On Representative Social Capital

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ABSTRACT

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This paper analyzes data for a random sample drawn from the Dutch population who reveal their propensity to invest and reward investments in building up social capital by means of an economic experiment. We find substantial heterogeneity in the propensity to invest and in the propensity to reward investments. In particular, we find strong evidence that the young, elderly, and low educated individuals invest relatively less, but are relatively more likely to reward investments in social capital. On the other hand, labor market participation, income, and religion do not have any significant impact on behavior in the experiment.

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1 Introduction

According to Bowles and Gintis (2002),

> Social capital refers to trust, concern for one's associates, a willingness to
> live by the norms of one's community and to punish those who do not.
> (p.1)

There is increasing empirical and theoretical evidence suggesting that a nation's level of social capital can influence its economic performance. The transactions cost paradigm remains the traditional way of thinking about the mechanism by which social capital affects economic performance. When societal capital is high, transactions costs are low which makes organizations and governments more efficient which ultimately leads to better economic growth (see Zak and Knack, 2001), Francois and Zabojnik, 2002) and improved organizational efficiency (La Porta, de Silanes, Shleifer, and Vishny, 1997).

Because of these important macroeconomic relationships, it becomes relevant for policy makers to investigate how levels of social capital are shifted as a result of changes in their characteristics. Changes in age and earnings distribution are two frequently cited structural changes western economies will have to deal with in the future (Gruber and Wise, 2001; Gottschalk and Smeeding, 1997). To perform these measurements, it is essential to measure with little error contributions of individuals to building up and sustaining social capital, and to perform these measurements on a sample of individuals randomly drawn from a country's population. The motivation for the first requirement follows from the law of large numbers, where average sample measures are consistent estimates of their population counterparts. The second requirement follows from the fact that the estimated parameters we make inferences on will, in general, be biased if contributions building up social capital are measured with error (see Bound, Brown, and Mathiowetz, 2001).

Survey questionnaires (Alesina and La Ferrara, 2002, Glaeser, Laibson, and Sacerdote, 2002) and laboratory experiments (e.g. Berg, Dickhaut and McCabe, 1995) have been the two main empirical methods employed so far to analyze the determinants of social capital or elements of social capital at the micro level. None of

these methods can provide researchers with both representative samples and accurate measures of the underlying behavior of interest. The main advantage of surveys is that it allows to make population inferences by observing the behavior of a randomly drawn sample of individuals from that population. The main drawback is that researchers run the risk of collecting answers to a vague and hypothetical question which can create a discrepancy between someone's answers and his actual behavior. Such discrepancies can in part be attributed to differences in the interpretation of the question across survey participants. Discrepancies may also arise because individuals do not answer truthfully the question. Laboratory experiments have the virtue of countering the effects associated with survey data by observing the behavior of individuals placed in a context in which they have incentives for making decisions revealing their true preferences. However, experiments suffer the drawback that subjects are generally drawn from homogenous pools of university students lacking the required variation in background characteristics to measure precisely their influence on observed behavior.

In this paper, we combine the strengths of survey and experimental methods by having a large representative sample of the Dutch population play a computerized version of the investment game of Berg, Dickhaut and McCabe (1995). The power of our approach lies in the capacity to approximate the environment of a traditional laboratory experiment while overcoming the need of bringing individuals in a lab. As will be discussed later, it has the methodological advantage over lab experiments of allowing to test for possible bias caused by using subjects selecting themselves in the experiment based on observable of unobservable characteristics which can be correlated with the decisions in the game, a topic on which very little is known.[1] Our choice of the investment game is particularly well suited for the analysis at hand as elements of trust, altruism, and reciprocity used by Bowles and Gintis to define social capital have also shown to characterize the decision of players in this game. Contrary to other work (e.g. Cox, 2004), our focus is on evaluating how social investments and returns to investments behavior vary across an heterogeneous population rather than on uncovering whether differences in behavior are the result

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[1]

of trust, trustworthiness or altruism. By doing so, we focus directly on determining the subgroups of the Dutch population who are more likely to contribute to social capital.

Our results are supportive of substantial heterogeneity in investment and returns to investment behavior in the Dutch population. One of the important findings of our paper is that holding everything else constant, the young and the elderly invest relatively less *but* reward investments relatively more than middle aged individuals. We suggest an explanation of this unexpected effect which relies on comparing responders expectations to the possible realizations. More specifically, an event might provoke disappointment or surprise depending on what the individual expected ex-ante. If emotions of surprise following unexpected generous offers trigger positive responses, than it is expected that the reward of an individual will be higher the lower are his expectations. In the case of our age profile, we show that the relatively lower investment levels of the young and elderly are associated with relatively *lower* expectations about receiving investments.

An important aspect of social capital highlighted in the citation of Bowles and Gintis above is the role played by social norms in determining socially conscious behavior. Gauging from the growing literature on the role of social norms in generating efficient social outcomes (e.g. Ostrom, 2000), we have good reasons to believe that they are likely to play a role in determining the decisions in our experiment. Controlling for their effect will be particularly important if other determinants of investments are correlated, hence empirically confounded with social norms. Estimation of the effect of norms on economic behavior is complicated by severe identification problems which occur when an individual's perceived expected behavior of others is inferred from the sample choice data (see e.g. Brock and Durlauf (2001) for an overview of the identification problems). We tackle these problems by eliciting for relevant subjects their subjective expectations of the average behavior of other individuals in the same role and find that social norms do play an important role in explaining investment behavior in our experiment. Interestingly, we show that not controlling for beliefs in our experiment leads to wrong inferences on the investment behavior of subjects with respect to their gender and level of education. Related work include Ortmann, Fitzgerald, and Boeing (2000) elicited subjective ex-

pectations of proposers in the investment game as a means to enforce comprehension of the game, but they do not investigate how these beliefs affected the actions in the game.

Because social preferences like altruism, fairness, trust, and reciprocity are relevant concepts to explain many other forms of strategic interactions involving social dilemmas (see Camerer, 2003 for a general overview), we believe that our results are relevant to understand many other social dilemma games. This is particularly relevant for public good games, of which the present experiment can be seen as a special two player case where only one player makes socially efficient contributions but who may end up worse off if the other player in some sense "free rides" on his investment in the social good.

Our experiment also has the virtue of providing additional evidence on the parallelism between the lab and the field. This parallelism has generally been tested using newspaper experiments (e.g., see Bosch–Domènech, Montalvo, Nagel, and Satorra, 2002), and by comparing samples of students to specialized samples including professional traders (Haigh and List, 2004) or CEO's (Fehr and List, 2004), none of which are representative of a nation's population. Three noteworthy experiments have recently been run with representative samples. Harrison, Lau, and Williams (2002) use a random sample of the Danish population to investigate the heterogeneity in individual discount rates. Hey (2002) used the CentERpanel of Tilburg University (more on this panel later on) to have a random sample of the Dutch population play an experiment on decision making under risk and uncertainty. Fehr, Fischbacher, Rosenbladt, Schupp, and Wagner (2003) present results from an interview based experiment with a social dilemma structure using the German Socio–Economic Panel.

The remainder of the paper is organized as follows. Section 2 describes the design of the experiment, the experimental procedure, and our sample. Section 3 presents our experimental results investment and returns to investment behavior in the Dutch population. Section 3.3 presents empirical evidence suggesting that the age patterns of the previous section can be rationalized by a surprise effect. Section 4 concludes.

2 The Experimental Design and the Sample

The recruitment of our subjects 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 a telepanel: the CentERpanel (hereafter CP), consisting of approximately 2000 representative Dutch households. Every Friday, CP's household members receive a questionnaire which they are asked to fill in at any time between Friday and Tuesday of the following week. This questionnaire is filled at home either on a computer or on a television set which is connected to a set–up box linking the household to the CentERdata server. In order to keep the sample representative of the Dutch population, low income households without a computer or a television set are given the necessary equipment in order to complete the weekly questionnaire.[2]

There are many reasons why the CP is an attractive medium to conduct experiments. First, it gives us access to a representative sample of a population, which is one of the key features of our study. Second, because participants answer questions on a computer or a television set, we are able to replicate as closely as possible the environment of a laboratory experiment, which simplifies comparisons of our results with those of the existing literature. Third, because participants communicate with 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. Finally, as CentERdata reimburses the weekly telephone costs for answering the questionnaire by crediting CentERpoints (1 CentERpoint = 0.01 Euro) to their private bank accounts four times a year, our participants are already familiar to payment in fictitious currency. This allows us to use CentERpoints as the experimental currency unit and reimburse our participants in a very convenient way.

Our design closely follows the investment game proposed by Berg, Dickhaut

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[2]For a description of the recruitment, sampling methods, and past usages of the CentERpanel see: www.centerdata.nl . Children below 16 years of age as well as immigrants are excluded from the panel, the latter group for the reason that their language proficiency in Dutch makes it difficult for them to answer the questions on a weekly basis.

and McCabe (1995). One attractive aspect of using their framework is that this allows us to contrast our results with existing student-based studies. Moreover, this design has proven to be robust to several framing effects (Ortmann, Fitzgerald, and Boeing, 2000) and role reversals (Burks, Carpenter, and Verhoogen, 2003). A sender and a responder were both endowed with 500 CentERpoints.[3] The sender could send money to the responder from his endowment. We discretized the choice set of the sender to 11 investment possibilities $I \in \{0, 50, ..., 450, 500\}$. The amount the sender sent was doubled by the experimenters and added to the endowment of the responder. Responders made their choices 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. The response which corresponded to the actual decision of the sender was chosen to be the effective action and determined the payoff of both participants. After all participants made their decisions, senders and responders were randomly matched and payoffs were computed based on the decisions of the pair. The final payoffs were computed as follows: a sender received the initial 500 CentERpoints reduced by the amount invested I plus the amount received from the responder, while the responder received her initial endowment of 500 CentERpoints, the amount sent by the sender multiplied by 2 minus the amount returned to the sender.[4]

The strategy method, dating back to Selten (1967) was chosen to overcome the difficulty of having CP members interact in real time. This method has several additional advantages. First, it facilitates data acquisition as the complete strategy plan for all 11 possible amounts received is elicited. Second, 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

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[3]For ease of reading we keep the terms "sender" and "responder" for the different roles. In the experiment we omitted suggestive labels and referred to the person itself or to its opponent as "the matched panel member." Computer screens of the original experiment (in Dutch) are available upon request. The translated text of all screens is enclosed in appendix A.

[4]The multiplier represents efficiency gains of the social interaction. Multipliers vary across studies. Berg, Dickhaut, and McCabe for instance use a multiplier of three, whereas Glaeser, Laibson, Scheinkman and Soutter (2000) apply a multiplier of two.

data from uninformed subjects.[5]

Under the assumption that both players maximize their monetary payoffs, the Nash equilibrium of the game is for the sender to send nothing to the responder, as the responder's dominant strategy is to return nothing to the sender. Hence, observing positive amounts sent is interpreted as evidence that people invest in others. Likewise, observing amounts returned is taken as evidence of reciprocity and altruistic behavior. It is important to stress that repeated game effects, retaliation strategies, and game experience effects are deliberately excluded by our experimental design. Thus, one can think of the current design as measuring the basic investment propensity of an individual at a given point in time.[6]

We elicit player's beliefs with a series of questions, all of which were asked after players made their decisions. This was done in order to circumvent the possibility that belief elicitation induces non–cooperative behavior when asked before the play of the game. Senders were first asked to state how much they think their responder will return to them. They were then asked to state their subjective expectation about what the average sender sends. Responders on the other hand simply had to state how much they thought of receiving from senders. Subjects were not rewarded based on the accuracy of their expectations.[7] This concluded the experimental part of the session.

All players were then asked to answer two survey questions. The first question asked players to state their average experience with trust

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[5]There is weak evidence suggesting that a hot environment triggers stronger responses in two player games. 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.

[6]Results from a multiple period investment experiments can be found in Willinger, Keser, Lohmann, and Usunier (2003).

[7]There are theoretical grounds suggesting that, assuming subjects are risk neutral and do not distort probabilities, beliefs elicited using the quadratic scoring rule should be more accurate than unpaid elicited beliefs. The empirical evidence seems not very supportive of this. Ortmann, Fitzgerald, and Boeing (2000) find that unpaid senders in their trust game had surprisingly accurate estimates of the average amounts sent by other senders while both Friedman and Massaro (1998), and Sonnemans and Offerman (2001) find insignificant differences between elicited beliefs of paid and unpaid subjects.

Lifetime trust experience question In the past, when you trusted someone, was your trust usually rewarded or usually exploited?

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

This question will be used to test for the presence of state dependent behavior whereby differences in past experiences with trust may lead to different investment behavior. The second question was the WVS trust question

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.

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 CentER bank 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 matched to one of the roles. The role was revealed to panel members once they had agreed to participate. We contacted 541 panel members from which 42 declined to participate.[8] Of the

[8]Compared to other experiments, we are in the unique position to observe the characteristics of those who deliberately decide not to participate in the experiment. In an extended version of this paper (Bellemare and Kröger, 2003), we tested for the presence of participation bias in our experiment. Participation bias is present if the behavior of participants in the experiment is not representative of the average behavior in the population, presumably because participants have a predisposition to gamble or lower risk aversion, to name only two possible explanations for this effect. Applying the approach by Heckman (1978) we re-estimated all models presented in this paper controlling for sample selection. We found no evidence of participation bias in any decision. Estimation results are available at the authors upon request.

499 panel members who completed the experiment, 276 were senders and 223 were responders.[9]

Table 1 gives the description of the variables and descriptive statistics of the 541 household members contacted for senders, receivers and non–participants. 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.

3   Experimental Results

The distribution of amounts sent in the experiment I is shown in figure 1. As we can see, this distribution is characterized by the fact that it is heavily skewed to the left with a mode at the equal split category (i.e. 250 CentERpoints), and the majority of subjects send positive amounts. Our first important result is that this distribution is familiar to that usually found in lab–experiments with student samples (e.g. Berg, Dickhaut, and McCabe, 1995, Ortmann, Fitzgerald, and Boeing, 2000).

We measure propensity to respond to investments along the lines suggested by Glaeser, Laibson, Scheinkman and Soutter (2000) using the return ratio, defined as the amount returned divided by the amount available to return. In our experiment, the available amount to return equals the amount received multiplied by two, plus the experimental endowment of 500 CentERpoints. Because we use the strategy method, responders were asked to state how much they will give back for each of

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[9]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. As all other participants, those 53 responders received payments resulting from only one (the first) matching.

the 11 possible amounts they can receive from the sender. This implies that we observe a sequence $\{r_a \in [0,1] \,|\, a \in \{0,50,...,500\}\}$ for each responder, where r_a denotes the return ratio when receiving a CentERpoints from the sender. 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 increases with a. Figure 2 presents two curves of the return ratio. The curve with empty squares presents the return ratio computed by taking the median return for each possible outcome sent for all 223 responders. The two important features of this figure are that the ratio 1) monotonically increases and is concave in the amounts received, 2) a significant fraction of the responders return nothing (especially in low categories) while practically no responder returns the entire possible amount. This differs substantially from existing results on the investment game (e.g. Berg, Dickhaut, McCabe, 1995; Cox, 2004) where it was found that no relation existed between amounts sent and amounts returned. It could be argued that the monotonicity observed is a consequence of using the strategy method which forced respondents to sequentially enter their choices for outcomes in increasing order of the amounts sent. To check this possibility, we also computed for each outcome the return ratio using only the decisions associated with responders who believed that this outcome would materialize in the play of the game. Hence, increases in return rations across two consecutive outcomes in the later case cannot be the result of having the same individuals reporting monotonically increasing return ratios. Results are graphed along the previous curve in Figure 2. We find that the shape of both curves are surprisingly similar across all outcomes,[10] reconfirming that rewards to investment do increase monotonically with the amounts invested.

Would it pay to invest ? Figure 3 presents density estimates of senders returns of investments, computed as $(r_I - I)/I$ for all investment possibilities I. Each line represents the estimated distribution of returns for a given number of CentERpoints sent. If responders return to senders exactly what they sent, the return on investment is 0. If responders do not return anything to the senders, the return on investment is -1. Apart from the distribution of returns when 50 CentERpoints are

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[10]Outcomes associated with amounts sent of 400 and 450 CentERpoints were believed possible by only 3 and 0 responders respectively, and were not added to the graph.

sent, all distributions have roughly the same shape. The common finding in laboratory investment experiments is that investment barely pays, as responders return to senders what they have sent (Camerer, 2003). Even though, we find that investing pays although efficiency gains are smaller, these gains are only on a small scale. Again, our results reconfirm the findings based on student populations. We find that the median return on investment is slightly above 0 for every amount sent. Furthermore, the probability of getting nothing back from a receiver (return on investment of 0) is not zero. Thus, it seems the aggregate behavior of our heterogeneous sample parallels to some extent that based on student samples. We next investigate how the heterogeneous characteristics of our subjects explains differences in their individual behavior.

3.1 Empirical Results on Investment Behavior

We begin our empirical analysis by first focusing on the amounts invested I, an ordinal and discrete variable. A standard model which maps an individuals unobserved (latent) investment propensity I^* into our observed experimental outcome is the ordered probit model (e.g. Maddala, 1983):

$$I_i^* = x_i'\beta + \varepsilon_i \tag{1}$$

$$I_i = j \quad \text{if} \quad m_{j-1} < I_i^* \leq m_j, \quad j = 0, \ldots, K \tag{2}$$

$$\varepsilon_i | x_i \sim N(0, 1). \tag{3}$$

The index i denotes the individual, x_i is a vector of explanatory variables including a constant term, β is the vector of parameters of interest, and ε_i is the error term. We make the usual identifying normalizations $m_{-1} = -\infty$, $m_0 = 0$, and $m_{10} = \infty$. The bounds $m_0, \ldots, m_9$ can be seen as nuisance parameters. The standard way to estimate this model is maximum likelihood (ML). However, the ordered probit model requires a sufficient amount of observations for each outcome to estimate the threshold parameters. As can be seen from figure 1, categories 300 to 450 CentERpoints have very little observations. In our empirical application, we merge these outcomes and estimate an ordered probit model with $K = 8$ outcomes.

The first specification of table 2 presents estimates of the ordered probit model using a regressors a standard set of background characteristics, reported life experience with trust (TRUSTEXP), subjects' beliefs about the amount they expect to be returned to them (STHINK), and the average amount they expect other senders will send (SMEANS).[11] Contrary to the earlier findings based on survey trust questions (Alesina and La Ferrara, 2002), we do not find that gender of subjects to influence their levels of investments in others. We also do not find any effect of the family size (HSIZE), whether an individual is retired from the labor force (RETIRED) or whether an individual works.

Both the linear and quadratic term in age are significant and indicate that, all else constant, the propensity to invest increases until the age of 37, beyond which it starts to decline. This reconfirms the inverted–U shape pattern usually found in the social capital literature (e.g. Putnam, 2000, although those studies report that social capital reaches a high at 45 years of age).

Education also has an inverted–U profile. We find that individuals with secondary and technical training are more likely to make higher invests than subjects with either low education levels (the omitted category) and subjects with university degrees. We classified subjects either as protestants, catholics, or atheists. We find no evidence that either catholics or protestants invest differently than atheists (the omitted category).

Both belief variables, STHINK and SMEANS, have positive effects on investment behavior and are highly significant. These results indicate that senders who expected to receive more sent more, and senders who thought senders on average would send more increased their amount sent. The latter result corroborates the presence of social norms geared at increasing efficiency and social cooperation. In order to asses the contribution of beliefs to the empirical model, we computed a likelihood–ratio test comparing specification 1 and 3. The estimation results of the specification omitting beliefs change in two noteworthy ways: gender has a significantly positive impact and the education pattern is not as clearly pronounced. The test value of 227.28 (5% χ^2 critical value of 5.99) indicates that apart from being

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[11]We have experimented with a specification including cross–terms but none was found to be statistically significant.

statistically significant, beliefs substantially improve the predictive fit of the model.

Contrary to our expectation, previous experiences when having trusted others in the past (TRUSTEXP) do not affect investments in our experiment, conditional of their background characteristics. This contrasts with the predictions of the indirect evolutionary approach to adaptation through experience literature (for a recent survey see Ostrom, 2000) which suggests that someone's trust behavior is directly related to his past experiences. One possible explanation for this is that part of our investments are motivated by altruistic preferences which are stationary, rather than by trust behavior, the later which in ultimately a matter of beliefs about responses of others which can be updated over time as the result of past experiences.

A final surprising result is that individuals answering "Most people can be trusted." to the WVS trust question are found to have significantly higher investment behavior in others. This is in sharp contrast with results of Glaeser, Laibson, Scheinkman and Soutter (2000) who did not find a significant effect.

Finally, it is possible that including the survey measure in our regression causes multicollinearity amongst the regressors, which could have an ill-effect on our inferences if both investment behavior, which is partly determined by trust behavior, and answers to survey trust questions are correlated and determined by the same background characteristics. In order to check for this possibility, we estimated our model excluding this variable. Specification 2 in table present the results. We see that all parameter estimates of the model retain their sign and significance when survey trust answers are excluded from the model, suggesting that multicollinearity is not a problem.

3.2 Empirical Results on Reward to Investment

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

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\begin{aligned}
r_{ai}^* &= z_i'\eta + \gamma_1 a + \gamma_2 a^2 + \epsilon_i & (4) \\
r_{ai} &= r_{ai}^* \quad \text{if } r_{ai}^* > 0 & (5) \\
&= 0 \quad\;\; \text{if } r_{ai}^* \le 0 & (6) \\
&\quad \epsilon_i | z_i \sim N(0, \sigma^2) & (7)
\end{aligned}
$$

where equation (4) describes an individual's latent propensity to reward investments, and equations (5) and (6) describe the censoring rule which allows responders with extremely low propensities to return nothing with positive probability. In a similar way to the investment propensity (1), the return propensity is modelled as a function of background characteristics z_i and an unobservable component ϵ_i. The quadratic form in a is added to capture the monotone increasing shape of amounts returned observed in the data.[12]

The estimation results are presented in table 3.[13] The first specification includes standard background characteristics of the responder, reported trust experience, their beliefs about what they expect to receive from the sender (RTHINK),[14] and responders' answers to the WVS trust question. The second specification extends the first specification by adding interaction terms between age and trust experience, answers to the WVS trust question, as well as responders' beliefs about the expected amount sent to them. We compare the two specifications using a log–likelihood ratio

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

[13]It is well known (e.g. Goldberger, 1983) that the Tobit model is sensitive to the normality and homoscedasticity assumptions of equation (7). We tested these assumptions using a specification test proposed by Newey (1987) which is based on the comparison of parameter estimates of the Tobit model and those of the Symmetrically Trimmed Least Squares estimator (STLS) of Powell (1986), the later which relaxes both the normality and homoscedasticity assumptions of the Tobit model. The test values (3.105 (χ_{17}^2, p=0.999) for specification 1 and 15.656 (χ_{29}^2, p=0.738) for specification 2) never reject the null hypothesis that the Tobit model is well specified. All results based on the STLS estimator are available from the authors.

[14]RTHINK is coded from 0 to 10, where each unit is worth 50 CentERpoints.

test. The extended specification which includes interaction terms is clearly preferred to the first specification.[15] Accordingly, our analysis below will focus on the results of the extended specification.

As could be seen from the raw data in figure 2, amounts returned monotonically increase and are concave in *a*, the amounts received. This is also reflected in the Tobit estimates, where the first order term γ_1 is positive and the second order term γ_2 is negative, both significant at the 1% level. The aging component of rewards to investments is captured by the parameters of retirement status (RETIRED), age (AGE), and the three interaction terms (AGESQ, AGE $\times$ WVS, AGE $\times$ RTHINK). Both the linear and quadratic terms of age are significant, indicating a non-linear relationship between the propensity to reward investments and age. We find that this propensity reaches its lowest level when individuals reach the age of 34 years, and increases beyond that.[16] The shape of this relationship is very different than that of investment behavior. There, we found that the propensity to invest increased until the age of 34 and decreases beyond that. We next evaluated the age turning points for those who report trusting others (WVS=1) and those who do not (WVS=0). The age profile of individuals who state they do not trust others reaches a low at 21 years of age, while it reaches a low at 43 years of age for those who declare trusting others. Section 3.3 discusses further this result and offers a potential explanation.

Previously, we found that the relationship between investments and education was inverted U shape, with subjects without a secondary degree and those with university degrees investing significantly less than low educated individuals. The relation between education and the propensity to reward investments is very different. Less educated subjects (the omitted category) return significantly more than educated subjects, all degrees confounded. Moreover, the parameter estimates suggest an U shape relationship, with individuals with university degrees rewarding more that subjects with technical education degrees. The effect of gender also distinguishes investment behavior from reward behavior. While gender was found to

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[15]The log–likelihood ratio test value is 19.9, significant at the 1% level.

[16]Because of the interaction terms, computation of these turning points requires that we fix the values of WVS and RTHINK. In order to get an overall picture we evaluate those variables at their sample means.

have no impact on investments, we find here that men return on average significantly less than women.

Like for the case of investment behavior, beliefs of responders play an important role in determining reward behavior. Responders who believed they would receive more had higher average return ratios. To gain some insights on the importance of beliefs on reward behavior, we estimated our extended specification omitting beliefs.[17] A log–likelihood ratio test (value of 85.88, significant at the 1% level) confirms that omitting beliefs substantially lowers the predictive fit of the model. As we show in the next section, these beliefs play an important role to interpret the age patterns of our experiment.

One of the interesting findings of Glaeser, Laibson, Scheinkman and Soutter (2000) was that answers to the WVS trust question did not correlate with responders behavior in the investment game but correlated rather well with the return ratio. We find that this effect is reconfirmed in our data.

We end by noting that some individual characteristics have no effect on their propensity to reward investments. This is the case of subjects' income, whether they work or not, their retirement status, religion, and their lifetime trust experience. Interestingly, none of these characteristics were found to explain investment behavior.

3.3 Discussion

In the literature on social capital, trust, trustworthiness, and altruism go hand in hand, suggesting that individuals investing more in others should be expected to be those relatively more likely to reward investments. Thus, contrary to the results of the previous section, we should not expect that the young and elderly invest relatively less but are reward investments relatively more than middle aged individuals. In this section, we show that one potential explanation of this paradox is that the young and elderly, apart from making relatively lower investments, also have

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[17]The only notable change is that the reward behavior of those who report trusting others continues to decline with age while it no longer declines for those who report not trusting others. Estimation results are available upon request.

the relatively lowest expectations of the average amounts that would be invested in them by proposers in the game. Hence, any amounts invested in them is more likely to exceed their expectations and be perceived are generous investments, triggering a relatively stronger emotional response which translates into relatively higher premiums on rewarding investments. For this explanation to be consistent with the investment-age and the reward-age relationships found in our data, it must be that the young and elderly have relatively lower subjective expectations of the levels invested in them as compared to middle-age individuals.

In order to test this hypothesis, we model the relationship between beliefs and age using the following semiparametric regression model (Robinson, 1988)

$$RTHINK_i = w_i'\theta + g(AGE_i) + \zeta_i \tag{8}$$

where $RTHINK_i$ represents responders' beliefs about the amount to be invested in them by proposers, w_i is a vector of observable characteristics including gender, education, religion, the work decision, $g(\cdot)$ is an unknown function, ζ_i is an error term assumed to have mean 0, and θ is a vector of unknown parameters. The main advantage of this semiparametric model is that the relationship between beliefs and age does not need to be specified a priori, but is rather determined by the data. The top panel of figure 4 presents graphs of the investment-age and the reward-age relationships based on results in tables 2 and 3. The bottom graph of figure 4 presents the nonparametric estimate of the function $g(\cdot)$ along with the 95% (pointwise) confidence bounds.[18] The relation between beliefs and age supports our conjecture: it is roughly concave, with beliefs progressively rising until the age of 40 and progressively declining after. A striking feature of Figure 4 is that the turning points of the investment, reward, and beliefs schedules are relatively well aligned around the 35-40 years range. This symmetry is clear evidence that age groups with relatively lower propensity to invest are those with both the relatively lowest expectations about the levels invested in them and the relatively strongest propensity to reward investments.

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[18]Results were found to be robust to the choice of conditioning variables. Parameter estimates are available upon request.

4 Conclusions

This study presented results from a novel computerized experiment combining the strengths of experiments and survey data collection methods which allowed to collect data of investment behavior and reward to investment behavior for a random sample of the Dutch population. Our experiment adds to the scarce body of literature which has made attempts to lift experimental economics out of its more traditional laboratory context which relies on homogeneous student subjects pools to broad and representative samples of a population. The key advantage of this approach is the ability to analyze revealed preference data for a random sample of the Dutch population.

We found strong evidence supporting substantial heterogeneity in propensities to invest and to reward investments which build up social capital. In particular, education level of subjects was shown to lead to opposing investment and reward behavior. Our results also pointed to a rather paradoxical finding occurring when comparing investment and reward behavior across age groups– we found that the young and elderly have both a relatively lower propensity to invest and a relatively higher propensity to reward investments than middle-aged individuals. Interestingly, we showed that these inefficiencies result from subjects lowering both their propensity to invest in others and their subjective expectations about receiving investments from others, increasing the odds that investments received exceed their expectations and induce a stronger propensity to reward investments. Despite that this conjecture is supported by the data, we do not rule out that other possible mechanism can explain our paradox. In particular, it has been argued elsewhere (Dufwenberg and Gneezy, 2000) that not living up to someone's expectation of behavior might also play a role in determining behavior in environments involving trust. Investigation of this hypothesis in the context of our investment game requires the elicitation of second order beliefs which is task require much higher cognitive skills from respondents, and can be problematic when subjects have very heterogeneous backgrounds and capacities. We leave this interesting exploration as an avenue for future research.

Because the incentive structure of our game resembles that of other social dilemma

games in experimental economics, some of our results may well be present in these alternative settings. Contributions to public goods have raised considerable interest over the last decades. How these contributions evolve in an aging world is an important yet still unanswered question. We feel that much can be learned by extending our experimental approach to address these issues. However, running similar large scale experiments for social dilemma games, or any other game, may be a source of discouragement if it is felt that research budgets could be judiciously allocated to other projects. We feel that the insights which can be gained in other settings are well worth the effort, and that the appreciation of experiments within economics as a whole can benefit from such an approach. We hope to have demonstrated that the new insights on population behavior presented in this paper make this a line of research worthwhile pursuing in the years to come.

A   Instructions (Translation)

The first 3 screens of the experiment are the same for both senders and responders. 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 Catholic University of Brabant.[19]

  With this experiment you can make **real money** in terms of CentERpoints. You receive from the researchers **additional** CentERpoints (besides 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 to 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

___
[19]Now: Tilburg University. The Catholic University of Brabant changed its name after the experiment.

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

Senders

- *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 amount you sent multiplied by 2. 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.)

  ◯ **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:*

  (was 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 of their 500 CentERpoints

which is doubled and received by a person of the other position.

How many points do you think those panel members have sent?

(The sender could indicate one out of 11 possible amounts from 0 to 500).

Responders

- *Fourth screen:*

  You have been matched with another member of the panel. Like you, this person received 500 CentERpoints. This person is asked to send you a share from their own 500 CentERpoints. You will receive the amount of those points the other person has sent multiplied by 2.

  For example, if the other person sends 100 CentERpoints, you will receive 200 CentERpoints. Together with the 500 points you begin with, you will 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. The amount you receive is written in the next column. 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 11 possible amounts the sender could send what he would return. 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 **expect** 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.

	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 Monthly Income (Guilders)
GR_INCH	4175.59	7526.39	4342.07	4313.76	4302.52	6593.33	Gross Household Monthly 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	0.533	0.500	0.448	0.498	–	–	Dummy, 1 if Yes to WVS trust question
TRUSTEXP	3.16	1.18	3.45	1.31	–	–	1-7, 1 Always rewarded 7, Always disappointed
Nobs	276		223		42		

Table 1: Descriptive statistics

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Specification	(1) Coef.	Std.	(2) Coef.	Std.	(3) Coef.	Std.
CONSTANT	−1.828***	0.687	−2.018**	0.736	−0.314	0.681
GENDER	0.217	0.160	0.182	0.133	0.229*	0.134
AGE	0.060***	0.031	0.067**	0.031	0.052*	0.029
AGE_SQ/1000	−0.803**	0.349	−0.897**	0.367	−0.662**	0.335
RETIRED	−0.251	0.287	−0.193	0.374	0.271	0.293
SECONDEG	0.845***	0.318	0.908***	0.396	0.626*	0.308
TRAINDEG	0.761**	0.332	0.809**	0.404	0.589*	0.312
UNIVDEG	0.678	0.395	0.729	0.457	0.716*	0.361
WORK	−0.189	0.208	−0.219	0.193	−0.189	0.208
LN(GR_INCP)	0.019	0.039	0.024	0.026	0.007	0.028
NUMCHILD	−0.129	0.099	−0.118*	0.064	−0.043	0.066
CATHOLIC	0.149	0.171	0.101	0.170	0.002	0.165
PROTEST	0.042	0.208	−0.016	0.164	−0.109	0.149
TRUSTEXP	−0.032	0.082	0.045	0.059	0.090	0.065
STHINK	0.003***	0.000	0.003***	0.0001		
SMEANS	0.433***	0.044	0.423***	0.061		
WVS	0.362**	0.169				
Log-Likelihood	−403.58		−406.42		−520.06	

Table 2: Sender results. Specifications (1) to (3) refer to the ordered probit model. The significance of parameters is based on robust standard errors and bootstrap empirical quantiles of the t-statistics (1000 repetitions), N=276.

Table 3: Responder results – Tobit estimator. N = 2453.

	(1)		(2)		(3)	
	Coef.	Std.	Coef.	Std.	Coef.	Std.
CONSTANT	−0.076***	0.035	−0.042	0.032	−0.056	0.035
γ_1	0.087***	0.004	0.087***	0.004	0.088***	0.004
γ_2	−0.0045***	0.0003	−0.005***	0.0003	−0.005***	0.0003
GENDER	−0.017***	0.0068	−0.018***	0.007	−0.008	0.007
AGE	−0.0014	0.0014	−0.002*	0.001	0.0003	0.001
AGESQ/1000	0.032*	0.017	0.033***	0.017	0.004	0.017
RETIRED	0.0052	0.014	−0.004	0.015	0.017	0.015
SECONDEG	−0.054***	0.012	−0.053***	0.012	−0.052***	0.012
TRAINDEG	−0.067***	0.012	−0.065***	0.012	−0.062***	0.012
UNIVDEG	−0.058***	0.015	−0.053***	0.015	−0.058***	0.015
WORK	0.019**	0.0089	0.016*	0.009	0.013	0.009
LN(GR_INCP)	0.0001	0.0011	0.0006	0.0011	0.0002	0.001
CATHOLIC	−0.0062	0.0073	−0.009	0.007	−0.002	0.007
PROTEST	0.0051	0.0076	0.006	0.008	−0.0005	0.008
TRUSTEXP	−0.017	0.009	−0.003	0.003	−0.011	0.009
RTHINK	0.0255***	0.0044	0.013***	0.001		
WVS	0.1035***	0.022	0.045***	0.007	0.149***	0.021
AGE×TRUSTEXP	0.0003	0.0002			0.0003	0.0002
AGE×WVS	−0.0014***	0.0005			−0.002***	0.0005
AGE×RTHINK	−0.0003***	0.0000				
σ^2	0.0207***	0.0006	0.144***	0.002	0.021***	0.0006
Log-Likelihood	946.47		936.52		903.53	

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

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B   Figures

Figure 1: Distribution of amounts invested
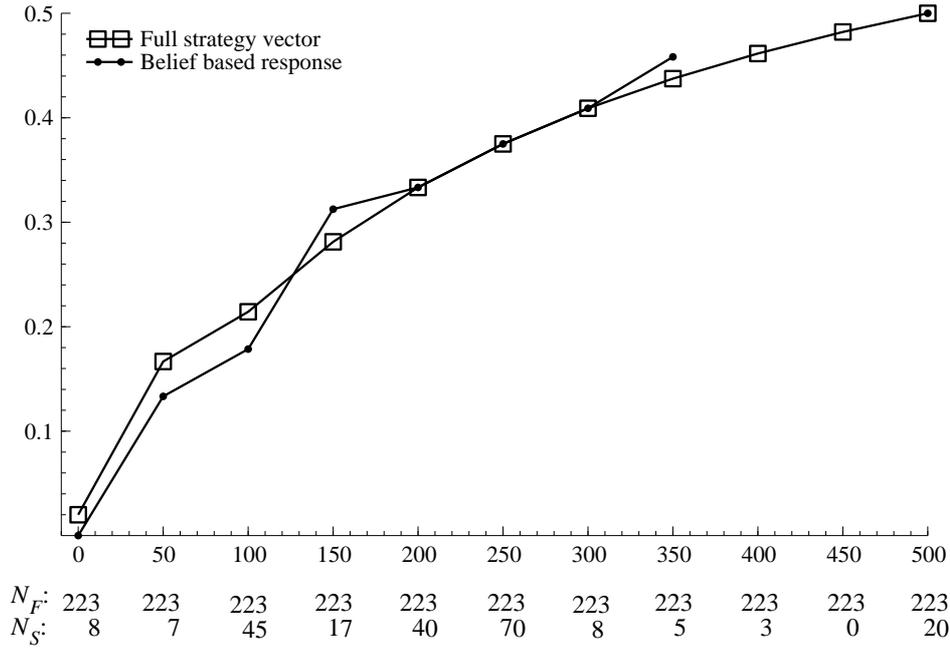
Figure 2: Curve with squares represents the return ratio of responders using the full strategy vector. The curve with full circles represents the return ratio computed using subjects response only for that outcome they expected to occur during the play of the game. N_F denotes the number of subjects used to compute the return ratio for each outcome based on the full strategy vector, N_S denotes the number of subjects used to compute the return ration for each outcome based on expected responses.
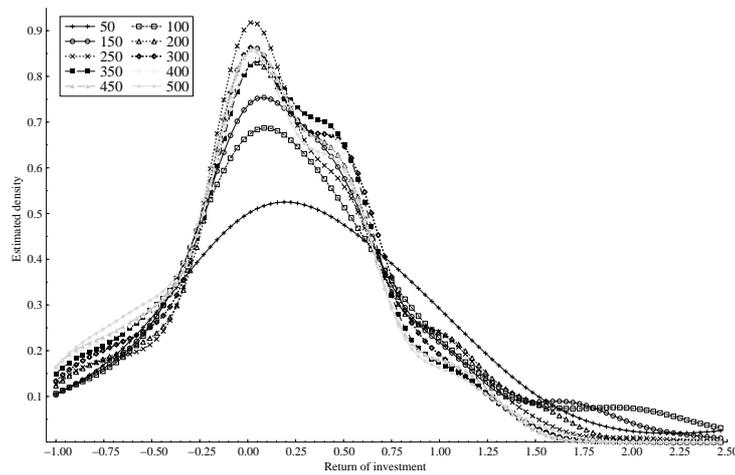
Figure 3: Estimated density of potential returns on investments for each amount sent. Gaussian kernel density estimation. Rate of return computed as $(r_I − I)/I$ for each possible investment I. The rate of return is infinity when the amount sent is zero and is not plotted here.
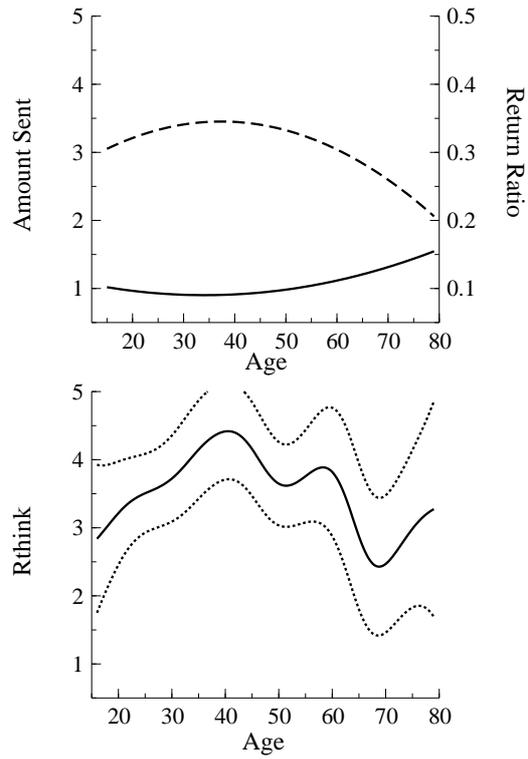
Figure 4: Top panel: Relationship between investment behavior and age (dashed line/left axis), and reward behavior and age (full line-right axis). Bottom panel: Nonparametric relation between responder's expected amounts invested and age (partial linear model). Gaussian kernel used, and bandwidth chosen by cross-validation.

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