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IS A FRIEND OF MY FRIEND MY FRIEND? VARIATION OF ALTRUISM WITHIN THE SOCIAL CHAIN

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Variation of Altruism within the Social Chain

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Abstract

Experimental dictator games have been used numerous times to measure altruism of individuals. The purpose of our paper is to investigate the effect of social distance on altruism. We measure social distances in an international student community in Latvia, and then conduct a modified dictator game under double-blind anonymity. Total altruism is decomposed into baseline, or giving to strangers, and directed altruism, or additional giving directed towards friends. Furthermore, we introduce two "prices" of altruism and; hence, investigate generosity under two ratios of token values. We find evidence that social distance is negatively related to generosity and baseline altruism is positively related to directed altruism. Moreover, people increase their giving when it becomes relatively cheaper.

Keywords: altruism, generosity, social distance, social network, friendship, dictator game.

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1 Introduction

Numerous charity and non-profit organizations exist worldwide, and they constantly raise various resources. In 2000, people donated an estimated $200 billion to charities in the United States alone (Lindahl and Aaron, 2002), while in 2001 89% of Americans donated to charity (Sullivan, 2002). Altruism of individuals is considered to be one of the main reasons for such behavior. Dictionaries define altruism as "a way of thinking and behaving that shows you care about other people and their interests more than you care about yourself" (Macmillan English Dictionary, p. 40). However, this is directly opposite to the narrow interpretation of one of the fundamental economic assumptions of individuals being completely selfish, pure wealth maximizers[i].

This behavioral deviation from the economic fundamentals has been one of the most researched topics among behavioral economists for several decades now. Scholars of different disciplines have posed numerous possible explanations for the existence of altruism as such. The most frequently offered are utility gains and reciprocal incentives. Both explanations have supporters as well as critics and final conclusions are still yet to be made.

Undoubtedly different individuals have different altruistic preferences. However, our preferences also vary with the identity of the recipient in a given situation. We hypothesize that, ceteris paribus, people behave more altruistically towards their close friends than towards strangers. This hypothesis implies that altruistic preferences should be directly related to the social distance - the shortest path connecting two individuals within the social chain. This in turn implies that individuals tend to give more to their direct friends than to friends of their friends and so on. All other things held constant, social distance is one factor that determines relationship value, which

can also be expressed in terms of altruistic preferences. Hence we pose the first research question for this study: **"How does generosity of individuals depend on the social distance?"** We seek to determine if individuals differ in their generosity to people at various social distances, how much generosity varies if it does, and at which point in the social chain a person becomes equally generous towards people at increasing social distances. Moreover, we believe that the same level of generosity does not create the same value in different situations. Thus we raise the second research question: **"Do individuals adjust their generosity according to its price?"** We expect to see increase in giving when it becomes relatively cheaper.

In order to answer the research questions raised, we employ a modified dictator game, which is commonly used to measure altruism. In the simplest form of a dictator game, a subject has to decide how much, if any, of initial endowment to donate to another subject, usually in one-shot anonymous setting (Camerer, 2003). Studies by Branas-Garza, Cobo-Reyes, Paz Espinosa, and Jimenez (2006); Branas-Garza, Duran, and Paz Espinosa (2006); Leider, Mobius, Rosenblat, and Do (2007), to name a few, use modified dictator games to estimate the value of having friends in terms of payoff relevant decisions. The value is measured by the share of endowment the dictator decides to give away. The point of departure for this thesis is the study by Leider et al. (2007) who distinguish between the effects of baseline altruism, the general altruism of a person, and directed altruism, the additional altruism towards friends. This study aims to contribute to the understanding of altruism by investigating altruistic preferences of a student community. We employ a similar methodology to theirs, but in a different setting. Our research is done in an international student community in Latvia, to our best knowledge making it the first study of this kind in Eastern Europe or any former Soviet Union country. As shown

by Henrich et al. (2001), outcomes of dictator games differ greatly in different cultures and societies. Additionally, in our experimental design we rely on a real classroom setting, as compared to online setting in Leider et al. (2007). Finally, our experimental design allows us to control for several demographic covariates while still ensuring full anonymity. We believe that our findings not only contribute to a deeper understanding of human behavior, but also are valuable for creation of social government policies and fundraising strategies of various funds.

The rest of the paper is organized as follows. In the next section we discuss the previous research on the topic. Afterwards we present the methodology used both to investigate the friendship ties and measure the altruistic preferences. Sample choice and characteristics follow next. Then we provide data for the first methodological part. The next section describes the experimental design as well as its summary statistics. Finally, we present empirical findings and we discuss them. The last part concludes the paper.

2 Literature review

Behavioral game theory as a field of study has grown rapidly in the last two decades, with experimental games as the main building block. One of the most commonly used games is the ultimatum game, starting with Guth, Schmittberger, and Schwarze in 1982. In this game, one player is the “Proposer” and the other is “Responder”. The proposer is given some amount of money, and then makes a take-it-or-leave-it offer on how to divide this money with the other person. The responder then either accepts or rejects this offer. If the offer is rejected, both players get nothing. The dictator game then is just a modification of the ultimatum game, with the responder’s ability to reject the offer removed. In this way the effect of fear of rejection is removed, with only altruism left to explain the generous offers (Camerer, 2003).

A great deal of research has been done in social sciences assessing altruism using dictator games. The first attempt dates back to 1986 in Kahneman, Knetsch, and Thaler's work "Fairness as a constraint on profit seeking: entitlements in the market". As Henrich et al. (2004) claim "Over the past decade, research in experimental economics has emphatically falsified the textbook representation of Homo economicus, with hundreds of experiments that have suggested that people care not only about their own material payoffs but also about such things as fairness, equity, and reciprocity."

Camerer (2003) points out that during numerous replications of dictator games one trend stays the same. Usually approximately 60% of dictators pass positive amount of the endowment while the average mean passed often amounts to high 20% of the initial endowment. However, Henrich et al. (2001) conducted a study in 15 small-scale societies and have shown that results differ greatly in different cultures depending on payoff to cooperation and market integration. One of the most important examples that illustrate the existence of altruistic preferences is Hoffman, McCabe, and Smith (1996) who manipulate different levels of anonymity of the dictator in double-blind and single blind experiments. They find that given guaranteed anonymity, no strategic sharing incentives, and no reciprocal incentives, dictators still tend to share their endowments. These evidence let us form a hypothesis that **each person has some particular level of altruism, which in our study we shall call baseline altruism**, following Leider et al. (2007).

Eckel and Grossman (1996) provide evidence that identity of the dictator's partner matter. They find that on average people donate more to the known and respected American Red Cross than to anonymous counterpart. They conclude that individuals in fact make rational decisions by incorporating fairness. Similarly,

Bohnet and Frey (1999) show that payoffs increase substantially in dictator game when the recipient talks briefly about himself to the dictator before the experiment. Andreoni and Miller (2002) conclude that altruism can be captured by the economic theory and is characterized by a quasi-concave utility function. Their work is influential because they generalize altruistic preferences using six different utility functions, which allow generalizing every single individual's altruistic preferences. Jacobsson, Johannesson, and Borgquist (2007) investigate how, if at all, giving differs when during double-blind dictator games, recipients are asked to allocate money, nicotine patches or food tickets. They find that people are much more reluctant to share money than to make transfers of nicotine patches and food tickets; their results suggest that altruism is predominantly paternalistic.

In contrast to research on altruism in general, the field of assessing altruism within individuals related via friendship ties is still relatively new. Evidence of increased altruism towards non-anonymous counterparts is found by Branas-Garza, Cobo-Reyes, Paz Espinosa, and Jimenez (2006); Branas-Garza, Duran, and Paz Espinosa (2006); Leider et al. (2007). This result is a clear deviation from the predictions by the game theory (Gibbons, 1997). There have been numerous attempts to explain it. Most work on social ties tries to understand why experimental behavior of the subjects deviates from pure selfishness predicted by the simple economic theory. Social integration of the subject and reciprocal incentives show the strongest explanatory power for increased giving towards friends during dictator games (Branas-Garza et al., 2006). Berg, Dickhaut, and McCabe (1995) conduct the experiment as an investment game and investigate whether trust and reciprocity can explain behavioral deviation from pure selfishness. They also find evidence that

reciprocity can explain such deviations. However, in dictator games under double-blind anonymity reciprocal incentives are eliminated but giving still remains positive.

So far arguably the greatest contribution to understanding payoff-based decisions within social communities has been done by Leider et al. (2007). They investigate how worthy it is to have a friend instead of a complete stranger to make payoff relevant decisions. The authors study the community of Harvard undergraduate students. They add to the knowledge of altruism by decomposing it into baseline altruism – willingness to give to strangers - and directed altruism – additional giving to friends. The authors find significant evidence showing that enforced reciprocity is crucial in explaining the existence of directed altruism. Their most influential findings are that the baseline and directed altruism are correlated and that subjects with high baseline altruism have more altruistic friends. Having these evidence in mind, we form another hypothesis that **directed altruism is negatively related to the social distance and at some point in the social chain it approaches zero, with baseline altruism left to explain the generosity**.

The fundamental question in experimental economics as a whole is the external validity of laboratory findings, also called parallelism (Levitt & List, 2007). This issue has been addressed by a number of scholars. Arguably the most influential study has been carried out by Levitt and List (2007). They claim that behavior is related not only to preferences but also to a large extent to situations. Hence establishing control over predefined conditions of morality, scrutiny, context, self-selection of individuals in the experiment, and stakes of the game, the laboratory findings can be successfully generalized for the outside world.

3 Methodology

The methodology to be used in this paper consists of two parts. The first explains the method to find the social distances between agents in the community while the other introduces the model employed to investigate altruistic preferences among the members of this community.

3.1 Mapping friendship ties

We consider a community, which consists of N actors:

$$N = \{n_1, n_2, \ldots, n_n\}$$

The members of the community interact and create friendship ties between each other. Hence, a binary relationship value can be assigned for each pair of actors – 1 if the relationship, R, exists and 0 if not. This allows us to create an adjacency matrix with n_n rows and n_n columns and $(n_n^2 - n_n)$ binary values for relationships. If nodes n_1 and n_2 are adjacent, cell (1, 2), $x_{1,2}$, indicates an existing relationship, R = 1, otherwise R = 0. The rows in the matrix represent the ties an actor sends while the columns represent the ties an actor receives; further we shall call these ties out-degrees and in-degrees respectively (Faust & Wasserman, 1997).

The adjacency matrix can be either symmetric where $x_{1,2} = x_{2,1}$; or asymmetric if $x_{1,2}$ is not equal to $x_{2,1}$ (see Figure 1 below). If the adjacency matrix is symmetric, ties are not directed[ii].

Figure 1.

Example of symmetric and asymmetric matrices

(1)

	n_1	n_2	...	n_n
n_1		1		
n_2	1			
...				
n_n				

(2)

	n_1	n_2	...	n_n
n_1		0		
n_2	1			
...				
n_n				

Note. (1) illustrates a symmetric tie between n_1 and n_2. (2) illustrates a directed tie where n_2 considers n_1 a friend, while n_1 does not.

If a friendship adjacency matrix has symmetric data, it means that all ties are reciprocated. Hence, it is practically impossible and almost never exists. In most cases people do not reciprocate all the incoming ties due to various reasons - some people may have more friends than they were asked to name, some may have forgotten to name the person, some may not share the idea of being friends, to name a few reasons. This means that in order to investigate an OR-network for friendship ties, the adjacency matrix first of all has to be made symmetric – the upper part of the matrix has to be replicated in the lower part[iii].

The aim of performing analysis of the nodes and ties is to obtain social distances between all the individuals within a community. In social network theory social distances are proxied by geodesic distances, which are defined as the shortest path between a pair of nodes (Faust & Wasserman, 1997). However, as mentioned above, the adjacency matrices might be both, symmetric and asymmetric. This in turn implies that geodesic distances between two nodes do not necessarily need to be the same. It

is not uncommon for geodesic distances from n_1 to n_2 and from n_2 to n_1 to be different in asymmetric matrices.

In this paper we are going to use both, symmetric and asymmetric adjacency matrices to estimate the geodesic distances between nodes. For direct friends, those at social distance one (SD = 1), we are going to estimate directed (asymmetric) and reciprocated (AND-network) geodesic distances. We believe that giving towards these three might be statistically different.

The major part of the analysis; however, will be done by dealing with symmetric matrix – the on without directed ties. We are going to estimate geodesic distances from a symmetric matrix for any SD > 1. This strongly simplifies the research and even though differences might exist, experimental design would become too complicated if we differentiate between reciprocated, symmetric and asymmetric ties further in the social chain as well.

The last step in the analysis of the relationships within the community is to calculate degree centrality. The degree centrality is defined as a total amount of outgoing and incoming ties an individual possesses (Faust & Wasserman, 1997). The interpretation of out-degree and in-degree measures is expansiveness (amount of outgoing ties) and popularity (incoming ties) levels respectively for each individual in the community. This potentially might become and influential explanatory variable for the empirical model of altruistic preferences.

3.2 Measuring altruistic preferences

This part introduces the model which we are going to use to measure the altruistic preferences of the members of the community.

If people indeed are more generous towards socially close individuals, this effect is then captured by directed altruism. Leider et al. (2007) have decomposed the total level of altruism as follows:

$$y_{MP} = y_1 D_{MP} + y_M \qquad (1)$$

where y_{MP} denotes total altruism level of the decision maker (M) towards partner (P) and y_M stands for baseline altruism. y_1 denotes the amount of directed altruism towards P, which depends on the social proximity between M and P. This proximity is captured by a set of dummy variables to denote social distances, D_{MP}.

Data to measure directed and baseline altruism will be gathered by conducting a modified dictator game. The empirical model suggested by Leider et al. (2007) and to be used in our research is as follows:

$$x_{MP} = \alpha + \beta Z + y_1 D_{MP} + y_M x_{MN} + \varepsilon_{MP} \qquad (2)$$

where x_{MP} denotes M's decision of how much money to transfer to P during the dictator game. Z stands for a set of demographic covariates of the dictator and the recipient used in different specifications of the model. D_{MP} is a set of dummy variables for particular geodesic distance. y_1 is the coefficient for D_{MP} and measures the effect of directed altruism. x_{MN} is a decision made towards nameless or fictitious partners (to be explained in greater detail in the experimental design) depending on the specification. It is our proxy for baseline altruism of the dictator, which is measured by the coefficient y_M. The intercept is α. The error term of the model is normally distributed and denoted ε_{MP}.

4 Sample

After comparing various communities in terms of size, approachability, frequencies of agent interactions and other criteria, we selected the student community of the Stockholm School of Economics in Riga. This sample has a large desirable variety in

terms of nationality, gender, social background and other factors. Students as a group have also yielded results very similar to the whole population in previous experiments (Bennet, 1987). Being part of this community ourselves and knowing its members quite well, we can expect much easier and faster data collection, as well as considerably higher response and participation rates. Finally, the community has a desirable size (348 individuals), which is big enough for significant analysis but not too big as to cumbersome the field work too much.

Although there are great benefits of doing insider research, we are also fully aware of the possible problems that it might cause. Various biases can arise when the authors are members of the researched community. Respondents might be reluctant to provide accurate information to authors if they know them personally, or they might be afraid that the information will be shared with other members of the community. Also, respondents might feel pressure to provide some certain answers or avoid others. Confidentiality issue becomes very important. We tried to ensure as high confidentiality as possible throughout the research in order to minimize the negative effects the insider research might cause.

5 Friendship ties

5.1 Collection

All the current students in the community of the Stockholm School of Economics were approached. They were asked to fill in a self-administered questionnaire either personally or via email (see Appendix A). The questionnaire was targeted to extract information about friendship ties between the students.

When designing the questionnaire we tried to ensure the highest possible anonymity. Each student was assigned a unique number in a list, which consisted of all community members. When a student was approached with a request to fill in the

questionnaire, he/she was provided with both, the survey itself and the previously mentioned list. The respondent then had to write down only his/her own number and those of his/her friends. This technique not only motivated respondents to reveal their real friends more openly, but also substantially reduced the time and effort needed to remember all friends. The questionnaire also contained such demographic control variables as gender, age, year of studies, nationality, hometown and religious beliefs. It will allow us to check for differences in altruism levels not only between genders, as in Leider et al. (2007), but for other factors as well.

5.2 Summary statistics

85.6% of the community members filled in the questionnaire. The remaining 14.4% consisted largely of persons who did not respond to our email. Such a high response rate allows us to considerably reduce the probability of "missed" ties as compared to Leider et al. (2007), who had a response rate of 71%. This achievement can be partly attributed to the fact that we have conducted insider research and used personal approach.

54% of respondents were male and 46% female. Division by nationalities was as follows: 44.6% were Latvians, 22.3% Lithuanians, 16.2% Russians, 11.5% Estonians and 5.4% other. All other collected demographic variables are provided below (in Tables 3 and 4).

The most important information extracted from the surveys is about the friendship ties. This information allowed us to compose adjacency matrices and in turn to find geodesic distances. The summary is provided in the table below.

Table 1.

Distribution of geodesic distances

Geodesic distance	Proportion in the community	
	Asymmetric	Symmetric
1	2.6%	3.3%
2	10.4%	18%
3	28.5%	47.2%
4	38.3%	28.4%
5	17.4%	3.1%
6	2.7%	0.1%
7	0.1%	-

Not surprisingly, the original data in the adjacency matrix was asymmetric with 36.01% of ties reciprocated. Leider et al. (2007) had almost the same proportion of reciprocated ties, 36.66%, in a community of 800 students. They also allowed naming up to 10 friends in their questionnaire, while creating additional monetary incentives for naming real friends.

After the data had been made symmetric the average distance between reachable pairs decreased from 3.663 to 3.103. Social distance 7 disappeared and social distance 6 became hardly noticeable. This is attributable to the fact that symmetrization created more ties than actually reported because it evened out directed ties by artificially making them mutual. However, this is not a disadvantage as this simplification makes the data much easier to handle and especially simplifies the matching process for the experiment[iv].

Another important information extracted from the friendship ties is Freeman's degree centrality measure. It roughly shows the most popular and the most disconnected actors within the community (Faust & Wasserman, 1997). This measure might become an important control variable in the empirical model on altruistic preferences to be estimated. The summary statistics on degree centrality is provided in the table below.

Table 2.

Statistics on Freeman's degree centrality

	Freeman degree centrality	
	Out-degree	In-degree
Mean	7.534	7.534
Std. deviation	3.660	4.108
Sum	2622	2622
Variance	13.392	16.875
Minimum	0	0
Maximum	10	23

In total respondents of the questionnaire named friends 2622 times. Logically, the amount of outgoing and incoming ties is the same (what is outgoing to one actor is incoming to another). However, the variance and, in turn, standard deviations differ. This is due to the fact that not all people name the same number of friends. It is also not surprising that variance in in-degree is bigger than in out-degree as people were bounded by 10 outgoing ties in making their decisions. However, certainly more than 10 individuals could have named the same actor as their friend.

Some actors were named as frequently 23 times, which implies that these people are the most popular in the network. The maximum of 10 outgoing ties was limited by the instructions of the survey.

This kind of centrality measure was estimated for each separate actor in the network and will be used in testing the empirical model of the altruistic preferences.

Having these data and measures, we can now move to the core part of the research – an experiment where the main empirical variables for the model will be gathered.

6 Experimental design

The experiment was a modified dictator game, with two different exchange rates (ratios of token values or "price" of altruism) and double-blind anonymity, which

means that neither the recipient nor the moderator are able to associate a particular decision with an exact person. The experiment was carried out as follows.

A list of individuals eligible to participate in the experiments was composed from the social map[v]. All of them were invited by email to the experiment a few days in advance but only those who confirmed their attendance were allowed to participate.

6.1 Flow of the experiment

All the participants were gathered in a classroom and seated. Each participant was given an instructions page (see Appendix B) and a closed envelope. Both authors were present during the experiment as moderators. One of the moderators explained the rules of the game. Afterwards participants were asked to open their envelopes, where they found two sheets of paper – a list of names and a decision sheet, both prepared individually for every participant. The former contained a list of nine people chosen according to social distances from the whole community, a fictitious person Andris Berzins, and a nameless partner who was a randomly chosen person from the community. The exact name for the fictitious partner was chosen to prevent students from suspecting that this fictitious person actually does not exist in their community. The name is of Latvian origin, since majority of students in the community are Latvians. Also, the chosen name and surname are very common in Latvia. These facts thus increase the probability that participants would believe that such person actually exists. Fictitious and nameless partners are going to be used as proxies for baseline altruism in the empirical specifications. See Appendix C for the decision sheet, which also contained a list of coded demographic variables of the dictator, so that they could be used later in data analysis.

The dictator had to make 22 decisions in total. Each dictator was matched with 11 partners and had to make a decision for each person under two different exchange

rates. There was only one experimental session. During that session all decisions were made, and all the participants made the decisions at the same time. The experimental setting was the same for everybody. In each case, the dictator was allocated 50 hypothetical tokens with real monetary value and had to decide how many of them to donate to the matched agent. Writing down numbers of hypothetical tokens on paper instead of using real money during the experiment was inevitable in order to ensure double-blind anonymity in our experimental design. The two different exchange rates were 1:1 and 1:3. Under the 1:1 exchange rate, each token was worth the same for both the dictator and the partner. Under 1:3 exchange rate each token was worth three times as much for the agent if donated. Giving (donating) is said to be neutral under 1:1 exchange rate and efficient under 1:3 exchange rate (Leider et al., 2007).

6.2 Confidentiality

Both because the experiment involved making personal decisions, which directly affected one's friends, and because of the insider research, it was of crucial importance to ensure a double-blind setting of the experiment. Moreover, controlling for scrutiny is one of the key issues for external validity of the laboratory findings. In this part we summarize the steps made to maximize the confidentiality of the participants during the experiment.

Firstly, the list of names did not have to be handed in, participants were asked to give in only the decision sheet. It was enough to conduct data analysis because the social distances were listed in a pre-defined sequence in the names sheet. The sequence was the same for all participants but due to obvious reasons they were not informed about it. As shown in Appendix B, no names were used in it, which means that the identity of the recipients could not be revealed. The only exception was the name of one agent for which money had to be allocated. As the decision sheets had to

be given back inside envelopes, it was not possible to know which decision sheet belonged to whom. Participants were informed about this fact.

Secondly, we used a password system when implementing decisions and giving out cash to participants. This ensured that we, the researchers, could not know how much money any particular participant would receive. This was done in the following way. At the end of the experiment, each participant had to draw a leaflet with a number and a password on it. The participant wrote the number in the appropriate place on decision sheet and kept the leaflet. After the experiment, we put the required sum of money (cash) into envelopes with appropriate password numbers, without knowing to whom they belong. These envelopes were given to the school's receptionist together with a numbered list of passwords. For two weeks after the experiment, participants could approach the receptionist with their password and get the envelope with their money.

Finally, the agent to whom the money was donated also did not know who donated this money. One of the main conditions of the experiment is that dictator remains anonymous to the recipient.

With all these steps we ensured that the only person who could know his exact decisions and payoffs is the dictator himself; hence we succeeded in ensuring a double-blind setting of the experiment.

6.3 Real monetary payoffs

Each participant was entitled to 3 Lats (LVL) which is around average hourly wage in Latvia[vi]. This amount of fixed compensation was promised in the invitation in order to ensure a high enough participation rate. It was paid after the experiment. Under 1:1 exchange rate, the value of each token was 0.10 LVL. Under 1:3 exchange rate, each token was worth 0.05 LVL if kept by the dictator and 0.15 LVL if donated to the

agent. Thus, under 1:1 exchange rate dictators were dividing 5 LVL, while under 1:3 exchange rate they could get up to 2.5 LVL by keeping all tokens to themselves or donate up to 7.5 LVL by donating all 50 tokens.

Although each dictator had to make 22 decisions, only one of them was actually implemented and the one implemented was determined randomly. This was done due to budget constrains. Before submitting the decision sheet, each participant had to blindly draw a leaflet from an envelope with a number from 1 to 22. The drawn number determined which of the 22 decisions were to be implemented. Sampling with replacement was used, so that the probability of each decision to be implemented would remain the same for every participant. The dictator had to write down the name of the person (from the list of names) for which this decision was made in the given place in decision sheet, and put the drawn leaflet to the envelope together with the decision sheet. It determined how much cash had to be given to which person after the experiment.

6.4 Summary statistics of the participants

The experiment was held on February 22, 2009. Out of all invited members of the community, 79 people replied to the email invitation confirming their participation and 75 of them actually participated in the experiment. 54.7% of participants were male and 45.3% female. According to nationality, 41.3% of the participants were Latvians, 30.7% Lithuanians, 16% Estonians, 9.3% Russians and 2.7% of other nationalities (see Tables 5 and 6 for more details).

Under 1:1 exchange rate, participants on average allocated 27.88 (out of 50) tokens to SD = 1 reciprocated partners, and the average gradually decreased to 4.09 for socially farthest (SD = maximum observed) recipients. Even less was donated to fictitious partners, only 3.28 tokens on average, while giving to nameless partners was

higher – 8.56. No tokens were allocated to reciprocated SD = 1 partners by 13.3% of participants, and no tokens were donated to SD = maximum by 62.7% of participants. All 50 tokens were allocated to reciprocated SD = 1 partners by 22.7% individuals, and nobody gave all tokens to SD = maximum partners (see Table 7 column (1) and Table 8 for more details).

With 1:3 exchange rate, when some giving was efficient, participants donated 36.49 tokens on average to SD = 1 reciprocated partners, and giving again gradually decreased to 8.85 to maximum observed social distance partners. Average transfer to fictitious partner was 6.79, and to nameless partner 13.13. Only 5.3% of participants gave nothing to SD = 1 reciprocated partners, while to SD = maximum 56% chose to do so. 50 tokens were allocated to SD = 1 reciprocated partners by almost half, 48% of individuals, and 4% donated everything to SD = maximum partners (see Table 7 column (2) and Table 9 for more details).

7 Empirical findings

We have used Tobit regressions to estimate the empirical model in different specifications. In the model estimated the dependent generosity variable – allocation the dictator made (a proxy for altruism) – was bounded from two sides – 0 from the left and 50 from the right. This kind of censored data requires Tobit estimators, as OLS estimates are inconsistent[vii].

All the empirical specifications were estimated using Tobit regressions with clustered robust standard errors[viii]. We believe that there is correlation between decisions of each single individual. Hence, the observations cannot be treated as totally separate when calculating the standard errors. As long as the observations are not identical it is essential to cluster them according to dictator's identity. Hence, we obtain 75 clusters (75 people participated in the experiment).

Even though we report pseudo R-squares, it should be noted that they are not the best estimates of model fit when we are investigating human behavior, which is usually influenced by a lot of exogenous variables that are not possible to observe. Hence, the reader should not be particularly concerned that the reported pseudo R-squares are rather small.

First, we have run the Tobit regression for the specification without any demographic variables in order to see the general patterns of the model.

$$x_{MP} = \alpha + y_1 D_{MP} + y_M x_{MN} + \varepsilon_{MP} \qquad (3)$$

where x_{MP} is a decision made by the dictator of how much money to share with the counterpart. This variable captures dictator's generosity towards partners at different social distances as well as the generosity towards nameless and fictitious partners. It is crucial to recognize that even though we are investigating altruism, generosity is only its proxy, as altruism cannot be directly observed. D_{MP} denotes a set of eight dummy variables: three for social distance one (SD = 1), two for social distance two (SD = 2), two for social distance three (SD = 3), and one for social distance four (SD = 4). The omitted dummy variable is the one which denotes maximum possible social distance (SD = max) within the community. Hence, y_1 should be interpreted as additional generosity towards a partner at a particular social distance as compared to allocation for the person at the maximum existing social distance within the community.

We have estimated the latter specification for two exchange rates, 1:1, where tokens are worth the same for the dictator and the counterpart, and 1:3, where for the counterpart the tokens are worth three times as much as for the dictator. Moreover, we have also estimated different specifications by changing the control variable for baseline altruism. We have run the model two times under each exchange rate, once with the decision towards nameless partner to control for baseline altruism and then

by including the decision towards fictitious – non-existing – person to control for baseline altruism. The results for the exchange rate 1:1 are reported in Table 10 columns (1) and (2) and for exchange rate 1:3 in Table 11.

Giving steadily decreases as social distance increases within the social chain in accordance with our expectations. Hence, we fail to reject our hypothesis that generosity is negatively related to the distance within the social chain. Most of the results are statistically significant at 1% level both jointly and separately. There are significant differences between the means of increased giving for different social distances as well. This is evidence for the existence of directed altruism as such – people adjust their generosity depending on the social distance.

Finding 1. *As the distance in the social chain increases, people decrease their generosity. Hence, this adjustment of giving shows evidence for the existence of directed altruism.*

We also find evidence for the existence of baseline altruism, as the coefficients in front of variables denoting decision towards nameless and fictitious partners are positive and statistically significant. We find that one's baseline altruism (giving to nameless or fictitious partners) also positively influence a dictator's giving to named partners (within the social chain). In different specifications coefficients for a nameless partner vary from 0.457 to 0.545 while coefficients for a fictitious partner vary from 1.007 to 1.3. This means that as giving to nameless/fictitious partners increases by one unit, dictators donate more for other social distances as well, on average by 0.5/1.15 token. All of these coefficients are statistically significant at 1% level; hence, we see evidence that more altruistic people as such (with higher baseline altruism) donate more to individuals at any social distance than people with less baseline altruism. What is not clear though, is which giving, towards nameless or

fictitious partners, is a better proxy for baseline altruism. On one hand nameless is an anonymous person and seizes generosity towards someone a dictator does not know. In such manner one's general altruistic preferences, baseline altruism, is captured. On the other hand, both named and nameless partners are pooled together in one model; therefore, the identity effect cannot be captured. Fictitious partner is advantageous because it possesses an identity by having a "real" name. However, the identity effect in this case can be either positive or negative. Thus, we report results of both control variables for baseline altruism and do not argue in favor of either.

Finding 2. *People possess baseline altruism, which means that they are willing to behave altruistically even towards unknown people. Baseline altruism has statistically significant positive effect on generosity towards named partners. One token increase in giving to nameless/fictitious partners is associated with an increase in giving to named partners of 0.5/1.15 tokens on average.*

It is evident that statistical significance tends to diminish when we move farther along the social chain (see Tables 10 and 11). This illustrates that at some point within the social chain directed altruism diminishes and only baseline altruism is left to explain generosity. These points vary across different specifications. For the exchange rate 1:1 SD = 4 is significant only at 5% level, which is much smaller as compared to significance of other distances (mostly 1% significance level). For the exchange rate 1:3, increased giving towards the person of the other nationality at SD = 3 is significant at only 10% level while generosity towards partner at SD = 4 is not statistically different from generosity towards the socially most distant person. When we look at the results of both exchange rates pooled together (Table 12), we again see that SD = 3 other nationality demonstrates large drop in significance level, while SD = 4 is significant at 10% level (nameless as a control variable for baseline altruism) or

not statistically different from zero (giving to fictitious partner used as a control variable for baseline altruism). Hence, we see that dummy indicating increased giving is no longer significant either at SD = 3 other nationality or most frequently at SD = 4, depending on the specification. These are the points where directed altruism effect diminishes and generosity is explained by baseline altruism.

Finding 3. *At social distance 4 additional giving is not statistically different from 0. This means that giving for a person at social distance 4 and the socially most distant in the community is not statistically different. Hence, we find the point where directed altruism diminishes and only baseline altruism is left to explain generosity.*

Next, we pool giving under both exchange rates together and introduce another dummy variable to note the exchange rate effect. Now the specification looks as follows.

$$x_{MP} = \alpha + \beta D_{exchange} + y_1 D_{MP} + y_M x_{MN} + \varepsilon_{MP} \qquad (4)$$

The only difference from previous specification is the new dummy variable $D_{exchange}$, which takes a value of 1 when the decision x_{MP} was made under exchange rate 1:3 and 0 when x_{MP} was made under exchange 1:1. The coeficient β shows the effect of the exchange rate on directed altruism.

When we run this specification (see Table 12), we find that the coefficient β is positive and statistically different from zero at 1% significance level. For two specifications when nameless and fictitious partners were used as control variables the coefficient takes values of 6.566 and 5.005 respectively. This means that the experiment participants reacted to the exchange rate and donated more money when the giving was efficient.

Finding 4. *People recognized different exchange rates and positively adjusted their giving when it was relatively cheaper.*

Finally, we add a set of demographic variables and run different specifications with various demographic covariates for two exchange rates separately. Hence, the empirical specification now looks as the one presented in methodology.

$$x_{MP} = \alpha + \beta Z + y_1 D_{MP} + y_M x_{MN} + \varepsilon_{MP} \qquad (2)$$

Z denotes a set of demographic variables. We tried different specifications with covariates denoting gender, age, year of studies, nationality, degree centrality (number of incoming, outgoing, and recipocated ties), home city size, family composition (number of brothers or sisters), level of religiousness, and religion itself.

In columns (3) and (4) of Table 10 we report only the variables that were significant for the specification for exchange rate 1:1. Social distance dummies still remain the only significant variables in all specifications to explain variation in generosity. We observe weak evidence that gender and Christianity as religion influences generosity. On 10% significance level females transfer approximately 4.5 tokens more than males. Christians transfer approximately 4.55 tokens more than not religious people and people of other religions in the specification where generosity towards nameless partner is used as a control variable for baseline altruism. However, these demographic covariates become not statistically significant when we run the specification with fictitious partner as a control variable. The only demographic variable, which is significant in both specifications, is the dummy for the size of the dictator's home city. At 5% significance level we find that decision makers from large hometowns (larger than 100,000 inhabitants) give away 6.144 and 5.999 tokens less than decision makers from small hometowns in the specifications where nameless and fictitious are used as control variables respectively. In the specification with fictitious partner as a control variable the amount of incoming ties explains some variation at 5% significance level; however, in the specification with nameless partner as a control

variable, it is not statistically significant. What is more, this degree centrality measure (number of incoming ties – popularity of the actor) raises serious causality concerns. It is not clear whether a decision maker is more generous because the person is popular or the popularity itself is associated with more generous nature of the individual. All in all, only the hometown size is significant in both specifications for the exchange rate 1:1.

When we add demographic covariates to the model with 1:3 exchange rate, none of the demographics show statistical significance. The same is true when we add demographics for both exchange rates pooled together.

Finding 5. *Demographic factors fail to explain generosity on the aggregated level even though for separate specifications gender, home city size, incoming ties, and religion have some explanatory power.*

In the next section we are going to analyze the findings and explain them in greater detail. Additionally, we will present some implications and discuss the possibility of generalization of the findings for the world outside experimental laboratory.

8 Discussion

8.1 Analysis and inferences

In this section we are going to elaborate on the findings presented in the previous part. Moreover, we are also going to draw some implications and present directions for further research.

We observe the first interesting finding while analyzing frequencies of donations. We see very different generosity patterns in our experiment relative to the single comparable study of Leider et al. (2007). When we compare our findings with Leider et al. (2007), we notice much higher generosity towards people at SD =1 in our

experiment. As Leider et al. (2007) use one measure for each social distance and we have three for SD = 1 and two for each, SD = 2 and SD =3, we calculate averages for our sample. From Table 7 we calculate average donation for SD =1 (sum donations towards reciprocated, outgoing and incoming and divide this by 3). We see that under exchange rate 1:1 the average donation for SD = 1 is 23.11 tokens in our sample, while in Leider et al. (2007) the average donation for SD = 1 is only 11.96. Moving further along the social chain, generosity in our experiment exhibits a large drop of approximately 47% from SD = 1 to SD = 2, which amounts to 12.347 tokens (average between SD = 2 same and different nationality). In Leider et al. (2007) the drop between giving for SD = 1 and SD = 2 amounts only to a low 10%. Moving even further, at SD = 3 in our experiment we see another big drop of 39% and the mean donation is now only 7.51 tokens (average between same and other nationality). Leider et al. (2007) demonstrate the drop of only 13% and their average giving at SD = 3 amounts to 9.39, which already exceeds the one in our experiment.

In this study giving at SD = 1 significantly exceeds the one in Leider et al. (2007), which is then in our study followed by sharp decreases at increasing social distances. Leider et al. (2007) findings, on the other hand, exhibit much lower giving at SD = 1 followed by mild decreases for increasing social distances (see Figure 3). This kind of pattern holds for the exchange rate 1:3 as well (see Figure 4).

Inference 1. *In our study people share much more with friends at smaller social distances relative to the ones at larger distances as compared with Leider et al. (2007).*

56% of decision makers give away more than 0 to the nameless partners and the average giving amounts to 8.56, which is 17.12% of the total endowment. Giving for the fictitious partner is much less generous where only 34.67% of dictators allocate

more than 0 tokens and the average giving is only 3.28 tokens, which is a low 6.56% of the total endowment. When we compare giving to nameless and fictitious partners to the ones in the social chain, a very interesting thing can be observed. It is strange that on average dictators allocate less for SD = max, SD = 4, and SD = 3 than for the nameless partner under both exchange rates. This means that nameless partner is allocated with more tokens than socially far individuals. This result is consistent with findings of Leider et al. (2007) who also find that people act more generously towards nameless partner than towards named ones at larger social distances. This can be partly attributed to the negative effect of knowing who the person is and not liking the person much. As Jenni and Loewenstein (1997) argue in their paper, knowing the identity may evoke either positive or a negative effect.

Inference 2. *Our findings support the idea that knowing the identity of the recipient might yield not only positive but also negative effect on generosity.*

When we run a set of Tobit regressions, we clearly observe a significant negative relation between social distance and generosity – generosity gradually decreases when social distance increases. This is a rather expected and straightforward finding. However, we also observe differences in generosity within the distances themselves. We find significant differences in generosity between SD =1 reciprocated, outgoing, and incoming ties, which amount to increases as high as 6 additional tokens on average (see Table 10 column (1)). Even though differences are not unexpected, their magnitude indeed is. The results demonstrate gradual step-by-step decreases not only between distances but also within SD = 1, and the size of decrease is roughly the same both between the distances and within them. This evidence suggests that actually OR-network might not be the most suitable choice to measure social networks as it considers a friendship tie to be present if at least one direct tie exists. As we see from

our findings, ties of different direction yield largely different results where the difference within the distance amounts approximately to the difference between the distances. It is a rather interesting finding as OR – social network approach is very common among social scientists. Our findings should signal that in the future researchers have to deal with it with greater caution.

Inference 3. *OR-social network might not be the best approach to measure social networks. The researchers should be very cautious about which network approach to use.*

Another similar finding is that we observe differences in generosity towards people according to nationality at the same SD. This suggests that people differentiate their friends on the same social distance by nationality. To the authors' best knowledge, no other studies have investigated for different giving among nationalities in such experimental design. Hence, this finding adds to the knowledge of altruism in modified dictator games. The possible explanation for this finding is that in our community this difference is most probably associated with close communication among the nationalities. Usually bonds are formed among people from the same community. For the further studies it would also be interesting to check whether there are differences among some nationalities in particular; however, our experimental design does not allow that, as we had to sacrifice some potential findings in order to maintain the scrutiny and keep the experimental design as simple as possible for the participants. We do believe that investigating generosity towards people of the same social distance but different nationalities could be a very interesting and valuable path for further research.

Inference 4. *People differentiate their friends not only according to social distance but also nationality, which is an interesting topic for further research.*

In our methodology we have also employed a different exchange rate, or in other words, introduced another "price" of altruism – generosity became cheaper for the decision maker. In this setting the ratio of token values was 1:3, which meant that donated tokens were worth three times more than kept ones. This setting allowed us to observe whether dictators noticed the exchange rate and behaved more generously when it was cheaper. Moreover, as sharing the endowment was no longer a zero-sum game, by being generous decision makers could increase the social surplus. We have found that exchange rate indeed matters, which suggests a certain strategy to be effective both for fundraising institutions themselves and for government wishing to increase charitable activities. This finding supports the use of matching gifts, a fundraising strategy where some leadership donor promises to match any donation from other persons with additional donation from his side. This effectively creates a social surplus and the result is very similar to efficient giving under 1:3 exchange rate in our experiment, since a person effectively donates a larger sum of money while loosing the same amount. To express differently, the "price" of donating is reduced. This result is consistent with findings by Karlan and List (2007) who conduct a large-scale field experiment and find that such a matching gift increases the probability that an individual donates by 22%, and increases the average donation sum by 19%. However, they find that larger match rations than 1:1 have no additional impact. As it is the only such study, further research should be done to validate this finding.

Inference 5. *Matching strategies for fundraising should have a significant positive effect on donated amounts and frequencies.*

We do not find strong evidence in favor of significance of any demographic covariate and this result goes in line with Leider et al. (2007) who do not find evidence in favor of demographics as well. However, value added from our research

comes from the fact that we have tested additional variables such as Freeman's degree centrality. It is interesting that we see no significant effect of expansiveness and barely see any significance in popularity (Freeman's degree centrality). However, the issue of causality should be noted here and even if we saw significance in these effects, a separate research could be devoted to establish causality. To our best knowledge, our study was also the first one to test for such variables as type of religion and religiousness as such, size of home city and number of brothers or sisters.

If under the exchange rate 1:1 at least some demographic covariates show weak statistical significance, even they diminish when giving becomes cheaper (exchange rate 1:3).

Inference 6. *Our findings suggest that demographic covariates do not explain much variation in altruism as a lot of explanatory power comes from social distance variables.*

Probably the most important of our findings is the diminishing effect of directed altruism. This is evidence for all the hypotheses we have raised in our work – existence of directed altruism and baseline altruism as such. As we find that increase in giving at SD = 3 different nationality or SD = 4 upwards, depending on the specification, is not statistically different from zero, we find the point where the social chain breaks and only baseline altruism can explain the variation.

Inference 7. *In the investigated community the breaking point in the social chain is social distance 3 of other nationality or social distance 4, depending on the specification.*

8.2 Parallelism

After carrying out any study concerning behavioral game theory experiments, it is crucial to discuss the external validity of laboratory findings. As illustrated by Levitt

and List (2007) it is relatively easy to make judgments whether findings of physical sciences in the laboratory hold in general. However, human behavior requires much more consideration because people maximize their utility based not only on monetary gains but also considering moral costs, which are influenced by different factors. Levitt and List (2007) have grouped these factors into five categories: scrutiny, morality, context, self-selection to participate in the experiment, and the stakes involved in the game. If the experiment is designed in a way that control is established over all of these factors so that the laboratory environment is parallelized to the outside world, the findings could be successfully generalized[ix]. We are going to discuss each of these five factors with respect to our study and discuss inferences for our findings.

Scrutiny is one of the key factors to establish control upon in the laboratory experiment. Naturally, if people feel that their behavior is closely monitored and judged, the decisions will be biased towards more socially acceptable ones (e.g. people would be more generous than naturally). In our experiment we established double-blind procedure – neither moderators nor partners could associate a particular decision with some exact individual. We believe that extensive confidentiality measures introduced in our experimental design facilitated rather natural behavior from the scrutiny perspective.

Morality and context are the factors that the researcher cannot control but must be fully aware of. It is important to understand that the background and previous experience of participants as well as norms in the society have great influence on behavior. Behavioral deviations in various cultures with different market integrations illustrate this fact perfectly (Henrich et al., 2001). Hence, it is important to know to what extent the findings can be generalized. We chose the community to be

investigated in such a manner that we could obtain the greatest possible variation in terms of background and demographics; hence, we believe we can draw inferences to communities of similar context not only in Latvia but also in the countries with similar market integration and norms.

Self-selection of the participants is the only factor upon which we were not able to control for. People in our experiment were recruited on the first-come, first-served basis. This was done due to relatively small community size and a need for sufficiently large sample in experiment. The main problems self-selected participants can cause are the "punctual student's" bias and previous experience of participation in laboratory experiments[x]. We believe that we do not observe the latter in our sample, as the students were not experienced in previous participation in economic experiments. Moreover, as it was a one-shot game, there was no learning curve effect as well. Hence, we do not see serious problems associated with the fact that participants were self-selected.

Different sizes of stakes used in similar experiments can have an effect on how players perceive their payoffs relatively to others. There has been some evidence, though not strong, that larger stakes reduce the number of rejections in ultimatum games (Camerer, 2003). However, the size of the stakes used in dictator games is of less importance, as rejection from the recipient is not possible, and thus there is no fear of rejection. Nevertheless, as very small stakes can reduce the motivation of participants to take the game seriously, we tried to have as high stakes as possible while still having sufficient amount of participants for proper analysis. The resulting expected payoff from the experiment was approximately one to two hours' average wage in Latvia. After considering price level of the country, such stakes are very similar to other researches with dictator games, such as Leider et al. (2007) and

Andreoni and Miller (2002), who devoted approximately 10 US dollars for sharing per participant. We thus consider our stakes to be high enough not to have any negative effect on the results.

As this research is pivotal not only regionally but also globally, the goal of this paper was to find some behavioral patterns and induce further research on altruism. Hence, we do not draw strong inferences about possibilities of generalization. Nevertheless, we have established control over scrutiny and the stakes of the game and we are fully aware that the findings can be generalized only for communities with similar context and moral norms. Thus we believe that altruistic patterns that we find can be successfully generalized for similar communities in the countries of similar market-integration and moral norms.

9 Conclusions

In the last two decades the field of behavioral game theory has witnessed numerous researches. Scholars have found enormous amount of evidence in favor for the existence of altruism as opposed to the concept of “homo economicus”, a purely selfish wealth-maximizing individual. The purpose of this paper was to contribute to understanding of altruistic behavior between socially close individuals. We investigated the relationship of generosity and social distance under two “prices” of altruism – two exchange rates. Our hypothesis was that people possess both baseline and directed altruism, and that the latter is negatively related to the social distance and that they increase giving when it becomes relatively cheaper.

This study was conducted in 2009 in a unique international student community in Latvia. We have measured social distances in a community of 348 students, and then conducted an anonymous modified dictator game with 75 participants. To our best

knowledge this research is only the second work to carry out such experiment within a measured social network, after Leider et al. (2007).

We find strong evidence for existence of directed altruism towards friends. As the social distance between two individuals within a social chain decreases, altruism gradually increases. We also find that "price" of altruism has a significant effect, just as Andreoni and Miller (2002) and Leider et al. (2007).

Most importantly we find the breaking point of the social chain. People do not increase their generosity towards people at social distance four. This is the point where directed altruism diminishes and variation in generosity is explained by baseline altruism. Hence, y*our friend's friend's friend's friend is not your friend anymore!*

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Endnotes

[i] Some economists have been thinking about philanthropy and donations for a long time. For example Becker (1974) developed a theory of social interactions and even more than 30 years ago claimed that individuals care about each other.

[ii] Ties are mutual; hence, establishing direction does not make any sense.

[iii] OR-network requires for the existence of only one directed tie for the actors to be related. AND-network on the other hand requires mutual ties in order for the relationship to exist (Leider et al., 2007).

[iv] Participants have to be matched with actors on various social distances from them. As it has to be done manually, directed ties would be extremely difficult to handle without errors.

[v] We define eligible as having enough social ties to comply with the experimental requirements.

[vi] At a time of the research, February, 2009, 1 LVL (Latvian Lat) was fixed to Euro at app. 1.42 EUR per 1 LVL.

[vii] For more information about differences between Tobit and OLS estimators see STATA manual.

[viii] We have also tried random effects Tobit regressions, which yielded the results very similar to those obtained by Tobit with clustered robust standard errors.

[ix] Even though, there also exists a fundamentally opposite view. Some scholars argue that behavioral laboratory experiments cannot be generalized at all.

[x] However, some scholars do not find significant differences between those volunteering to participate and those randomly selected.

Appendix A

Questionnaire

This study is done by the students of the Stockholm School of Economics in Riga as a part of Bachelor Thesis research. Its purpose is to study traits of individuals within a close community. Even though you are asked to provide some private information, it will remain confidential. Information will be used by the authors for this study only and results will be presented only in aggregated form. No private information will be disclosed to any third parties.

1. You are provided with a list where each student is assigned with a specific number, so that no actual names would be used in the questionnaire.
Please provide your own number from the given list: ……

2. Please write down the numbers of UP TO 10 of your closest friends from the given list (SSE Riga students only). Please list your really best friends. The people you name will have a higher chance to earn money in a game experiment to be done later in this study. The order of your list is NOT important. Note: Authors of the study are not included in the list.

1) ..... **2)** ..... **3)** ..... **4)** ..... **5)** .....
6) ..... **7)** ..... **8)** ..... **9)** ..... **10)** .....

3. Your gender: □ Male □ Female

4. Your age: …….

5. Your year of studies: □ Year 1 □ Year 2 □ Year 3 □ other

6. Your nationality: □ Estonian □ Latvian □ Lithuanian □ Russian □ other

7. Which city/town are you from (specify)? …….....

8. Do you have any brothers/sisters? □ No □ One □ Two or more

9. How religious would you consider yourself (from 1 as absolutely not religious to 5 as deeply religious)**?**
□ 1 □ 2 □ 3 □ 4 □ 5
If you answered 2, 3, 4, or 5 in Question 9, please proceed to Question 10.
If you answered 1 in Question 9, please skip to Question 11.

10. What are your religious beliefs?
□ Lutheran □ Orthodox □ Catholic □ other (specify) …….....

11. Part of respondents will be selected randomly and invited for an experimental game for this study. It will take place in mid February and last for around 45 minutes. Attendees will be able to earn around 5 to 8 LVL for participation. **At which times are you available for participation in the experiment? (Tick all, which apply)**
□ Working days 12.00 – 13.00 □ Working days 17.00 – 19.00
□ Working days 19.00 – 21.00 □ Weekends
□ I would not like to participate in the experiment

Thank You for Your participation!

Appendix B

Instructions

Introduction:

You are participating in an experimental game. You were randomly selected from a group of students and invited to the experiment. You have voluntarily agreed to participate. All the participants must strictly **follow the guidelines** provided in the instructions and explained by the moderator.

From this moment in the experiment **you are strictly forbidden to talk**. Any attempt to communicate with another participant or to look at what others are writing will result in a monetary penalty of 1 LVL, which will be deducted from your final payoff.

Inside the envelope you will find two sheets of paper. The first sheet will contain an individual list of student names (names sheet). The second sheet is the decision sheet, where you will write down all your decisions.

What you need to do:

You are allocated 50 intangible tokens; each token is worth 0.10 Ls. You have to decide how many of these tokens you will donate (share) to another person (from 0 to 50). You have to repeat this decision for every person in the given list, and **write down the donated (shared) number of tokens in the decision sheet.**

You also need to repeat your decisions for each person under a new "exchange" rate of 1:3. It means that now every token that you keep for yourself is worth only 0.05 Ls, while every donated (shared) token is worth 0.15 Ls for that person.

In total you have to make 22 decisions (11 persons under 2 different exchange rates). At the end of the experiment one of these decisions will be selected randomly and implemented, meaning that both you and the recipient will get the appropriate sum of money based on your decision.

Write the decisions on decision sheet **only**, the names sheet **will not have to be submitted**.

Please take your time to make all the decisions carefully, as it will influence the results of the research. **You will have to wait for everyone to finish before you can leave your seat.**

When the moderator allows you to leave your seat, approach him/her with both of your sheets and the envelope.

Confidentiality:

All your decisions as well as your payoff will remain as confidential as possible; even the moderators will not know them. This will be ensured in the following way:

- You will only submit the decision sheet, which does not contain neither your own nor any other name.
- Before submitting the decisions, you will draw a leaflet with a random number from 1 to 22, which will determine which of your 22 decisions will be implemented in reality. Unless it is a nameless partner, you have to write down the name and surname of the person with that number from your names sheet into the decision sheet (near "selected person"), and underline/circle the appropriate decision. This is needed in order for the moderators to know to which person to allocate the amount of money you decided to donate (share). This person **will not be able to know who donated this money to them**. Do not discard the leaflet!
- You will also draw another leaflet with a unique **password and a number**. You will have to **write** down this number in the decision sheet (near "password number") and **keep** the password leaflet. After this, you have to put the decision

sheet **together with the drawn number leaflet** into your envelope, close it and hand it in to the moderators. You may throw away or keep the names sheet.

- You will get the 3 LVL fixed compensation in cash at the end of the experiment. However, the additional money according to one of the decisions you will get only on Monday the next week from Maija in the reception. You will have to hand in/say your password, and she will give you an envelope with appropriate sum of money in cash. This will ensure that nobody will know the exact payoff that you will get, as even the moderators will not know which decision sheet belongs to which person.
- All these procedures are done in order to ensure the highest possible confidentiality for the participants.

Appendix C

Decisions Sheet

- □ You are given 50 imaginary tokens, each worth 0.10 Ls
- □ Repeat the action below for every person from your names sheet:
- □ Decide how many tokens (from 0 to 50) you will donate/share to that person, write the donated/shared number of tokens in the appropriate place in the table (decisions 1-11). Remake and write down the decision under a different exchange rate (1:3), where each kept token is worth 0.05 Ls for you, while each donated (shared) token is worth 0.15 Ls for that person (decisions 12-22).

Number of the person from the names list	**Tokens donated/shared (from 0 to 50)**	
	Exchange rate 1:1 (each token is worth 0.10 Ls for both persons)	**Exchange rate 1:3** (tokens worth 0.05 Ls if kept and 0.15 Ls if donated/shared)
1	1)	12)
2	2)	13)
3	3)	14)
4	4)	15)
5	5)	16)
6	6)	17)
7	7)	18)
8	8)	19)
9	9)	20)
10	10)	21)
11	11)	22)

Take your time to do all the decisions and wait for the moderator to ask you to leave your seat.

After you draw a leaflet with a random number, circle/underline your decision with that number (from 1 to 22), and write down the name and surname of the person for which this decision was made here (from the names list):

…….

Leave this space empty if the underlined decision is for nameless partner!

After you draw the password leaflet, write down the number of your password leaflet here: …….

Demographic variables (You wrote them down in the questionnaire. They are needed for moderators to make data comparison in aggregated form, it does not reveal your identity!):
[coded demographic variables for separate people]

Table 3.

Summary statistics of the sample (1)

	Average	Standard deviation	Min	Max
Age N = 296	20.32	1.22	18	25
Religious index N= 296	2.13	1.04	1	5

Note. Statistics about age, and religiousness of the participants who filled in the survey

Table 4.

Summary statistics of the sample (2)

		Frequency	Percent
Gender N = 298	Female	137	45.97%
	Male	161	54.03%
Nationality N = 296[a]	Estonian	34	11.49%
	Latvian	132	44.59%
	Lithuanian	66	22.3%
	Russian	48	16.22%
	Other	16	5.41%
Home city/town[c] N = 293[a]	Big	207	70.65%
	Small	86	29.35%
Year of studies N = 298	Year 1	102	34.23%
	Year 2	98	32.89%
	Year 3	87	29.19%
	Exchange	11	3.69%
Number of brothers and sisters N = 297[a]	None	56	18.86%
	One	156	52.53%
	Two or more	85	28.62%
Religion N = 196[b]	Catholic	75	38.27%
	Lutheran	43	21.94%
	Orthodox	44	22.45%
	Other	34	17.35%

Note. Statistics about gender, nationality, home city, number of brothers and sisters, and religious beliefs of the people who filled in the survey

[a] Some people forgot to fill in this box

[b] People who indicated being not religious at all, did not have to provide their religion

[c] Cities that contained more than 100,000 inhabitants in 2008 are considered to be big

Table 5.

Summary statistics of experiment participants (1)

	Average	Standard deviation	Min	Max
Reciprocated ties N = 75	4.93	2.1	1	10
Outgoing ties N = 75	9.13	1.61	3	10
Incoming ties N = 75	8.67	4.04	1	23
Age N = 75	20.32	1.14	18	24
Religious index N= 75	2.11	2.11	1	4

Note. Statistics about degree centrality, age, and religiousness of the experiment participants

Table 6.

Summary statistics of experiment participants (2)

		Frequency	Percent
Gender N = 75	Female	34	45.33%
	Male	41	54.67%
Nationality N = 75	Estonian	12	16%
	Latvian	31	41.33%
	Lithuanian	23	30.67%
	Russian	7	9.33%
	Other	2	2.67%
Home city/town[b] N = 75	Big	53	70.67%
	Small	22	29.33%
Number of brothers and sisters N = 75	None	8	10.67%
	One	43	57.33%
	Two or more	24	32%
Religion N = 47[a]	Catholic	19	40.43%
	Lutheran	8	17.02%
	Orthodox	9	19.15%
	Other	11	23.4%

Note. Statistics about gender, nationality, home city, number of brothers and sisters, and religious beliefs of the experiment participants

[a] People who indicated being not religious at all, did not have to provide their religion

[b]Cities that contained more than 100,000 inhabitants in 2008 are considered to be big.

Table 7.

Average allocations in dictator game

	Exchange rate 1:1	Exchange rate 1:3
SD = 1 reciprocated	27.88	36.493
	(17.798)	(17.13)
SD = 1 outgoing ties	23.24	32.36
	(16.836)	(18.056)
SD = 1 incoming ties	18.213	25.747
	(15.811)	(18.929)
SD = 2 same nationality	13.413	19.92
	(12.774)	(17.042)
SD = 2 other nationality	11.28	14.427
	(13.615)	(16.518)
SD = 3 same nationality	8.32	13.787
	(9.329)	(15.159)
SD = 3 other nationality	6.693	10.827
	(8.425)	(14.122)
SD = 4	5.453	8.933
	(7.838)	(13.289)
SD = maximum observed	4.093	8.853
	(7.673)	(14.18)
Nameless	8.56	13.133
	(12.818)	(16.644)
Fictitious	3.28	6.787
	(6.068)	(11.458)

Note. Average transfer made by 75 dictators for individuals at different social distances and decisions towards nameless and fictitious partners. Standard deviations are reported in parentheses.

Table 8.

Frequencies of allocations in dictator game (exchange rate 1:1)

	SD = 1			SD =2		SD =3					
Interval	Recipr. ties	Outgoing ties	Incoming ties	Same nationality	Other nationality	Same nationality	Other nationality	SD = 4	SD = max	Nameless	Fictitious
0	10 (13.33%)	11 (14.67%)	14 (18.67%)	20 (26.67%)	28 (37.33%)	25 (33.33%)	32 (42.67%)	37 (49.33%)	47 (62.67%)	33 (44%)	49 (65.33%)
1-10	7 (9.33%)	12 (16%)	21 (28%)	20 (26.67%)	19 (25.33%)	34 (45.33%)	27 (36%)	26 (34.67%)	18 (24%)	23 (30.67%)	20 (26.67%)
11-20	8 (10.67%)	12 (16%)	8 (10.67%)	20 (26.67%)	12 (16%)	9 (12%)	10 (13.33%)	6 (8%)	5 (6.67%)	9 (12%)	3 (4%)
21-30	22 (29.33%)	21 (28%)	19 (25.33%)	9 (12%)	10 (13.33%)	5 (6.67%)	5 (6.67%)	6 (8%)	4 (5.33%)	6 (8%)	3 (4%)
31-40	3 (4%)	4 (5.33%)	4 (5.33%)	3 (4%)	2 (2.67%)	2 (2.67%)	1 (1.33%)	0	1 (1.33%)	0	0
41-49	8 (10.67%)	3 (4%)	3 (4%)	1 (1.33%)	2 (2.67%)	0	0	0	0	0	0
50	17 (22.67%)	12 (16%)	6 (8%)	2 (2.67%)	2 (2.67%)	0	0	0	0	4 (5.33%)	0

Note. Frequencies of donations falling into predetermined intervals. Proportions of donation to some particular interval are provided in parentheses.

Table 9.

Frequencies of allocations in dictator game (exchange rate 1:3)

	SD = 1			SD =2		SD =3					
Interval	Recipr. ties	Outgoing ties	Incoming ties	Same nationality	Other nationality	Same nationality	Other nationality	SD = 4	SD = max	Nameless	Fictitious
0	4 (5.33%)	3 (4%)	9 (12%)	15 (20%)	24 (32%)	21 (28%)	33 (44%)	37 (49.33%)	42 (56%)	28 (37.33%)	44 (58.67%)
1-10	5 (6.67%)	13 (17.33%)	16 (21.33%)	14 (18.67%)	19 (25.33%)	24 (32%)	17 (22.67%)	18 (24%)	13 (17.33%)	21 (28%)	13 (17.33%)
11-20	7 (9.33%)	7 (9.33%)	12 (16%)	15 (20%)	12 (16%)	10 (13.33%)	9 (12%)	8 (10.67%)	6 (8%)	10 (13.33%)	9 (12%)
21-30	11 (14.67%)	11 (14.67%)	7 (9.33%)	11 (14.67%)	8 (10.67%)	8 (10.67%)	10 (13.33%)	6 (8%)	6 (8%)	5 (6.67%)	6 (8%)
31-40	7 (9.33%)	9 (12%)	9 (12%)	9 (12%)	4 (5.33%)	7 (9.33%)	2 (2.67%)	2 (2.67%)	4 (5.33%)	1 (1.33%)	1 (1.33%)
41-49	5 (6.67%)	5 (6.67%)	3 (4%)	2 (2.67%)	1 (1.33%)	1 (1.33%)	0	1 (1.33%)	1 (1.33%)	1 (1.33%)	0
50	36 (48%)	27 (36%)	19 (25.33%)	9 (12%)	7 (9.33%)	4 (5.33%)	4 (5.33%)	3 (4%)	3 (4%)	9 (12%)	2 (2.67%)

Note. Frequencies of donations falling into predetermined intervals. Proportions of donation to some particular interval are provided in parentheses.

Table 10.

Tobit estimates (exchange rate 1:1)

	Exchange rate 1:1			
	(1)	(2)	(3)	(4)
Dummy SD = 1 reciprocated ties	36.418*	36.511*	36.575*	36.558*
	(3.675)	(3.694)	(3.694)	(3.691)
Dummy SD = 1 outgoing ties	30.727*	30.877*	30.88*	30.927*
	(3.553)	(3.583)	(3.563)	(3.579)
Dummy SD = 1 incoming ties	24.378*	24.581*	24.54*	24.618*
	(3.139)	(3.174)	(3.151)	(3.169)
Dummy SD = 2 same nationality	17.931*	18.112*	18.091*	18.107*
	(2.669)	(2.715)	(2.694)	(2.717)
Dummy SD = 2 other nationality	14.133*	14.320*	14.247*	14.267*
	(2.518)	(2.543)	(2.532)	(2.54)
Dummy SD = 3 same nationality	11.218*	11.271*	11.392*	11.359*
	(2.266)	(2.294)	(2.286)	(2.316)
Dummy SD = 3 other nationality	7.681*	7.972**	7.791*	7.981*
	(2.253)	(2.295)	(2.225)	(2.269)
Dummy SD = 4	5.041**	4.914***	5.159**	4.964**
	(2.141)	(2.225)	(2.134)	(2.195)
Dummy SD = maximum observed				
Fictitious partner		1.007*		1.2*
		(0.233)		(0.255)
Nameless partner	0.504*		0.545*	
	(0.095)		(0.098)	
Dictator is female			4.454***	3.828
			(2.676)	(2.704)
Dictator comes from large city			-6.144**	-5.999**
			(2.736)	(2.855)
Dictator is Christian			4.546***	2.056
			(2.686)	(2.722)
Dictator's incoming ties			0.226	0.694**
			(0.242)	(0.273)
Constant	-11.723*	-10.922*	-14.119*	-16.119*
	(2.844)	(2.699)	(4.2)	(4.25)
Pseudo R-square	0.0712	0.0734	0.0710	0.0701
Number of observations	675	675	675	675

1%* 5%** 10%***, Joint F test reports statistical significance at 1% level for all specifications.

Note. (1) and (3) report estimated values for the model where decision towards nameless partner was used to control for baseline altruism. (2) and (4) report estimated values for the model where decision towards fictitious partner was used to control for baseline altruism. Omitted social distance is the maximum observed one. (3) and (4) are specifications with additional covariates (only covariates that were significant in some specifications are reported). Clustered robust standard errors are reported in parentheses (standard errors adjusted for 75 clusters according to dictator's identity).

Table 11.

Tobit estimates (exchange rate 1:3)

	Exchange rate 1:3	
	(1)	(2)
Dummy SD = 1 reciprocated ties	48.088*	47.926*
	(5.695)	(5.661)
Dummy SD = 1 outgoing ties	40.859*	40.94*
	(5.447)	(5.441)
Dummy SD = 1 incoming ties	30.900*	31.16*
	(4.993)	(5.034)
Dummy SD = 2 same nationality	21.430*	21.877*
	(4.206)	(4.303)
Dummy SD = 2 other nationality	13.132*	13.48*
	(3.55)	(3.612)
Dummy SD = 3 same nationality	12.543*	12.928*
	(3.489)	(3.608)
Dummy SD = 3 other nationality	5.746***	6.188***
	(3.344)	(3.425)
Dummy SD = 4	1.991	2.013
	(2.552)	(2.661)
Dummy SD = maximum observed		
Fictitious partner		1.111*
		(0.164)
Nameless partner	0.457*	
	(0.091)	
Constant	-9.150*	-10.768*
	(3.683)	(3.413)
Pseudo R-square	0.0617	0.0849
Number of observations	675	675

1%* 5%** 10%***, Joint F test reports statistical significance at 1% level for all specifications.

Note. (1) reports estimated values for the model where decision towards nameless partner was used to control for baseline altruism. (2) reports estimated values for the model where decision towards fictitious partner was used to control for baseline altruism. Omitted social distance is the maximum observed one. Clustered robust standard errors are reported in parentheses (standard errors adjusted for 75 clusters according to dictator's identity).

Table 12.

Tobit estimates (both exchange rates pooled together)

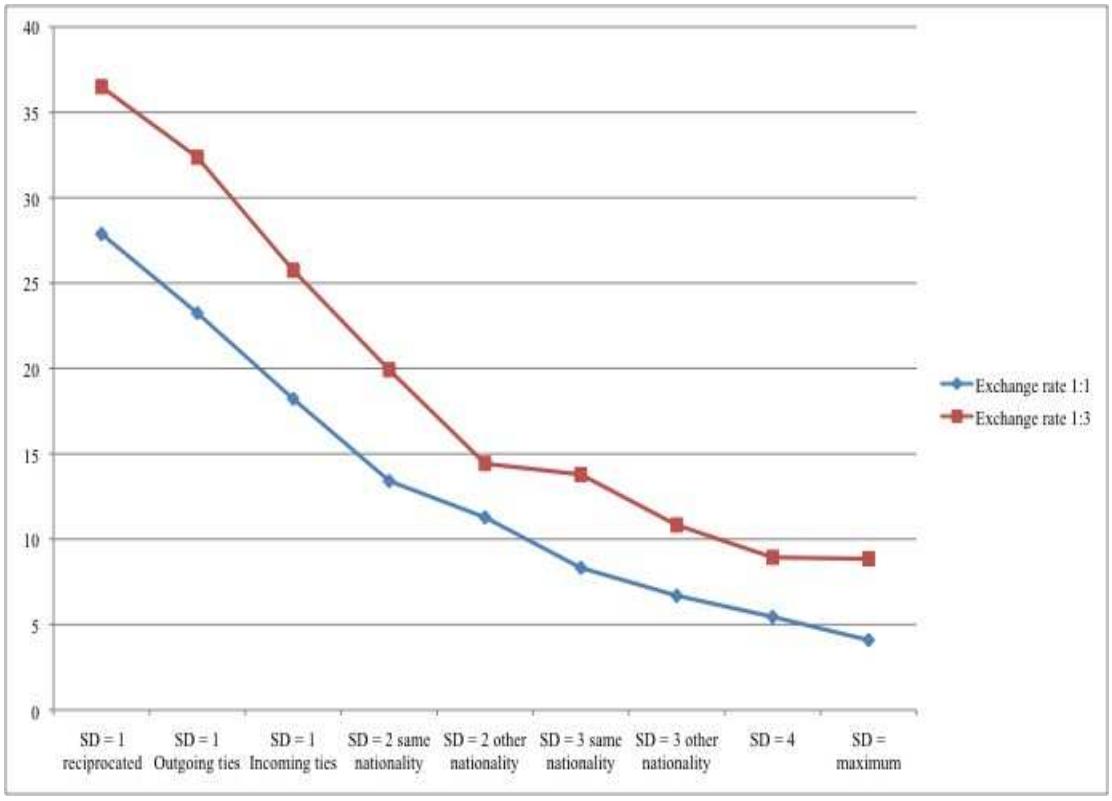
	Both exchange rates	
	(1)	(2)
Dummy exchange rate	6.566*	5.005*
	(1.976)	(1.481)
Dummy SD = 1 reciprocated ties	41.83*	41.82*
	(4.036)	(4.076)
Dummy SD = 1 outgoing ties	35.547*	35.673*
	(4.012)	(4.044)
Dummy SD = 1 incoming ties	27.55*	27.772*
	(3.637)	(3.689)
Dummy SD = 2 same nationality	19.714*	20.0*
	(3.072)	(3.152)
Dummy SD = 2 other nationality	13.678*	13.931*
	(2.703)	(2.75)
Dummy SD = 3 same nationality	11.964*	12.149*
	(2.617)	(2.691)
Dummy SD = 3 other nationality	6.902**	7.241*
	(2.657)	(2.711)
Dummy SD = 4	3.675***	3.61
	(2.191)	(2.286)
Dummy SD = maximum observed		
Fictitious partner		1.05*
		(0.151)
Nameless partner	0.474*	
	(0.071)	
Constant	-13.971*	-13.399*
	(3.176)	(3.128)
Pseudo R-square	0.0649	0.0775
Number of observations	1350	1350

1%* 5%** 10%***, Joint F test reports statistical significance at 1% level for both specifications.

Note. (1) reports estimated values for the model where decision towards nameless partner was used to control for baseline altruism. (2) reports estimated values for the model where decision towards fictitious partner was used to control for baseline altruism. Omitted social distance is the maximum observed one. Clustered robust standard errors are reported in parentheses (standard errors adjusted for 75 clusters according to dictator's identity).

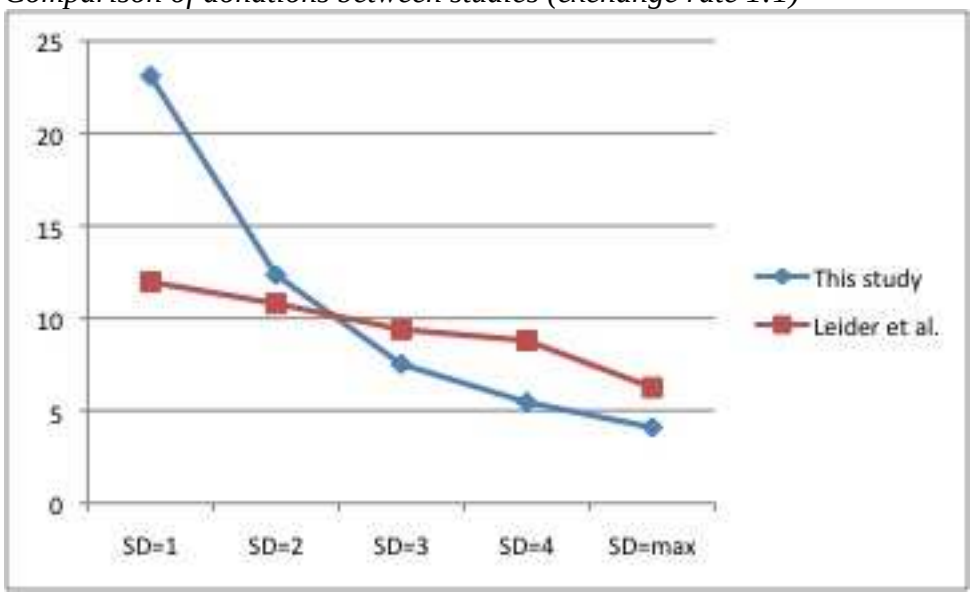
Figure 2.

Social distance effect on generosity



Note. Average number of tokens allocated for different social distances under two exchange rates

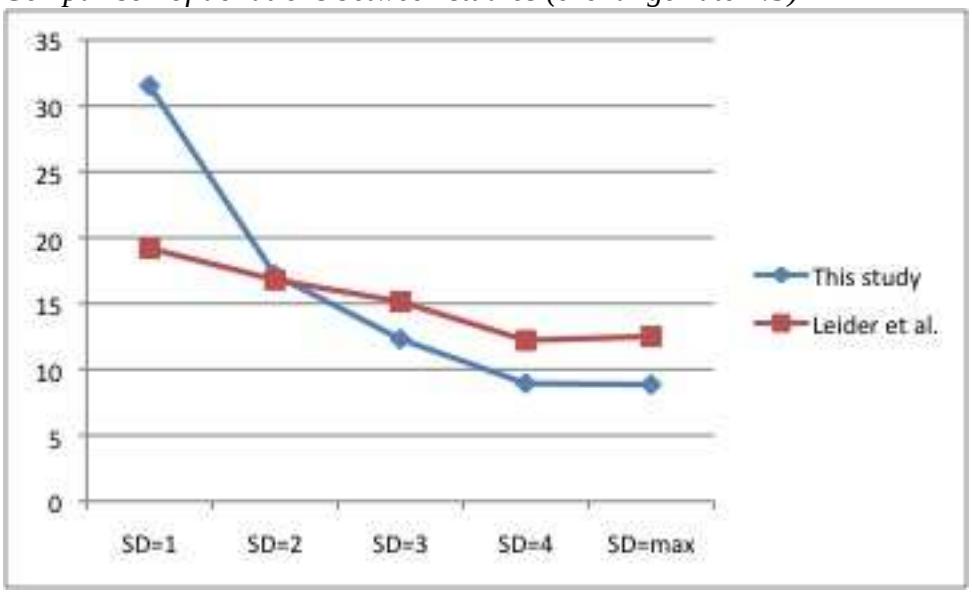
Figure 3.
Comparison of donations between studies (exchange rate 1:1)



Note. Results of this study are plotted together with findings of Leider et al. (2007). Average number of tokens passed denoted on Y-axis.

Figure 4.

Comparison of donations between studies (exchange rate 1:3)



Note. Results of this study are plotted together with findings of Leider et al. (2007). Average number of tokens passed denoted on Y-axis.