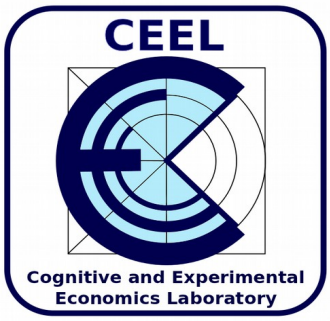


Giuseppe Danese and Luigi Mittone

On pledging one's trustworthiness through gifts: an experimental inquiry.

CEEL Working Paper 1-20

Cognitive and Experimental Economics Laboratory

Via Inama, 5 38100 Trento, Italy

http://www-ceel.economia.unitn.it
tel. +39.461.282313

On pledging one's trustworthiness through gifts: an experimental inquiry

Giuseppe Danese[1]

Philosophy, Politics, and Economics Program

University of Pennsylvania

313 Claudia Cohen Hall
249 South 36th Street, Philadelphia, PA 19104 U.S.A.
e-mail: gdanese@upenn.edu

Luigi Mittone
Dipartimento di Economia e Management and CEEL
Università di Trento
Via Inama, 5, 38121 Trento ITALY

ABSTRACT

The anthropological literature provides many instances of tokens donated in the form of a gift to woo potential trade partners, or to strengthen ties to existing partners. We study the role of gifts, as pledges of one's trustworthiness, through an experiment modeled on the trust game. We vary whether the trustee can send a token before the trustor decides whether to transfer money; whether one of the tokens is socially positioned; and whether the participants interact repeatedly or are randomly re-matched in each round. Participants in a fixed matching achieve comparable levels of trust and trustworthiness in the studies with and without tokens. In the studies with a token, trustors send significantly more points when the trustee has sent a token. A token is used more sparingly after it is socially positioned. We conclude that for institutional design, the time horizon of the relationship might be at least as important as the ability to make pledges.

JEL Codes: Z13; B52; C92.
Keywords: Pledges; Gifts; Marcel Mauss; Trust Game; Tokens.

[1] Corresponding author. We would like to thank Zev Berger, Cristina Bicchieri, Eugen Dimant, Alessandro Sontuoso, seminar participants at BEEMA V (Villanova) and three anonymous referees for helpful comments. The usual disclaimer applies.

1. Introduction

"*To breed an animal with the right to make promises-is not this the paradoxical task that nature has set itself in the case of man?*" These are the opening lines of the Second Essay of Nietzsche's *Genealogy of Morals* (Nietzsche, 1989 [or. ed. 1887], p. 57). Nietzsche reconducts morals (especially guilt) and legal institutions to the fundamental problem described in the *Genealogy*, i.e., how can the debtor make itself "calculable, regular, necessary" (*id.*, p. 58) – one might say trustworthy– in the eyes of the creditor. Creditor-debtor relations become pervasive once the economy becomes "political," i.e., when individuals transition from the simple household and kin-based structures of the subsistence economy and start to rely to a substantial degree on trading for their livelihood (Johnson and Earle, 2000, p. 26; cf. also the considerations in Graeber, 2014, pp. 76 and ff.). In his celebrated *Essai sur le don*, Mauss (2016 [or. ed. 1925]) describes the role of gift-tokens in creditor-debtor relations: "In Germanic law, every contract, every sale or purchase, loan or deposit, includes the making of a pledge; an object is given to the other contracting party, generally of little value: a glove, a coin (*Treugeld*), a blade – or, as is still the case in France, pins – that they will give back upon payment for the thing being handed over" (*id.*, pp. 172-173).

Mauss's example from Germanic law provides a glimpse into the ceremonial way in which exchanges took place in ancient societies. One finds a similar script, whereby tokens set the stage for trading relations, also in Malinowski's *Argonauts of the Western Pacific* (Malinowski, 2014 [or. ed. 1922]), which provided Mauss with ample ethnographic evidence to support Mauss's central contention: gifts are a highly obligatory "total" practice (cf. e.g., Mauss, 2016, p. 57). Malinowski gave a detailed account of the practice of the *kula*, involving exchanges of ornaments (*vaygu'a*, necklaces, and bracelets) and other goods and services. We learn from Malinowski's *Argonauts* that *kula* permeated the social life of the natives, creating opportunities for cooperation within each community (e.g., in the building of canoes), and across communities, in an area characterized by local chiefs often in conflict with each other. The local Kiriwinian language has a rich vocabulary to express the concept of gift: "…[t]he *vaga [elsewhere in the book translated as "opening gift"]* entails more wooing or soliciting than the *yotile [return gift]*. This process … consists among others of a series of solicitary gifts. One type of such gifts is called *pokala*, and consists of food … When a good valuable is known to be in the possession of a man, some of this food will be presented to him, with the words: 'I *pokala* your valuable, give it to me.' If the owner is not inclined to part with his valuable, he will not accept the *pokala*. If accepted, it is an intimation that the *vaygu'a* will sooner or later

be given to the man who offers the *pokala*. The owner, however, may not be prepared to part with it at once, and may wish to receive more solicitary gifts" (*id.*, p. 364). Rather than gloves or pins, the Trobriand Islanders described by Malinowski maintain and create relations by donating everyday consumption items.

Mauss (2016, p. 89) contended that the *kula* shares a common structural core with many other instances of competitive prestations documented in traditional societies, including the pledging observed in Germanic societies. The institutional economist might view the giving of tokens in the form of seemingly disinterested gifts as an instance of "rules in equilibrium" (Guala and Hindriks, 2015; Hodgson, 2015; Smith, 2015). The contribution of these rules and conducts to the development of a political economy was already noticed by Malinowski and many scholars who followed. Landa (1983) claims that the tokens signify one's belonging to a "club" of trustworthy traders. The attempt to establish one's trustworthiness through tokens seems especially common in societies characterized by weak formal contract-enforcement structures (cf. Greif, 1993; Voigt, 2013; Voigt, 2018; Robinson, 2013)[2].

In the absence of an external enforcer of contracts, the transfer of a token might help sustain "suitably concordant mutual expectations" (Lewis, 2008, p. 25) of trust and trustworthiness. Whether the vulnerable party of the exchange relationship places trust on the other party will crucially depend on these expectations (cf. Bicchieri et al., 2011). This is especially true in one-shot encounters, or among new partners. Among partners who interact repeatedly, trustworthiness can be established by sending a token, as in the one-shot encounters, but also through an examination of the other's past conduct[3].

Our experimental design is based on variants of the trust (or investment) game.[4] We introduce one crucial modification on this well-known game: the trustor, before making its decision,

[2] Cf. also Benveniste (2016 [or. ed. 1969], p. 76) on pledges and trust being part of the same "institution" (p. 76). Similar considerations can be found also in Graeber (2001, p. 126).

[3] Similar considerations can be found in Duffy & Feltovich (2002), discussing the effect of cheap talk, which one can conceive in the language of this paper as a verbal pledge, versus the effect of observation of actual play in experimental games.

[4] Cf. the seminal Kreps (1990) and Berg et al. (1995); for the repeated version of the game, Anderhub et al. (2002) and Engle-Warnick and Slonim (2004); for the trust game with communication, Bicchieri et al. (2010); for a discussion of the motives for trustworthiness, Cox et al. (2016); Vollan (2012) studies a "within-culture across-country" trust game; for a meta-analysis of findings, Johnson & Mislin (2011) and Holt (2019, pp. 280-298). On promises, a term semantically close to *pledge*, cf. Vanberg (2008); Charness & Dufwenberg (2006); Rousseau (2001); Habib (2018). On signals, another term closely related to pledge, cf. Fudenberg and Vespa (2019) and the

might receive a token from the trustee. Our experimental design allows us to study three questions: whether trustors place more trust on the trustees when the trustees donate a token; whether the trustees' donation of a token is positively related to trustees' trustworthiness, or rather a "bait" that trustees use in an attempt to induce the trustors to transfer money to them; and whether *any* gift can function as a pledge of one's trustworthiness. An ancillary question concerns how pledging among fixed partners differs from pledging among strangers. To anticipate, the results of this paper show that trustors send more points after receiving a token; "baiting" behavior is not the most common, but it happens in about one-third of the cases among random couples; couples in a fixed pairing are able to reap the benefits of trust and trustworthiness to a much larger extent than random couples, regardless of whether the tokens were available or not.

2. Materials and Methods

We recruited 120 University of Trento, Italy, (undergraduate) students to participate in the experiment. We randomly assigned participants to one of six studies (two controls and four treatments[5]). The sample of participants was gender balanced. Participants won on average 12 euros, not including a show-up fee of three euros. The experiments lasted on average 1h.

The control study with random couples (*Control-Random*) is a repeated trust game. Participants are randomly seated in the experimental room, after which instructions are read aloud. After a comprehension test has been individually checked, the experiment starts. Each participant is randomly matched with another participant from the pool of those present in the room. The system randomly assigns to one participant the role of the trustor[6] and to the other the role of the trustee. The randomizations of couples and roles are carried out independently of each other in each round. The couples can be re-matched but are informed that the system imposes an embargo period for re-matching of 10 rounds. In this way, we attempted to neutralize any re-encounter concern in this study. Each trustor and trustee is endowed with ten experimental points (10 euros) in each round. The trustor decides first how many points to send to the trustee.

essays in Skyrms (2010). We do not discuss here the subtle differences between these terms, and we stick to the term pledge throughout.

[5] The experiment was computer-based and programmed in *oTree* (Chen et al., 2016).

[6] The terms "trustor", "trustee", "give," "return" were never used in the experiment, replaced by the letters A and B for the roles, and a plain Italian verb that would roughly translate into "to pass" to denote the transfer of points from one participant to the other. The instructions do not use any "contextual" language (Alekseev et al., 2017). The choice of *not* using contextual language was dictated by the desire to compare results between the controls, to which no meaningful context can be attached apart from a generic situation of trust and vulnerability, and the treatments, inspired to the Maussian example from Germanic societies discussed in the Introduction.

The trustee receives the amount sent by the trustor, doubled by the experimenter. The trustee decides how many points to return to the trustor, from zero to his/her entire endowment. The participants are then shown their payoffs. The experiment at this point restarts. Points earned in earlier rounds are *not* carried forward to the next round. At the end of the 20th round, the participants fill in a debriefing and demographic questionnaire. We pay the participants according to their earnings in one randomly-extracted round, plus a show-up fee of three euros. All features of the experiment are communicated to the participants in the instructions.[7]

The control study with fixed couples (*Control-Fixed*) is equivalent to the *Control-Random* study, save for the fact that the participants in a couple know they will be interacting for the entire duration of the experiment. Roles are, as usual, randomized in every round[8].

In the treatment study *Tokens-Random,* each trustor and trustee is endowed with five tokens, shown as pictures on their screen, and ten experimental points (as in the controls described above). The tokens shown to the participants were an eraser, a pencil sharpener, a candy, a green bill with "1,000" and "Monopoli[9]" written on it, and a rubber elastic. The tokens were chosen for their simplicity, relatability, and low market price. We chose to use photographic depictions of actual items, rather than the purely "virtual" tokens used by Camera et al. (2013) and Bigoni et al. (2019)[10] in an attempt to approximate (highly imperfectly, we reckon) the ancient Germanic arrangement described in the Introduction.

Unlike in the control studies, in the *Tokens-Random* study the trustee moves first and decides which, if any, of the five tokens he/she wishes to send to the other participant. The trustor observes the choice of the trustee[11] and decides how many points to send to the trustee. The trustee receives the amount sent by the trustor, doubled by the experimenter (as in the control study). The trustee decides then how many points to return to the trustor, as in the control study. The participants are shown their payoffs. The experiment restarts, with random re-matching in every period, and the number of rounds set at 20, as in the control studies. Participants carry

[7] Instructions for this study are available upon request from the corresponding author.
[8] Instructions for this study are available upon request from the corresponding author.
[9] The spelling in Italian of the popular game.
[10] The other crucial difference between these two studies and ours is that our game is a sequential "trust" game, while theirs is a simultaneous-move "helping" game.
[11] If the trustee had no token available, the trustor was informed of the circumstance when his/her turn came. We tried to avoid in this way trustors assuming the trustees did not intend to send a token, when actually none was available for the trustee to send.

over their objects from one round to the next. Participants do not carry over their points from one round to the next, as in the Control studies. After the questionnaire, participants are paid their earnings in one randomly extracted round. Participants also received as part of their payout all the tokens in their availability at the end of the last round -- the actual tokens, not the photographic depictions of the tokens. All features of the game were common knowledge, including the payout rules for money and tokens.[12]

In the treatment study *Tokens-Random-Voting*[13], before the repeated trust game with tokens and random couples described just above for the study *Tokens-Random*, each participant was asked to vote for one of the tokens. The participant who voted for the most voted article in an experimental session, in the shortest amount of time, won three euros. After the choices of the participants were recorded, the system showed all participants the number of votes received by each token. To avoid creating inequality in endowments, we did not communicate who the winner was until the end of the experiment. This voting game is an adaptation of the newspaper "beauty contest" described in the *magnum opus* of J. M. Keynes (1973 [or. ed. 1936], p. 156; cf. also Nagel, 1995)[14]. We devised the photo competition to break the symmetry among the tokens, in such a way that a subset of the tokens becomes "socially positioned." We borrow the term "socially positioned" from Lawson (2016), who claims that social positioning is a key step in the process that leads to the constitution of mere tokens into "money." In this paper, we take socially positioned as meaning the same as salient in Lewis (2008)[15]: a "basis for common knowledge" *(id., p.* 56) exists that a subset of the tokens is special. This basis arises when we show to all participants the number of votes received by each token. For simplicity, we assume that the item that is socially positioned, through the voting manipulation, is the most voted token. The trustees might choose to send the socially positioned token to reinforce the value of their pledge.

The study *Tokens-Fixed* is, in all regards, equivalent to the study *Tokens-Random*, save for the fact that the couples are now fixed. The same is true for the study *Tokens-Fixed-Voting* and *Tokens-Random-Voting*.

[12] Instructions for this study are available upon request from the corresponding author.
[13] The instructions for this study can be found in Appendix.
[14] The beauty contest game used by Nagel involved the choice of numbers, rather than pictures. Several papers test the role of "labels" attached to the strategies (Andreoni, 1995; Larrick and Blount, 1997; Dufwenberg et al., 2011).
[15] While differences exist between the rise of conventions à la Lewis and Lawson's social positioning approach to money, we do not further elaborate on differences here and stick to "social positioning" throughout.

A feature of our design deserves re-stating. In all our studies, the participants *do not* carry earnings from one round to the next. In the treatment studies, the participants instead *do* carry the tokens over from one round to the next. Allowing participants to carry over their earnings would create differences in the strategy spaces from the second round and on. Some trustors could also start a round with no points at all. We believed this heterogeneity to be undesirable in the framework of our research questions. Substantial differences in endowments could give rise in fact to well-documented behavioral phenomena, such as inequity-aversion and envy. The way to interpret our choices regarding the availability of tokens and money throughout the experiment is that money is available in each period, to all participants, in the same amount. The tokens are instead relatively abundant (each participant starts with one each of the 5 item types), but not unlimited.

In the same spirit of guaranteeing a high degree of uniformity across participants and rounds, we randomized the roles of trustee and trustor in each round. In this way, we avoided having one participant, the trustor, always being the vulnerable one, and the other, the trustee, necessitating many tokens.

3. Results

3.1 Descriptive statistics

The first question we address is the effectiveness of our three manipulations: whether tokens circulated or not, and nested in this manipulation whether the token was socially positioned through the contest or not; and the matching technology. The first indication of manipulation effectiveness comes from nonparametric tests[16] on the median amount sent (by trustors) and sent back (by trustees) across the six studies. These tests find significant differences across the studies ($p<0.001$). There is no evidence from two-sample Wilcoxon rank-sum (Mann-Whitney) tests that amounts sent or returned were different when the tokens were circulating and when they were not. There is clear evidence that the matching technology affected both decisions ($p<0.001$). The vote appears to have marginally affected the amount sent ($p=0.041$), but not the amount returned.

[16] The nonparametric tests do not correct for the fact that participants make repeated choices in the course of the experiment. This is an aspect of our data that is explicitly tackled through panel regression analysis later in the paper.

In the treatments with voting, the Monopoly bill was the most voted item, receiving 50% of the votes. In the two studies with voting, the most common choice was *not* to send any object, which occurred in about one-third of the cases, followed by the rubber elastic, sent in about one-fifth of the cases. The Monopoly bill was the token sent least frequently. In the sessions with tokens circulating, but no vote, the most common choice was to send the Monopoly bill (in about one-fifth of the cases). Participants in all studies likely saw the Monopoly bill as salient, but we have initial evidence that only in the studies with voting the subjects sent the Monopoly bill sparingly.

Table 1 presents summary statistics related to the trustee, who might send the token in the treatments in which a token circulated, and send back money to the trustor. Upon first inspection, the amount returned (*sentbackamount*) in the studies with fixed couples appears much higher than in the studies with random couples. A token was sent in more than 70% of cases in all studies. The Monopoly bill appears to have been sent less when the study incorporated the voting procedure. The unavailability of the Monopoly token was an issue in some studies, but participants rarely had no token to send at the end of a period.

Table 1: Summary statistics for the trustees' behavior[17]

Control-Fixed					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
sentbackamount	200	7.315	5.974	0	30
Control-Random					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
sentbackamount	200	1.415	2.598	0	16
Tokens-Random-Voting					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
monopolyDummy[18]	194	0.113	0.318	0	1
tokenDummy[19]	194	0.716	0.452	0	1
noObjectEndofRoundDummy	200	0.045	0.208	0	1
noMonopolyEndofRoundDummy	200	0.315	0.466	0	1

[17] We use STATA 16 ® for all statistical analyses presented in the paper. Figures are also produced through the same software.
[18] The observation is set to missing when the participant had no Monopoly bill available.
[19] The observation is set to missing when the participant had no token available.

sentbackamount	200	1.7	2.675	0	15
Tokens-Fixed-Voting					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
monopolyDummy[20]	170	0.094	0.293	0	1
tokenDummy	200	0.71	0.455	0	1
noObjectEndofRoundDummy	200	0	0	0	0
noMonopolyEndofRoundDummy	200	0.2	0.401	0	1
sentbackamount	200	6.295	5.497	0	15
Tokens-Random					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
monopolyDummy[21]	196	0.255	0.437	0	1
tokenDummy[22]	196	0.826	0.37	0	1
noObjectEndofRoundDummy	200	0.035	0.184	0	1
noMonopolyEndofRoundDummy	200	0.395	0.49	0	1
sentbackamount	200	0.755	1.213	0	6
Tokens-Fixed					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
monopolyDummy[23]	142	0.317	0.467	0	1
tokenDummy[24]	188	0.798	0.402	0	1
noObjectEndofRoundDummy	200	0.105	0.307	0	1
noMonopolyEndofRoundDummy	200	0.415	0.494	0	1
sentbackamount	200	7.655	6.313	0	30

Table 2 shows the summary statistics for the trustor, who might send points to the trustee, and who observes whether a token was sent (in the studies with a token). Trustors send more points when they are in a fixed pair.

Table 2: Summary Statistics for the trustor's behavior

Control-Fixed					

[20] The observation is set to missing when the participant had no Monopoly bill available.
[21] The observation is set to missing when the participant had no Monopoly bill available.
[22] The observation is set to missing when the participant had no token available.
[23] The observation is set to missing when the participant had no Monopoly bill available.
[24] The observation is set to missing when the participant had no token available.

Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
amountSent	200	5.85	3.26	0	10
Control-Random					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
amountSent	200	2.09	2.564	0	10
Tokens-Random-Voting					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
amountSent	200	2.55	2.75	0	10
Tokens-Fixed-Voting					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
amountSent	200	5.075	3.45	0	10
Tokens-Random					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
amountSent	200	1.35	1.581	0	10
Tokens-Fixed					
Variable	*Obs*	*Mean*	*Std. Dev.*	*Min*	*Max*
amountSent	200	5.98	3.988	0	10

Figure 1 shows the average amount sent by trustors in each of the six studies. The mean and the standard errors are obtained by pooling observations of all participants in each study, from twenty rounds of play. The study *Tokens-Random* features lower levels of trust than all other studies. Studies with fixed couples exhibit higher levels of trust than studies with random couples. The same pattern is confirmed by inspection of the points returned in the six studies (Figure 2). We defer a discussion of the statistical significance of these effects to the next section.

Figure 1: amounts given in the six studies[25]

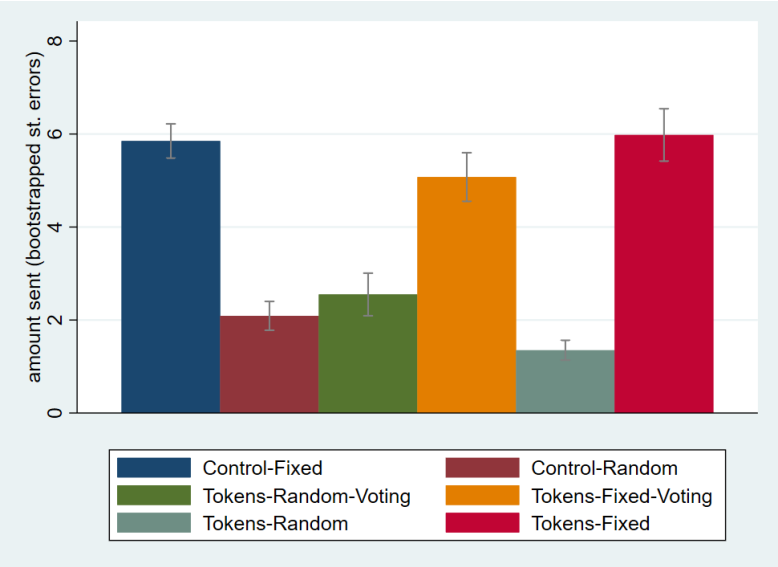


Figure 2: amounts returned in the six studies

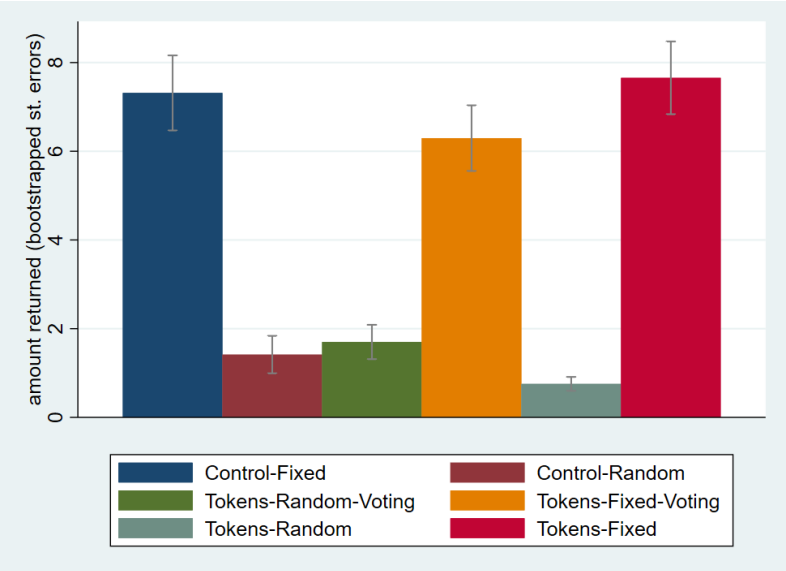
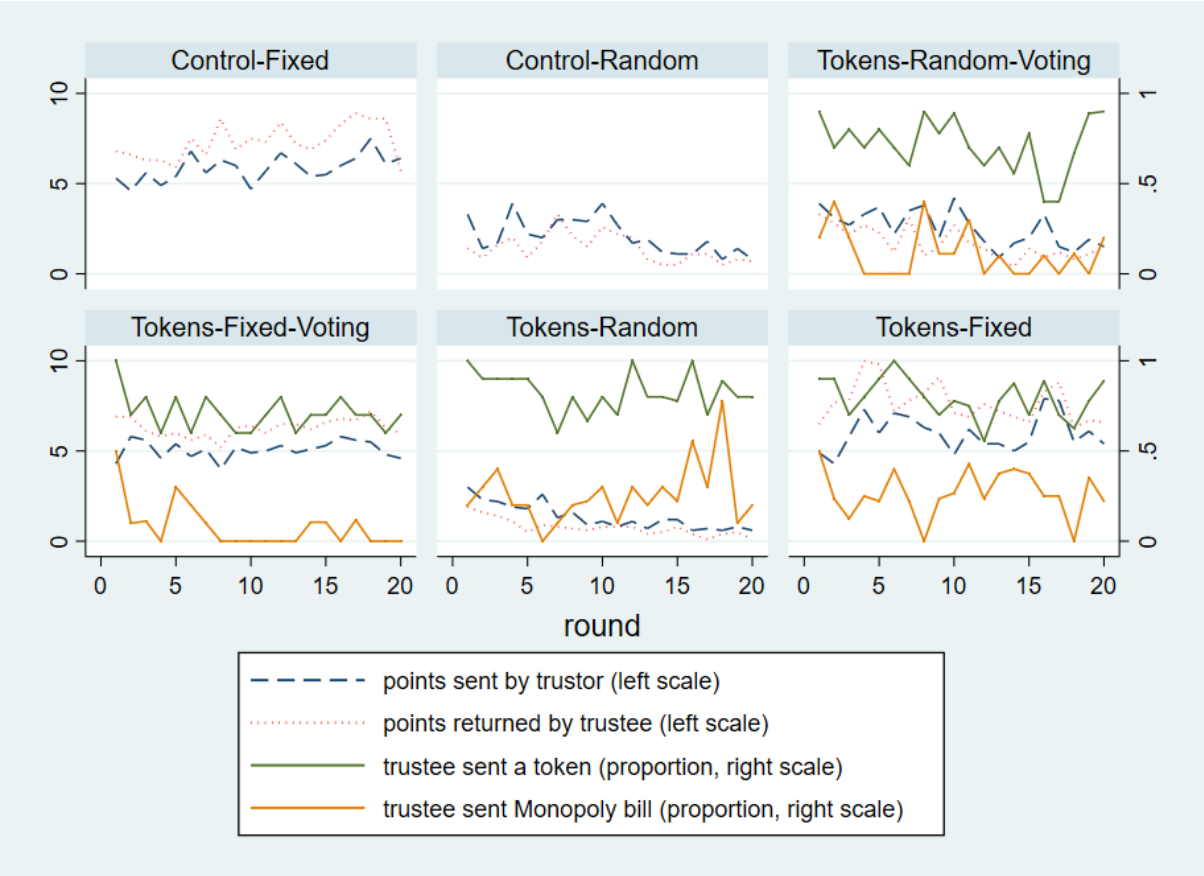


Figure 3 shows the evolution in the course of the twenty rounds of play of 4 keys variables: the points the trustor sent to the trustee (on average in each round, for each of the six studies), the points the trustee sent back, the proportion of tokens sent (when at least one token was available to be sent), and the proportion of Monopoly bills sent (when at least a Monopoly bill was available to be sent).

[25] Bootstrapped standard errors are based on 500 repetitions.

Figure 3: behavior in each round



In all studies with fixed couples, points returned were, on average, higher than points sent. The opposite is virtually always the case with random couples. In terms of the tokens, it is apparent that much experimentation took place in the course of the experiment. Interestingly, in the study *Tokens-Fixed-Voting* about half of the Monopoly tokens available to trustees in round 1 were sent. After round 5, however, very few Monopoly bills circulated.

Figures 4 and 5 show, respectively: the behavior of trustors in response to the trustees' decision to send or not one of the tokens; the behavior of trustors in response to the trustees' decision to send or not the Monopoly bill. In the study *Tokens-Fixed-Voting*, trustors sent *more* when they did *not* receive a token. Inspection of the trustees' behavior in this study (Figure 6) shows that trustors were correct in anticipating that receiving a token meant receiving fewer points back, hence lowering the willingness to send points after receiving a token. One might speculate that trustees in the study *Tokens-Fixed-Voting* used the tokens and money as substitutes rather than complements. We return to this consideration in the final remarks. In all other studies, trustors appear to send more points after receiving a token. The trustees' decision to send a Monopoly

bill (or not) seems not to have played any major role in the trustors' choices. On average, receipt of a Monopoly bill was associated with an increased willingness to send points, in all studies.

In Figure 6, we try to give a first answer to the question of the trustees' intentions when sending a token. In the study *Tokens-Fixed-Voting*, the trustees sent back sharply less when they had earlier in the round sent a token. In all other studies, trustees appear to send back more points after sending a token.

Figure 4: the trustor's decision in response to the trustee's decision to send a token (or not)

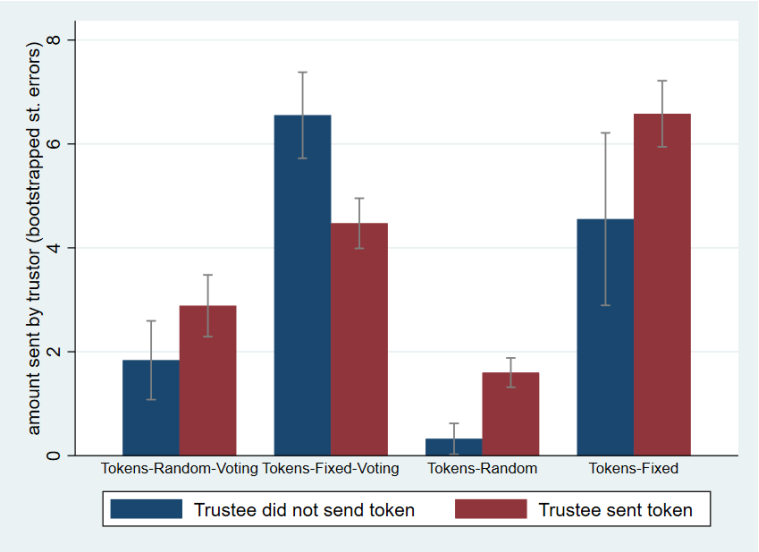


Figure 5: the trustor's decision in response to the trustee's decision to send the Monopoly bill (or not)

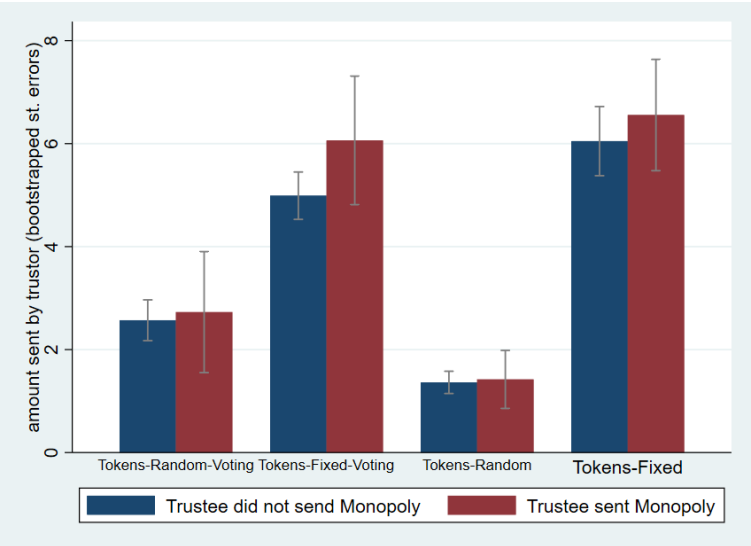
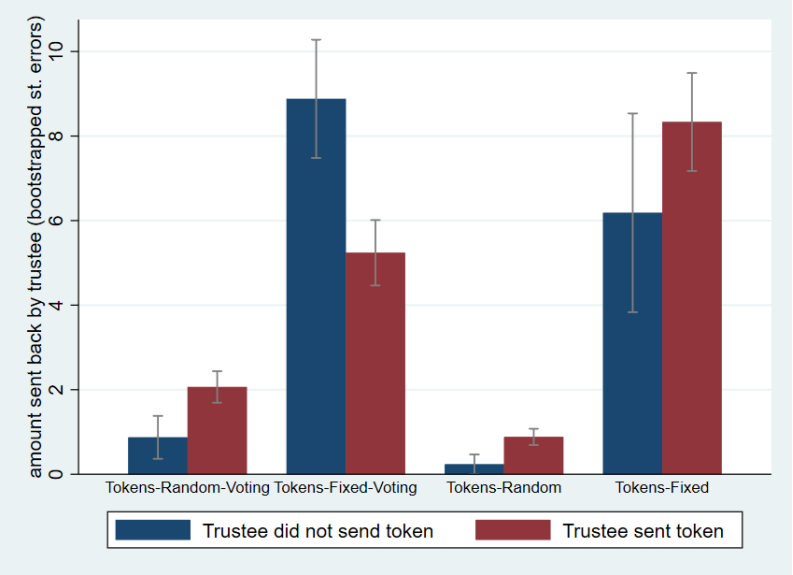


Figure 6: the trustee's "money" decision after the trustee's "token" decision



3.2 Regression analysis

Participants were observed for 20 rounds, on average ten times as trustees and ten times as trustors. We create two separate panels, one for trustees and one for trustors. In each round, a participant can either appear in the trustor or the trustee panel. The panel is, therefore, unbalanced and with gaps.

We first estimate, using a random-effects (RE) panel regression model, the impact of receiving a token (dummy-coded), the voting manipulation (dummy-coded), being in a fixed pairing (dummy-coded), cross-terms, a time trend, and controls, on the amount sent by trustors. The observations come only from the four studies in which the tokens circulated. The estimated sample size to achieve a power of 0.8 with eleven regressors, and the usual .05 significance level, is 44, well below our number of participants (80). The regression output is shown in Table 3.

Table 3: the dependent variable is the amount sent by the trustor

	Observed Coef.	*Bootstrapped Std. Err.*[26]
Regressor		
tokenDummy	1.300***	0.206
votingDummy	1.303*	0.546
tokenDummy & votingDummy	-0.321	0.445
fixedcouplesDummy	4.642***	0.917
tokenDummy & fixedcouplesDummy	-0.322	0.651
votingDummy & fixedcouplesDummy	-1.503	1.409
tokenDummy & votingDummy & fixedcouplesDummy	-0.464	0.999
timeTrend	-0.044*	0.019
maleDummy	1.243*	0.549
age	0.201	0.197
constant	-4.034	4.224

***p<0.001, *p<0.05. Overall R-squared = 0.34.

Receiving a token, the voting and the fixed-couple manipulations are associated with significantly higher amounts sent—greater trust.[27]

[26] The STATA ® command is: `xtreg sent_amount tokenDummy##votingDummy##fixedcouplesDummy timeTrend maleDummy age, vce(bootstrap, reps(500) seed(200)).`

[27] Dropping the first round, in which participants might have experimented to a larger degree than in later rounds, leads to equivalent inferences (using again RE as in Table 3). The dependent variable suffers from truncation at 0 and 10. Estimating the coefficients through panel Tobit (with bootstrapped standard errors, 500 repetitions) leads to equivalent signs and p-values, save for the gender dummy, whose coefficient is positive (as in Table 3) but insignificant. Fixed effects (FE) estimation allows only the estimation of *tokenDummy* (significant), some of the cross terms (all insignificant) and the time trend (significant). The Hausman test, conducted comparing regression coefficients that can be estimated through *both* FE and RE (i.e., only the time-varying regressors), finds significant differences between the two sets of coefficient estimates. We do not believe this brings a serious challenge to our RE estimates: the key regressor, *tokenDummy*, can be estimated through both methods, and it is significant in both. Also, Dieleman & Templin (2014, p.9) report, based on simulations, that occasionally it is preferable to use RE when this involves a small-enough bias, rather than opting for the less precise FE estimator. A regression of the amount sent on a dummy for the tokens (circulating or not), the matching technology, the interaction of these two dummies, controls, and a time trend, using data from *all* six studies, finds that amounts sent are significantly higher in fixed couples. The dummy for the tokens is positively related to the sending decision, but insignificantly so. Male trustors send significantly more points in this regression.

We move now to a RE regression of the amount returned by the trustee on manipulation dummy variables, the number of points sent by the trustor in that round (before the multiplication), interaction terms, controls, and the total number of tokens the trustee sent in the rounds up to and including the previous round in which he/she was a trustee[28]. The estimated sample size to achieve a power of 0.8, and the usual .05 significance level, with twelve regressors, is 18, again below our number of participants. The regression output is shown in Table 4.

Table 4: the dependent variable is the amount sent back by the trustee

	Observed Coef.	*Bootstrap Std. Err.*
Regressor		
amountTrustorSent	0.401**	0.137
votingDummy	-0.375	0.322
votingDummy & amountTrustorSent	0.244	0.186
fixedcouplesDummy	1.800	1.111
fixedcouplesDummy & amountTrustorSent	0.499*	0.240
votingDummy & fixedcouplesDummy	-1.120	1.289
votingDummy & fixedcouplesDummy & amountTrustorSent	-0.042	0.304
timeTrend	0.016	0.040
tokensTrusteeSent	-0.075	0.083
maleDummy	1.106*	0.501
age	0.018	0.124
constant	-0.453	2.675

**p<0.01, *p<0.05. Overall R-squared = 0.8.

[28] For example, the participant in our panel of trustees with personal identifier "1" (*Tokens-Random-Voting* study) played as a trustee 7 rounds (round numbers: 1, 3, 5, 12, 13, 18, 20). He/she sent a token in each round except round 5. The regressor $tokensTrusteeSent_{1,t}$ takes in our dataset the values: "." (missing, no previous history of tokens sent at round 1), 1, 2, 2, 3, 4, 5. One can interpret this variable as the trustee's track record of token behavior. This is a regressor that we have included to ascertain if the trustees use the tokens as a "bait."

We find a significant positive relationship between the amount sent and the amount returned. The size of the coefficient, below 1, tells us that for most trustors, the payback amount was lower than the amount sent. This result is in line with earlier results from trust games, showing that "responses are variable, with some second movers returning nothing and others returning a little more than what was passed. On average, amounts returned are close to the amounts originally passed (before being tripled [doubled in our case]). So the "investment" is generally not profitable ex post" (Holt, 2019, p. 282). We have already observed, while inspecting Figure 3, that in all studies with random couples points returned were less than points sent, a trend which counteracts the opposite trend observed in the sessions with fixed couples. We also find in Table 4 a positive and significant interaction term between the amount received and the fixed couples dummy: among fixed pairs, there is a tighter relationship between amounts sent and amounts returned. The number of tokens the trustee sent is negatively related to the dependent variable, but insignificantly so.[29] Male participants send back more points.[30]

We conducted a debriefing survey at the end of all our sessions. Roughly 40% of the respondents (in the treatment studies) reported that participants might have stockpiled tokens to use them "later." The same percentage answered that the participants did not send the tokens because they thought them unhelpful. The remaining respondents provided a free-form answer. We also elicited, for each token, the perceived usefulness in the experiment and the estimated market price of each token. The rubber elastic was chosen as the most useful token (the average

[29] Dropping the first round leads to equivalent inferences (using again random effects as in Table 4). The dependent variable suffers from truncation at 0 and 30. Estimating the coefficients through panel Tobit, the effect of the amount received is still positive and significant. All other regressors are insignificant, including the coefficient of *male*, which is of the same sign as shown in Table 4. The results of panel Tobit are essentially in line with the results of linear regression. FE estimation yields a significant relation between amount received and amount sent back, and significant positive interactions between the amount received and the vote and the fixed-couple dummies. The Hausman test, conducted comparing regression coefficients that can be estimated through both FE and RE, finds significant differences between the two sets of coefficient estimates. The key regressor, *amountTrustorSent*, can be estimated through both methods, and it is significant in both. A regression of the amount sent back on the amount sent, a dummy for the tokens (circulating or not), the matching technology, interactions, controls, and a time trend, using data from the *treatments and controls*, finds that amounts received are in a positive, significant, and below-unit relation to the amount sent back. The cross-term between the amount received and the fixed couples dummy is also significant and positive. All other regressors are insignificant.

[30] Regarding the effect of gender on the dependent variables (amount sent and amount sent back), the current consensus in trust games is that "male subjects tend to pass more in trust games, and female subjects return a higher percentage of funds available to them in the second round. This is generally interpreted to mean that male subjects are 'more trusting,' and female subjects are 'more trustworthy' in this context" (Holt, 2019, p. 289). RE estimation shows that our male participants were more trusting, as in earlier studies, but also more trustworthy. Tobit estimation finds the same signs, but cannot exclude a null effect of gender. The question of gender is peripheral to the scope of this paper, but we believe our results speak to the overall representativeness of our sample of participants.

score is 7.3 out of 10, with ten being "very useful"), but also the one with the lowest price (16 cents on average). The participants perhaps felt that the inexpensive tokens were the most helpful as a pledge, a mere "token of appreciation," one might say. The most expensive item, according to the respondents, was the sharpener (92 cents). The average price of the Monopoly bill, after removing the "1,000" that a small minority of respondents reported, was 36 cents.

3.3. Robustness check

We suspect that some participants might have grappled with the tokens. Some participants, as we saw from the questionnaire answers, thought that the tokens might have been useful in the future, perhaps in future experiments. Other participants seemed unsure about the usefulness of the tokens altogether. We speculate that some participants might have failed to see the logical progression from the vote on the tokens to the modified trust game. We were also concerned about our choice to give out the tokens for free at the beginning of the modified trust game. The tokens might have been perceived as a cheap means of pledging.[31]

We devised a new treatment study, *Tokens-Random-BDM*, to address these concerns. Instructions were amended in two essential regards[32]: first, the tokens in this new study were for purchase. Participants were given a larger endowment than in earlier studies, 10 euros, which they could use to purchase the same five tokens we used in the earlier studies with tokens. The participants stated a maximum buying price, between zero and two euros, in increments of ten cents. They could purchase each token if the buying price was lower than or equal to a random number between 0 and 2, in increments of ten cents. The extraction of the number was done in front of the participants by a confederate, with the replacement of the extracted numbers. Participants were explained in the instructions that under these rules – the well-known procedure of Becker, DeGroot & Marschak (1964) - they had no incentive in misrepresenting their valuation of the tokens. Second, we stated in the instructions that the participants would *not* receive the tokens at the end of the experiment. We further stated that the participants' willingness to pay to acquire the tokens should mirror the perceived usefulness of each token *in the experiment*. Through these amendments, we wished to convey a clear message to the participants: the tokens did not have a life outside the lab room, and they were

[31] On the issue of the costliness of signals, cf. Zollman et al. (2013).
[32] The instructions for this study can be found in Appendix.

not freely available. The participants were left free to decide whether to acquire the tokens for use in the experiment, and the price at which they wished to acquire the tokens.

After the participants express their prices, and the extractions, the system informs the participants of the tokens they had purchased and of the average buying prices of all the tokens. Calculations of average prices are based on the choices of those present in the experimental room. We showed the average prices to mimic the social positioning procedure in the studies with voting, where we showed the number of votes received by each token.

We recruited 24 new participants for this study, from the same pool of participants of the earlier studies (but excluding students who had taken part in our earlier studies). Table 5 shows descriptive statistics for the participants' (maximum) willingness to pay for the tokens.

Table 5: willingness to pay for the tokens

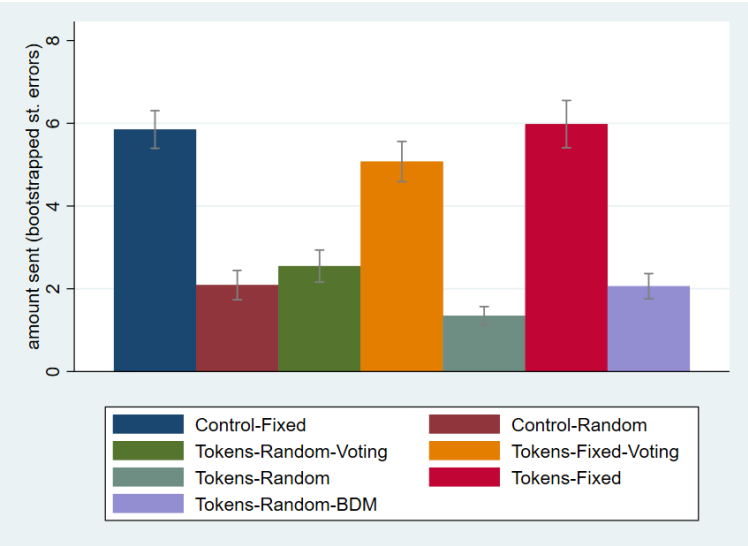
	Obs	*Observed Mean*	*Bootstrapped Std. Err.*	*Normal-based [95%] Conf. Interval*	
Objects					
Candy	24	37.727	7.054	23.902	51.552
Elastic	24	21.364	6.552	8.521	34.206
Eraser	24	10.364	0.875	8.650	12.078
Monopoly bill	24	66.364	13.938	39.045	93.682
Sharpener	24	114.546	9.577	95.775	133.315

The highest stated prices are for the sharpener (identified as the most expensive item in the questionnaire of the earlier studies) and the Monopoly bill. The confidence intervals of all tokens do not include zero. The participants thought it worthwhile to purchase the tokens, with a total willingness to pay for the 5 tokens, on average, of €2.50.

For comparison with the new study, the closest study is *Tokens-Random-Voting*. Both in *Tokens-Random-Voting* and the new *Tokens-Random-BDM* study, we introduced a manipulation to socially position the tokens. The trustors' giving behavior in the two studies appears indistinguishable (marginally significant differences according to a t-test, and

marginally insignificant according to the rank-sum test; cf. Figure 7, which replicates Figure 1, updated with the average number of points sent in the new treatment).

Figure 7: amounts given in the seven studies



No differences, using both parametric and nonparametric tests, can be detected in the trustees' decision regarding how many points to send back. Some differences are discernible in the use of the tokens. Table 6 displays a comparison of 4 variables pertaining to the trustees in the two studies *Tokens-Random-Voting* and *Tokens-Random-BDM*, with confidence intervals.

Table 6: comparison of the trustees' behavior in *Tokens-Random-Voting* and *Tokens-Random-BDM*

	Obs	*Observed Mean*	*Bootstrapped Std. Err.*	*Normal-based [95%] Conf. Interval*	
monopolyDummy					
Tokens-Random-Voting	388[33]	0.113	0.023	0.068	0.159
Tokens-Random-BDM		0.222	0.032	0.159	0.284
tokenDummy					
Tokens-Random-Voting		0.716	0.031	0.655	0.778
Tokens-Random-BDM		0.830	0.027	0.777	0.882

[33] The variable is set to missing whenever the trustee had no Monopoly bill available.

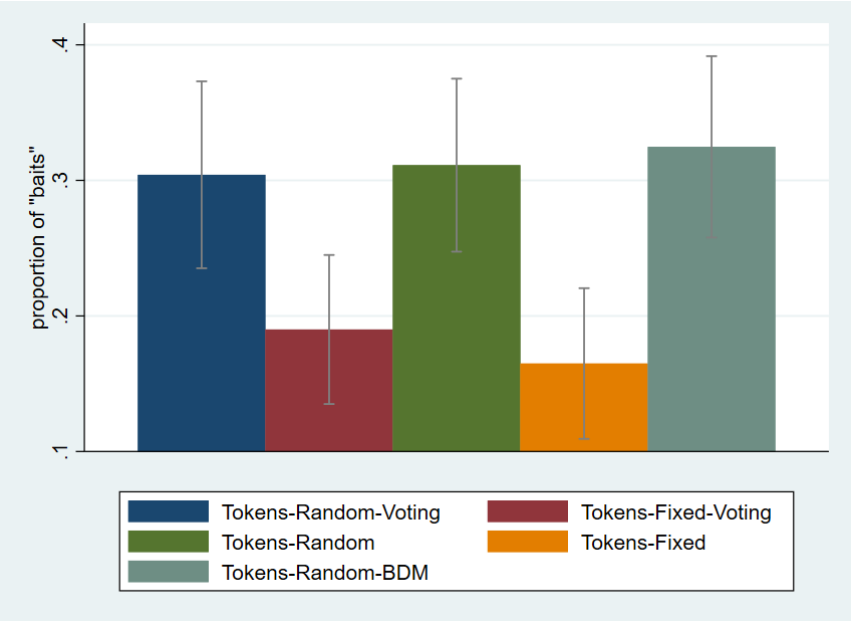
	388[34]				
noObjectEndofRoundDummy					
Tokens-Random-Voting	440	0.045	0.016	0.015	0.075
Tokens-Random-BDM		0.329	0.031	0.269	0.390
noMonopolyEndofRoundDummy					
Tokens-Random-Voting		0.315	0.033	0.250	0.380
Tokens-Random-BDM	440	0.592	0.032	0.529	0.654

More Monopoly bills, and more tokens in general, were sent in the *Tokens-Random-BDM* study. Perhaps as a result of this increased profligacy in *Tokens-Random-BDM*, but also as a result of the fact that fewer tokens were available to start with, in *Tokens-Random-BDM*, participants were left with no tokens at all in about one-third of the rounds, a substantial increase compared to *Tokens-Random-Voting*. Also, Monopoly bills were unavailable comparably more in the study with the BDM. Table 6 shows that, depending on how the tokens were acquired, the token behavior of the participants changed. There is no evidence, however, that these differences in token behavior translate in substantial differences in the amounts sent or returned.

Using data from the four earlier treatment studies plus the new treatment, we can inquire whether the trustees try to use the token as a "bait." The regression output shown in Table 4 did not lend evidence to this scenario. We created an indicator that takes the value of 1 when the trustee sent a token, but then returned to the trustor strictly *fewer* points than the trustor sent before the multiplication took place. This is the "bait" scenario. The variable is set equal to zero in all other cases:[35] this includes cases in which the tokens were later accompanied by a payback greater than or equal to what the trustor sent (before the multiplication), as well as cases in which a token was not sent, and hence in which there was no attempt to use a bait. Figure 8 shows that the proportion of baits is always less than 40%, with seemingly lower proportions in the treatments with fixed couples.

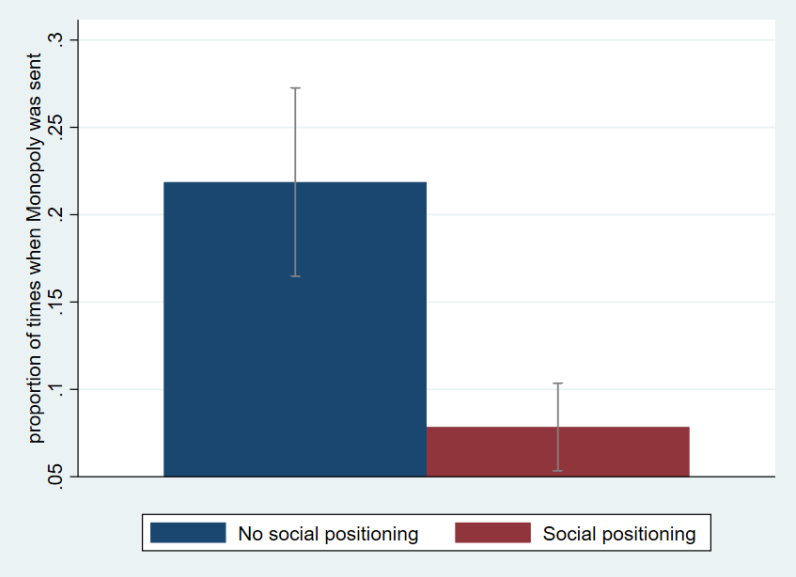
[34] The variable is set to missing whenever the trustee had no token available.
[35] The variable is set to missing when no token was available to the trustee.

Figure 8: the proportion of trustees' "baits"



We can also inquire whether the decision to send the Monopoly was different when the token was socially positioned. For this paper, social positioning is the result of the creation of a basis for common knowledge that a subset of the tokens is salient. Positioning is achieved in our experiments by announcing that we will show the votes received by each token in the studies with voting (in the studies *Tokens-Random-Voting* and Tokens-Fixed-Voting), and the average buying prices in the experiment with the BDM. Figure 8 shows that Monopoly bills, the most voted token in the studies with voting and the second most expensive token in the study with the BDM, were sent twice as frequently in the treatments *without* social positioning. We take these results as evidence that it is possible to influence the perception of the value of simple tokens in the lab through simple manipulations.

Figure 8: the Monopoly-bill sending decision with and without social positioning



4. Discussion

"Men knew how to pledge their honor and their names long before they knew how to write," according to Mauss (2018, p. 119). Our experimental design allows us to answer three questions concerning pledging. The first is whether trustors saw the token as a pledge of the trustee's trustworthiness. We have documented that trustors sent more points after having received a token. The second question concerns whether trustees followed up with their pledge after sending a token. We have no strong evidence that trustees who sent more tokens returned more points to the trustors. Around one-third of trustees in the studies with random pairs engage in a behavior that we have called "baiting." The third question is whether any token can act as a pledge. We do not find strong evidence that the socially positioned token can elicit higher levels of trust. We find that the social positioning manipulation decreases the frequency with which the socially positioned item is sent. We also find that fixed pairs display much higher levels of trust and trustworthiness than random couples. The relative magnitudes of the regression coefficients of the token dummy and of the fixed couples dummy show that the horizon of the relation stimulates trust to a much higher degree than receiving a token. Several explanations are possible for this set of results.

"Antieconomic pledge-tokens"

According to Mauss, the Greeks "left behind the obsolete morality and economy of the gift, which was too risky, too expensive, too extravagant, encumbered with consideration for people

incompatible with the development of the market, of commerce and production, and, fundamentally, at that time, antieconomic" (Mauss, 2016 [or. ed. 1925]), p. 157). One might wonder if pledges are remainders of the olden days in which exchanges were ceremonial and often wasteful, such as in the *potlatch* of the Natives of the Pacific Northwest that so impressed Mauss. Max Weber thought that one type of power, the charismatic one, was especially prone to antieconomic drifts (cf. e. g. Weber, 2019, p. 386). Many of the gift exchange societies described by Mauss, such as the Trobriand and the societies of the Pacific North West, are probably characterized by charismatic chiefs. In the context of our experiment, pledges might be antieconomic because they complicate the strategy space of those interacting. Choices have to be made regarding the utilitarian commodity (money) as well as the tokens one wishes to send. In the ancient Athenian society, which was uppermost in Mauss's mind as a society that rid itself of the vagaries of exchange, formal institutions such as a hard currency and officers tasked with certifying the quality of the bullion emerged as facilitators of exchanges (Ober, 2008, p. 239). The process that leads from informal pledges of one's trustworthiness to formal guarantees of trustworthiness is one that cannot be taken for granted, and which deserves further study.

"Cheap pledge-tokens"

As we have seen, on average, the trustor who received a token sent more points than the trustor who did not receive a token. It is undeniable, however, that the coefficient of the token is of modest magnitude. The status of social convention, according to Hume (cf. Epstein, 2015, p. 53), supplies what is an otherwise empty promise with the element of obligation. In the societies studied by Mauss, the tokens were imbued with symbolic and conventional content, a process we are only able to replicate in the lab very imperfectly[36]. Past actions might have been more informative than the token because of these limitations of the lab methodology. Another problematic feature of our experiment was that the tokens were highly stylized and chosen by the experimenter. It is possible that if the participants could produce the tokens, the pledge value of the tokens would be increased.[37]

[36] Similar considerations about the "artificial, impersonal, anonymous experimental environment" can be found in Vollan (2012, p. 372). Mauss in his *Essai* spoke at length of the "loss of face" (e.g. in Mauss, 2017, p. 129) when one does not reciprocate worthily. This is a clear exemplification of the tight limits that the lab methodology imposes.

[37] We have explored this issue in a past contribution by the same authors.

Another issue relates to the perceived value of the pledge-tokens. Bloom (2010, p. 83) noticed that displays of personal quality, such as a pledge, will only be relied upon if they involve some sacrifice or cost. In our studies, sending a token was costly, as the endowment of tokens was not replenished. It is likely, however, that the number of tokens available (5) meant that the pledge value of every single token was not very high.

Posner (1980, p. 24) remarked that: "A gift is a way of communicating information about one's wealth, tastes, and attitudes more credibly than by a statement, especially in circumstances where a statement would be difficult to verify and guarantees of its truth would not be enforceable." A future study might enlighten a question we have not tried to answer here, namely the comparative efficiency of tokens and cheap talk in increasing trust and trustworthiness, and corroborate (or not) the "Posner conjecture" that gifts are more helpful than words.

"Tokens and Money do not mix"

Both in traditional and modern societies, gifts and commodities do not often mix. This point is exemplified by Malinowski's contention that commodity exchange (*gimwali*) and *kula* typically do not happen at the same time in the Trobriand society. This insight proved fundamental in the discussion in Bohannan and Bohannan (1968, p. 228) of so-called "spheres of exchange" among the Tiv of Nigeria. In Bohannan and Bohannan's terminology, a conveyance is an exchange within a sphere, such as a chicken for another daily-use item, while a conversion is an exchange between spheres, such as a chicken for a brass rod. They notice that "Conveyances are morally neutral; conversions have a strong moral quality in their rationalization" (*id.*, p. 234). Commodities have an exchange ratio; gifts have an exchange order, a ranking (Gregory, 1982). A ranking of the various spheres exists, and powerful Tiv can convert items of lower spheres into items of higher spheres (Bohannan and Bohannan (1968, p. 237). The difficulty of conversions is likely to create a sophisticated strategy space for the participants in our experiment. They might be unsure of what is appropriate in return for a token. They might also resent receiving a token that was not the most voted (Monopoly), or the most expensive tokens when they were for sale (sharpener and Monopoly bill). Conditioning their strategies on the previous history of play might have been a comparatively easier strategy. Future research might try to separate the two spheres of commodities and tokens, for example with a gift-token exchange followed by a monetized exchange.

APPENDIX 1

Instructions of the study *Tokens-Random-Voting*

You will receive €2 for your participation and you will be able to earn an additional sum of money, and some items, with the games that will be presented to you. All earnings are paid at the end of the experiment. During the experiment, it is not allowed to talk to other participants and the use of mobile phones.

ANONYMITY:

The experiment will take place in conditions of anonymity and using a computer. You will only be identified during the experiment by the code you picked upon entering the room, and that we ask you to keep for the duration of the experiment. The researchers will not be able to associate your real name with the choices you make during the experiment.

EXPERIMENT

The experiment is divided into three phases. In the first phase, you will take part in a game. In the second phase, you will take part in another game. In the third phase, you will be asked to answer a questionnaire.

FIRST PHASE

You will see images of 5 items on the screen. All the participants present in the room at this time will see the same images on their screen. Everyone chooses one item from the screen. The participant who chooses the item most chosen by all those who are in the room at this time, in the shortest possible time, wins 3 euros. We will then show you the votes received by each item. The winning participant learns that he won at the end of the experiment.

SECOND PHASE

Through a random draw at the beginning of each round, pairs of players are formed. The draw includes players you have already played with. It is, therefore, possible to play more than once with the same player in this phase. However, the minimum interval between the two matches will be ten rounds. In this phase, there are a total of 20 draws made before each of the 20 repetitions of the same game.

Within each pair, in each round, there will be a player A and a player B. The probability of being player A and B is the same, and the draw takes place in each round. It is, therefore,

possible that you have the same role for several successive rounds. On average, you will play 10 rounds as player A and 10 as player B.

Both player A and player B begin this phase with 10 experimental points; and 5 items shown in photos (the same ones you saw in phase 1). Each experimental point corresponds to 1 euro.

Player B moves first and decides whether to send an item to player A and, if so, which item from those available. Player A observes the choice of B and decides how many points to pass to player B, by entering a number from 0 to 10 in the appropriate box.

The number of points passed from A to B will be multiplied by 2. So, B will receive twice the number of points passed by A. If we indicate with *x* the number of points passed by A, 2 * *x* points are delivered to player B.

Player B will wait for the choice of A. Once A has made his/her choice, a message will appear on the monitor of player B with the information on the number of points passed by A and on the number of points that he/she actually received (2 times the number of points passed by A).

Then player B will decide how many points to pass to A. B can send any number of points between zero and all the points available, 10 points plus 2 * *x*.

The round earnings of each player will then be shown on the monitor.
The monetary earnings of the players in each round will be the following:

Player A:

Earnings of A = 10 points - points passed to B + points passed by B.

Player B:

Earnings of B = 10 points + points passed by A and multiplied by 2 - points passed to A.
You will also be shown the items available to you at the end of each round.

This phase is repeated 20 times. The experimental points accumulated in the previous rounds do not accumulate. In each round, all players have an initial amount of 10 points. The items available to each player at the end of the previous round are transferred to the next round.

All choices (which item B sends to A, how many points A passes to B, and how many points B passes to A) must be completed in a maximum of 40 seconds. If no choice is made, the system automatically selects the options of no item sent, and zero points passed.

THIRD PHASE

You will complete a questionnaire.

END OF EXPERIMENT

We will randomly choose one round among the twenty of the second phase, and we will pay the earnings of the players in that round, plus the two euros for participation, the possible earnings from phase 1, and the items that the players had at the end of the twentieth round. The objects will be physically delivered (not photos of the objects).

If you have any questions, please raise your hand and one of us will answer confidentially.

APPENDIX 2

Instructions of the study *Tokens-Random-BDM*

You will receive €10 for your participation and you will be able to earn an additional sum of money with the games that will be presented to you. All earnings are paid at the end of the experiment. During the experiment, it is not allowed to talk to other participants and the use of mobile phones.

ANONYMITY:

The experiment will take place in conditions of anonymity and using a computer. You will only be identified during the experiment by the code you picked upon entering the room, and that we ask you to keep for the duration of the experiment. The researchers will not be able to associate your real name with the choices you make during the experiment.

EXPERIMENT

The experiment is divided into three phases. In phase 1, you will have the opportunity to purchase items. In the second phase, you will take part in a game where you can use the items purchased in the first phase. In the third phase, you will be asked to answer a questionnaire.
We will explain the rules of the game first (phase 2), so that it is clear to you what is the function of the items you can buy in phase 1.

THE GAME (PHASE 2)

Through a random draw at the beginning of each round, pairs of players are formed. The draw includes players you have already played with. It is, therefore, possible to play more than once with the same player in this phase. However, the minimum interval between the two matches will be ten rounds. In this phase, there are a total of 20 draws made before each of the 20 repetitions of the same game.

Within each pair, in each round, there will be a player A and a player B. The probability of being player A and B is the same, and the draw takes place in each round. It is, therefore, possible that you have the same role for several successive rounds. On average, you will play 10 rounds as player A and 10 as player B.

Both player A and player B begin this phase with 10 experimental points (1 experimental point = 1 euro) and the items purchased in the first phase.

Player B moves first and decides whether to send an item to player A and, if so, which item from those available. Player A observes the choice of B, and decides how many points to pass to player B, by entering a number from 0 to 10 in the appropriate box.

The number of points passed from A to B will be multiplied by 2. So B will receive double the points passed by A. If we indicate with *x* the number of points passed by A, 2 * *x* points are delivered to player B.

Player B will wait for the choice of A. Once A has made his/her choice, a message will appear on the monitor of player B with the information on the number of points passed by A and on the number of points that he/she actually received (2 times the number of points passed by A).

Then player B will decide how many points to pass to A. B can send any number of points between zero and all the points available, 10 points plus 2 * *x*.

The round earnings of each player will then be shown on the monitor.
The monetary earnings of the players in each round will be the following:

Player A:

Earnings of A = 10 points - points passed to B + points passed by B.

Player B:

Earnings of B = 10 points + points passed by A and multiplied by 2 - points passed to A.

You will also be shown the items available to you at the end of each round.

This phase is repeated 20 times. The experimental points accumulated in the previous rounds do not accumulate. In each round, all players always have an initial amount of 10 points.

The items available to each player at the end of the previous round are instead transferred to the next round.

All choices (which item B sends to A, how many points A passes to B, and how many points B passes to A) must be completed in a maximum of 40 seconds. If no choice is made, the system automatically selects the options of no item sent / zero points passed.

THE ITEMS (FIRST STAGE)

In phase 1, which precedes the game described above, you can purchase items. Items can be purchased using your participation amount. The items will be shown to you in photos and are everyday objects. All participants present in the room at this time will have the opportunity to purchase the same items. You should know that if you buy any of the items, it will be exclusively to use them in the game described above. There will be no payment of the physical items at the end of the experiment.

You are asked to indicate the maximum price at which you are willing to purchase each item. The price must be between 0 and 2 experimental points, in increments of ten cents of experimental point.

For each item, a random number between 0 and 2 experimental points will be generated in a public extraction, in increments of ten cents. The random number is the selling price of each item. If the selling price is less than or equal to the purchase price you indicated for that object, you will purchase the item at the randomly extracted selling price. However, if the selling price is higher than the maximum purchase price you indicated for that item, you will not buy the item.

Keep in mind that it is not in your interest to underestimate the item because this reduces the chances of you being able to buy it, but at the same time, it does not influence in any way the determination of the sale price. Likewise, it is not in your best interest to overestimate the item because it could force you to buy it at a price that is higher than the valuation you give to the item itself. We remind you that your price evaluations must be made keeping in mind that the item are used only in the game described above, and will not be physically given to you.

An example shows you why the rules should convince you to express a purchase price exactly equal to your valuation of each item. Imagine that the first item you can buy is an apple. Now imagine that you have attributed to the apple a value in the game described above equal to 1

experimental point (= 1 euro). This means that you would not be willing to buy the apple for 1 euro and 10 cents but you would be happy to buy it for 1 euro or, of course, at prices below 1 euro. You may be tempted to offer to buy the apple for less than 1 euro, for example 50 cents. However, if you did so, you would forego the possibility of only buying the apple at the price you consider right, again considering the exclusive use of the items within the experiment. You would also not be able to buy the apple at prices between 50 cents and 1 euro, all prices which would be convenient for you. It follows that the only way that you can avoid wasting this opportunity to buy the apple is to write a purchase price exactly equal to your valuation of the object (1 euro in this purely illustrative example). It should be clear to you that it is not in your interest to express a purchase price higher than one euro. Your purchase price must reflect the value of each item in the phase 2 game: the more useful it is for you to have the item in the phase 2 game, the higher the purchase price you should declare.

The unspent money in the purchase of the items remains in your initial amount, paid at the end of the experiment. If you wish to keep your initial amount intact, you can indicate as the purchase price "0" (zero) for all items. In this way you are sure that you will never purchase any of the items. If you want to purchase all items, you can do by indicating the maximum purchase price of 2 euros for each item. There are enough items to ensure that all participants can buy them.

At the end of this phase, we will show each participant the items he/she has purchased. We will show everyone the average purchase prices for each item indicated by the participants in the experiment in this room.

THIRD PHASE

You will complete a questionnaire.

END OF EXPERIMENT

We will choose a random round among the twenty of phase 2, and we will pay the players' winnings in that round, plus the initial amount (minus what you spent in phase 1). There is no physical payment of the objects. If you have questions, raise your hand and one of us will answer confidentially.

References

Alekseev, A., Charness, G., & Gneezy, U. (2017). Experimental methods: When and why contextual instructions are important. *Journal of Economic Behavior & Organization*, 134, 48-59.

Anderhub, V., Engelmann, D., & Güth, W. (2002). An experimental study of the repeated trust game with incomplete information. *Journal of Economic Behavior & Organization*, *48*(2), 197-216.

Andreoni, James. (1995). Warm-Glow versus Cold-Prickle: The Effects of Positive and Negative Framing on Cooperation in Experiments. *Quarterly Journal of Economics*, 110(1): 1-21.

Becker, G. M., DeGroot, M. H., & Marschak, J. (1964). Measuring utility by a single-response sequential method. *Behavioral Science*, 9(3), 226-232.

Benveniste, É. (2016 [or. ed. 1969]). *Dictionary of Indo-European concepts and society*. Hau Books.

Berg, J., Dickhaut, J., & McCabe, K. (1995). Trust, reciprocity, and social history. *Games and Economic Behavior*, 10(1), 122-142.

Bicchieri, C., Lev-On, A., & Chavez, A. (2010). The medium or the message? Communication relevance and richness in trust games. *Synthese*, 176(1), 125-147.

Bicchieri, C., Xiao, E., & Muldoon, R. (2011). Trustworthiness is a social norm, but trusting is not. *Politics, Philosophy & Economics*, *10*(2), 170-187.

Bigoni, M., Camera, G., & Casari, M. (2019). Partners or Strangers? Cooperation, monetary trade, and the choice of scale of interaction. *American Economic Journal: Microeconomics*, 11(2), 195-227.

Bloom, P. (2010). *How pleasure works: The new science of why we like what we like*. Random House.

Bohannan, P., & Bohannan, L. (1968). *Tiv Economy*. Northwestern University Press.

Camera, G., Casari, M., & Bigoni, M. (2013). Money and trust among strangers. *Proceedings of the National Academy of Sciences*, 110(37), 14889-14893.

Charness, G., & Dufwenberg, M. (2006). Promises and partnership. *Econometrica*, 74(6), 1579-1601.

Chen, D. L., Schonger, M., & Wickens, C. (2016). oTree—An open-source platform for laboratory, online, and field experiments. *Journal of Behavioral and Experimental Finance*, 9, 88-97.

Cox, J. C., Kerschbamer, R., & Neururer, D. (2016). What is trustworthiness and what drives it?. *Games and Economic Behavior*, 98, 197-218.

Dieleman, J. L., & Templin, T. (2014). Random-effects, fixed-effects and the within-between specification for clustered data in observational health studies: a simulation study. *PloS One*, 9(10), e110257.

Duffy, J., & Feltovich, N. (2002). Do actions speak louder than words? An experimental comparison of observation and cheap talk. *Games and Economic Behavior*, 39(1), 1-27.

Dufwenberg, Martin, Simon Gächter and Heike Hennig-Schmidt. (2011). The Framing of Games and The Psychology of Play. *Games and Economic Behavior*, 73(2): 459-478.

Engle-Warnick, J., & Slonim, R. L. (2004). The evolution of strategies in a repeated trust game. *Journal of Economic Behavior & Organization*, *55*(4), 553-573.

Epstein, B. (2015). *The ant trap: Rebuilding the Foundations of the Social Sciences*. Oxford University Press.

Fudenberg, D., & Vespa, E. (2019). Learning Theory and Heterogeneous Play in a Signaling-Game Experiment. American Economic Journal: Microeconomics, 11(4), 186-215.

Graeber, D. (2001). *Toward an anthropological theory of value: The false coin of our own dreams*. Palgrave.

Graeber, D. (2012). *Debt: The first 5000 years*. Penguin UK.

Gregory, C. A. (1982). *Gifts and Commodities*. Academic Press.

Greif, A. (1993). Contract enforceability and economic institutions in early trade: The Maghribi traders' coalition. *The American Economic Review*, 525-548.

Hodgson, G. (2015). On defining institutions: Rules versus equilibria. *Journal of Institutional Economics*, 11(3), 497-505.

Habib, A. (201). Promises (Stanford Encyclopedia of Philosophy). Stanford Encyclopedia of Philosophy, URL = https://plato.stanford.edu/archives/spr2018/entries/promises/.

Hindriks, F., & Guala, F. (2015). Institutions, rules, and equilibria: A unified theory. *Journal of Institutional Economics*, *11*(3), 459-480.

Holt, C. A. (2019). *Markets, Games, and Strategic Behavior: An Introduction to Experimental Economics*. Princeton University Press.

Johnson, A. W., & Earle, T. K. (2000). *The evolution of human societies: from foraging group to agrarian state*. Stanford University Press.

Johnson, N. D., & Mislin, A. A. (2011). Trust games: A meta-analysis. *Journal of Economic Psychology*, *32*(5), 865-889.

Keynes, J.M. (1973 [or. ed. 1936]). *The Collected Writings of John Maynard Keynes: Vol. VII. The General Theory of Interest. Employment and Money*. London: Macmillan.

Kreps, D. M. (1990). *Corporate culture and economic theory*. In J. E. Alt & K. A. Shepsle (Eds.), *Firms, Organizations and Contracts* (pp. 221-275). Oxford, UK: Oxford University Press.

Landa, J. T. (1983). The enigma of the Kula Ring: gift-exchanges and primitive law and order. *International Review of Law and Economics*, *3*(2), 137-160.

Leach, J. W., & Leach, E. (Eds.). (1983). *The Kula: new perspectives on Massim exchange*. Cambridge University Press.

Larrick, Richard P. and Sally Blount. (1997). The Claiming Effect: Why Players Are more Generous in Social Dilemmas than in Ultimatum Games. *Journal of Personality and Social Psychology*, 72(4): 810-825.

Lawson, T. (2016). Social positioning and the nature of money. *Cambridge Journal of Economics*, 40(4), 961-996.

Lewis, D. (2008 [or. ed. 1969]). Convention: A Philosophical Study. John Wiley & Sons.

Malinowski, B. (2014). *Argonauts of the Western Pacific*. Routledge.

Mauss, M. (2016 [or. ed. 1925]). *The gift*. Chicago: The University of Chicago Press.

Mauss, M. (2017 [or. ed. 1928]). Wette, wedding. *Journal of Ethnographic Theory*, *7*(3), 443-450.

Nagel, R. (1995). Unraveling in guessing games: An experimental study. *The American Economic Review*, *85*(5), 1313-1326.

Nietzsche, F. W. (1989). *On the genealogy of morals*. Vintage.

Ober, J. (2008). Democracy and Kowledge: Innovation and Learning in Classical Athens. Princeton University Press.

Posner, R. A. (1980). A theory of primitive society, with special reference to law. *The Journal of Law and Economics*, 23(1), 1-53.

Robinson, J. A. (2013). Measuring institutions in the Trobriand Islands: a comment on Voigt's paper. *Journal of Institutional Economics*, *9*(1), 27-29.

Rousseau, D. M. (2001). Schema, promise and mutuality: The building blocks of the psychological contract. Journal of Occupational and Organizational Psychology, 74(4), 511-541.

Skyrms, B. (2010). *Signals: Evolution, learning, and information*. Oxford University Press.

Smith, V. (2015). Conduct, rules and the origins of institutions. *Journal of Institutional Economics*, *11*(3), 481-483.

Vanberg, C. (2008). Why Do People Keep Their Promises? An Experimental Test of Two Explanations. *Econometrica*, *76*(6), 1467-1480.

Voigt, S. (2013). How (not) to measure institutions: a reply to Robinson and Shirley. *Journal of institutional economics*, *9*(1), 35-37.

Vollan, B. (2012). Weird reciprocity? A 'within-culture across-country' trust experiment and methodological implications. *Journal of Institutional Economics*, 8(3), 371-397.

Weber, M. (2019 [or. ed. 1921]). *Economy and society*. Harvard University Press.

Weiner, A. B. (1983). 'A world of made is not a world of born': doing kula in Kiriwina. In J. W. Leach & E. Leach, (Eds.), *The Kula: new perspectives on Massim exchange*. Cambridge University Press.

Zollman, K. J., Bergstrom, C. T., & Huttegger, S. M. (2013). Between cheap and costly signals: the evolution of partially honest communication. *Proceedings of the Royal Society B: Biological Sciences*, 280(1750), 20121878.