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Abstract

It is common practice to have subjects make decisions which pay out in a fictitious experimental currency. Earnings in the experimental currency are then converted to cash at the end of the experiment. Like many practices in experimental economics, however, these procedural choices seems to be driven more by habit or tradition than by empirical evidence that more desirable outcomes are produced. In this paper, we report the results of an induced value experiment in which we manipulated the exchange rate between the experimental currency and cash. We find virtually no relationship between a stronger/weaker experimental currency and the ability of theory to predict observed outcomes. The only significant effect that was generated relates to the comparison of the cash-only condition to the 1-to-1 exchange condition, with the latter producing greater behavioral deviations from theoretical predictions. The results suggest that experimenters might be able to spread scarce research dollars over more subjects by using weaker experimental currency but using a 1-to-1 conversion between the experimental currency and cash might, in the words of Davis and Holt (1993), "create an artificial 'game-board' sense of speculative competitiveness."

Keywords: experimental currency units; ECU; tokens; Vickrey auction; induced values.

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1 Introduction

Denominating subjects' payoffs using a laboratory currency (e.g., "tokens" or "francs") which are later converted into cash, is a practice often employed in experimental economics. For example, a search of the term "Experimental Currency Unit(s)" or "ECU(s)" in just two of the journals that specialize in publishing experimental papers i.e., *Experimental Economics* and *Journal of Economic Behavior & Organization*, returns a list of 22 and 32 published papers, respectively.[1]Yet, there appears to be scant research focusing on the implications of using ECUs rather than cash in lab experiments.

Davis and Holt (1993, pp. 25) were perhaps the first to outline some hypotheses about the use of ECUs in their now classic "Experimental Economics" book. Suprisingly, they advice against using ECUs in experiments, unless "the researcher has a specific design motivation for using a laboratory currency". So why would researchers use ECUs anyway? Davis and Holt (1993) argue that for one, a very low exchange rate (e.g., 1000 ECUs per € earned) can create a continuous price grid that would more accurately approximate theoretical results. A second advantage is that when a researcher wishes to minimize interpersonal payoff comparisons of subjects, this can easily be achieved by using different exchange rates across subjects as part of their private information. Third, ECUs can control the location of focal payoff points when, for example, sessions are conducted in countries with different currencies.

Davis and Holt (1993) also mention that ECUs might lead to a money illusion effect (Fehr and Tyran, 2001). On the one hand, such an effect might be desirable, because it can increase incentives (and reduce experiment costs) if the subject puts an effort to the tasks proportional to the nominal value of the currency. Related to this desirable money illusion effect is the fact that increasing the nominal value of the reward may increase the perceived opportunity cost of "misbehavior" (Harrison, 1989, 1992). In this respect, Lusk et al. (2007) have shown that incentives for bidding the weakly dominant strategy in several auction mechanisms depend on the expected cost of misbehaving (expected forgone payoff) which tends to be steeper for higher (nominal) values. On the other hand, this money illusion effect can become detrimental if it creates, what Davis and Holt (1993) call, an artificial "game-board" sense of speculative competitiveness. Financial incentives could then be masked behind the veil of ECUs.

[1]The papers are listed by journal in Appendix B. Search was performed on May 7, 2013 and list was compiled the same day.

An additional way that ECUs can affect behavior in the lab is the idea that a medium of exchange can disguise the final outcome of an action (Hsee et al., 2003). This is related to the medium maximization hypothesis which states that subjects will often try to maximize the medium of exchange while forgetting the real target of maximizing the outcome. Hsee et al. (2003) have termed the three medium effects they studied as the illusion of advantage, the illusion of certainty and the illusion of linearity. They describe cases in which a medium makes a less advantageous option appear more advantageous, a risky choice to seem like a riskless one, and a concave payoff relationship appear to be seemingly a linear one.

Mazar et al. (2008) studied how a medium of exchange (tokens) can affect dishonest behavior by making the moral implications of dishonesty less accessible and thus making it easier for participants to cheat more often. In one treatment, subjects received one token per correct task (i.e., find two numbers in a matrix that add up to 10), to be exchanged with $0.5 at the end of the experiment. Mazar et al. (2008) found that compared to a no-tokens treatment, subjects cheated more (i.e., they reported to have solved more questions) in the tokens treatment. The implication is that if tokens (or ECUs) facilitate elicitation of insincere responses then the task of discovering subjects' "true" preferences would be made more difficult using ECUs.

Given all the uncertainty and competing hypotheses about how ECUs may affect subjects' behavior in experiments, we decided to further explore the use of ECUs as a design choice. Although the types of experiments in the literature that use ECUs is widely varied, we chose to further explore this issue using an induced value 2nd price auction. We made this choice for a number of reasons. First, we opted for an experiment where theory has a clear prediction about behavior, so we can test whether behavior matches prediction. The 2nd price auction is theoretically incentive compatible and, as such, theory predicts that subjects will bid their induced value. Second, the 2nd price auction is an institution for which theory has a clear prediction but for which there is debate about whether the theory holds in practice. For example, Kagel et al. (1987) and Harstad (2000) show that subject's tend to overbid in 2nd price auctions while Parkhurst et al. (2004) did not find overbidding to be prevalent. Lusk and Shogren (2007, see particularly Table 2.3) present several studies that tested the theoretical prediction of the 2nd price auction and concluded that the results are mixed. The reason we are interested in an institution for which debate is still active is because if previous research found that the 2nd price auction was always demand revealing, then there would be little room for tokens and exchange rates to impact behavior vis-a-vis the theory. The fact that the debate is still active suggests that at the very least, there are a variety of factors or incentives that might come into play that would help us learn more why and when the theory accurately predicts behavior.

In our experiment, we conduct several treatments where we vary the experimental currency-Euro exchange rate and the use/non use of tokens. Our benchmark is a treatment where no tokens are used (i.e., subjects bid in the euro currency and no conversion takes place). In the other treatments, the exchange rate varies by treatment as follows: i) 10 tokens for 1 €, ii) 25 tokens for 1 €, iii) 0.25 token for 1 € and iv) 1 token for 1 €. Our results can be boiled down to these: First we find that most token treatments do not differ substantially with respect to the cash Euro treatments. However, the 10 token: 1 € treatment does slightly better in terms of demand revelation especially in the last few rounds. On the other hand, the 1 token: 1 euro treatment does much worse than any other treatment. We believe this is due to the fact that an otherwise transparent situation is turned into a game for no obvious reason. Therefore, subjects decide to "play" and "compete" more in this particular treatment. This is consistent with the "game board" speculative competitiveness hypothesis of Davis and Holt (1993). A few other results indicate that demand revelation improves over rounds and with higher induced values. The former result is consistent with a learning hypothesis while the latter is consistent with the hypothesis that the cost of misbehaving becomes larger with higher induced values. In what follows, we describe our experimental design, then present some descriptive analysis and econometric results. We conclude in the last section.

2 Experimental design

To investigate some of the issues discussed above, we conducted a laboratory experiment to compare behavior between treatments in which we vary the exchange rate of tokens and Euros. We use a control treatment in which no tokens are used and subjects bid in Euros. The next sub-section describes the subjects, recruitment procedures, and the experimental environment. We then discuss the different treatments used in the study.

2.1 Description of the experiment set-up

In order to test our research hypothesis, we conducted non-hypothetical induced value auction experiments. We chose to do induced value auctions because this provides a benchmark against which to compare bidding behavior between different treatments. In induced value experiments, accurate or optimal bidding is referred to as bids that are close to induced values. Thus, the main variable of interest in our study is the revelation ratio, i.e. the distance between the revealed preference (the bid) and the true preference (the induced value). Mis-bidding is measured as the differences between bids and induced values across all treatments.

We have five treatments in our experiment. Each treatment consisted of four sessions with exactly eight subjects each (i.e., 32 subjects per treatment) for a total of 160 subjects.[2] Our experiment was conducted from November 20 to December 5, 2012 during the middle of the week (Tuesday, Wednesday, Thursday). Subjects in the experiments took part in a multi-round auction where they submitted non-hypothetical bids for their assigned induced values. Prior to the auction, we carefully explained how a second price auction works. In every round, each participant was assigned his/her unique induced value for the "good". For the induced value procedure, we sold the unspecified "good" and resold it to the experimenter at the market clearing price (2nd highest bid) after the end of the round. Our induced value auction experiment follows procedures similar to Jacquemet et al. (2013), Cherry et al. (2004) and Shogren et al. (2001).

One of the treatments is the control treatment in which subjects bid in the Euro nominal currency. In the other treatments, subjects bid in tokens with the understanding that tokens are changed into Euros at the end of the experiment. We varied the exchange rate of tokens and Euros as follows: i) Treatment 1: 0.25 tokens are exchanged with 1 €, ii) Treatment 2: 1 token is exchanged with 1 €, iii) Treatment 3: 10 tokens are exchanged with 1 €, iv) Treatment 4: 25 tokens are exchanged with 1 €. Treatment 5 is the control treatment.

At the end of the auctions, participants were asked to complete a questionnaire containing basic demographic questions. Full anonymity was ensured by asking subjects to choose a unique four-digit code from a jar. The code was then entered as soon as the computerized experiment started. The experimenter only knew the correspondence between digit codes and profits. Profits and participation fees (a standard 10€participation fee was given to all subjects) were put in sealed envelopes (the digit code was written on the outside) and were exchanged with printed digit codes at the end of the experiment. No names were asked at any point of the experiment. Average total payouts were 9.85 € (S.D.=1.05, min=1.8, max=11.72).

[2]Our sample size was determined via a conventional power calculation (Kupper and Hafner, 1989). Assuming α=0.05, β=0.20, and a standard deviation of the variable of interest (i.e., the difference between bid and expected value) of 0.8 (which is the value observed by Shogren et al. (2001) who used a similar range of induced values in their study of the 2nd price auction), the minimum sample size required to detect a difference of 0.2 is 250 subjects per treatment. The number increases to 446 for a minimum detectable difference of 0.15. Our per treatment sample size is (32 subjects)$\times$(24 rounds)=768, which is sufficiently large to detect these differences, should they exist.

2.2 Experimental procedures

We only used one proctor (i.e., one of the authors) in all our sessions. A conventional lab experiment was conducted using the z-Tree software Fischbacher (2007). Subjects consisted of undergraduate students at the University of Ioannina. They were recruited using the ORSEE recruiting system (Greiner, 2004). The nature of the experiment was not mentioned during the recruitment. Each session had exactly eight subjects, with each subject participating in 24 auction rounds. Our per treatment sample size is comparable to other induced value auction experiments (e.g., Cherry et al., 2004).

Each subject participated in one session only. In every round, each subject was endowed with a different induced value. The sets of induced values were randomly drawn from 8 values. The induced demand curve is identical in all treatments and is defined by: 0.30, 1.32, 2.75, 3.91, 4.79, 5.54, 6.06, 6.72. Monetary values are expressed in Euros (€) for the Euro nominal currency (control) treatment. In the rest of the treatments induced values were expressed as tokens and were properly scaled up or down.[3]

By the 8th round, each subject experienced the full range of the induced values permitting all possible permutations among individual induced values. In rounds 9 to 16, subjects experienced again the set of the induced values albeit in a different (random) order. The set of induced values was repeated for rounds 17 to 24 in a random order. Thus each bidder experienced each induced value three times, and the whole demand curve was induced in every round. None of the bidders knew anything about the other bidders' induced value or the induced demand curve. They also were not aware that they were going to be assigned the same induced value multiple times or about the order of assignment of induced values.

Feedback on others subjects' earnings or about the highest bid in the auction was not provided between rounds. Profits are equal to the difference between the induced value and the price the bidder pays for the good (the second highest bid). If the bidder did not purchase the good, profits were zero for that round. The only information posted between rounds was the loss/profit of the previous round, if any.

Each session consisted of a the training phase and the auction phase. A questionnaire followed at the end of each session. After arriving at the lab, subjects were randomly assigned to a computer. In all treatments subjects received a 10 € fee, for a session that lasted about 45 minutes. All transactions were completed at the end of the experiment. Before the sessions started, subjects were given detailed written instructions (shown in Appendix A) which were also read aloud and explained by the proctor. Subjects also participated in a three round hypothetical practice auction to fully familiarize themselves with the procedure.

[3]For example in the 10 tokens:1 € treatment the set of induced values was: 3.0, 13.2, 27.5, 39.1, 47.9, 55.4, 60.6, 67.2.

All random draws (e.g., drawing the binding round) were performed right in front of subjects by asking one of the subjects to draw a number from an urn.

3 Results

Before we proceed with the analysis and results, it is useful to know whether the demographic profile of subjects differs across treatments. An ANOVA test shows that this is not the case for age at the 5% level (F-statistic=2.12, p-value=0.08) and for household size (F-statistic=1.10, p-value=0.36). In addition, a Pearsons chi-squared test shows that gender is not significantly different between treatments at the 5% level ($\chi^2 = 8.46$, p-value=0.08).

3.1 Descriptive analysis

We first consider aggregate behavior by round in each treatment. Table 1 provides raw data on observed behavior by treatment and round. All bids and induced values from the token treatments are converted into Euros using the exchange rate applied in each treatment. In each treatment, aggregate induced demand (ID) equals 125.56 (this is the sum of induced values times the number of sessions per treatment i.e., $4 * (0.30 + 1.32 + 2.75 + 3.91 + 4.79 + 5.54 + 6.06 + 6.72)$). Table 1 shows the aggregate revealed demand (RD), which is equal to the sum of observed bids as well as the ratio of RD/ID in percentage points. With respect to the control treatment (i.e., Euros, no tokens) it is evident that aggregate behavior is not perfectly demand revealing, although it gets very close in the last few rounds (see for example R20, R23 and R24 for which the RD/ID ratio is below 105%). While in theory a 2nd price auction is incentive compatible, it is far from clear from the literature when it might or might not be incentive compatible in practice. For instance, Lusk and Shogren (2007, see particularly Table 2.3) presented several studies that tested the incentive compatibility of the 2nd price auction and concluded that the results are mixed. The conventional wisdom in the literature is that it takes several rounds before subjects learn the dominant strategy and our results support this view. Across all rounds, total demand revelation ratio for the control treatment is 109.86%.

When we compare the token treatments with the control treatment, we do observe a similar pattern for all treatments with the exception of the 1 token:1 € treatment. It appears that the revelation ratio is much worse for this treatment. Aggregate total RD/ID ratio for the rest of the treatments is comparable to the control treatment albeit it is slightly better for the 10 tokens:1 € treatment. Figure 1 illustrates the RD/ID ratio graphically. The red solid line in the figure signifies perfect demand revelation and is the benchmark. The fact that the revelation ratio is > 100 across (almost) all rounds implies that people tend to overbid.[4] This is consistent with the findings of Kagel et al. (1987) and Harstad (2000) suggesting that there is overbidding in second price auctions, albeit in the context of private values.

So why does the 1 token:1 € treatment generate more misbidding than the other treatments? A possible explanation can be found in what Davis and Holt (1993, pp. 26) call "speculative competitiveness" wherein payoffs in laboratory currency may create an artificial "game-board" sense. Arguably, the 1-to-1 conversion rate makes it appear as if a transparent situation is turned into a game for no obvious reason. Therefore, subjects decide to "play" and "compete" more in this particular treatment.

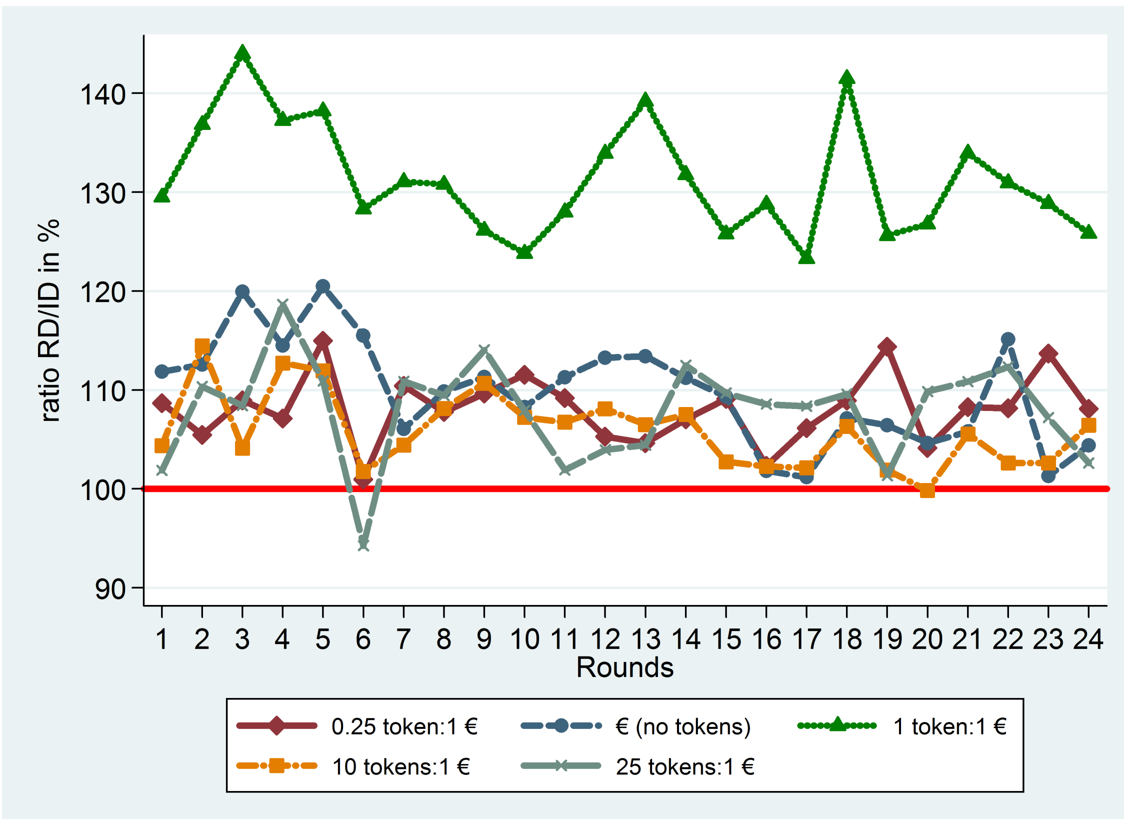

Figure 1: Ratio of revealed demand over induced demand in % per round and treatment

To examine how demand revelation depends on the magnitude of induced values, Table 2 shows revealed demand and the RD/ID ratio by induced values. First, it is obvious that with larger induced values the revelation ratio improves so that for most of the treatments (with

[4]Note that this finding concerns aggregate behavior. At the individual level, 27.8% of all submitted bids are lower than the assigned induced value, 9.9% of all bids are exactly equal to the induced value and 62.3% of all bids are greater than the induced value.

Table 1: Induced value bidding behavior by treatment and round

	Treatments									
	0.25 token:1 €		€(no tokens)		1 token:1 €		10 tokens:1 €		25 tokens:1 €	
	Revealed demand	(%)	*Revealed demand*	(%)	*Revealed demand*	(%)	*Revealed demand*	(%)	*Revealed demand*	(%)
Total	**3258.12**	**108.12**	**3310.46**	**109.86**	**3955.42**	**131.26**	**3190.66**	**105.88**	**3251.82**	**107.91**
R1	136.44	108.67	140.43	111.84	162.61	129.51	131.06	104.38	127.91	101.87
R2	132.40	105.45	141.34	112.57	171.81	136.83	143.70	114.45	138.55	110.34
R3	136.80	108.95	150.58	119.93	180.84	144.03	130.72	104.11	136.12	108.41
R4	134.52	107.14	143.74	114.48	172.34	137.26	141.54	112.73	148.99	118.66
R5	144.36	114.97	151.27	120.48	173.54	138.21	140.54	111.93	139.21	110.87
R6	126.76	100.96	145.02	115.50	161.11	128.31	127.77	101.76	118.35	94.25
R7	138.64	110.42	133.18	106.07	164.57	131.07	131.10	104.41	139.21	110.87
R8	135.32	107.77	137.93	109.85	164.23	130.80	135.78	108.14	137.44	109.46
R9	137.68	109.65	139.75	111.30	158.39	126.15	138.97	110.68	143.20	114.05
R10	140.04	111.53	135.94	108.27	155.45	123.81	134.66	107.25	135.49	107.91
R11	137.08	109.17	139.71	111.27	160.72	128.00	134.03	106.75	127.96	101.91
R12	132.20	105.29	142.20	113.25	168.17	133.94	135.74	108.11	130.52	103.95
R13	131.36	104.62	142.39	113.40	174.80	139.22	133.71	106.49	131.12	104.42
R14	134.40	107.04	139.66	111.23	165.47	131.79	135.03	107.54	141.24	112.49
R15	137.00	109.11	137.26	109.32	157.93	125.78	129.00	102.74	137.73	109.69
R16	128.48	102.33	127.84	101.82	161.70	128.78	128.40	102.26	136.31	108.56
R17	133.28	106.15	127.05	101.19	154.79	123.28	128.22	102.12	136.06	108.36
R18	136.76	108.92	134.50	107.12	177.66	141.49	133.50	106.32	137.62	109.60
R19	143.60	114.37	133.65	106.44	157.73	125.62	127.96	101.91	127.23	101.33
R20	130.76	104.14	131.38	104.64	159.18	126.78	125.37	99.85	137.93	109.85
R21	135.96	108.28	132.84	105.80	168.18	133.94	132.53	105.55	139.19	110.85
R22	135.80	108.16	144.53	115.11	164.42	130.95	128.86	102.63	141.05	112.33
R23	142.76	113.70	127.20	101.31	161.79	128.85	128.84	102.61	134.59	107.19
R24	135.72	108.09	131.07	104.39	157.99	125.83	133.63	106.43	128.84	102.61

Notes: For each treatment the left column gives the aggregate revealed demand (sum of bids) in each round (in rows) and across all rounds (first row). The right column (the % column) gives the ratio of the revealed demand to the aggregate induced demand which was 125.56 in each round.

the exception of the 1 token:1 € treatment), the revelation ratio is close to 100% for the 6.72 induced value. On the other hand, for smaller induced values like the 0.30 €, the revelation ratio ranges from 252% (25 tokens: 1 € treatment) to 603% (1 token: 1 € treatment). This is consistent with the prediction in Lusk et al. (2007) which state that the incentives for an individual to bid optimally in a second price auction can be very weak, unless an individual's true value is relatively large because then the second price auction punishes sub-optimal bids more severely. The RD/ID ratio is graphically depicted in Figure 2. The upper part of the figure shows the ratio across the full set of induced values while the lower part depicts the ratio for induced values larger or equal to 3.91. Figure 2 largely reconfirms our discussion above. The 1 token: 1 € treatment has a larger RD/ID ratio across the full range of induced values.

Table 2: Induced value bidding behavior by treatment and induced value

	Treatments									
	0.25 token:1 €		€(no tokens)		1 token:1 €		10 tokens:1 €		25 tokens:1 €	
	Revealed demand	(%)	*Revealed demand*	(%)	*Revealed demand*	(%)	*Revealed demand*	(%)	*Revealed demand*	(%)
Total	**3258.12**	**108.12**	**3310.46**	**109.86**	**3955.42**	**131.26**	**3190.66**	**105.88**	**3251.82**	**107.91**
IV=0.30	86.80	301.39	98.44	341.81	173.73	603.23	54.26	188.41	72.59	252.04
IV=1.32	175.88	138.79	190.28	150.16	261.78	206.58	151.83	119.81	173.56	136.96
IV=2.75	310.88	117.76	306.19	115.98	386.42	146.37	296.76	112.41	313.57	118.78
IV=3.91	405.52	108.03	411.63	109.66	475.13	126.58	393.71	104.89	406.99	108.43
IV=4.79	501.04	108.96	493.75	107.37	578.04	125.70	488.95	106.33	499.59	108.65
IV=5.54	536.72	100.92	567.18	106.64	646.46	121.55	556.75	104.68	544.93	102.46
IV=6.06	593.40	102.00	616.03	105.89	710.52	122.13	602.06	103.49	597.65	102.73
IV=6.72	647.88	100.43	626.96	97.19	723.34	112.12	646.34	100.19	642.94	99.66

Notes: For each treatment the left column gives the aggregate revealed demand (sum of bids) for each assigned induced value (IV) given in rows and across all induced values (first row). The right column (the % column) gives the ratio of the revealed demand to the aggregate induced demand. Aggregate induced demand was 3013.44 in each treatment (i.e., [sum of induced values=31.39] x [24 rounds] x [4 sessions]) and per induced value was [IV] x [24 rounds] x [4 sessions].

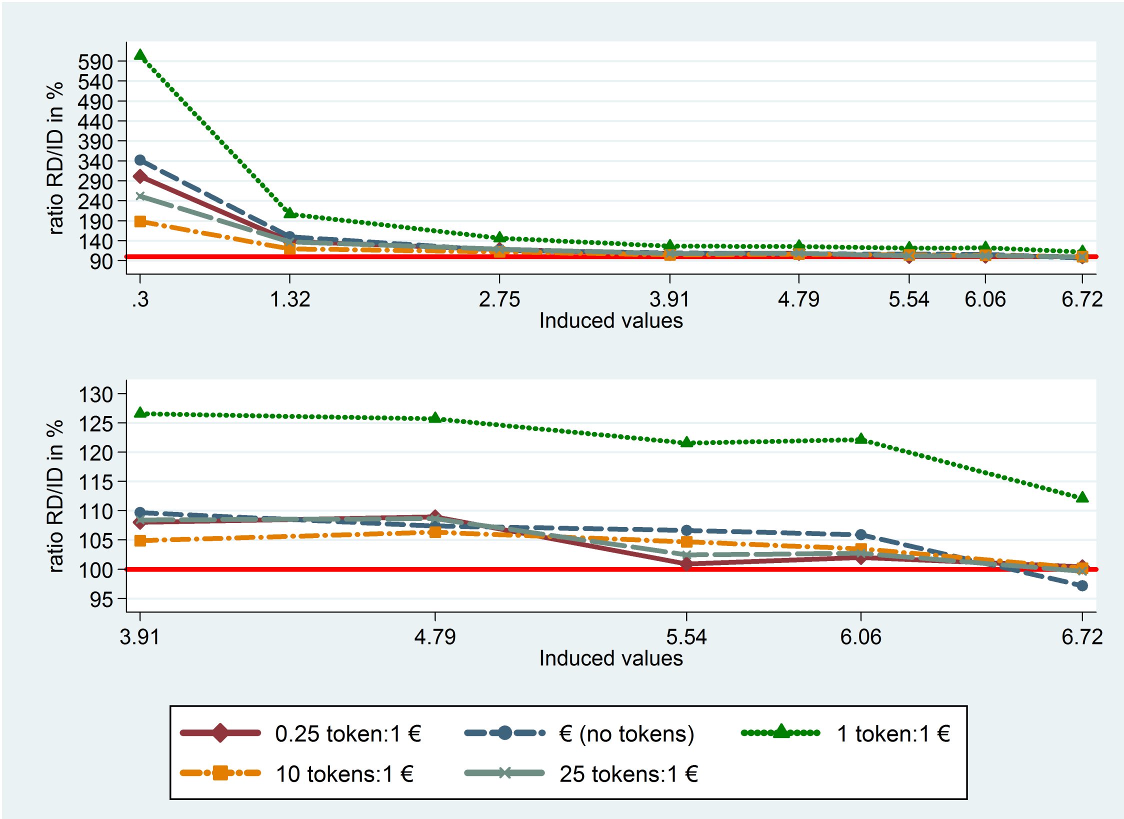

Figure 2: Ratio of revealed demand over induced demand in % per induced value and treatment

A similar picture can be drawn from Table 3 which shows the summary statistics of experimental data. With the exception of the 1 token: 1 € treatment, mean and median bids were only slightly larger than mean and median induced values, respectively. Depending on the treatment, the mean difference of bids and induced values ranges from 0.23 (S.D.=1.06) to 0.39 (S.D.=1.36) and the median is from 0 to 0.42. The difference in the 1 token: 1 € treatment is more than threefold that of the control (no tokens) treatment. The ratio of bids to induced values is lower for the 10 tokens:1 € treatment (mean ratio is just 1.18 and the median is exactly 1) and largest for the 1 tokens:1 € treatment (mean is 1.96 and median is 1.10). It is worth noting that a t-test on whether the bid-to-induced value ratio is equal to 1 is highly rejected for all treatments. Similarly, t-tests of whether bid minus induced value equals zero are highly rejected as well. Kolmogorov-Smirnov tests on whether the distributions of induced values and bids are equal, are consistent with the t-tests. A similar story is in place when we test for medians using a Sign test (Snedecor and Cochran, 1989). The last two rows of Table 3 show ANOVA tests and its non-parametric version, the Kruskal-Wallis test, of whether the means of bid, bid minus induced value and bid to induced value ratio are equal between treatments. The null is highly rejected in all cases.

Figure 3 shows the scatter diagrams of the bids and induced values by treatment.

Table 3: Descriptive statistics

		Bid	IV	Bid-IV	Bid-to-IV
0.25 token:1 €	Mean	4.24	3.92	0.32***	1.35***
	S.D.	2.36	2.15	1.32	1.53
	Median	4.76	4.35	0.10***	1.05***
€(no tokens)	Mean	4.31	3.92	0.39***	1.42***
	S.D.	2.36	2.15	1.36	2.09
	Median	4.73	4.35	0.08***	1.02***
1 token:1 €	Mean	5.15	3.92	1.23***	1.96***
	S.D.	3.05	2.15	2.30	3.15
	Median	5.30	4.35	0.42***	1.10***
10 tokens:1 €	Mean	4.15	3.92	0.23***	1.18***
	S.D.	2.36	2.15	1.06	0.81
	Median	4.50	4.35	0.00**	1.00**
25 tokens:1 €	Mean	4.23	3.92	0.31***	1.29***
	S.D.	2.38	2.15	1.30	1.89
	Median	4.72	4.35	0.05***	1.02***
ANOVA	F-statistic	20.72***	-	55.98***	16.91***
Kruskal-Wallis	χ^2	54.86***	-	182.21***	159.51***

Notes: *** for the means denotes rejection of the null that the Bid-to-IV ratio equals 1 or that Bid-IV equals 0 at the 1% level according to a t-test.
***(**) for the medians denotes rejection of the null that the sample median of Bid-to-IV ratio equals 1 or that the sample median of Bid-IV equals 0 at the 1%(5%) level according to a Sign test (Snedecor and Cochran, 1989).
*** for the ANOVA and Kruskal-Wallis test denotes rejection of the null that the means of Bid, Bid-IV and Bid-to-IV respectively, are equal between treatments at the 1% level.

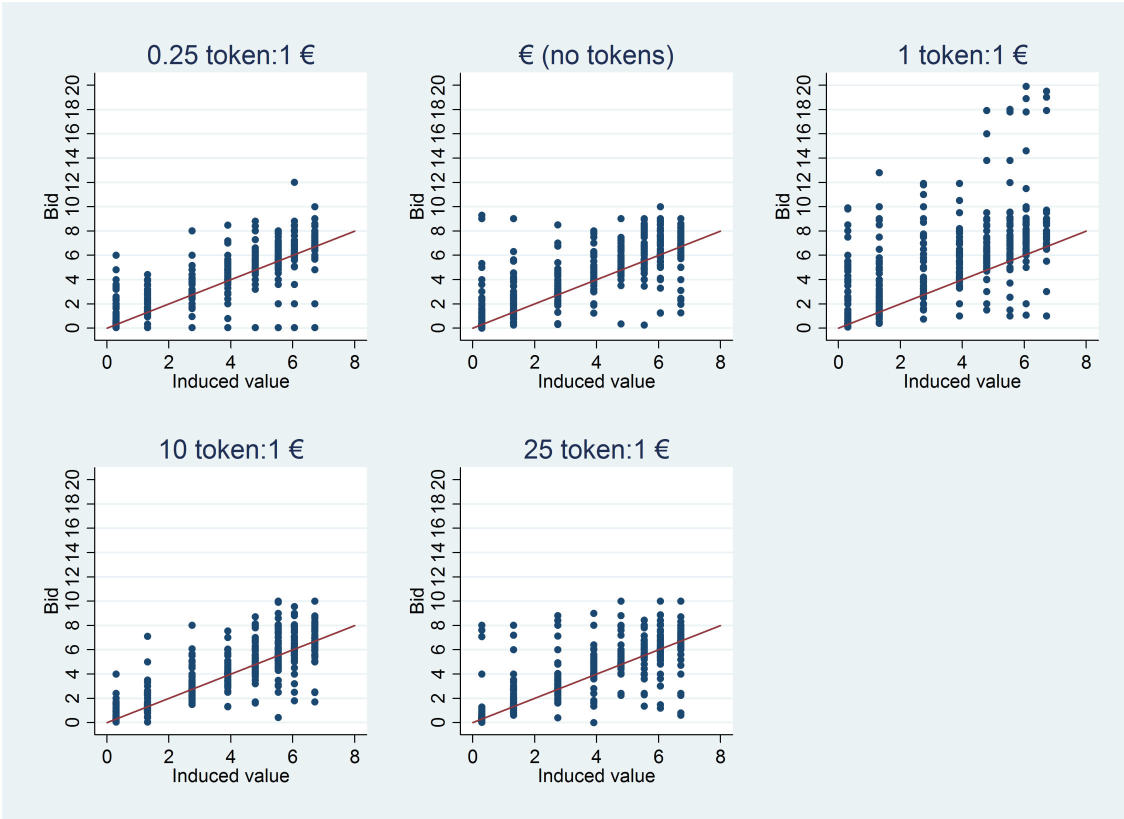

Figure 3: Distribution of bids by treatment (solid lines represent perfect demand revelation)

3.2 Econometric analysis

By specifying the bidding function as linear in induced value, we can directly test the assumption of perfect revealing bids. Since we have individual-level observations (bids from each subject) for multiple rounds, we have a panel data set. Similar procedures were followed in Cherry et al. (2004) and Jacquemet et al. (2013). We specify the bid function as:

$$bid_{it} = b_0 + b_1 IV_{it} + \rho_t + u_i + \varepsilon_{it} \tag{1}$$

In equation (1) bid_{it} is subject i's bid in round t, IV_{it} denotes subject i's induced value in round t, ρ_t are round fixed effects, u_i are individual specific random effects and ε_{it} is a period specific error term. Wald tests of $H_0 : b_0 = 0, b_1 = 1, \rho_t = 0$ can provide a formal test of perfectly demand revealing bids.[5] Econometric results of model (1) are displayed in Table 4 while Wald tests are displayed in Table 5.

[5]Since we omit round 1 fixed effect from the regression, this particular H_0 is a test of whether bid equals induced value in the first round of the experiment. We also test on a round-by-round basis by adding the relevant round fixed effect to the intercept (i.e., by generating the round-specific mean) in the Wald test.

Table 4: Random effects regressions of bids on induced values

	0.25 token:1 €	€(no tokens)	1 token:1 €	10 tokens:1 €	25 tokens:1 €
Constant	0.690***	0.861***	1.423***	0.265	0.356
	(0.242)	(0.248)	(0.416)	(0.195)	(0.241)
IV	0.911***	0.899***	0.932***	0.976***	0.928***
	(0.016)	(0.017)	(0.018)	(0.012)	(0.020)
Round dummies	Yes	Yes	Yes	Yes	Yes
σ_u	0.930***	0.913***	2.042***	0.787***	0.558***
	(0.123)	(0.122)	(0.262)	(0.103)	(0.084)
σ_ε	0.942***	0.994***	1.094***	0.724***	1.165***
	(0.025)	(0.026)	(0.029)	(0.019)	(0.031)
N	768	768	768	768	768
Log-likelihood	-1104.790	-1143.173	-1235.796	-912.102	-1242.410
AIC	2263.581	2340.346	2525.591	1878.205	2538.820
BIC	2388.963	2465.728	2650.973	2003.587	2664.203

Notes: Standard errors in parentheses. * p<0.1, ** p<0.05 *** p<0.01.

The Wald tests exhibited in Table 5, indicate that perfect demand revelation is rejected in all treatments and rounds with the exception of the 10 tokens:1 € treatment. In the 10 token:1 € treatment, we fail to reject the null for some rounds (paticularly the latter ones), indicating that in aggregate, demand revelation was close to perfect for these rounds. The penultimate columns labeled "cross model demand revelation" show Wald tests of the null that demand revelation is perfect across all five models of Table 4 simultaneously. This is highly rejected in all rounds. The last two columns exhibit Wald tests of the null that the constant and coefficient of the induced value variable is equal with each of the respective coefficients of the rest of the treatments. The null is highly rejected in all cases indicating that there are differences between treatments in terms of the extent of demand revelation.

Another way to look at our data is to regress absolute deviations from induced values $|bid_{it} - IV_{it}|$ on the treatment dummies. Table 6 shows the results from random effects regressions with and without demographic variables. Results show that once we account for demographic effects, the 1:1 treatment remains statistically significant, albeit at the 10% level. However, the difference with the rest of the treatments remains highly significant. For example, the coefficient of the 1 token:1 € treatment is statistically different from those in the 0.25:1, 10:1 and 25:1 treatments at the 5%, 1% and 5 % level, respectively (p-values are 0.034, 0.003 and 0.019 respectively). Therefore, the case remains that the results from the 1:1 treatment are quite different from those of the other treatments.

We also analyzed our data separately for rounds 1-12 and rounds 13-24 to check if these results will hold. We find that results are consistent across early vs later rounds. That

Table 5: Wald tests for perfect demand revelation by round

	Treatments													
	0.25 token:1 €		€(no tokens)		1 token:1 €		10 tokens:1 €		25 tokens:1 €		Cross model demand revelation		Cross model equality of coefficients	
	χ^2	p-value	χ^2	p-value	χ^2	p-value	χ^2	p-value	χ^2	p-value	χ^2	p-value	χ^2	p-value
R1	34.08***	0.00	40.44***	0.00	21.57***	0.00	4.62*	0.10	13.66***	0.00	225.52***	0.00	48.06***	0.00
R2	32.80***	0.00	40.92***	0.00	26.04***	0.00	12.79***	0.00	16.72***	0.00	228.42***	0.00	44.47***	0.00
R3	34.22***	0.00	47.38***	0.00	31.38***	0.00	4.52	0.10	15.65***	0.00	227.08***	0.00	49.49***	0.00
R4	33.40***	0.00	42.31***	0.00	26.32***	0.00	10.78***	0.00	23.84***	0.00	229.46***	0.00	46.57***	0.00
R5	38.28***	0.00	47.98***	0.00	26.99***	0.00	9.93***	0.01	17.05***	0.00	230.89***	0.00	46.98***	0.00
R6	31.99***	0.00	43.14***	0.00	20.94***	0.00	3.93	0.14	14.54***	0.00	223.40***	0.00	48.52***	0.00
R7	35.02***	0.00	37.64***	0.00	22.44***	0.00	4.63*	0.10	17.05***	0.00	227.05***	0.00	48.32***	0.00
R8	33.67***	0.00	39.27***	0.00	22.29***	0.00	6.65**	0.04	16.20***	0.00	227.77***	0.00	47.10***	0.00
R9	34.59***	0.00	40.10***	0.00	19.86***	0.00	8.71**	0.01	19.39***	0.00	227.36***	0.00	46.60***	0.00
R10	35.71***	0.00	38.49***	0.00	18.78***	0.00	6.06**	0.05	15.41***	0.00	226.33***	0.00	47.21***	0.00
R11	34.34***	0.00	40.08***	0.00	20.78***	0.00	5.76*	0.06	13.67***	0.00	228.11***	0.00	47.78***	0.00
R12	32.75***	0.00	41.39***	0.00	24.15***	0.00	6.63**	0.04	14.02***	0.00	225.41***	0.00	46.34***	0.00
R13	32.57***	0.00	41.50***	0.00	27.70***	0.00	5.61*	0.06	14.14***	0.00	225.13***	0.00	46.91***	0.00
R14	33.36***	0.00	40.05***	0.00	22.86***	0.00	6.25**	0.04	18.17***	0.00	228.40***	0.00	47.39***	0.00
R15	34.30***	0.00	38.99***	0.00	19.68***	0.00	4.12	0.13	16.33***	0.00	225.37***	0.00	48.48***	0.00
R16	32.12***	0.00	36.74***	0.00	21.19***	0.00	4.02	0.13	15.73***	0.00	222.73***	0.00	47.72***	0.00
R17	33.03***	0.00	36.69***	0.00	18.56***	0.00	3.99	0.14	15.62***	0.00	224.02***	0.00	48.11***	0.00
R18	34.21***	0.00	38.02***	0.00	29.39***	0.00	5.52*	0.06	16.28***	0.00	227.98***	0.00	47.99***	0.00
R19	37.78***	0.00	37.77***	0.00	19.61***	0.00	3.95	0.14	13.61***	0.00	226.83***	0.00	49.53***	0.00
R20	32.45***	0.00	37.23***	0.00	20.16***	0.00	3.80	0.15	16.42***	0.00	224.64***	0.00	49.35***	0.00
R21	33.90***	0.00	37.56***	0.00	24.16***	0.00	5.12*	0.08	17.04***	0.00	226.01***	0.00	47.83***	0.00
R22	33.84***	0.00	42.82***	0.00	22.37***	0.00	4.09	0.13	18.05***	0.00	227.09***	0.00	49.38***	0.00
R23	37.25***	0.00	36.69***	0.00	21.22***	0.00	4.09	0.13	15.09***	0.00	226.66***	0.00	49.48***	0.00
R24	33.81***	0.00	37.17***	0.00	19.70***	0.00	5.58*	0.06	13.76***	0.00	224.66***	0.00	46.49***	0.00

Notes: * p<0.1, ** p<0.05 *** p<0.01.

is, we still find that the results from 1:1 treatment are quite different from those of other treatments.

Table 6: Random effects regression of misbidding (absolute difference of bid minus induced value)

	(1)		(2)	
Constant	0.824***	(0.202)	1.408	(1.758)
0.25 token:1 €	-0.079	(0.269)	-0.098	(0.269)
1 token:1 €	0.592**	(0.269)	0.481*	(0.275)
10 tokens:1 €	-0.231	(0.269)	-0.352	(0.275)
25 tokens:1 €	-0.165	(0.269)	-0.172	(0.278)
Males			-0.478***	(0.177)
Age			-0.042	(0.054)
Household size			0.054	(0.108)
$Income_2$			0.041	(1.142)
$Income_3$			0.154	(1.125)
$Income_4$			0.341	(1.094)
$Income_5$			0.443	(1.103)
R2	0.209**	(0.100)	0.209**	(0.100)
R3	0.190*	(0.100)	0.190*	(0.100)
R4	0.237**	(0.100)	0.237**	(0.100)
R5	0.254**	(0.100)	0.254**	(0.100)
R6	0.114	(0.100)	0.114	(0.100)
R7	0.039	(0.100)	0.039	(0.100)
R8	-0.015	(0.100)	-0.015	(0.100)
R9	0.081	(0.100)	0.081	(0.100)
R10	-0.013	(0.100)	-0.013	(0.100)
R11	-0.000	(0.100)	-0.000	(0.100)
R12	-0.047	(0.100)	-0.047	(0.100)
R13	-0.082	(0.100)	-0.082	(0.100)
R14	0.037	(0.100)	0.037	(0.100)
R15	-0.026	(0.100)	-0.026	(0.100)
R16	-0.098	(0.100)	-0.098	(0.100)
R17	-0.106	(0.100)	-0.106	(0.100)
R18	0.018	(0.100)	0.018	(0.100)
R19	-0.090	(0.100)	-0.090	(0.100)
R20	-0.158	(0.100)	-0.158	(0.100)
R21	-0.009	(0.100)	-0.009	(0.100)
R22	0.028	(0.100)	0.028	(0.100)
R23	-0.058	(0.100)	-0.058	(0.100)
R24	-0.194*	(0.100)	-0.194*	(0.100)
σ_u	1.060***	(0.062)	1.051***	(0.063)
σ_ε	0.891***	(0.010)	0.891***	(0.010)
N	3840		3840	
Log-likelihood	-5317.434		-5317.051	
AIC	10694.867		10708.101	
BIC	10882.464		10939.471	

Standard errors in parentheses. * p<0.1, ** p<0.05 *** p<0.01

4 Conclusion

Despite the widespread use of tokens, Francs, and other non-cash experimental currency units (ECUs), relatively little is known about the effects of such "money veils" in economic experiments. The results presented here suggest that using ECUs causes little harm but also conveys few benefits insofar as generating behavior consonant with theoretical predictions. One potential implication of these results is that experimenters might be able to spread scarce research dollars over more subjects by using a weaker ECU. However, our findings also suggest one danger in this approach. When experimenters use ECUs for no obvious reason (i.e., use a 1-to-1 conversion between the ECU and cash), we find that subjects are less likely to behave as theory predicts. Such an "obviously" unnecessary move might cause subjects to view the experiment as a game or to seek retribution (or other non-experimental rewards) when experimenters purposefully and transparently make an experiment more confusing than need be.

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A Appendix: Experimental Instructions

[This is an English translation of the original instructions written in Greek]
[Instructions were appropriately adjusted for the different token treatments]

Welcome!
Thank you for agreeing to participate in this survey. The survey concerns the economics of decision making.

Before we begin, I will ask each of you to draw a four digit number from this urn.

This four digit number is unique for each one of you across all sessions we will be conducting. That is, no other subject participating in this survey will have the same number. We will use this ID number for your payment.

After I explain the tasks, you will start making decisions using your computer. Before we begin with the tasks, you'll have to enter the four digit ID in an input screen to your computer. After we finish the session I will only know earnings corresponding to each ID number. You'll exit the room temporarily, I will then put the money in an envelope, seal the envelope, write the number on the back of the envelope and call you back one-by-one to the room to exchange your printed ID number with the corresponding envelope. I will not ask for your name at any point of the procedure. Thus, I cannot link your name with any profits you make today and this guarantees that the whole procedure is anonymized. All records and published results will be linked to four digit numbers, and not to your name. Please keep your printed codes private and do not share the information with anyone else.

[Each subject picks a number]

You should read all instructions carefully and answer any questions accurately. I will also read instructions loudly. It is very important to understand instructions because you might earn or lose money depending on your decisions.

It is very important that you do not communicate with any other participant. We want to know how you make decisions alone and not under the influence of someone else.

For your participation in the experiment you will receive the amount of 10€. The money given for your participation is yours to use as you wish.

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The procedure

In the tasks to follow you will participate in a type of auction known as a 2nd price auction. The 2nd price auction has 4 basic steps:

Step 1: We'll describe to you the product to be auctioned

Step 2: Each one of you, will submit a bid for buying the product

Step 3: The computer will rank all bids from highest to lowest

Step 4: The person(s) that submit the highest bid buys the product **but will pay the price of the second highest bidder**. If you don't bid the highest price then you dont purchase the good.

Consider this numerical example:

Suppose 8 people bid in an auction in order to buy a USB memory stick (16GB). Each bidder submits a bid separately. The submitted bids are given in the table below:

Person	**Bid**
1	12
2	15
3	20
4	18
5	30
6	25
7	35
8	32

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After ranking bids from highest to lowest (this is done by the computer internally), we have:

Person	**Bid**
7	**35**
8	32
5	30
6	25
3	20
4	18
2	15
1	12

Person 7 purchases the good because s/he bid the highest price (35) but s/he only pays 32 (second highest bid). All the other participants in the auction pay nothing and do not receive a memory stick.

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The training

We will now do a training task. This task is designed to allow you to familiarize yourself with the 2nd price auction and it doesn't count toward your earnings. We will repeat this task for three rounds. We will then select one round as binding by having one of you selecting a number from 1 to 3 from an urn. The numbers correspond to rounds, so if s/he picks number 1 then round 1 is binding, if s/he picks number 2 then round 2 is binding etc. Although we say binding, this is just for demonstration since the task will not count toward your earnings.

In this training task and the real task that will follow, you will bid in **Lab Currency Units (LCUs)**. That is, if you bid a number of 4, this means 4 LCUs. Lab currency units will be exchanged with Euros at the end of the session at this exchange rate: **10 LCU = 1 €**. That is, if you make a profit of 20 LCUs you will get 2 €on top of your participation fee. On the other hand if you make a loss of 10 LCUs we will deduct 1 €from your participation fee.

In this task we will proceed as follows:

You will bid for a fictitious product. This fictitious product has a **resale value**. The resale value is the value which we will pay you to buy back the fictitious product, if you actually purchase it. If you do not purchase the product then you cannot trade back the product to us. Resale values may be different for each participant. An example of this procedure can be described with the following steps:

Step 1: Each bidder looks at his/her *resale value* displayed at the computer screen

Example screen:

Round 1
Your resale value is 28.4 LCUs

Please submit your bid (in LCUs):

[input]

Step 2: Each bidder then submits a bid for the resale value displayed on the screen

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Step 3: Computer ranks bids from highest to lowest

Step 4: The second highest bid determines the price at which the good will be purchased from the highest bidder

Step 5: The buyer who bids the highest price purchases the good at the second highest bid

Step 6: The person who purchases the good, sells the good back to us **at the resale value**

Therefore, your net position is **resale value - 2nd highest price**.

You make a profit if **resale value>2nd highest price**, since:

Profit= resale value - 2nd highest price>0

or a loss if **resale value<2nd highest price**, since:

Loss= resale value - 2nd highest price<0

Step 7: Bidders at or below the 2nd highest price do not purchase the good and thus will make zero profit/loss

Step 8: Auction ends after the last round

After each round you will get some feedback. In case you are not the highest bidder in any given round your feedback screen will look something like this:

Your resale value in round 1 was 28.4 LCUs

Your bid was **AA** LCUs

You were not the highest bidder. Your profit/loss for the previous round is zero.

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In case you are the highest bidder in any given round your feedback screen will look something like this:

Your resale value in round 1 was 28.4 LCUs

Your bid was **AA** LCUs
Second highest price was **BB** LCUs

You are the highest bidder. Your profit/loss for the previous round is **28.4-BB=CC** LCUs.

[Training task is performed]

[One person is randomly selected to pick a number from the urn]

[Proctor asks: Are there any questions? Is everybody absolutely sure that s/he understood the task?]

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The actual auction

We will now move on to the actual task. This time it is for real. That is, profits will be added to your endowment while losses will be deducted. We will do **24 rounds** for this task. In each round each person will submit his/her bid for the object. After the last round we will randomly select one round as binding by having one of you selecting a number from an urn. The numbers correspond to rounds, so if s/he picks number 1 then round 1 is binding, if s/he picks number 2 then round 2 is binding etc. Only the binding round will count toward your profits/losses.

[Proctor asks: Are there any questions?]

[Real task is performed]

[One person is randomly selected to pick a number from the urn]

[Profits/losses are determined from the computer which also converts tokens in €(only for the token treatments)]

[Subjects leave the room temporarily; the experimenter puts money into envelopes and writes the corresponding four digit code on the back of the envelope; subjects return to the room and exchange their code with the envelope]

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B Appendix: Literature using Experimental Currency Units

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