The veil of experimental currency units in second price auctions

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Abstract Economic experiments often entail subjects making decisions with payoffs in experimental currency units (ECUs). Earnings in ECUs are converted to cash at the end of the experiment. Such a procedural choice seems to be driven more by habit or tradition than by empirical evidence that this is more appropriate to use. We report results of a private, induced value second price auction (SPA) experiment in which we manipulate the exchange rate between ECUs 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 relates to the comparison of the cash-only condition to the one-to-one exchange condition. The latter produced greater behavioral deviations from theoretical predictions. However, we find that this effect is largely driven by a handful of subjects. The

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results suggest that the use of ECUs is not a hindrance for experimental practice, at least not in the context of an induced value SPA.

Keywords Experimental currency units · ECU · Tokens · Vickrey auction · Induced values

JEL Classification C90

1 Introduction

Denominating subjects' payoffs using an experimental currency unit (ECU), which are later converted into cash, is a practice often employed in experimental economics.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. 25–26) were perhaps the first to outline some hypotheses about the use of ECUs in their now classic “Experimental Economics” book. Surprisingly, they advise against using ECUs in experiments, unless “the researcher has a specific design motivation for using a laboratory currency”. They reason that ECUs may “mask or even dilute financial incentives” and create an artificial “game-board sense” promoting “speculative competitiveness”.

This paper explores the use of ECUs as a design choice. Although the types of experiments in the literature that use ECUs are widely varied, we chose to further explore this issue using a private, induced value 2nd price auction. Theory has a clear prediction about behavior in the second price auction (i.e., theory predicts that subjects will bid their induced value). However, there is an active debate about whether the theory holds in practice [see for example the contrasting evidence in Kagel et al. (1987), Harstad (2000), Parkhurst et al. (2004), Lusk and Shogren (2007)]. The fact that the debate is still active suggests that at the very least, there are a variety of factors or incentives that might explain when and why the theory accurately predicts behavior.

Most relevant to our work is the possible money illusion effect from the use of ECUs as suggested by Davis and Holt (1993). There is a growing literature which finds evidence of money illusion, which can be broadly defined as subjects responding differently to conditions framed in nominal vs. real terms.² A money illusion effect might be desirable because it can increase incentives (and reduce experiment costs) if the subject puts effort into the tasks proportional to the nominal value of the currency. For example, 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.

¹ A non-exhaustive list of possible synonyms one may encounter in the literature are ‘tokens’, ‘francs’, ‘talers’ and laboratory currency units—‘LCUs’.

² This research stream uses survey data [e.g. Shafir et al. (1997)] as well as experimental data [e.g. Boes et al. (2007), Fehr and Tyran (2001, 2007), Petersen and Winn (2014), Tyran (2007), Weber et al. (2009), Wertenbroch et al. (2007)].

We conduct an experiment where we vary the experimental currency-Euro exchange rate and the use/non use of ECUs. Our benchmark is a treatment where no ECUs are used (i.e., subjects bid in the euro currency). In the other treatments, the exchange rate varies by treatment as follows: (i) 0.25 ECUs for 1 €, (ii) 1 ECU for 1 €, (iii) 10 ECUs for 1 € and (iv) 25 ECUs for 1 €. The distribution of actual stakes in Euros, is the same across all treatments and only the framing of the stakes in ECUs changes across treatments.

In what follows, we briefly discuss the relevant literature, then describe our experimental design, and present some descriptive analysis and econometric results. We conclude in the last section.

2 Literature review

The work most related to our study is that by Reinstein and Riener (2012) and Shen and Takahashi (2013). Reinstein and Riener (2012) vary the tangibility of the payment in the context of a modified Dictator game where contributions go to a charity instead of other subjects: they either give cash to the subjects before they decide how much to donate (subjects have to physically hold and place any donations they make into envelopes) or they allocate their donation from the computer screen and are paid cash at the end of the experiment (only Euros where used in their experiment; not ECUs). Shen and Takahashi (2013) examine behavior in an ultimatum game where proposers physically place offers in an envelope (from money they hold in their hands) or they allocate points which are exchanged to cash at the end of the experiment. Thus, both experiments examine cash effects (i.e., physically holding cash) by comparing behavior with an ECU treatment which uses a 1:1 exchange rate (this is similar to one of our treatments). Reinstein and Riener (2012) find that in the dictator game, subjects donated significantly less in the cash condition while Shen and Takahashi (2013) find that compared to the point sessions, proposers in the ultimatum game offered more and responders rejected less frequently in the cash sessions.

An additional way that ECUs can affect behavior is described by Mazar et al. (2008) who studied how tokens can affect dishonest behavior by making the moral implications of dishonesty less accessible. In one treatment, subjects received one token per correct task, 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 ECUs facilitate elicitation of insincere responses, then the task of discovering subjects' "true" preferences would be made more difficult using ECUs.³

³ There is also the suggestion of Hsee et al. (2003) that a medium of exchange (like ECUs) can disguise the final outcome of an action. The medium maximization hypothesis 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 to appear linear.

3 Experimental design

3.1 Description of the experiment set-up

In order to test our research hypothesis, we designed an experiment with five treatments. One of the treatments is the control treatment in which subjects bid in the Euro nominal currency. In the other treatments, subjects bid in ECUs with the understanding that ECUs are exchanged into Euros at the end of the experiment. We varied the exchange rate of ECUs and Euros as follows: (i) Treatment 1: 0.25 ECUs are exchanged with 1 €, (ii) Treatment 2: 1 ECU is exchanged with 1 €, (iii) Treatment 3: 10 ECUs are exchanged with 1 €, (iv) Treatment 4: 25 ECUs are exchanged with 1 €. Treatment 5 is the control treatment.

Subjects in the experiments took part in a multi-round auction where they submitted non-hypothetical bids for their private 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).

3.2 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 2015). 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 treatment consisted of four sessions with eight subjects each (i.e., 32 subjects per treatment) for a total of 160 subjects.⁴ Our experiment was conducted from November 20 to December 5, 2012 during the middle of the week (Tuesday, Wednesday, Thursday).

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 (ID) 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 ECUs and were properly scaled up or down.⁵

⁴ Our sample size was largely determined via a conventional sample size calculation which is laid out in “Appendix”.

⁵ For example in the 10 ECUs:1 € treatment the set of induced values was: 3.0, 13.2, 27.5, 39.1, 47.9, 55.4, 60.6, 67.2.

By the 8th round, each subject experienced the full range of the induced values permitting all possible permutations among individual induced values. In rounds 9–16, subjects experienced the same set of induced values, albeit in a different (random) order. The same is true for rounds 17–24. In all, subjects were assigned a set of 8 induced values in blocks of 8 rounds. Each block of 8 rounds was repeated 3 times so that $3 \text{ blocks} \times 8 \text{ rounds} = 24 \text{ rounds}$. Thus, each bidder experienced each induced value three times across the range of 24 rounds. In any given round, each subject experienced a different induced value from those of other subjects (drawn randomly from the set of the 8 induced values) so that the whole demand curve was induced in every round. None of the bidders knew anything about the other bidders' induced value or the ID 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 were 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 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 3€ show-up fee and a 7€ participation fee, for a session that lasted about 45 min. All transactions were completed at the end of the experiment. Before the sessions started, subjects were given detailed written instructions (shown in the Online Supplementary Material S.3) 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. 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.

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, participation and show-up fees 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 (including participation fees) were €9.85 (SD = 1.05, min = 1.8, max = 11.72).

4 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

Fig. 1 Ratio of revealed demand over induced demand in % per round and treatment

chi-squared test shows that gender is not significantly different between treatments at the 5 % level ($\chi^2 = 8.46$, p-value = 0.08).

4.1 Descriptive analysis

We first consider aggregate behavior by round in each treatment. First, we convert all bids and induced values from the ECUs treatments into Euros using the exchange rate applied in each treatment. In each treatment, aggregate ID equals 125.56 (this is the sum of induced values times the number of sessions per treatment i.e., $4 * (0.30 + 1.32 + 2.75 + 3.91 + 4.79 + 5.54 + 6.06 + 6.72)$). Figure 1 graphs the ratio of the (aggregate) revealed demand (RD) over ID. The horizontal solid line in the figure signifies perfect demand revelation and is the benchmark. With respect to the control treatment (i.e., Euros, no ECUs) it is evident that aggregate behavior is not perfectly demand revealing, although it becomes very close in the last few rounds (for example, in rounds 20, 23 and 24 the RD/ID ratio is below 105 %). This result is consistent with conventional wisdom that it takes several rounds before subjects learn the dominant strategy. Across all rounds, the total demand revelation ratio for the control treatment is 109.86 %.

When we compare the ECUs treatments with the control treatment, we observe a similar pattern for all treatments with the exception of the 1 ECU:1 € treatment. The revelation ratio is much worse for this treatment. Aggregate RD/ID ratios for the rest of the treatments are comparable to the control treatment. The fact that the revelation ratio is >100 across (almost) all rounds implies that people tend to overbid.⁶ This is

⁶ 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.

Fig. 2 Ratio of revealed demand over induced demand in % per induced value and treatment

consistent with the findings of Kagel et al. (1987) and Harstad (2000) which find overbidding in second price auctions in the context of private values.

To examine how demand revelation depends on the magnitude of induced values, Figure 2 graphs the RD/ID ratio by induced values. First, with larger induced values the revelation ratio improves so that for most of the treatments (with the exception of the 1 ECU:1 € treatment), the revelation ratio is close to 100 % for the highest induced value. On the other hand, for smaller induced values like the 0.30 €, the revelation ratio ranges from 252 % (25 ECUs: 1 € treatment) to 603 % (1 ECU: 1 € treatment). This is consistent with the prediction in Lusk et al. (2007) that the incentives for an individual to bid optimally in a second price auction increase with the individuals' values. This results, in part, because the chances of winning of a low-value individual in a second price auction is low even if they overbid by a relatively large amount. The upper part of Fig. 2 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 S.1 in the Online Supplementary Material, graphs the scatter diagrams of the bids and induced values by treatment as well as the linear predictions with their respective slopes against the 45° line. Table S.2 in the Online Supplementary Material, shows the summary statistics of the experimental data along with several statistical tests that support the discussion above.

4.2 Econometric analysis

The theoretical prediction for the SPA is that bids should reflect a bidder's private value. For this reason, although the behavior in Fig. 1 raises questions about the linear relationship between induced values and bids, we specify the bidding function

as linear in induced value. This allows us to directly test the assumption of perfect demand revelation. Because we have individual-level observations (bids from each subject) for multiple rounds, we have a panel data set. Similar to Cherry et al. (2004) and Jacquemet et al. (2013), we specify the bid function as:

$$bid_{it} = b_0 + b_1 IV_{it} + \sum_{m=2}^5 (b_m Treat_i^m + b_{m+4} IV_{it} Treat_i^m) + \rho_t + u_i + \varepsilon_{it} \quad (1)$$

In Eq. (1) bid_{it} is subject i 's bid in round t , IV_{it} denotes subject i 's induced value in round t , $Treat_i^m$ are dummies for the treatments, ρ_t are round fixed effects, u_i are individual specific random effects and ε_{it} is a round specific error term. The specification also includes the interaction of the induced value variable and the treatment variables. Wald tests of $H_0 : (b_0 + b_m) = 0, (b_1 + b_{m+4}) = 1, \rho_t = 0$ can provide a formal test of perfectly demand revealing bids.⁷ Econometric results of Eq. (1) are displayed in column (2) of Table 1.

Column (1) in Table 1 shows estimation results without the interaction terms specified in Eq. (1). As evident, bids increase with the magnitude of induced value and there is a significant positive treatment effect for the 1:1 treatment. Estimation results from column (2) in Table 1 further support these results. We find that subjects bid on average 0.71€ [based on specification (2)] more in the 1 ECU:1€ treatment as compared to the no ECUs treatment and that bids increase more rapidly with higher induced values in the 10 ECUs:1€ treatment than in the no ECUs treatment. However, Table 1 by itself does not provide information about the demand revelation properties of the different treatments.

Table S.1 in the Online Supplementary Material, presents results from Wald tests [based on the model estimated in column (2)] of the demand revealing hypothesis we have sought to test with the specification we have set forth in Eq. (1). As shown, the hypothesis of perfect demand revelation is rejected in all treatments and rounds with the exception of the 10 ECUs:1 € treatment. In the 10 ECUs:1€ treatment, we fail to reject the null across almost all rounds.

Another way to look at our data is to regress absolute deviations from induced values $|bid_{it} - IV_{it}|$ on the treatment dummies. Table S.3 in the Online Supplementary Material shows the results from random effects regressions with and without demographic variables. Results show once more a significant effect for the 1:1 treatment. In addition to that, Table S.3 shows a highly significant effect of the male variable. This effect is also evident in column (3) of Table 1 and prompted us to check whether the treatment effects are gender specific. Specification (3) in Table S.3 and specification (4) in Table 1 interact the male dummy with the treatment variables and reveal an *economically* significant interaction effect for the 1:1 treatment. Table S.3 shows that the effect of the 1:1 treatment is *statistically* significant as well. This signifies that females exhibit a different behavior than males in the 1:1 treatment.

⁷ Because 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 1 Random effects regression of bids on induced values

	(1)	(2)	(3)	(4)	(5)
Constant	0.611*** (0.222)	0.730*** (0.229)	0.794*** (0.239)	0.806*** (0.297)	0.798*** (0.228)
IV	0.929*** (0.007)	0.899*** (0.017)	0.929*** (0.007)	0.929*** (0.007)	0.930*** (0.007)
0.25 token: 1 €	-0.068 (0.291)	-0.114 (0.305)	-0.068 (0.288)	-0.231 (0.403)	-0.231 (0.301)
1 token: 1 €	0.840*** (0.291)	0.709** (0.305)	0.760*** (0.291)	0.945** (0.371)	0.281 (0.286)
10 tokens: 1 €	-0.156 (0.291)	-0.459 (0.305)	-0.213 (0.289)	-0.278 (0.378)	-0.278 (0.283)
25 tokens: 1 €	-0.076 (0.291)	-0.190 (0.305)	-0.042 (0.288)	-0.150 (0.426)	-0.150 (0.318)
0.25 token: 1 € × IV	-	0.012 (0.023)	-	-	-
1 token: 1 € × IV	-	0.033 (0.023)	-	-	-
10 tokens: 1 € × IV	-	0.077*** (0.023)	-	-	-
25 tokens: 1 € × IV	-	0.029 (0.023)	-	-	-
Males	-	-	-0.365* (0.188)	-0.390 (0.403)	-0.390 (0.301)
0.25 token: 1 € × males	-	-	-	0.325 (0.570)	0.325 (0.426)
1 token: 1 € × males	-	-	-	-0.677 (0.603)	-0.013 (0.456)
10 tokens: 1 € × males	-	-	-	0.177 (0.585)	0.177 (0.438)
25 tokens: 1 € × males	-	-	-	0.186 (0.575)	0.186 (0.430)
Round dummies	Yes	Yes	Yes	Yes	Yes
σ_u	1.147*** (0.066)	1.147*** (0.066)	1.133*** (0.065)	1.122*** (0.065)	0.829*** (0.049)
σ_e	0.991*** (0.012)	0.990*** (0.012)	0.991*** (0.012)	0.991*** (0.012)	0.973*** (0.011)
<i>N</i>	3840	3840	3840	3840	3768
Log-likelihood	-5695.546	-5689.230	-5693.685	-5692.053	-5472.982

IV stands for induced value; Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Specification (5) excludes three female subjects which significantly misbid i.e., their absolute difference between bid and induced value was >1 standard deviation of the misbids in the same treatment

This result prompted us to conduct a more thorough investigation of the role of the gender variable. By simply tabulating mis-bids in the 1 ECU:1€ treatment with a value greater than a standard deviation of misbids in the same treatment, we identified three subjects which significantly misbided across all 24 rounds (for two out of three subjects) and over 22 rounds (for one out of three subjects). These three subjects are all females. If we exclude these three female subjects from model estimation, the significant effect of the 1 ECU:1€ treatment disappears [model (5) in Table 1 and model (4) in Table S.3]. This leads us to conclude that although on average subjects exhibit different behavior in the 1 ECU:1 € treatment, this result is driven by a handful of subjects.

5 Conclusion

Despite the widespread use of ECUs, relatively little is known about the effects of such “money veils” in economic experiments. The results presented here suggest that using ECUs in a second price auction setting causes little harm but also conveys few benefits insofar as generating behavior consonant with theoretical predictions. Because we do not find an effect on misbidding for the 10:1 and 25:1 treatments with respect to the control (no ECUs) treatment, our data do not support a ‘money illusion’ explanation.

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 across 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. In addition, using ECUs may allow researchers to reduce the cognitive load of the experimental instructions as well as provide ease in explaining the random lottery incentive payoff mechanism. Although our design does not allow us to distinguish between these mechanisms, this is a fruitful area for future research. In addition, we find that the 10:1 is demand revealing while none of the other treatments is (including the no ECUs treatment). 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 in auction experiments. However, it remains to be seen why one exchange rate produced this effect while an even weaker exchange rate (i.e., the 25:1 treatment) did not. One possible, albeit speculative, explanation is that the 10:1 treatment facilitated comparisons due to an easier conversion to the nominal currency while the 25:1 treatment did not.

However, our findings also suggest one potential danger in the use of ECUs. When experimenters use ECUs for no obvious reason (i.e., use a one-to-one conversion between the ECU and cash), we find that subjects are less likely to behave as theory predicts. The 1 ECU:1€ treatment generates more misbidding than the other treatments. A possible explanation can be found in what Davis and Holt (1993, p. 26) call “speculative competitiveness” wherein payoffs in laboratory

currency may create an artificial “game-board” sense. Arguably, the one-to-one 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. This “game board” explanation is also plausible under *cognitive* Experimenter Demand Effects (Zizzo 2010), if one assumes that the 1:1 exchange rate prompts subjects to try to make sense of the experimental environment or if it draws attention to the experimental variable of interest.

Our data unfortunately do not allow us to distinguish between these two explanations. Subjects’ experience cannot explain any of the results we observe, since about 70 % of our sample (112 out of 160 subjects) had never participated in an experiment before, while only 30 % of our sample had participated before in a risk preferences experiment. Unfortunately, since both experiments were anonymous, we cannot directly link this past experience with behavior in the current experiment. In addition, when we scrutinized our data further, we find that the result regarding the 1:1 treatment is mainly driven by three female subjects. Although, the 1:1 conversion rate remains an anomaly for a handful of subjects, we do not expect this to be a significant hindrance for future experimental practice, at least in second price auction contexts, given that the number of experiments that use such an exchange rate is likely small, if not zero.

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Appendix

Sample size calculations

The sample size calculations presented below are considering as the variable of interest absolute deviations from induced values $|bid - IV|$. To determine our sample size we followed two approaches. The first approach requires specifying less parameters albeit at the expense of a stronger assumption (i.e., that the correlation ρ of bids between rounds for the same subject is zero). Assuming $\alpha = 0.05$ (Type I error) and $\beta = 0.20$ (Type II error), the per group (treatment) minimum sample size required to compare two means μ_0 and μ_1 , with common variance of σ^2 in order to achieve a power of at least $1 - \beta$ is given by Kupper and Hafner (1989):

$$n^A = \frac{2(z_{1-\alpha/2} + z_{1-\beta})^2}{\left(\frac{\mu_0 - \mu_1}{\sigma}\right)^2} \quad (2)$$

For $\alpha = 0.05$ and $\beta = 0.20$ the values of $z_{1-\alpha/2}$ and $z_{1-\beta}$ are 1.96 and 0.84, respectively; and $2(z_{1-\alpha/2} + z_{1-\beta}) = 15.68$, which can be rounded up to 16. The formula then collapses to $n = \frac{16}{\Delta^2}$ (with $\Delta = \frac{\mu_0 - \mu_1}{\sigma}$), which is known as Lehr’s (1992) equation [see also Chapter 2 in van Belle (2008)]. To calculate a minimum sample size one needs to feed the above formula with values for σ and the minimum meaningful difference $\mu_0 - \mu_1$.

The formula above does not take into account the fact that subjects submit multiple bids in our experiment i.e., it just treats data as cross-sectional data. To take into account the repeated measurement, the formula is augmented to include the number of repeated measurements M (i.e., auction rounds in our case) as well as a value for the correlation ρ between observations on the same subject. The per group/treatment minimum sample size (i.e., number of subjects per group, *not* number of observations per group) is then given by Diggle et al. (2002, p. 30) and Liu and Wu (2005):

$$n^B = \frac{2(z_{1-\alpha/2} + z_{1-\beta})^2(1 + (M-1)\rho)}{M\left(\frac{\mu_0 - \mu_1}{\sigma}\right)^2} \quad (3)$$

Note, that formula 2 is a special case of formula 3, so that for $\rho = 0$ it is $n^B = \frac{n^A}{M}$. To specify the necessary parameters for the above formulas we used data from two older 2nd price induced value experiments (Corrigan et al. 2012; Lusk and Rousu 2006). Since these two studies used different sets of induced values, we first scaled down induced values by a factor necessary to scale the largest induced value in each dataset to 6.72 (which is the largest induced value in the present study). We then scaled down by the same factor bids and calculated the standard deviation (which will be used in place for σ) of $|bid - IV|$. This transformation basically scales down the standard deviation by the same factor. We calculated a value of $\sigma = 1.32$ for Corrigan et al. (2012) and $\sigma = 0.99$ for Lusk and Rousu (2006). We also calculated intraclass correlation coefficients of $\rho = 0.54$ and $\rho = 0.33$ for Corrigan et al. (2012) and Lusk and Rousu (2006), respectively.

Consequently, we calculated the minimum sample size for a range of values of σ between 0.8 and 1.4 with a step of 0.1 and for three values of ρ (0.1, 0.3 and 0.5). As the minimum meaningful difference, $d = \mu_0 - \mu_1$, we used values of 0.1 to 0.5 with steps of 0.05 which matches well with the range of treatment effects reported in Table S.3.

Table 2 shows sample size calculations for different values of σ , ρ and d . Entries for n^A show number of observations⁸ while entries for n^B show number of subjects. It is obvious that the lower the minimum meaningful difference d , the higher the standard deviation σ and the higher the correlation ρ , a larger sample size is needed to detect the desired effect size with 80 % power. The first row of each panel of minimum meaningful difference, lists the calculation for n^A which is basically equivalent to n^B (adjusted for M) with a $\rho = 0$. Table 2 highlights in bold all cases for which the per treatment sample size of our experiment (768 observations or, equivalently, 32 subjects) is lower or equal than the corresponding calculation i.e., cases for which the sample size of the present study can safely detect the minimum meaningful difference with 80 % power.

⁸ For this calculation we treat multiple observations per subject as independent i.e., $\rho = 0$; if n^A is then divided by 24 which is the number of rounds, the resulting number corresponds with number of subjects.

Table 2 Sample size calculations for different values of σ , ρ and d

	$\sigma = 0.8$	$\sigma = 0.9$	$\sigma = 1$	$\sigma = 1.1$	$\sigma = 1.2$	$\sigma = 1.3$	$\sigma = 1.4$
$d = 0.1$							
n^A	1005	1272	1570	1899	2261	2653	3077
n^B							
$\rho = 0.1$	138	175	216	261	311	365	423
$\rho = 0.3$	331	419	517	625	744	873	1013
$\rho = 0.5$	523	662	818	989	1177	1382	1602
$d = 0.15$							
n^A	447	565	698	844	1005	1179	1367
n^B							
$\rho = 0.1$	61	78	96	116	138	162	188
$\rho = 0.3$	147	186	230	278	331	388	450
$\rho = 0.5$	233	294	363	440	523	614	712
$d = 0.2$							
n^A	251	318	392	475	565	663	769
n^B							
$\rho = 0.1$	35	44	54	65	78	91	106
$\rho = 0.3$	83	105	129	156	186	218	253
$\rho = 0.5$	131	166	204	247	294	345	401
$d = 0.25$							
n^A	161	203	251	304	362	424	492
n^B							
$\rho = 0.1$	22	28	35	42	50	58	68
$\rho = 0.3$	53	67	83	100	119	140	162
$\rho = 0.5$	84	106	131	158	188	221	256
$d = 0.3$							
n^A	112	141	174	211	251	295	342
n^B							
$\rho = 0.1$	15	19	24	29	35	41	47
$\rho = 0.3$	37	47	57	69	83	97	113
$\rho = 0.5$	58	74	91	110	131	154	178
$d = 0.35$							
n^A	82	104	128	155	185	217	251
n^B							
$\rho = 0.1$	11	14	18	21	25	30	35
$\rho = 0.3$	27	34	42	51	61	71	83
$\rho = 0.5$	43	54	67	81	96	113	131
$d = 0.4$							
n^A	63	79	98	119	141	166	192
n^B							
$\rho = 0.1$	9	11	13	16	19	23	26
$\rho = 0.3$	21	26	32	39	47	55	63

Table 2 continued

	$\sigma = 0.8$	$\sigma = 0.9$	$\sigma = 1$	$\sigma = 1.1$	$\sigma = 1.2$	$\sigma = 1.3$	$\sigma = 1.4$
$\rho = 0.5$	33	41	51	62	74	86	100
$d = 0.45$							
n^A	50	63	78	94	112	131	152
n^B							
$\rho = 0.1$	7	9	11	13	15	18	21
$\rho = 0.3$	16	21	26	31	37	43	50
$\rho = 0.5$	26	33	40	49	58	68	79
$d = 0.5$							
n^A	40	51	63	76	90	106	123
n^B							
$\rho = 0.1$	6	7	9	10	12	15	17
$\rho = 0.3$	13	17	21	25	30	35	41
$\rho = 0.5$	21	26	33	40	47	55	64

Numbers in bold indicate cases for which the per treatment sample size of our experiment (768 observations or, equivalently, 32 subjects) is lower or equal than the corresponding calculation

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