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Abstract

Obviousness around the clock

by Yves Breitmoser and Sebastian Schweighofer-Kodritsch*

Li (2017) supports his theoretical notion of obviousness of a dominant strategy with experimental evidence that bidding is closer to dominance in the dynamic ascending clock than the static second-price auction (private values). We replicate his experimental study and add three intermediate auction formats to decompose this behavioral improvement into cumulative effects of (1) seeing an ascending-price clock (after bid submission), (2) bidding dynamically on the clock and (3) getting drop-out information. Li's theory predicts dominance to become obvious through (2) dynamic bidding. We find no significant behavioral effect of (2). However, both (1) and (3) are highly significant.

Keywords: strategy proofness, experiments, private value auction

JEL classification: C91, D44, D82

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1 Introduction

Why do people often fail to play a dominant strategy? Li (2017) suggests that such failures occur when recognizing dominance requires contingent reasoning. He proposes a formal definition for when dominance should be obvious from the extensive form of a game, calling a strategy *obviously* dominant, if, at any information set that may be reached with it, its worst outcome is still at least as good as even the best outcome under any alternative strategy that deviates there.[1] Empirical support for the behavioral validity of this concept comes from the prominent and otherwise puzzling experimental finding that, with private values, ascending-clock auctions produce strikingly more dominance play than sealed-bid second-price auctions (e.g., Kagel et al., 1987), since despite their strategic similarity, dominance is theoretically obvious only in the former but not in the latter.[2] Li's own experimental evidence confirms the earlier finding for auctions (with increased statistical power), and also adds further support from a comparison of random serial dictatorship mechanisms. Given the importance of strategy proofness (SP) in practical mechanism design, Li's work suggests that the analogously strengthened requirement of "obvious strategy proofness" (OSP) should be applied as a general selection principle that would prevent costly mistakes and mis-allocation, with potentially huge welfare benefits.[3]

In this paper, we revisit and augment the existing evidence on this theory of obviousness. Our starting point is the observation that this evidence, including Li (2017), is concerned with the *joint* effect of changing multiple design features at once, without cleanly isolating any of the changes that should theoretically cause dominance to become obvious. Thereby, it constitutes an unnecessarily weak test of the theory, in particular with regards to its reliability in selecting simpler mechanisms, and it provides little guidance for practical mechanism design subject to implementation constraints and trade-offs. Our study is designed to overcome these limitations. The resulting findings fundamentally challenge prior conclusions—in particular, we find that theoretical obviousness fails our stronger test—and our analysis also provides a deeper understanding of how design can help reduce mistakes.

Our experiment uses the well-documented online materials accompanying Li (2017) to replicate his high-powered experimental comparison of the sealed-bid second-price auction (2P) and the ascending-clock auction (AC) with the same sets of (private) valuation draws. Our innovation is to augment this prominent baseline comparison with three novel auction formats that allow us to decompose the overall behavioral difference between 2P and AC, as a joint effect, into the contributions of each of three basic design steps: *clock presentation* (bidders watch an ascending price clock resolve the bidding), *dynamic bidding* (bidders bid live on the clock), and *drop-out information* (bidders observe the number of opponents still bidding on the clock).[4]

Figure 1 offers a quick overview of our experimental design and decomposition results. The novel auction formats are AC-B, 2PAC and 2PAC-B. AC-B is an ascending-clock auction just like AC, except that it is "blinded," so that bidders do not observe their opponents' dropping out. Given private valuations, this information is theoretically irrelevant. This is true also for theoretical obviousness—AC-B is also OSP—and in this sense such information introduces a potential confound in attributing behavioral effects to the theory,

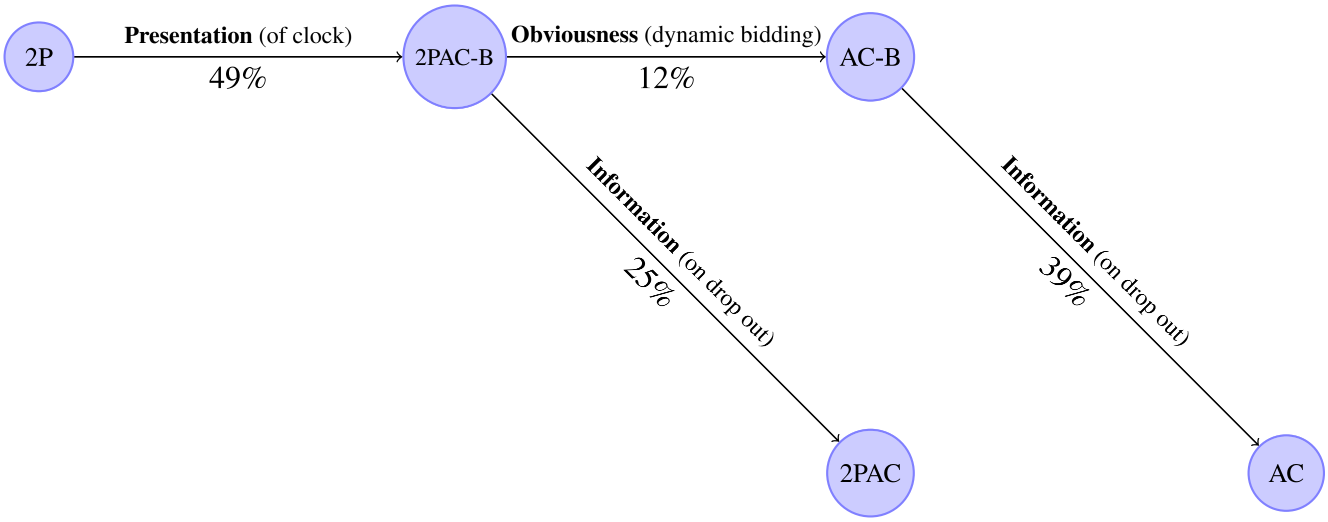
[1] Thus, "obvious dominance" operationalizes the (stronger) normal-form notion of "absolute dominance" (Weber, 1987) via the extensive form.

[2] The ascending-clock auction we refer to here is a special case of an English auction, sometimes called Japanese auction (Milgrom and Weber, 1982), where bid increments are exogenous and bidders simply indicate when they exit the auction.

[3] See Rees-Jones (2018) or Hassidim et al. (2018) for field evidence of important such mistakes.

[4] While we focus on the most prominent application of auctions with private valuations, where we can rely on a characterization of OSP mechanisms (Li, 2017, Theorem 3), our main lessons are general in nature.

Figure 1: Overview of auction formats analyzed in the experiment and their contributions to reduction in mean absolute deviations from truthful bidding



Note: The decomposition of the total effect reported here is derived from the decomposition of the mean absolute deviation of bids from values when we move from second-price auctions (2P) to ascending-clock auctions (AC). Specifically, using the "standardized" mean absolute deviations as reported in Table 2 (Appendix), the presentation effect is the average effect of moving from 2P to 2PAC-B after the first three auction rounds, and all other values are similarly derived, by moving from 2PAC-B via 2PAC or AC-B, respectively, to AC.

which our comparison of AC-B and AC isolates.[5] 2PAC and 2PAC-B both have bidders submit sealed bids just like 2P. However, these bids serve as automatic exit prices in a subsequently run ascending-clock auction, similar to "proxy bidding" introduced by eBay (e.g., Roth and Ockenfels, 2002), which is displayed either with drop-out information as in AC or blinded as in AC-B. Since bidders then cannot act anymore, these merely present the auction's outcome the same way as the dynamic ascending-clock formats; indeed, 2PAC and 2PAC-B are both formally identical to 2P and accordingly SP but not OSP. Importantly, moving from 2P to 2PAC-B isolates the effect of mere *clock presentation*, and then moving from 2PAC-B to AC-B isolates the effect of *dynamic bidding*. This latter step is here in fact necessary and sufficient for theoretical obviousness of dominance, whereby the comparison of 2PAC-B and AC-B affords a both cleaner and stronger test of the prediction that OSP reliably reduces mistakes. 2PAC, by comparison with 2PAC-B, allows us to also measure the role of drop-out information as part of the passive clock-presentation, where, in contrast to AC, bidders cannot respond to it live. This summarizes what we consider the most informative comparisons. However, following Li (2017, p. 3276), we also test the basic theoretical prediction that either OSP format significantly outperforms *any* of the mere SP formats in terms of dominance play, where the latter also includes 2PAC, of course.

Reassuringly, we find that our 2P and AC replicate the respective behavior in Li's study almost exactly. Also following his analysis, we focus on mean absolute deviations from truthful bidding for non-winning bids. We find that, after a brief initial phase of around three auction rounds, where participants underbid severely in *all* formats (as observed also by Noussair et al., 2004), the relative contributions stabilize: Clock-presentation (2P $\rightarrow$ 2PAC-B) accounts for around 50% in reducing deviations from truthful bidding, dynamic bidding (2PAC-B $\rightarrow$ AC-B) promotes this further by 12%, which is statistically insignifi-

[5]This confound is also captured by standard game theory: AC when including live drop-out information does not have the same reduced normal form as 2P (see section 2.3). While some authors have previously pointed at its potential importance for behavior (Kagel and Levin, 2009), it has not been systematically studied.

cant, and the remaining share of almost 40% is due to live drop-out information (AC-B → AC). Even the passive consumption of drop-out information reduces deviations further, by 25% (2PAC-B → 2PAC), which is twice the effect of dynamic bidding.

Theoretical obviousness therefore fails our stronger test. The live drop-out information under AC turns out to be a highly important design element and confound of OSP in prior evidence. Once this confound is removed, however, the entire benefit of the dynamic ascending-clock format (OSP) boils down to the effect of clock-presentation alone, as in AC-B v. 2PAC-B. This result bears important good news: The 2PAC-B auction is also a strategy-method implementation of AC-B, but Li's theory fundamentally challenges the validity of this widely used experimental method, by predicting that it would render any obvious dominance non-obvious. Our finding that this prediction fails and the strategy method does not distort behavior therefore supports the method's validity, in line with the survey by Brandts and Charness (2011).

What about initial behavior? As indicated, in the first three auction rounds, subjects underbid severely in all auction formats, and here both OSP formats significantly reduce this *under*-bidding compared to the three non-OSP ones.[6] Is the theory of obviousness therefore predictive of initial play? Li recognizes the issue that such initial differences might also be driven by greater familiarity with the dynamic auctions. Indeed, small differences in familiarity can produce very strong effects: We find that deviations from dominance play generally drop sharply after the first round, and after only two rounds, all other formats improve over the initial play under AC (even 2P does so weakly). To address this issue, we also follow Li's study and replicate his innovative "X-auctions," which perturb the standard formats by a random mark-up element to remove any familiarity.[7] For these presumably unfamiliar auctions, we find no such initial differences, suggesting that those in our standard auctions are in fact driven by familiarity, rather than theoretical obviousness.

Finally, we move beyond the descriptive analysis to gain a deeper understanding of how design affects behavior in response to monetary incentives via game cognition. We analyze a structural model of bidding behavior, allowing for different decision processes and incorporating a notion of obviousness as additional decision weight on the dominant strategy beyond expected payoffs. This delivers the following insights: First, bidding behavior is well-explained by actual monetary incentives (see also Harrison, 1989). Second, the effect of clock-presentation is to change how bidders process these incentives (expected payoffs), from a static evaluation of all possible bids at once under 2P, to a dynamic evaluation of iteratively deciding whether to continue to increment the bid or stopping under all formats involving clock-presentation, even the formally static 2PAC-B and 2PAC. In contrast to the static evaluation, the dynamic evaluation "mechanically" entails a tendency towards some underbidding, since incentives to further increment the bid vanish when approaching one's valuation. Third, bidders quickly recognize that underbidding is dominated, but not that overbidding is dominated. Moreover, the only significant difference in such obviousness is that they recognize this more strongly under AC than the other formats in standard auctions (and only there). Hence, the static decision process under 2P leads to overbidding, clock presentation reduces this mistake by changing game cognition to the dynamic decision process, and the live drop-out information under AC mitigates the resulting tendency to make underbidding mistakes. This shows how theoretical obviousness does not directly affect behavior.

[6]First-round behavior is indistinguishable for both AC and AC-B on the one hand, and 2P, 2PAC and 2PAC-B on the other hand, so there was no "instruction effect" in our additional treatments.

[7]As in Li's experiment, X-auctions follow the above standard auctions, always for the same format.

Related Literature The auctions we consider are mechanisms to allocate a single indivisible object among a set of agents with private valuations. This economic problem is both practically important and particularly suitable for testing OSP, as Li's Theorem 3 characterizes the class of OSP mechanisms for precisely this allocation problem: Essentially, given quasi-linear preferences, all OSP mechanisms take the form of an ascending-clock auction.

More broadly, Li provides a formal argument speaking to the larger literature suggesting that indirect implementations often have the advantage of being simpler for participants than direct ones (see, e.g., Ausubel, 2004, or Kagel and Levin, 2009, for the case of auctions).[8] The OSP requirement is supposed to circumvent cognitive limitations in contingent reasoning about hypothetical scenarios, which itself is a well-established phenomenon (Charness and Levin, 2009; Esponda and Vespa, 2014), though it may alternatively relate to violations of Savage's "sure-thing-principle" (Esponda and Vespa, 2017; Martínez-Marquina et al., 2017).[9]

Kagel et al. (1987) were the first to demonstrate that the ascending-clock auction outperforms the sealed-bid second-price auction in terms of dominance play. Li replicated their experiment with substantially enhanced statistical power and confirmed the results. Harstad (2000) found that prior experience with the ascending-clock auction partially carries over to sealed-bid auctions. He also investigated so-called "p-list" auctions: Bidders face an ordered list of prices and indicate which are acceptable/unacceptable, and their highest acceptable price serves as bid in a second-price auction. This sealed-bid design generated underbidding, with great variation, and experience with it was also not as helpful for the second-price auction, suggesting that it is not merely the yes/no nature of decisions in ascending-clock auctions that leads bidders to quickly adopt their dominant strategy. Kagel and Levin (2009) studied ascending-clock auctions without drop-out information, similar to our AC-B auctions, though with only 13 participants as a small add-on treatment in a study of multi-unit auctions. They already pointed to drop-out information as an important source of the greater prevalence of dominance play in the usual ascending-clock auctions, in line with our findings. Both Harstad (2000) and Kagel and Levin (2009) emphasized the general role of feedback information for whether participants recognize their dominant strategy, which our design disentangles into the effects of clock-presentation, dynamic bidding, and drop-out information.[10]

2 The experiment

Our experiment exactly replicates Li's 2P and AC auctions by using the same random numbers, interface, and instructions (aside from translation). We added the three novel treatments making minimal adaptations to instructions, and excluded the third part of his experiment (random serial dictatorship mechanisms) to focus on auctions. All treatments were run strictly between subjects, using 66–72 participants per treatment, also similar to Li, and all treatments were evenly allocated across time slots and weekdays within a short time span of three weeks in November and December 2017. Average payment was substantial, amounting to €24 per subject for 75–90 minutes, of which €5 was a show-up fee and the remainder was the sum of profits in the experimental auctions.

[8] Glazer and Rubinstein (1996) much earlier made a closely related theoretical argument.

[9] Zhang and Levin (2017) show how obvious dominance has a formal interpretation in terms of preferences.

[10] 2PAC and 2PAC-B can be seen as normal-form version of an ascending-clock auction. Schotter et al. (1994) experimentally compared behavior in simple extensive-form games and their normal-form versions and found strong differences. However, their games all had multiple Nash equilibria, and the differences appear due to greater use and fear of non-credible threats in the normal-form versions (also see Rapoport, 1997).

2.1 Experimental design

The experiment consists of 20 auctions, all of which are paid. In each auction, participants bid for a money prize worth up to €130. As in Li, individual values are affiliated by being the sum of two random draws: a group draw, which is identical for all members of a group, and a private adjustment, which is drawn independently for each individual. The group draw is uniformly distributed between €10 and €110, and the private adjustment is uniformly distributed between €0 and €20, both with a smallest monetary unit of €0.25. Before each round, participants learn their own valuations, but neither the group draw nor the private adjustment. All auctions are played anonymously in randomly matched groups of four participants. Our sessions had between 4 and 6 such groups, and we had three sessions per treatment. The treatments are as follows.

Treatment 1 (2P: Second Price). *All subjects submit sealed bids, between €0 and €150, in multiples of €0.25. The highest bidder wins the auction and pays the second-highest bid. No bidder wins if there is a tie for the highest bid.*

Treatment 2 (AC: Ascending Clock). *A price clock ticks upwards from a low starting value, in increments of €0.25, up to a potential maximum of €150.*[11] *By default, all subjects participate in the auction. At each price, they decide whether to irreversibly exit, the number of remaining bidders is updated and displayed on the screen. The auction ends once there is a single remaining bidder, or all remaining bidders exit simultaneously, or the maximal price is reached. If there is a single remaining bidder, she wins the auction and pays the current price. Otherwise, no bidder wins.*

Following each auction, each subject observes a results summary, containing all submitted bids or exit prices, respectively, her own profit, and the winner's profit. Our additional treatments adapt the baseline auctions as follows.

Treatment 3 (2PAC: Second Price with Ascending Clock). *Exactly like 2P, except that bidding is followed by an ascending-clock auction, as in AC, but where the prior bids serve as automatic exit prices and subjects cannot act anymore.*

Treatment 4 (2PAC-B: Second Price with Ascending Clock – Blind). *Exactly like 2PAC, except that the clock does not display the number of remaining bidders. Instead, it always displays the total original number of bidders.*

Treatment 5 (AC-B: Ascending Clock – Blind). *Exactly like AC, except that the clock does not display the number of remaining bidders. Instead, it always displays the total original number of bidders.*

A detailed discussion of the theoretical properties follows, but essentially, the two dynamic auctions (AC and AC-B) are OSP and the three static auctions (2P, 2PAC and 2PAC-B) are not OSP.

Finally, each session starts with 10 "standard auctions" as defined above, and these are followed by 10 "X-auctions." X-auctions adapt the above definitions by adding a random mark-up X that is uniformly distributed between €0 and €3. It is newly drawn before each round, but not revealed to the participants until the results summary. In the X-versions of dynamic auctions (AC and AC-B), once there is a single bidder left, the price continues to increase for another €X and stops only then; if the last remaining bidder is still in the auction, she wins the prize and pays that final clock price; otherwise, no one wins.[12] In the

[11] The starting price is the highest multiple of €25 that is below the group draw.

[12] In the X-auctions of treatment AC, once the number of remaining bidders falls to 2, the clock tells subjects only that "1 or 2" bidders are left.

X-versions of the static auctions (2P, 2PAC and 2PAC-B), the highest bidder wins the prize only if her bid exceeds the second-highest bid amount by more than X; otherwise, no one wins. Following Li (2017), X-auctions help address potential confounds due to familiarity with particular auction designs, as none of the X-auctions would be familiar. Since X-auctions always follow the standard auctions, our data on X-auctions reflect behavior of somewhat experienced bidders.

2.2 Logistics

The sessions were run in November and December, 2017, at the WZB-TU lab in Berlin. Participants were recruited via ORSEE (Greiner, 2015) from a large pool of students at various universities in Berlin. Upon arrival at the laboratory, all subjects were seated randomly by an experimental assistant at computer working places. The assistant handed out the instructions, which where then read aloud. The instructions are close translations of Li's originals, with straightforward adaptations for the novel treatments, and are provided in Appendix C. Individual questions were answered discretely. The remainder of the experiment was fully computerized using z-tree (Fischbacher, 2007); Appendix C provides screenshots. After finishing the 20 auctions, subjects were paid individually by an experimental assistant in a separate room.

Besides their show-up fee of €5, subjects were paid the sum of their profits from *all rounds* (if positive). They were paid only their show-up fee if they made an overall loss. The instructions contained no examples, nor was there any "dry run;" every round counted towards the total payment. The variable payoff had a wide range, from a minimum of €0 (an overall loss) to a maximum of around €70, with an average of around €19, for an average total payout of €24.

2.3 Theoretical properties

The theoretical background for our study basically follows from Li's theory. He proposes a formal notion of when a strategy is *obviously* dominant (OD), strengthening (weak) dominance and leading to his selection of obviously strategy-proof (OSP) mechanisms. Here, we briefly and informally discuss these properties, as they apply to our study, relegating formal definitions and proofs to appendix A. There, we also explicitly deal with the discreteness of the experimental implementation, and the "X-auctions," though the logic is similar.

2P and AC are the usual sealed-bid second-price and ascending-clock auctions, respectively. It is well-known that truthful bidding—i.e., bidding one's valuation in 2P, or quitting once the clock price reaches one's valuation in AC—is a dominant strategy in either of these auctions, hence they are strategy-proof.

OSP requires that dominance be "obvious" in the following sense: At any information set possibly reached under a player's dominant strategy, the worst possible outcome under this strategy is at least as good as the best possible outcome of any alternative strategy that deviates there. In the only information set of 2P, the minimal payoff from truthful bidding is zero while the maximal payoff from any positive bid is one's own value. Since the former is less than the latter, truthful bidding is not OD in 2P, violating OSP.

The dynamic AC auction meets the OSP criterion. In information sets with standing prices below one's valuation, quitting is clearly no better than truthful bidding, and when the price has reached one's valuation, staying makes a difference only in case of winning, but then comes at a loss. Thus, truthful bidding is here OD.

AC provides updated information on the number of bidders still in the auction. Given private valuations, there is nothing to learn from it, so AC-B, which suppresses it, shares the above properties with AC. In contrast to AC, however, where bidders may nonetheless condition their exit decision on the number of others remaining, the reduced normal form of AC-B actually coincides with that of 2P (see Thompson, 1952, or Elmes and Reny, 1994). The comparison of behavior between 2P and AC-B rather than AC therefore removes possible confounds with "obviousness" that are due to this additional behavioral possibility.

2PAC and 2PAC-B both have normal forms identical to 2P, thus sharing the aforementioned properties. They may be viewed as different presentations of 2P, where bids unravel and feedback comes via an ascending clock. They also represent normal-form implementations of AC and AC-B, respectively, after removal of theoretically irrelevant strategies where bidders respond to opponents' dropping out.[13] Either way, they introduce an ascending clock without letting bidders act in all contingencies, as OD would require, thereby allowing us to evaluate to what extent this different presentation affects behavior.

Proposition 1. *In all auction formats, an agent's strategy is weakly dominant if and only if it is truthful. Whereas any truthful strategy in AC and AC-B is obviously dominant, no obviously dominant strategy exists in 2P, 2PAC or 2PAC-B. All considered auction formats are strategy-proof, whereas only AC and AC-B are obviously strategy-proof. All auction formats except AC have the same reduced normal form.*

3 Results

3.1 Deviations from truthful bidding

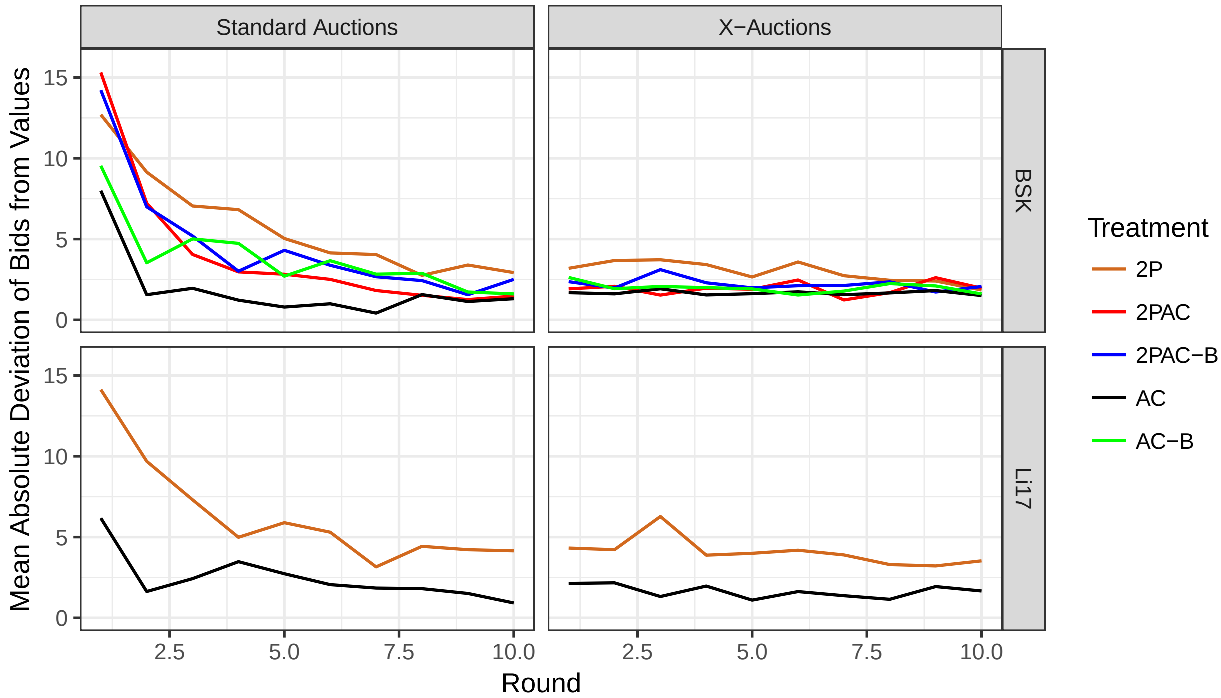
Figure 2 plots the mean absolute deviations of bids from values over time, for our experiment and Li's. Initial levels are similar across all static auction formats (2P, 2PAC, 2PAC-B), 12–15 Euros, and also similar across both dynamic ones (AC, AC-B), 6–10 Euros, indicating that our additional treatments did not introduce "instruction effects," and they generally decline swiftly over the very next few rounds.

At the same time as we closely replicate Li's results on 2P and AC, differences across the static and also across the dynamic formats emerge after the first two rounds. 2PAC and 2PAC-B quickly outperform 2P, and AC quickly outperforms AC-B, with all of our three novel formats (2PAC, 2PAC-B, AC-B) performing very similarly at intermediate levels from round 3 onwards.

The results indicate that subjects find the dominant strategy in AC easiest to identify. To understand the evolution of behavior, we evaluate the null hypothesis of equality of any given auction format with AC for six different time intervals, rounds 1–3, 4–6, 7–10, which are standard auctions, and rounds 11–13, 14–16, 17–20, which are X-auctions. We account for the panel character of the data using (two-sided) tests controlling for unobserved heterogeneity, at the session level and also at the subject-level within session. Despite the large differences initially, the difference between AC and 2PAC becomes (and remains) insignificant from the final four auctions of the first stage onwards, periods 7–10 of the standard auctions. The other auction formats catch up successively: AC-B auctions stop differing significantly from AC with the start of the X-auctions (periods 11–13), and the two other variants of static auctions stop differing significantly toward the end of the X-auctions (periods 17–20). The difference between plain AC and 2P auctions remains close to being significant, with a p-value of 0.064 in two-sided tests over the last four rounds. Hence,

[13] This is similar to the strategy method whose validity is surveyed by Brandts and Charness (2011); Esponda and Vespa (2017) study a related design.

Figure 2: Mean absolute deviations over time



Note: The plots report mean absolute deviations of bids from values for each round of the experiment, where, for comparability between static and dynamic formats (also following Li), we only use non-winning bids and set all static bids below the analogous clock's starting value equal to the latter.

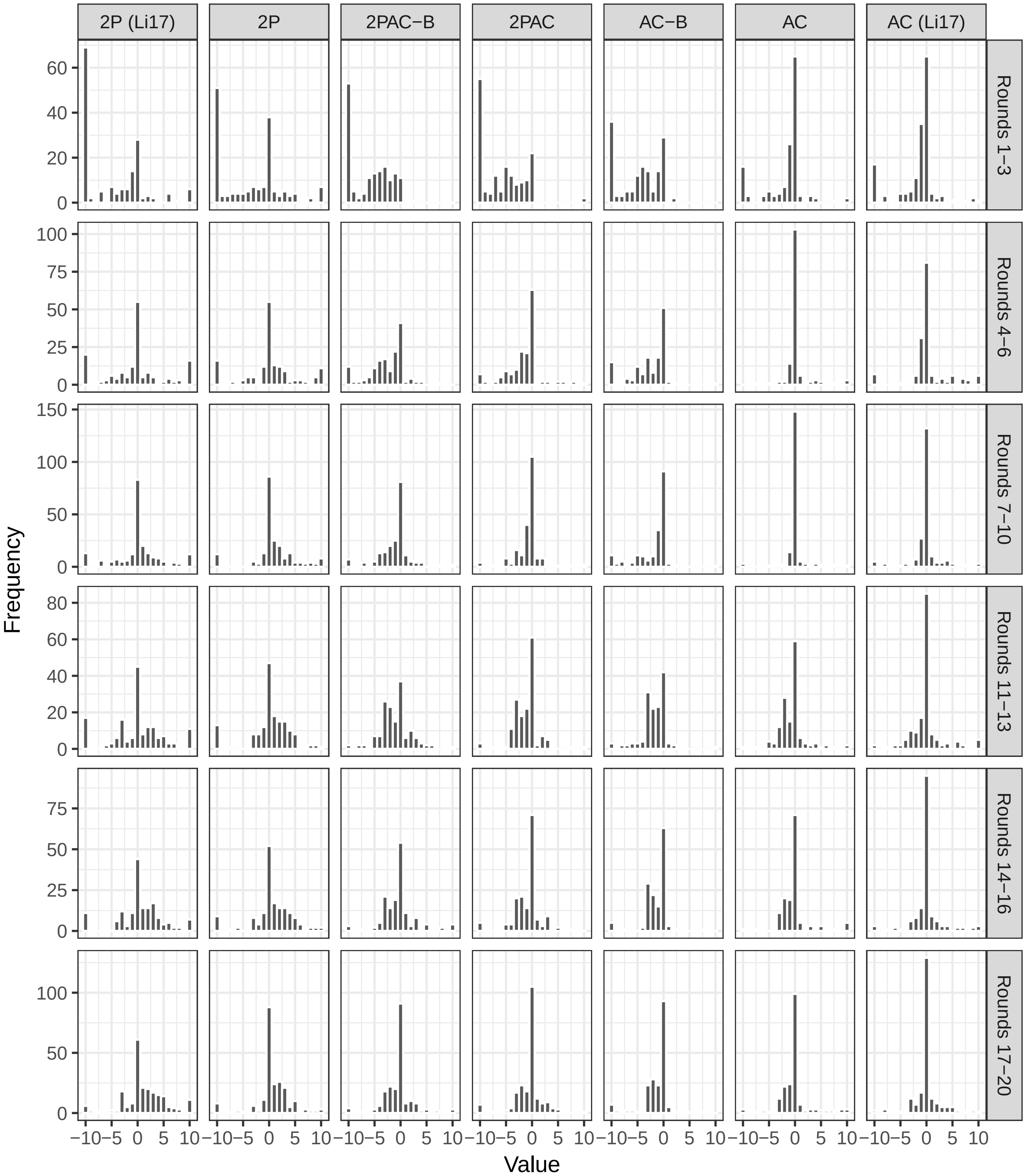
the contrast to Li's result—where the difference remains significant also in the final rounds of the X-auctions—may be considered marginal (see Tables 2 and 3 in Appendix B for details).

Result 1. *In all auction formats, absolute differences between bids and values decline over time, converging to small and eventually insignificantly different values.*

Figure 3 shows the distributions of bids in detail, plotting histograms of the differences between bids and values. This highlights that behavior in our experiment resembles behavior in Li's at an enormous level of detail: in both 2P and AC auctions the initial bid distributions are virtually equivalent to Li's, and subjects initially tend to underbid in all auction formats (as in comparable experiments, e.g., Noussair et al., 2004). The tendency to overbid develops almost exclusively in 2P, both in our experiment and in Li's. In 2P, eventually around 40% of bids exceed the respective bidders' values by more than €1 (a conventional threshold for overbidding), and in all other auction formats, this value remains below 20%. Apparently, the simple modification of showing a passive clock after bid submission (2PAC, 2PAC-B) substantially helps subjects not to develop the tendency to overbid – even if the auction itself formally maintains sealed bids.

Result 2. *Initially, subjects tend to underbid, and only in 2P auctions, subjects learn to systematically overbid. Exposing them to an ascending clock, be it actively (during bidding) or passively (after bidding), largely prevents the shift toward overbidding.*

Figure 3: Distributions of actual deviations (bid − value) over time



3.2 Economic incentives in static and dynamic auctions

In order to better understand this observed behavior, let us relate it to economic incentives and "optimization premia" in the sense of Battalio et al. (2001). Specifically, we seek to evaluate the monetary costs associated with deviations from the dominant strategy, loosely following Harrison (1989), and based on this, we shall set up a simple structural model evaluating to which extent and also how subjects' behavior reflects these incentives.

Given a subject with value v and any bid b, define the "relative bid" $a = b - v$ as the amount by which this bid exceeds the subject's value. Let A denote the set of possible relative bids. The highest opponent bid is a random variable B^*, and the conditional probability that it equals b^* is $\Pr(B^* = b^*|v)$. We make the conventional assumption that subjects abstract from distortions at the bounds of the signal space, so we can capture subjective beliefs about B^* in terms of relative bids (i.e., independently of their own signal). Formally, a belief is then a function $\sigma_b \in \Delta(A)$, and $\sigma_b(a^*)$ denotes the probability that the highest relative bid of an opponent is a^*, where relative refers to v (i.e., the highest actual opponent bid is $b^* = v + a^*$).

Expected payoffs of bids The subjective probability that bidding a wins the auction is $\sum_{a'<a} \sigma_b(a')$. The winning probabilities faced by subjects in our experiment are presented in panel (A) of Figure 4 for rounds 4–10 of the standard auctions (for further plots see appendix B).[14] For example, bidding $a = 0$ yields values very close to the theoretically predicted winning probability of 0.25 in all treatments except 2P, where it is slightly lower, at 0.19, due to subjects' overbidding. Given belief σ_b, the ex-ante expectation of the profit from bidding a is

$$\Pi(a) = -\sum_{a'<a} a' \cdot \sigma_b(a'), \tag{1}$$

which, for our data, is displayed in panel (B) of Figure 4. The (weakly) dominant strategy of bidding truthfully, $a = 0$, generally maximizes expected payoffs. Close to bidding truthfully, the empirical payoffs are fairly symmetric and flat (see also Harrison, 1989), but further away from truthful bidding, overbidding is substantially more costly than underbidding. This suggests that subjects would tend to underbid in static auctions.

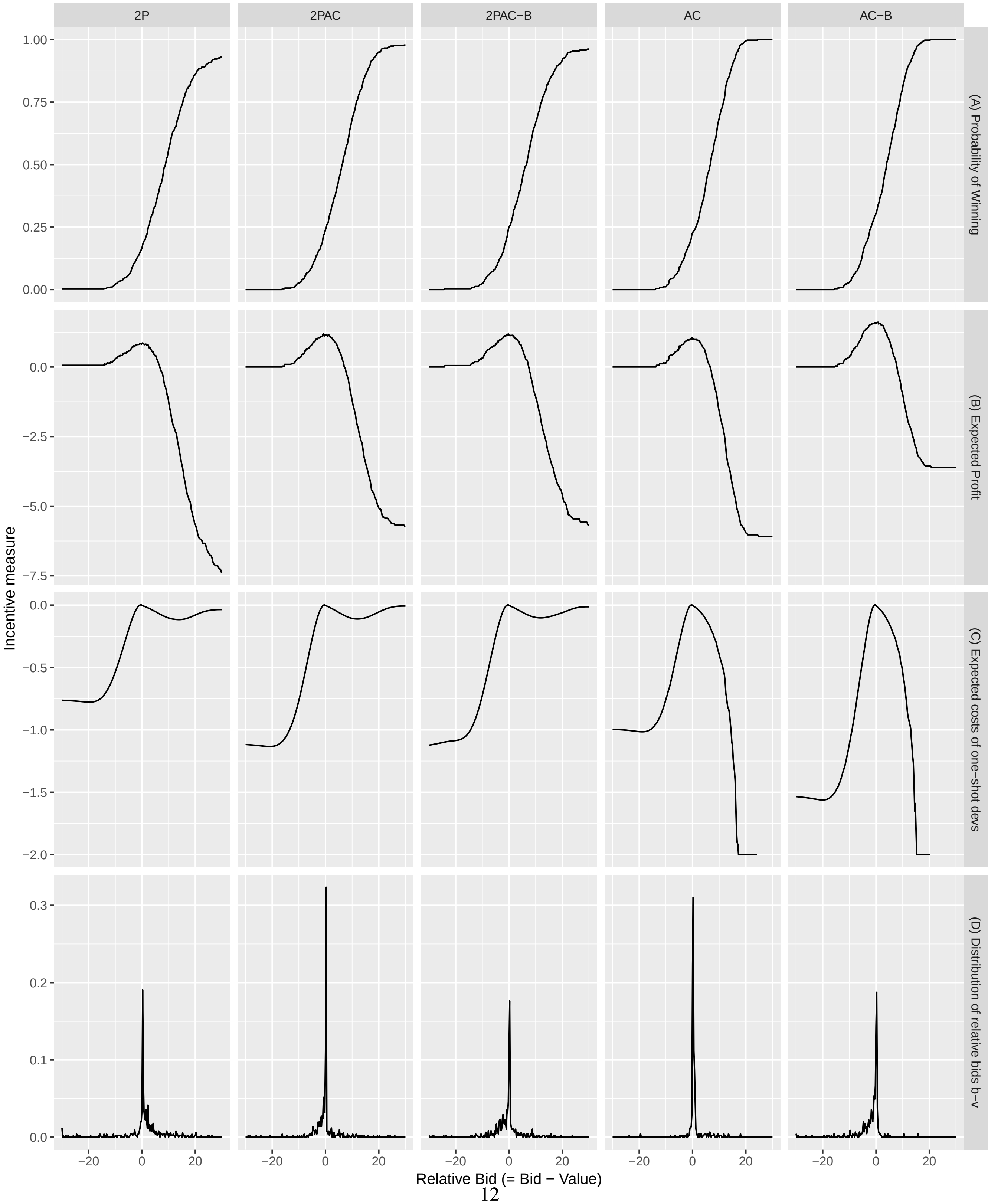
Expected payoffs of bid increments In ascending-clock auctions, a subject is sequentially presented various values of a as the clock ascends, starting at relatively low bids $a < 0$, up to potentially high bids $a > 0$. Here, we directly quantify the costs of enacting one-shot deviations from the dominant strategy. At bids $a < 0$, the one-shot deviation requires the subject to stop bidding, thus forfeiting the expected profits associated with the dominant strategy. At bids $a \geq 0$, the one-shot deviation requires the subject to keep bidding for exactly one bid increment, and thus to risk winning the auction at a price above her value v. We denote the expected costs of such one-shot deviations, conditional on the current bid a, as $L^-_{AC}(a)$ and $L^+_{AC}(a)$ for the cases $a < 0$ and $a \geq 0$, respectively.

$$a < 0: \quad L^-_{AC}(a) = \frac{\sum_{a'=a}^{0} a' \cdot \sigma_b(a')}{\sum_{a'=a}^{\infty} \sigma_b(a')} \qquad a \geq 0: \quad L^+_{AC}(a) = \frac{-a \cdot \sigma_b(a)}{\sum_{a'=a}^{\infty} \sigma_b(a')} \tag{2}$$

We can evaluate analogously such expected costs of "one-shot deviations" in static auctions. Intuitively, we will think of a subject engaging in a hypothetical thought process

[14] In dynamic auctions, where the winning bid was not observed, it was imputed as being equal to the maximum of the winner's value, and the highest observed bid plus 0.25.

Figure 4: Beliefs, profits and incentives under rational expectations (periods 4–10 of standard auctions)



that mimicks the ascending clock: Starting at low bids, she iteratively evaluates whether to increment her bid or not, up to a point where she decides to stop, which yields the bid she eventually submits in the auction. Since this thought process takes place ex ante, so would be expectations, implying

$$a<0: \quad L^{-}_{2P}(a)=\sum_{a'=a}^{0} a'\cdot\sigma_b(a') \qquad\qquad a\geq 0: \quad L^{+}_{2P}(a)=-a\cdot\sigma_b(a). \tag{3}$$

The expected costs of deviating from the dominant strategy for our experimental subjects are provided in panel (C) of Figure 4. The differences across treatments are striking. Ex-ante, the probability that a one-shot bid increment beyond one's value turns out costly is very low. Hence, subjects have little reason not to increment their bid by a tick when deliberating their choice based on unconditional expected costs L_{2P} in static auctions. Ex-post, i.e. conditional on the standing price being above the own value, further increments likely win the auction, thereby yielding a substantial loss even in expectation and providing subjects with strong incentives to play the dominant strategy and exit. This relates closely to the intuition of Cooper and Fang (2008), that bidders perceive the benefits and costs of raising their bids differently in sealed-bid and ascending-clock auctions, and indeed clarifies that this intuition is not solely an implication of bounded rationality (as conjectured by Cooper and Fang). It represents the difference between unconditional and conditional monetary incentives.

Both unconditional and conditional expected costs are contained as special cases in

$$L^{-}(a)=\frac{\sum_{a'=a}^{0} a'\cdot\sigma_b(a')}{\left(\sum_{a'=a}^{\infty}\sigma_b(a')\right)^{\beta}} \qquad\qquad L^{+}(a)=\frac{-a\cdot\sigma_b(a)}{\left(\sum_{a'=a}^{\infty}\sigma_b(a')\right)^{\beta}}, \tag{4}$$

where the unconditional expectation obtains for $\beta=0$ and the conditional expectation obtains for $\beta=1$. Since $\beta=1$ refers to the case that subjects compute expected costs (of one-shot deviations) contingent on having reached the information set where this deviation is implemented, and $\beta=0$ refers to the case where subjects do not account for this contingency, we refer to β as the degree of contingent reasoning. By estimating β, we will thus be able to assess if there are differences in the degree of contingent reasoning between treatments, potentially as a function of whether the auction is OSP—thus testing if obviousness amplifies contingent reasoning.

3.3 Analysis of behavior in relation to economic incentives

Given the previous definitions, we can use a structural model to analyze how behavior reflects these incentives, and in particular, whether it corresponds to the static perspective in Eq. (1), the ex-post incremental perspective in Eq. (2), or the ex-ante incremental perspective in Eq. (3). Our prior hypothesis is that subjects bid according to incentives from the static perspective in static auctions and according to the ex-post incremental perspective in dynamic auctions.

In addition, our analysis of incentives allows us to examine further the cognitive channels through which theoretical obviousness (might) affect behavior. One possible channel was introduced already: it could help subjects in reasoning contingently such that they compute conditional rather than unconditional expected payoffs. Another channel might be that it heightens the perception of payoff differences across the board, which we can capture through allowing for differences in the usual precision parameter λ below. A third, slightly more subtle but also more direct channel is that it helps subjects find the dominant action such that it is chosen disproportionately often, i.e., more often than payoff differences predict. Here, we follow Huck et al. (2015), amongst others, who capture choice effects due to roundedness of numbers by allowing for additive increments to choice propensities (or, utilities) when numbers are round. If these increments are significantly positive, then the numbers are chosen more often than say utility differences predict. We will estimate similar additive increments for the dominant action, to then test for their significance and also for differences across auction formats. Our hypothesis is, naturally, that subjects react to dominance more when it is formally obvious.

Our structural model directly implements the monetary incentives quantified above assuming logistic errors. Allowing for logistic errors follows McKelvey and Palfrey (1995, 1998) and is standard practice in behavioral analyses of laboratory experiments (Goeree et al., 2008), in particular also in analyses of auctions (Goeree et al., 2002; Crawford and Iriberri, 2007; Turocy, 2008). In addition to the monetary incentives, our specification will include terms ν^- and $\nu^+ \geq 0$ that denote the aforementioned additional weight awarded to the dominant action to continue if $a < 0$ and not to continue if $a \geq 0$ (or, in static auctions, to not deviate from bidding one's value toward either $a < 0$ or $a > 0$, respectively). These weights capture the degree to which dominance as such is choice-relevant—beyond elevating expected payoffs.

Thus, given the current price is a, a subject holding the "dynamic perspective" in Eq. (4) does not exit (or, continues bidding) with probability

$$a < 0: \quad \Pr_{cont}(a) = \frac{\exp\{-\lambda \cdot L^-(a) + \nu^-\}}{\exp\{0\} + \exp\{-\lambda \cdot L^-(a) + \nu^-\}},$$
$$a \geq 0: \quad \Pr_{cont}(a) = \frac{\exp\{\lambda \cdot L^+(a) - \nu^+\}}{\exp\{0\} + \exp\{\lambda \cdot L^+(a) - \nu^+\}}.$$

Here, $\lambda \geq 0$ denotes the weight of monetary incentives in decision making. The probability that the subject ends up bidding $a \in A$ is

$$\Pr(a) = \big(1 - \Pr_{cont}(a)\big) \cdot \prod_{a' < a} \Pr_{cont}(a'). \tag{5}$$

A subject with a static perspective chooses her bid simply based on expected payoffs, implying

$$\Pr(a) = \frac{\exp\{\lambda \cdot \Pi(a) - \nu^- \cdot I_{a<0} - \nu^+ \cdot I_{a>0}\}}{\sum_{a'} \exp\{\lambda \cdot \Pi(a') - \nu^- \cdot I_{a'<0} - \nu^+ \cdot I_{a'>0}\}}, \tag{6}$$

using the same parameters (λ, ν^+, ν^-) as before. We estimate these parameters by maximum likelihood and then evaluate our statistical hypotheses using the robust likelihood-ratio tests of Schennach and Wilhelm (2017). The Schennach-Wilhelm test is robust to misspecification and arbitrary nesting structures, while allowing us to cluster at the subject level.

The results of this analysis are provided in Table 1. In the upper panel of the table, we report the estimates for the full model and for basic tests of treatment effects, on "Monetary Incentives" (λ), "Obviousness" (ν^-, ν^+) and "Contingent Reasoning" (exponent β in Eq. (4)). We find that treatment differences seem to show up only with respect to obviousness of dominance regarding underbidding (ν^-). The respective p-values are reported for each test and each phase of the experiment. The second panel then evaluates a refined model that we obtain after ruling out treatment differences in ν^+ and β, so we can focus on treatment differences in the weight on monetary incentives (λ) and obviousness that underbidding is

Table 1: Results of the structural analysis

			Standard auctions				X-auctions			
Parameter/Label	Null hypothesis	Alternative	1–3		4–10		1–3		4-10	
Estimates after ruling out differences in α_1 and α_2										
$(\lambda,\beta)_{2P}$			0 (0.04)	0 (–)	0.66 (0.19)	0 (–)	1.39 (0.35)	0 (–)	1.21 (0.31)	0 (–)
$(\lambda,\beta)_{2PAC-B}$			0.28 (0.14)	6.77 (0.43)	1.15 (0.67)	0 (0.43)	2.79 (2.72)	0 (0.74)	0.73 (0.49)	0 (0.69)
$(\lambda,\beta)_{2PAC}$			0 (0)	4.17 (0.1)	1.55 (1.14)	0 (0.53)	3.44 (1.84)	0 (0.32)	2.98 (1.67)	0 (0.44)
$(\lambda,\beta)_{AC}$			0 (0)	4.45 (0.1)	0 (0)	0 (0.48)	2.81 (2.29)	0 (0.52)	0.65 (0.36)	0 (0.53)
$(\lambda,\beta)_{AC-B}$			0.02 (0.01)	3.54 (1.51)	0.71 (0.4)	0 (0.29)	3.8 (0.93)	0 (0.18)	3.19 (1.28)	0 (0.31)
$(\nu^+,\nu^-)_{2P}$			2.36 (0.22)	2.44 (0.21)	0.98 (0.22)	2.8 (0.28)	0.57 (0.25)	2.07 (0.34)	0.62 (0.23)	2.72 (0.33)
$(\nu^+,\nu^-)_{2PAC-B}$			0.58 (7.7)	0.44 (0.57)	0 (0.17)	2.3 (0.33)	0 (0.2)	1.85 (0.82)	0 (0.23)	3.02 (0.17)
$(\nu^+,\nu^-)_{2PAC}$			0 (0.46)	1.73 (0.11)	0 (0.27)	2.42 (0.47)	0 (0.32)	1.75 (0.52)	0 (0.22)	2.28 (0.51)
$(\nu^+,\nu^-)_{AC}$			0 (0.27)	3.22 (0.14)	0 (0.19)	4.87 (0.29)	0 (0.22)	1.96 (0.7)	0 (0.24)	3.28 (0.13)
$(\nu^+,\nu^-)_{AC-B}$			2.03 (0.11)	0.72 (0.64)	2.43 (0.27)	0 (0.27)	1.23 (0.29)	0 (0.78)	1.86 (0.39)	0 (0.93)
BIC			**1889.74**		**4026.35**		**1929.99**		**4177.37**	
Basic tests of treatment effects (*p*-values)										
(Monetary Inc)	H_A : all λ equal	H_{Base}	0.925		0.769		0.826		0.772	
(Obviousness +)	H_B : all ν^+ equal	H_{Base}	0.39		0.236		0.927		0.81	
(Obviousness −)	H_C : all ν^- equal	H_{Base}	0		0.014		0.752		0.808	
(Cont Reason)	H_D : all β equal	H_{Base}	0.76		0.826		0.986		0.699	
(Combined)	$H_E : H_B \wedge H_D$	H_B	0.729		0.84		0.986		0.812	
$(\lambda,\nu^-)_{2P}$			0.02 (0.12)	1.91 (0.32)	0.72 (0.19)	2.14 (0.28)	1.38 (0.34)	1.7 (0.34)	1.28 (0.32)	2.28 (0.54)
$(\lambda,\nu^-)_{2PAC-B}$			0.41 (0.16)	0.2 (0.61)	1.25 (0.64)	2.24 (0.31)	2.66 (1.32)	1.9 (0.46)	0.49 (6.66)	3.11 (2.39)
$(\lambda,\nu^-)_{2PAC}$			0 (0)	1.71 (0.11)	1.59 (1.1)	2.4 (0.46)	3.39 (1.56)	1.75 (0.46)	2.87 (6.56)	2.32 (1.5)
$(\lambda,\nu^-)_{AC}$			0 (0)	3.22 (0.14)	0 (1.13)	4.88 (0.66)	2.82 (1.43)	1.99 (0.49)	0.46 (7.33)	3.35 (2.99)
$(\lambda,\nu^-)_{AC-B}$			0.16 (0.43)	1.65 (1)	0.68 (0.38)	2.45 (0.27)	3.74 (0.97)	1.25 (0.31)	3.12 (5.62)	1.88 (1.24)
β,ν^+			1.04 (0.34)	6.16 (0.17)	0 (0.1)	0 (0.5)	0 (0.11)	0 (0.21)	0 (0.59)	0 (5.16)
BIC			**1924.39**		**4034.86**		**1912.07**		**4166.14**	
Treatment effects after ruling out effects on ν^+,β (Hypothesis H_E)										
(Monetary Inc)	$H_F : H_E \wedge$ all λ equal	H_E	0.966		0.767		0.964		0.383	
(Obviousness −)	$H_G : H_F \wedge$ all ν^- equal	H_F	0.002		0.003		0.582		0.655	
(Obviousness −)	$H_H : H_F \wedge$ all ν^-_{-AC} equal	H_F	0.648		0.679		0.716		0.809	
Are 2PAC and 2PAC-B played as if dynamic or sealed-bid? (BIC of refined model H_H)										
2P sealed bid, Rest dynamic			**1929.41**		**4031.55**		**1909.92**		**4176.63**	
2P* sealed bid, AC* dynamic			**1921.28**		**4161.51**		**1944.03**		**4289.78**	
All dynamic			**1959.49**		**4153.51**		**1979.35**		**4291.31**	

Note: The table contains "label columns" on the left-hand side and "data columns" on the right-hand side. These data columns distinguish the four phases of the experiment discussed in the text. The table reports all parameter estimates, Huber-Sandwich standard errors (clustering at subject level), Bayesian Information Criteria (BIC, Schwarz, 1978) of model adequacy, and the results of the Schennach-Wilhelm likelihood-ratio tests for model discrimination. For each likelihood-ratio test, we specifically report which null hypothesis is tested against which alternative and all p-values.

dominated (ν^-). We will focus on the results obtained for this refined model. Let us first summarize the results before we discuss the statistical support.

Result 3 (Behavior in relation to incentives)**.** *Initially (rounds 1–3), bidding is not correlated with monetary incentives ($\lambda = 0$). Subsequently:*

1. *Bidding is significantly correlated with monetary incentives in all formats ($\lambda > 0$), without significant differences between formats.*
2. *Bidding is dynamic in all formats featuring a price clock, including 2PAC and 2PAC-B. It is static only in 2P.*
3. *Bidders evaluate incentives unconditionally ($\beta = 0$) in all formats, including AC and AC-B.*
4. *In all formats, dominance is recognized only regarding underbidding, not overbidding ($\nu^- > \nu^+ \approx 0$).*
5. *There are no significant differences between formats in the extent to which subjects recognize dominance, with the sole exception of AC's standard auctions, where dominance regarding underbidding is recognized significantly better than in the others.*

Point 1 reviews the estimates of the weight on monetary incentives, λ, which is zero initially and then increases substantially. In Table 1, the lines labeled "(Monetary Inc)" report the *p*-values of tests for significance of differences in λ between treatments. The *p*-values are always above 0.3 and mostly even above 0.7, showing that differences are statistically minor. That is, winning an additional Euro is considered similarly valuable across treatments, and in this sense subjects are similarly rational in all conditions.

Point 2 summarizes the results reported in the bottom panel of Table 1. We say that bidding in a format is static if it is best explained by static expected payoffs, Eq. (6), and we say that bidding is dynamic if it is best explained by incentives under the incremental choice process in Eq. (5). The bottom panel evaluates the hypothesis that only 2P is played as static against two alternatives: either that bidding is static in all formally static formats (2P, 2PAC and 2PAC-B, summarized as "2P*") and dynamic in both dynamic ones, or that bidding is dynamic in all formats. The results are fairly clear-cut: Aside from the first three auctions, where monetary incentives bear no weight in any treatment ($\lambda = 0$), the differences in the Bayesian Information Criterion (BIC) are substantial in all phases of the experiment.

Point 3 follows immediately from the fact that $\beta = 0$ is estimated in all phases of the experiment where monetary incentives carry positive weight (i.e., after round 3). It means that subjects fail to update winning probabilities as they increase their own, even in the dynamic auctions. In Figure 3, we can directly see the main implication of it: Conditional on overbidding in AC auctions, we observe a rather flat, uniform distribution in both our AC treatment and Li's. That is, once subjects move above their values, they seem to believe in a low probability of winning the auction with the next bid increment; otherwise, they would face strong incentives to exit, as shown in Figure 4).

Point 4 follows from the observation that the weight ν^+, which captures the extent to which subjects account for overbidding being dominated in excess of the payoff difference, does not differ between treatments and is estimated to be zero after round 3. The underlying statistical test is reported in line "(Obviousness +)" in the top panel of the table. In turn, the estimates for ν^- are generally positive and large in relation to their standard errors.

Point 5 follows from the observation that the corresponding weights for underbidding, ν^-, differ highly significantly between treatments in "standard" auctions—although there are no differences between auction designs other than AC. The results (p-values) of the underlying statistical tests are reported in the two lines "Obviousness $-$" in the middle panel of the table (with ν^-_{-AC} the vector of all ν^- excluding AC).

Discussion The structural analysis, Result 3, offers a specific explanation of the differences in behavior between AC and 2P auctions. After an initial learning phase of three auctions, where underbidding is prevalent, subjects understand better that underbidding is dominated than that overbidding is dominated, quantified as $\nu^- > \nu^+$. Considering that expected payoffs are rather symmetric around truthful bidding, see Figure 4, this implies that subjects will tend to overbid when they approach bidding from a static perspective, Eq. (6). This is the case only for plain 2P auctions, however. Subjects approach all other auctions, including 2PAC and 2PAC-B, from a dynamic perspective. That is, presentation with the ascending clock instils an iterative reasoning process, where subjects iterate through possible bids in ascending order, even when bidding itself is ultimately static. Similarly to 2P auctions, also in these other auctions subjects understand dominance better with regards to underbidding ($\nu^- > 0$) than overbidding ($\nu^+ \approx 0$), but there is a substantial difference: Walking through multiple prices/bids below one's value in ascending order mechanically implies the observed bias toward underbidding, given that choice is stochastic.[15]

We observe that dominance as such statistically affects behavior, and contrary to monetary incentives, it does so from the very start of the experiment. Further, its effect is stronger in AC than in the other formats, as the ν^- differ, and in this sense we can confirm the basic idea of obviousness. However, we observe this difference only for the dominatedness of underbidding, not overbidding, not for the unfamiliar X-auctions, and not for the other format in which dominance should be obvious as well (AC-B). Thus, overall, the structural analysis confirms our earlier basic results by indicating that theoretical obviousness of dominance does not robustly help predicting when dominated actions are avoided by subjects.

4 Conclusion

Li's theory of obvious dominance is a remarkable approach towards formally grasping game cognition, and, though coarse, it provides a first theoretical guide to mechanism selection based on limited cognition. The theory's prediction is that any OSP mechanism produces significantly less deviations from dominance play than any SP mechanism that is not OSP. By its nature, there is no single way of testing this prediction, and we agree with Li that a meaningful test should compare mechanisms implementing the same allocation rule. Taking this further, not every such comparison is equally informative about the behavioral content of the theory, however, and our study permits a both cleaner and stronger test. Contrary to earlier conclusions based on the joint effect of changing multiple design features, we find that the theory's prediction does not survive this stronger test based instead on varying these features one by one and identifying the effect of the single step that theoretically should make dominance obvious.

For the same reason as differences in familiarity are a potential confound in initial behavior—especially if OSP mechanisms work better, they should be more prevalent, hence familiar—we focus our analysis on behavior after a few initial rounds of play. While the theory is confirmed for this initial phase, this is because the dynamic OSP formats reduce *under*-bidding mistakes relative to the static SP-but-not-OSP ones, which is the opposite of

[15]Take the extreme case of purely noisy behavior (uniform randomization), which implies that subjects would very likely exit only few ticks above the starting bid in the AC auction.

the common wisdom that sealed-bid second-price auctions lead to overbidding. We confirm the latter mistake after the initial phase, however. Moreover, our findings are suggestive of greater familiarity with dynamic auctions as driving the differences in initial play.[16]

Thus, overall, our study demonstrates that OSP should not be relied upon as a stand-alone knock-out criterion, and in particular, that for practical purposes, it remains important to behaviorally understand mechanism design at a finer level. While OSP insists on dynamic implementation, obviousness is really a matter of what one gets to see (such as an ascending clock as part of the results presentation), not how one formally gets to decide, and this is comforting for the widely used strategy method. From this perspective, we may well be optimistic that static SP mechanisms, which often have practical advantages, might generally be improved by exploiting analogies to their dynamic OSP counterparts in how they are presented. Considering also that OSP implementations often fail to exist (e.g., for two-sided matching, see Ashlagi and Gonczarowski, 2018) and how quickly agents learn,[17] future behavioral research in mechanism design may lead to substantial improvements if it systematically investigates how agents' understanding can be supported by small-scale dry runs with optimized presentation of results, prior to running the actual large-scale implementations of, say, multi-object auctions or school-choice problems.

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[16]This assumes that the X-auctions' manipulation worked as intended. Moreover, all participants had accumulated prior experience with the corresponding standard format already, possibly attenuating differences. The theory of obviousness may therefore still be important and valid for initial behavior in mechanisms, but this awaits further empirical investigation tailored to this question.

[17]Already in the third auction round of our experiment, all mechanisms weakly improve on the best mechanism from the first round. That is, running two practice runs may be considered a substitute for implementing an OSP mechanism in our case.

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A Formal Description of the Experimental Auctions and Proof of Proposition 1

Formally, we study the problem of allocating a single indivisible object among a set $N = \{1,\ldots,n\}$ of agents. Each agent i knows her own monetary valuation for the object, v_i, but not the valuations of others. It is commonly known, however, that all valuations are iid draws from a distribution with full support on the interval $[\underline{v},\overline{v}]$, $0 < \underline{v} < \overline{v}$. We consider auction mechanisms, as games to be played by those agents, that determine who wins the object, $w \in N \cup \{0\}$ ($w = 0$ means no one wins), and how much the winner pays for it, p^*.

We will now formally define the auctions that we implemented in our experiment. Hence, all money amounts are multiples of some smallest monetary unit $\varepsilon > 0$, and we abuse notation by writing $[a,b]$ for the set of multiples of ε that are weakly above a and weakly below b. To also cover the perturbed versions of our basic games, let X denote a random mark-up with support $[0,\overline{x}]$, whose value x is drawn according to a commonly known probability distribution F_X before the auction begins, but not revealed to the agents until after the auction has ended. (For the unperturbed versions, simply set $\overline{x} = 0 = X$.)

Our so-called AC and AC-B formats both specify an ascending-clock auction as follows. There is a publicly observed running price $p(t)$, which starts below $\underline{v}$ at the beginning of the auction, time $t = 0$, and subsequently increases in steps of $\varepsilon > 0$ every $\Delta > 0$ units of time, up to a maximum amount that exceeds $\overline{v} + \overline{x}$. At any running price p, those agents that still remain in the auction, denoted by set $A(p) \subseteq N$ and initially equal to N, decide whether to quit or remain in the auction. If, at p, all except for a single agent i quit the auction, and this agent i subsequently continues to remain in the auction until the end of the period where the running price equals $p + x$, then the auction ends after that period; in this case $w = i$ and $p^* = p + x$. If i, however, quits already at a price $p' \leq p + x$, then the auction ends at this point, and no one wins. The remaining case has multiple remaining agents all quitting at the same price p (including when the running price hits its maximum); in this case the auction ends after this period, and no one wins.

The two formats differ only in terms of whether agents receive "drop-out" information. In an AC auction, the number of remaining bidders at the running price p, denoted by $k \equiv |A(p)| \in N$, is public, whereas in an AC-B auction no one observes this information. Note, however, that from the mere fact that the auction is still going on, any remaining agent can infer that she has not won yet and could guarantee herself a zero payoff by quitting now.

Denote then the starting and maximum running prices of the ascending clock by p_0 and p_T, respectively. An agent i's strategy in any such auction, given her valuation v_i, assigns to all of her possible information sets when she is still in the auction, one of her two available actions $\{0,1\}$, where 0 means to quit and 1 means to remain in. Information sets in an AC auction take the form of sequences $(p(t),k(t))_{t=0}^{\tau}$ up to a current period τ such that $p(t)$ is increasing from $p(0) = p_0$ to $p(\tau) \leq p_T$ and $k(t)$ is weakly decreasing from $k(0) = n$ to $k(\tau) \geq 1$. The only difference in an AC-B auction is that her information sets omit the unobserved sequence $k(t)$; note, however, that $k \geq 1$ is implied by the fact that the information set is reached. We say here that i's strategy is **truthful** if it specifies quit if $p(\tau) > v_i$, and only if $p(\tau) \geq v_i$.

Our 2P-formats (2P, 2PAC, 2PAC-B) all specify that every agent i, simultaneously with all others, chooses a bid $b_i \in [0, p_T]$, where p_T is a maximal possible bid amount, similar to p_T in the ascending clock formats. If there is an agent i whose bid b_i satisfies $b_i > \max_{j \in N, j \neq i}\{b_j\} + x$, then $w = i$ and $p^* = \max_{j \in N, j \neq i}\{b_j\} + x$. Otherwise – i.e., the single highest bid does not exceed the second highest by more than x, or there are multiple highest bids – no one wins.

The formats differ only in terms of how information on the auction outcome is presented, subsequent to the bidding stage. Since the way information on the outcome is provided after all choices have been made is irrelevant to the set of possible strategies, all three formats have the same strategy spaces, namely $[0, p_T]$. We say here that i's strategy is **truthful** if it specifies $b_i \in \{v_i, v_i + \varepsilon\}$.

Throughout the following analysis of the five auction formats, we assume that each agent i maximizes her expected utility u_i, which is an increasing function of her profit from the auction ($v_i - p^*$ if $i = w$, and zero otherwise), and we normalize $u_i(0) = 0$. Moreover, it suffices to consider only pure strategies, as defined above. Denoting a strategy of agent i by s_i and her opponents' strategy profile by s_{-i}, we will abuse notation and also write $u_i(s_i, s_{-i}, x|v_i)$ for the utility she obtains under play of these strategies when her valuation is v_i and $X = x$.[18]

Definition 1. Given an auction and her valuation v_i, agent i's strategy s_i is **weakly dominant** if, for any alternative strategy s'_i and any opponent strategy profile s_{-i},

$$\mathbb{E}_{F_X}\left(u_i(s_i, s_{-i}, X|v_i)\right) \geq \mathbb{E}_{F_X}\left(u_i(s'_i, s_{-i}, X|v_i)\right).$$

An auction is **strategy proof** if every agent, given any valuation, has a weakly dominant strategy.

Defining obvious dominance, following Li (2017), requires the notion of **earliest points of departure** of any two distinct strategies of an agent in the extensive-form representation of a game. Since in the simultaneous-move games specified by 2P, 2PAC and 2PAC-B, every agent has a single information set, this is also the earliest point of departure of any two distinct strategies of hers. Consider then either of the sequential-move games specified by AC and AC-B, and take any two strategies s_i and $s'_i \neq s_i$ of an agent i. In AC (resp., AC-B), an information set $(p(t), k(t))_{t=0}^{\tau}$ (resp., $(p(t))_{t=0}^{\tau}$) is an earliest point of departure of the pair (s_i, s'_i) if (i) at this information set one of them specifies 1 for "remain in" whereas the other specifies 0 for "quit", and (ii) at any strict sub-sequence $(p(t), k(t))_{t=0}^{\tau'}$ (resp., $(p(t))_{t=0}^{\tau'}$), $\tau' \leq \tau - \Delta$, both specify 1 for "remain in". Given any earliest point of departure α of (s_i, s'_i), let $(S_{-i}, X)(\alpha)$ denote the set of all opponent strategy profiles and values of X such that information set α is reached whenever i plays according to s_i (or s'_i).

Definition 2. Given an auction and her valuation v_i, agent i's strategy s_i is **obviously dominant** if, for any alternative strategy s'_i and any earliest point of departure α of (s_i, s'_i),

$$\inf_{(s_{-i}, x) \in (S_{-i}, X)(\alpha)} u_i(s_i, s_{-i}, x|v_i) \geq \sup_{(s_{-i}, x) \in (S_{-i}, X)(\alpha)} u_i(s'_i, s_{-i}, x|v_i).$$

An auction is **obviously strategy proof** if every agent, given any valuation, has an obviously dominant strategy.

The fundamental notion for obtaining a game's reduced normal form is that of equivalent strategies. Two strategies of a player are equivalent, if they lead to the same material

[18] Recall that we consider here only private value settings. Given her opponents' strategies, their respective private information (valuations) is therefore irrelevant to i.

outcome, for any given profile of opponents' strategies, and the reduced normal form summarizes such equivalence classes into single strategies (see Thompson (1952), or Elmes and Reny (1994), for technical details).

We are now ready to establish the theoretical properties of our five auction formats that are summarized in Proposition 1 and form the basis of our empirical test.

Proof. Given any auction, take any agent i with valuation v_i, and let s_i be a truthful strategy and s_i' be a non-truthful strategy, where α is an earliest point of departure of (s_i, s_i'). Two observations are immediate, given any auction, whatever s_{-i} and x: First, all truthful strategies are payoff-equivalent, and second, truthfulness ensures a non-negative payoff.

Consider then AC and AC-B, and suppose that $s_i(\alpha) = 1$ and $s_i'(\alpha) = 0$. Since s_i is truthful, it must be that the running price p satisfies $p \leq v_i$. Given such an information set α is reached, the worst possible outcome under s_i yields zero utility (any s_{-i} and x such that $p^* \geq v_i$ or $w = 0$), as does the choice to quit under s_i'. Suppose then the alternative case of $s_i(\alpha) = 0$ and $s_i'(\alpha) = 1$, where s_i's truthfulness implies a running price $p \geq v_i$. Given such an information set α is reached, the best possible outcome under s_i' yields zero utility (since $p^* \geq p \geq v_i$), as does the choice to quit under s_i. Thus, we have established that any truthful strategy is obviously dominant, which implies its weak dominance (see Corollary 1 of Li (2017)). Moreover, for any strategy that fails to be truthful, one can easily construct s_{-i} such that it performs strictly worse than a truthful one. Hence, truthfulness, weak dominance and obvious dominance are equivalent in AC and AC-B.

For 2P, 2PAC and 2PAC-B, standard arguments establish the equivalence between truthfulness and weak dominance of strategies. What remains to show is that no truthful strategy is obviously dominant here, where α is the single information set at which agent i takes an action, for any pair of strategies. We will provide a simple counter-example. Let $v_i < p_T - \varepsilon$, take any truthful strategy s_i, which is a bid $b_i \in \{v_i, v_i + \varepsilon\}$, and compare it with the strategy s_i' of bidding the maximal possible amount $b_i' = p_T > b_i$. Any worst possible outcome under s_i yields value zero, whereas the best possible outcome under s_i' has every opponent bid and also x equal to zero, so $i = w$ with $p^* = 0$, which yields positive value $u_i(v_i) > 0$. Thus 2P, 2PAC and 2PAC-B have no obviously dominant strategies.

It is clear that 2P, 2PAC and 2PAC-B have the same normal form. In AC-B, all strategies that first specify quit for the same price are equivalent. Hence, they may be summarized by this price, similar to a bid, upon which the normal form becomes the same. In AC, this is not possible, however, for any $n > 2$. Let $k(p)$ denote the number of bidders still in the auction at a clock price equal to p, and consider the following strategy by player 1, for some prices $0 < p' < p''$ and some $\hat{k} \in [3, n-1]$: "quit at p' if $k(p') \leq \hat{k}$, and otherwise quit at p''". To reduce the normal form of AC to that of 2P, one would need to find a single-quitting-price strategy that is payoff-equivalent. Clearly, this would have to be either p' or p''. We will now show that neither of them is so, even upon restricting any other player $j > 1$ to quitting unconditionally at some price p_j. Denoting $\max\{p_j\}_{j>1} = \hat{p}$, if $p'' > \hat{p} \geq p'$ and $k(p') > \hat{k}$, player 1 wins under the conditional strategy but not under the unconditional strategy of quitting at p'. On the other hand, if $p'' > \hat{p} \geq p'$ and $k(p') \leq \hat{k}$, player 1 wins under the unconditional strategy of quitting at p'' but not under the conditional strategy. □

B Details on statistical tests

Let s denote a generic subject, t denote the current round, $v_{s,t}$ as subject s' value in round t, $b_{s,t}$ as bid, $v_{s,t|Winner}$ and $b_{s,t|Winner}$ are value and bid of the actual winner in s' group in round t In this section, we provide detailed information on the following range of statistics.

- **Mean absolute deviation** Mean of $|bs,t - v_{s,t}|$ over all s in round t
- **Mean efficiency loss** Mean of $\max_s v_{s,t} - v_{s,t|Winner}$ across all groups in round t
- **Relative Frequency of Overbidding by more than 1 Currency Unit** Estimate of $\Pr(b - v > 1)$
- **Relative Frequency of Underbidding by more than 1 Currency Unit** Estimate of $\Pr(v - b > 1)$
- **Relative Frequency of Misbidding by more than 1 Currency Unit** Estimate of $\Pr(|b - v| > 1)$
- **Mean Profit per Round** Mean of $v_{s,t|Winner} - b_{s,t|Winner}$ across groups in round t
- **Mean Cumulative Profit** cumulative sum of actual profits of subject s, up to round t, averaged across all subjects
- **Relative Frequency of Making Losses** Estimate of $\Pr(v_{s,t|Winner} - b_{s,t|Winner} < 0)$
- **Relative Frequency of Cumulative Losses** Probability of the cumulative profit (up to round t) of a random subject being negative
- **Mean Profit Forfeited** Fix subject s and determine the difference between maximal profit possible given the co-players' actual bids (i.e. by s bidding her value) and s' actual profit in round t, then average across all subjects.

For each of these statistics, we first plot the evolution across in our treatments and in Li's treatments, for both standard auctions and X-auctions, and then report on the results of statistical tests on differences between treatments. We report on the results of tests for all pairs of treatments in either experiment and in Li's experiment, after pooling rounds 1–3 and rounds 7–10, respectively. Specifically, we report the p-values of the null of zero difference after controlling for random effects at group level and subject-within-group level.

Table 2: Behavior over time by experiment I: BSK

	Standard auctions			X-auctions			H_0: Equality with AC (p-value)			
	1–3	4–6	7–10	11–13	14–16	17–20	1–3	7–10	11–13	17–20
Mean absolute deviation										
2P	9.63	5.33	3.28	3.53	3.22	2.36	0	0.006	0.022	0.063
2PAC-B	8.8	3.56	2.29	2.48	2.13	2.07	0	0.007	0.052	0.255
2PAC	8.86	2.77	1.52	1.84	2.11	1.87	0	0.328	0.689	0.622
AC	3.84	1	1.11	1.74	1.63	1.64				
AC-B	6.03	3.7	2.26	2.21	1.82	1.93	0.007	0.022	0.196	0.484
Relative frequency of overbidding ($b-v>1$)										
2P	0.17	0.31	0.38	0.38	0.44	0.4	0.015	0	0	0
2PAC-B	0.01	0.08	0.12	0.18	0.17	0.17	0.004	0.335	0.166	0.139
2PAC	0.02	0.07	0.1	0.09	0.14	0.15	0.03	0.945	0.727	0.479
AC	0.07	0.11	0.1	0.12	0.13	0.11				
AC-B	0.01	0.03	0.02	0.04	0.01	0.02	0.025	0.005	0.058	0.005
Relative frequency of underbidding ($b-v<-1$)										
2P	0.54	0.22	0.12	0.2	0.17	0.1	0	0.131	0.007	0.027
2PAC-B	0.86	0.52	0.34	0.48	0.35	0.29	0	0	0.316	0.393
2PAC	0.78	0.43	0.25	0.41	0.34	0.29	0	0	0.938	0.31
AC	0.3	0.07	0.05	0.39	0.24	0.22				
AC-B	0.69	0.49	0.33	0.52	0.44	0.36	0	0	0.084	0.05
Mean Profit Forfeited										
2P	2.82	1.37	0.46	0.18	0.28	0.38	0.003	0.101	0.029	0.06
2PAC-B	3.08	0.63	0.42	0.36	0.4	0.42	0.002	0.045	0.005	0.13
2PAC	1.66	0.65	0.27	0.24	0.25	0.25	0	0.429	0.099	0.204
AC	0.4	0.21	0.17	0.02	0.13	0.09				
AC-B	0.82	0.42	0.31	0.26	0.12	0.08	0.074	0.237	0.023	0.836
Mean Efficiency Loss										
2P	3.59	2.16	1.41	0.55	0.5	0.75	0	0.009	0.01	0.031
2PAC-B	2.86	2.19	1.51	1.2	0.87	1.28	0.001	0.002	0	0.002
2PAC	2.54	1.93	0.92	0.58	0.68	0.59	0.001	0.239	0.003	0.054
AC	1.24	0.87	0.68	0.08	0.35	0.23				
AC-B	2.49	1.21	0.96	0.64	0.31	0.09	0.002	0.199	0.019	0.104
Mean cumulated profit (censored as paid out)										
2P	3.21	5.97	7.63	8.71	9.59	10.81	0.723	0.462	0.091	0.003
2PAC-B	4.45	7.57	10.09	12.2	13.81	15.4	0.129	0.49	0.869	0.356
2PAC	5.93	9.19	12.33	14.8	16.49	18.11	0.027	0.082	0.176	0.779
AC	2.87	5.98	8.92	11.89	14.51	17.43				
AC-B	3.59	7.6	11.98	14.98	17.08	20.05	0.461	0.073	0.108	0.255
Relative frequency of cumulative profits (uncensored) below zero										
2P	0.11	0.25	0.3	0.3	0.29	0.29	0.368	0.006	0.001	0
2PAC-B	0.06	0.14	0.17	0.19	0.2	0.18	0.794	0.336	0.071	0.047
2PAC	0.02	0.13	0.15	0.16	0.15	0.12	0.052	0.55	0.181	0.232
AC	0.07	0.12	0.11	0.08	0.08	0.07				
AC-B	0.03	0.04	0.02	0.02	0.01	0	0.227	0.027	0.098	0.034

Table 3: Behavior over time by experiment II: Li17

	Standard auctions			X-auctions			H_0: Equality with AC (p-value)			
	1–3	4–6	7–10	11–13	14–16	17–20	1–3	7–10	11–13	17–20
Mean absolute deviation										
2P	10.37	5.39	3.99	4.94	4.02	3.48	0	0	0	0.001
AC	3.41	2.75	1.52	1.87	1.56	1.53				
Relative frequency of overbidding ($b-v>1$)										
2P	0.11	0.28	0.32	0.36	0.4	0.45	0.343	0	0	0
AC	0.07	0.19	0.13	0.15	0.17	0.16				
Relative frequency of underbidding ($b-v<-1$)										
2P	0.65	0.31	0.2	0.31	0.22	0.18	0	0.089	0.168	0.724
AC	0.3	0.13	0.1	0.2	0.14	0.15				
Mean Profit Forfeited										
2P	2.74	1.17	1.05	0.73	0.56	0.82	0	0.011	0.731	0.018
AC	0.58	0.98	0.34	0.62	0.19	0.23				
Mean Efficiency Loss										
2P	3.65	2.1	2.81	1.41	0.98	1.29	0	0	0.635	0.008
AC	1.9	1.9	1.35	1.6	0.57	0.6				
Mean cumulated profit (censored as paid out)										
2P	5.08	7.75	8.03	9.37	10.31	11.35	0.062	0.692	0.989	0.43
AC	2.81	5.29	7.36	9.34	10.85	13.03				
Relative frequency of cumulative profits (uncensored) below zero										
2P	0.08	0.21	0.32	0.35	0.36	0.35	0.177	0.04	0.051	0.066
AC	0.04	0.14	0.18	0.2	0.22	0.22				

Figure 5: Distributions of actual deviations (bid − value) over time: Standard auctions 1–3

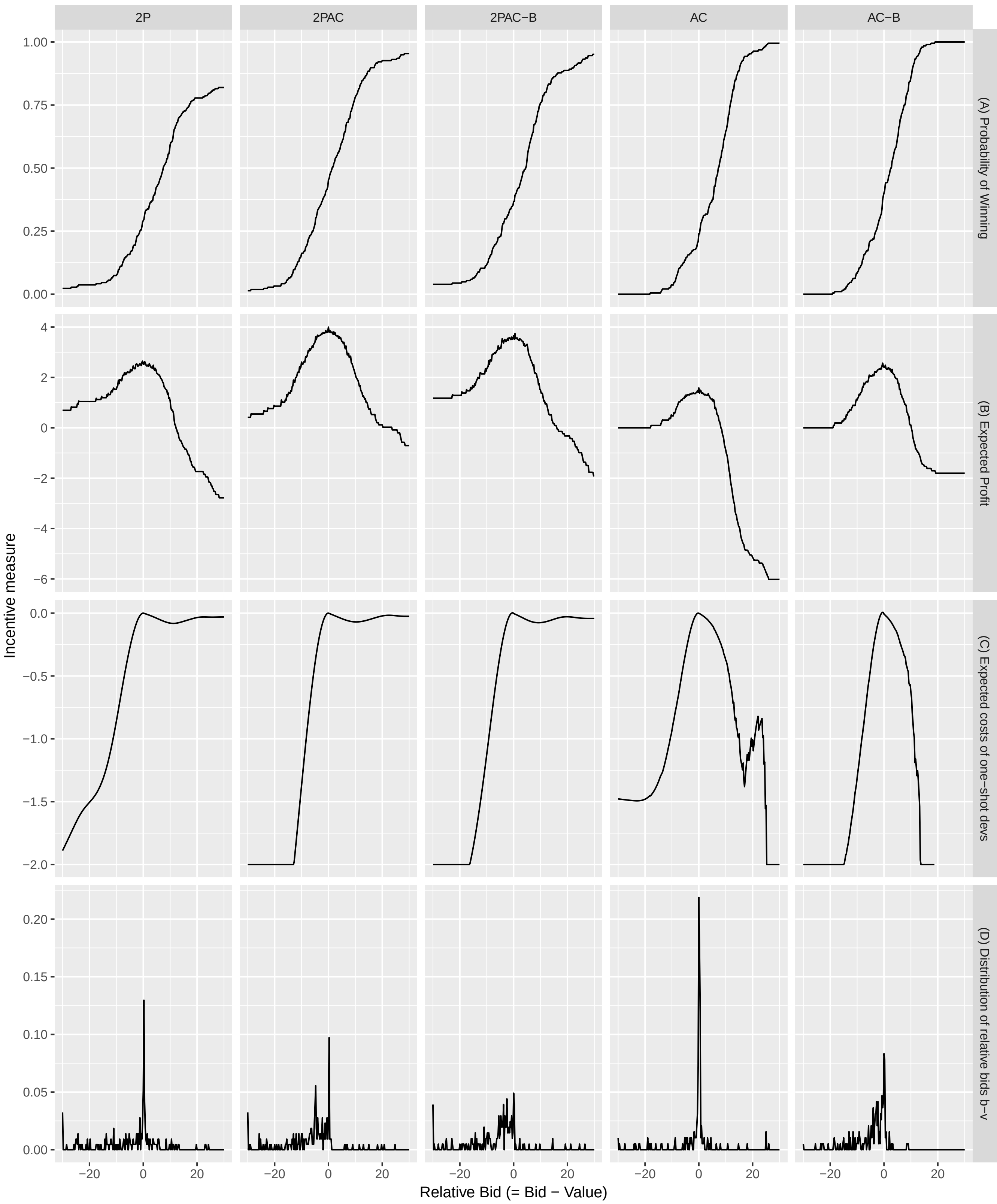


Figure 6: Distributions of actual deviations (bid − value) over time: Standard auctions 4–10

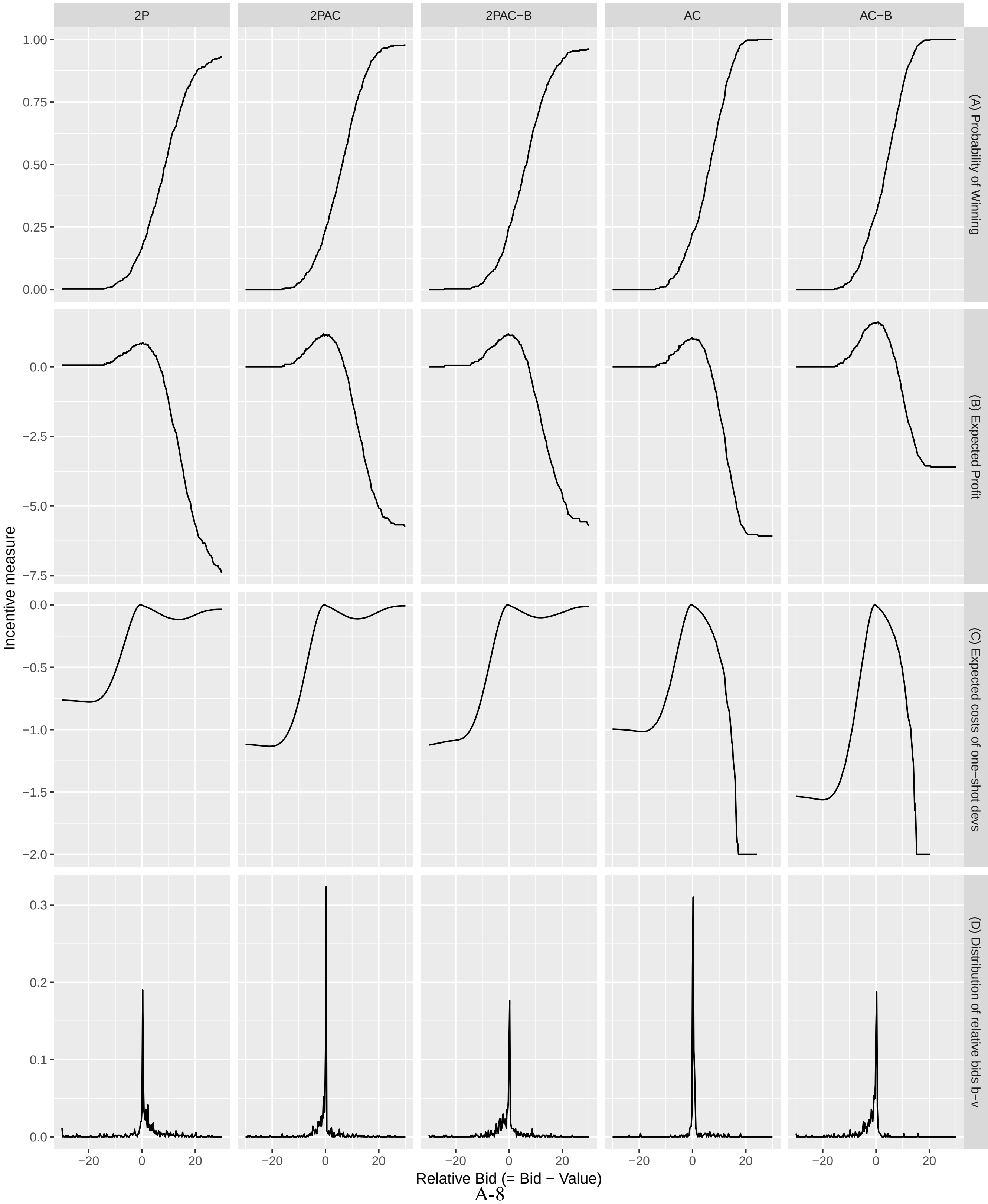


Figure 7: Distributions of actual deviations (bid − value) over time: X-auctions 1–3

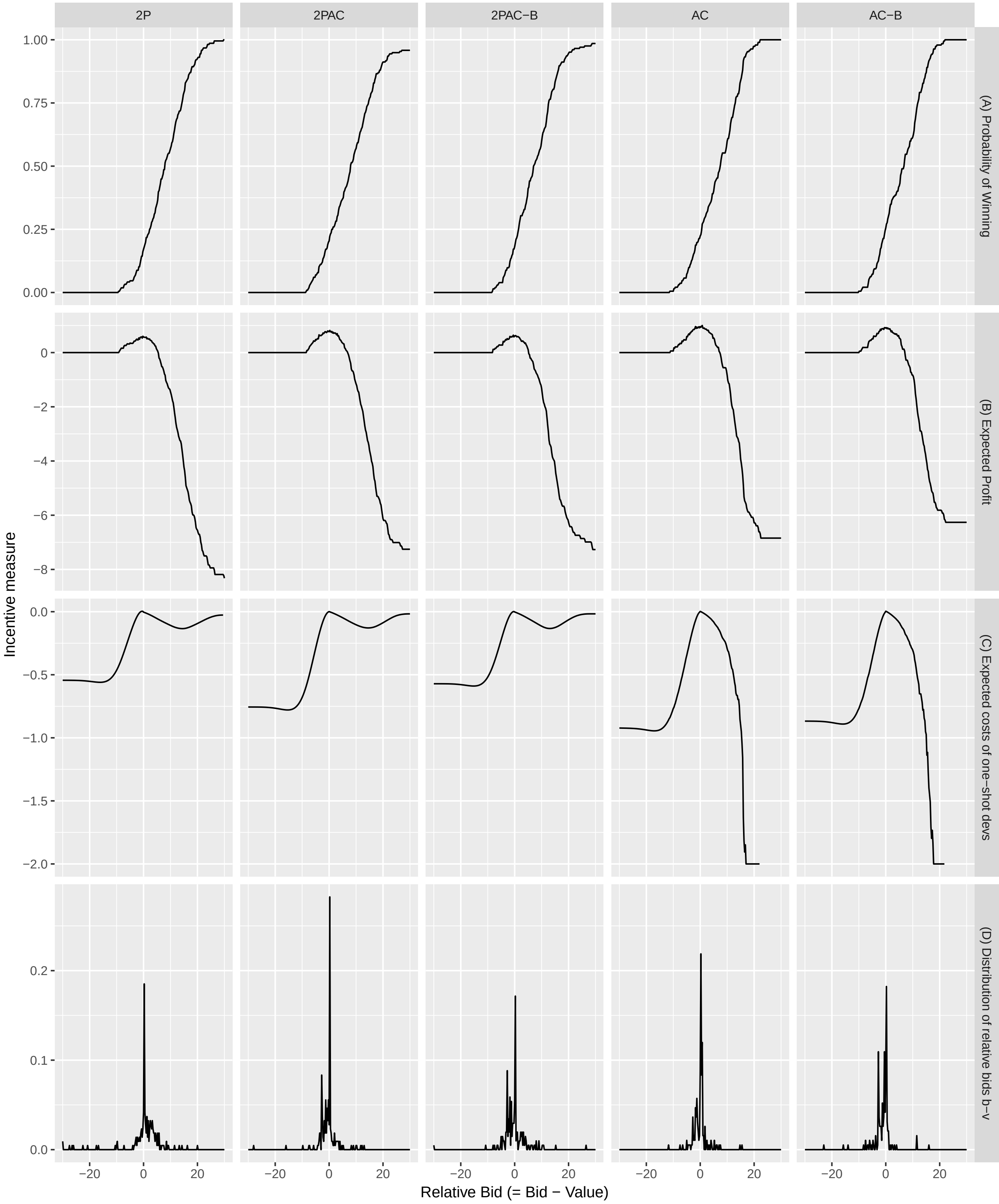


Figure 8: Distributions of actual deviations (bid − value) over time: X-auctions 4–10

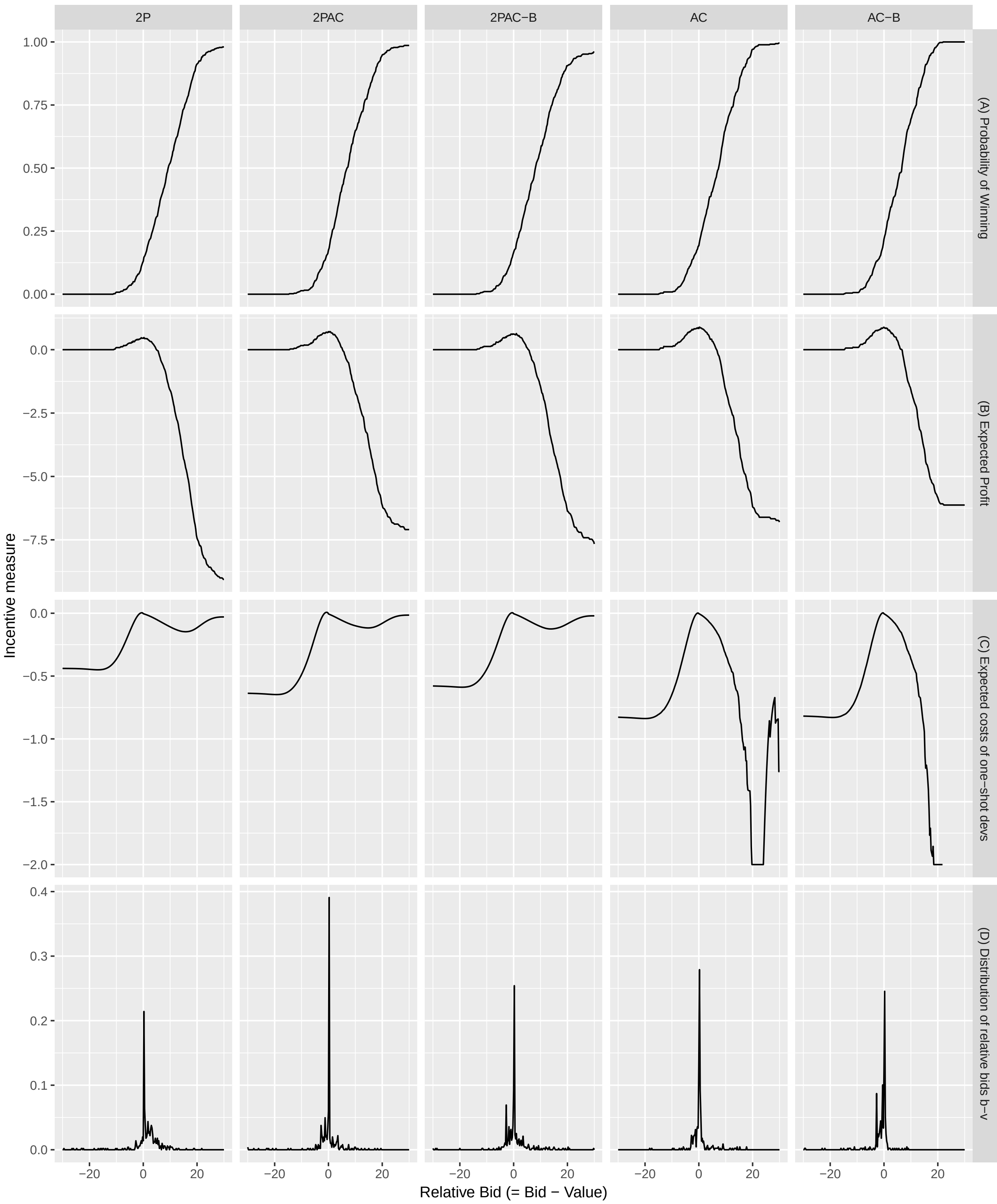


Figure 9: Relative frequencies of key events

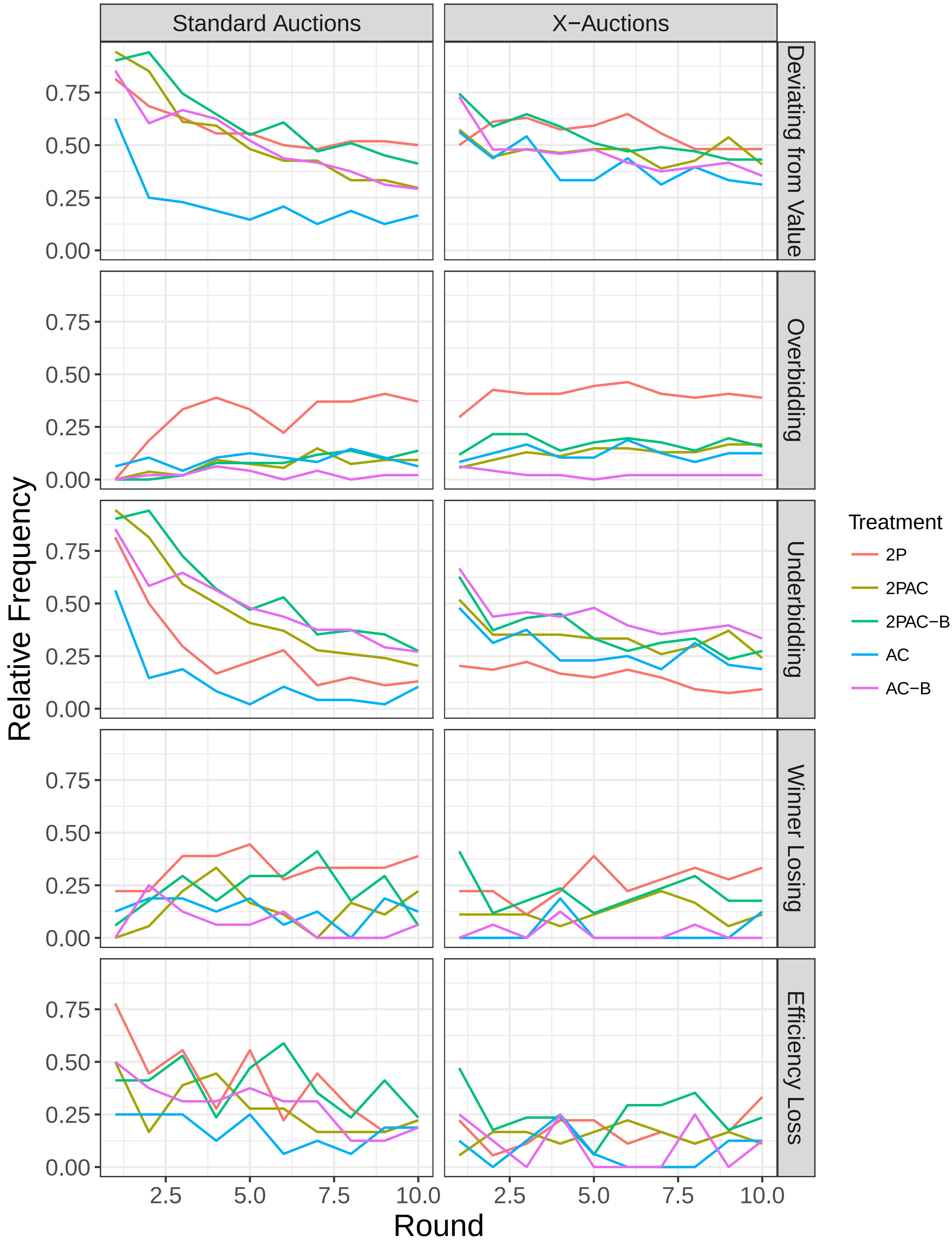
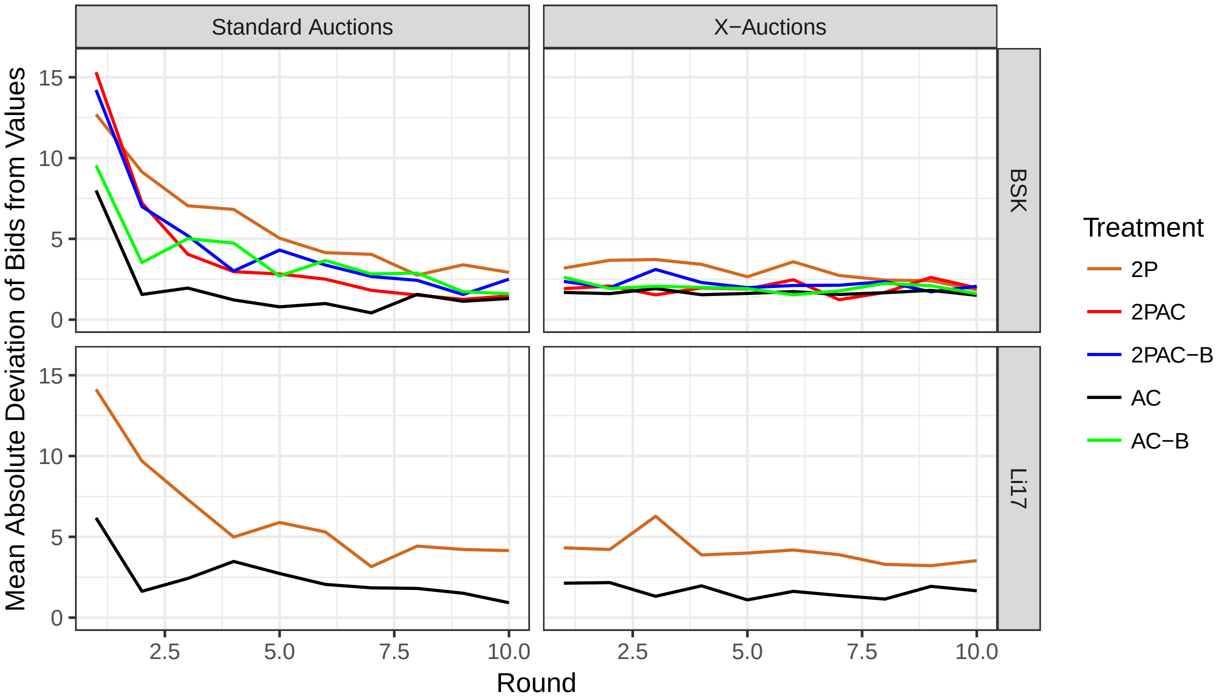


Figure 10: Mean absolute deviation over time



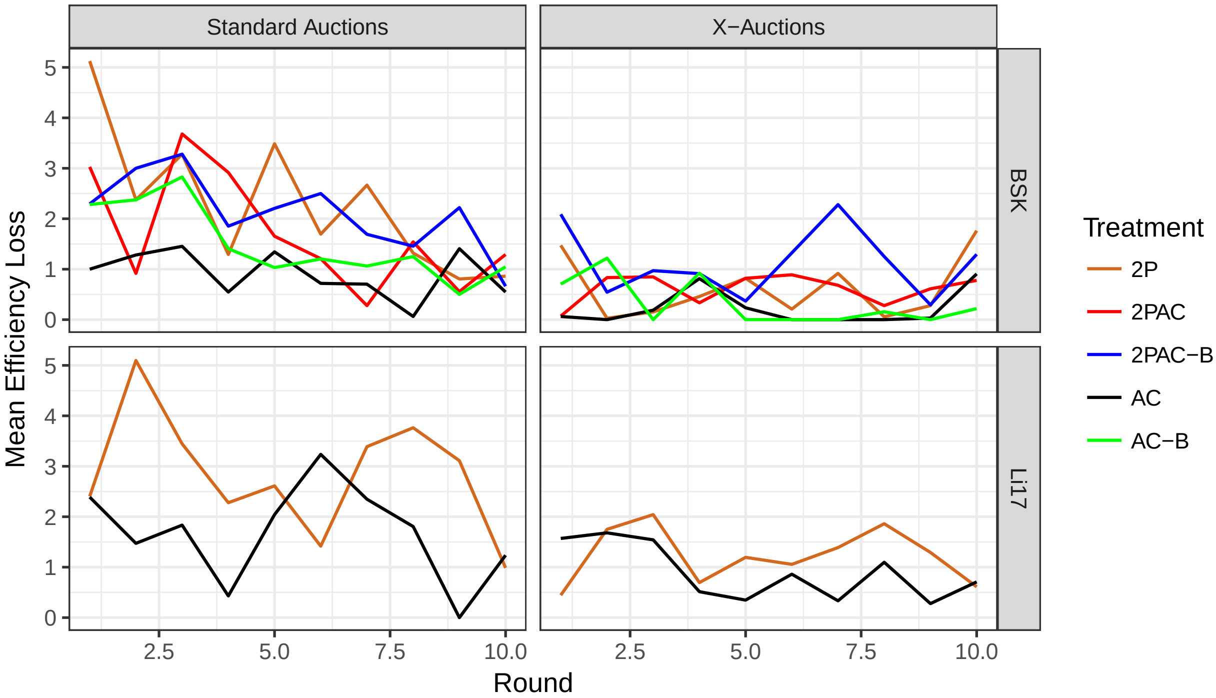
(a) BSK: Statistical tests

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	9.63	5.33	3.28	–	0.552	0.638	0	0.007	–	0.341	0.014	0.006	0.165
2PAC-B	8.8	3.56	2.29	0.552	–	0.895	0	0.004	0.341	–	0.044	0.007	0.685
2PAC	8.86	2.77	1.52	0.638	0.895	–	0	0.002	0.014	0.044	–	0.328	0.126
AC	3.84	1	1.11	0	0	0	–	0.007	0.006	0.007	0.328	–	0.022
AC-B	6.03	3.7	2.26	0.007	0.004	0.002	0.007	–	0.165	0.685	0.126	0.022	–
X-2P	3.53	3.22	2.36	–	0.203	0.045	0.022	0.13	–	0.586	0.229	0.063	0.225
X-2PAC-B	2.48	2.13	2.07	0.203	–	0.16	0.052	0.563	0.586	–	0.563	0.255	0.789
X-2PAC	1.84	2.11	1.87	0.045	0.16	–	0.689	0.43	0.229	0.563	–	0.622	0.995
X-AC	1.74	1.63	1.64	0.022	0.052	0.689	–	0.196	0.063	0.255	0.622	–	0.484
X-AC-B	2.21	1.82	1.93	0.13	0.563	0.43	0.196	–	0.225	0.789	0.995	0.484	–

(b) Li17: Mean absolute deviations of values and bids

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	10.37	5.39	3.99	–	0	–	0
AC	3.41	2.75	1.52	0	–	0	–
X-2P	4.94	4.02	3.48	–	0	–	0.001
X-AC	1.87	1.56	1.53	0	–	0.001	–

Figure 11: Mean Efficiency Loss



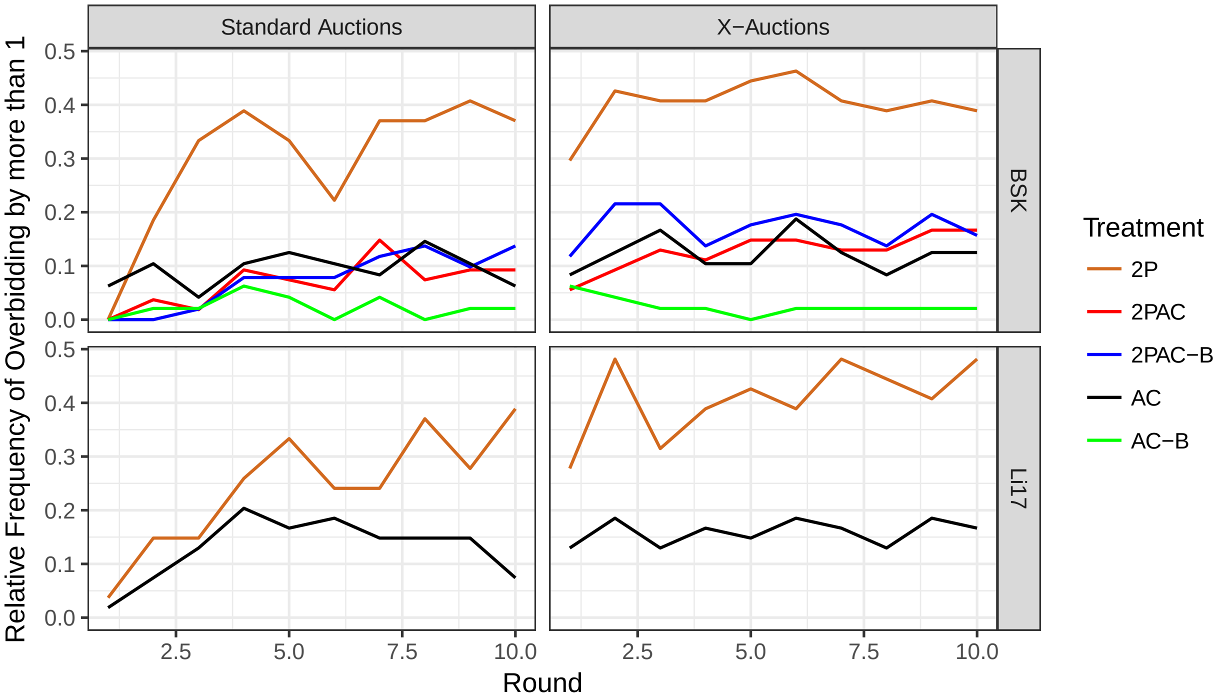
(a) BSK: Mean efficiency loss

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	3.59	2.16	1.41	–	0.165	0.028	0	0.031	–	0.571	0.077	0.009	0.115
2PAC-B	2.86	2.19	1.51	0.165	–	0.596	0.001	0.43	0.571	–	0.042	0.002	0.095
2PAC	2.54	1.93	0.92	0.028	0.596	–	0.001	0.874	0.077	0.042	–	0.239	0.978
AC	1.24	0.87	0.68	0	0.001	0.001	–	0.002	0.009	0.002	0.239	–	0.199
AC-B	2.49	1.21	0.96	0.031	0.43	0.874	0.002	–	0.115	0.095	0.978	0.199	–
X-2P	0.55	0.5	0.75	–	0.016	0.884	0.01	0.679	–	0.134	0.461	0.031	0.001
X-2PAC-B	1.2	0.87	1.28	0.016	–	0.023	0	0.039	0.134	–	0.044	0.002	0
X-2PAC	0.58	0.68	0.59	0.884	0.023	–	0.003	0.917	0.461	0.044	–	0.054	0
X-AC	0.08	0.35	0.23	0.01	0	0.003	–	0.019	0.031	0.002	0.054	–	0.104
X-AC-B	0.64	0.31	0.09	0.679	0.039	0.917	0.019	–	0.001	0	0	0.104	–

(b) Li17: Mean efficiency loss

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	3.65	2.1	2.81	–	0	–	0
AC	1.9	1.9	1.35	0	–	0	–
X-2P	1.41	0.98	1.29	–	0.635	–	0.008
X-AC	1.6	0.57	0.6	0.635	–	0.008	–

Figure 12: Relative Frequency of Overbidding by more than 1 Currency Unit



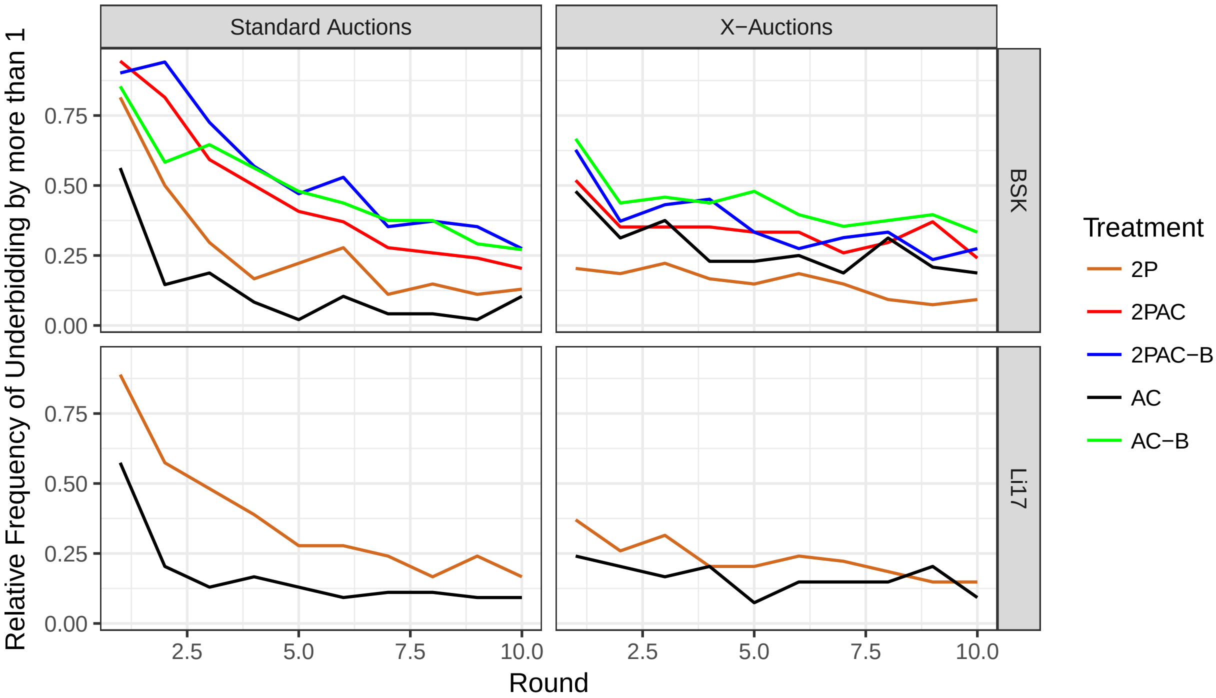
(a) BSK: Relative frequency of $b - v > 1$

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	0.17	0.31	0.38	–	0	0	0.015	0	–	0	0	0	0
2PAC-B	0.01	0.08	0.12	0	–	0.34	0.004	0.529	0	–	0.32	0.335	0.001
2PAC	0.02	0.07	0.1	0	0.34	–	0.03	0.79	0	0.32	–	0.945	0.022
AC	0.07	0.11	0.1	0.015	0.004	0.03	–	0.025	0	0.335	0.945	–	0.005
AC-B	0.01	0.03	0.02	0	0.529	0.79	0.025	–	0	0.001	0.022	0.005	–
X-2P	0.38	0.44	0.4	–	0.013	0	0	0	–	0.001	0	0	0
X-2PAC-B	0.18	0.17	0.17	0.013	–	0.091	0.166	0.003	0.001	–	0.379	0.139	0
X-2PAC	0.09	0.14	0.15	0	0.091	–	0.727	0.137	0	0.379	–	0.479	0.002
X-AC	0.12	0.13	0.11	0	0.166	0.727	–	0.058	0	0.139	0.479	–	0.005
X-AC-B	0.04	0.01	0.02	0	0.003	0.137	0.058	–	0	0	0.002	0.005	–

(b) Li17: Relative frequency of $b - v > 1$

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	0.11	0.28	0.32	–	0.343	–	0
AC	0.07	0.19	0.13	0.343	–	0	–
X-2P	0.36	0.4	0.45	–	0	–	0
X-AC	0.15	0.17	0.16	0	–	0	–

Figure 13: Relative Frequency of Underbidding by more than 1 Currency Unit



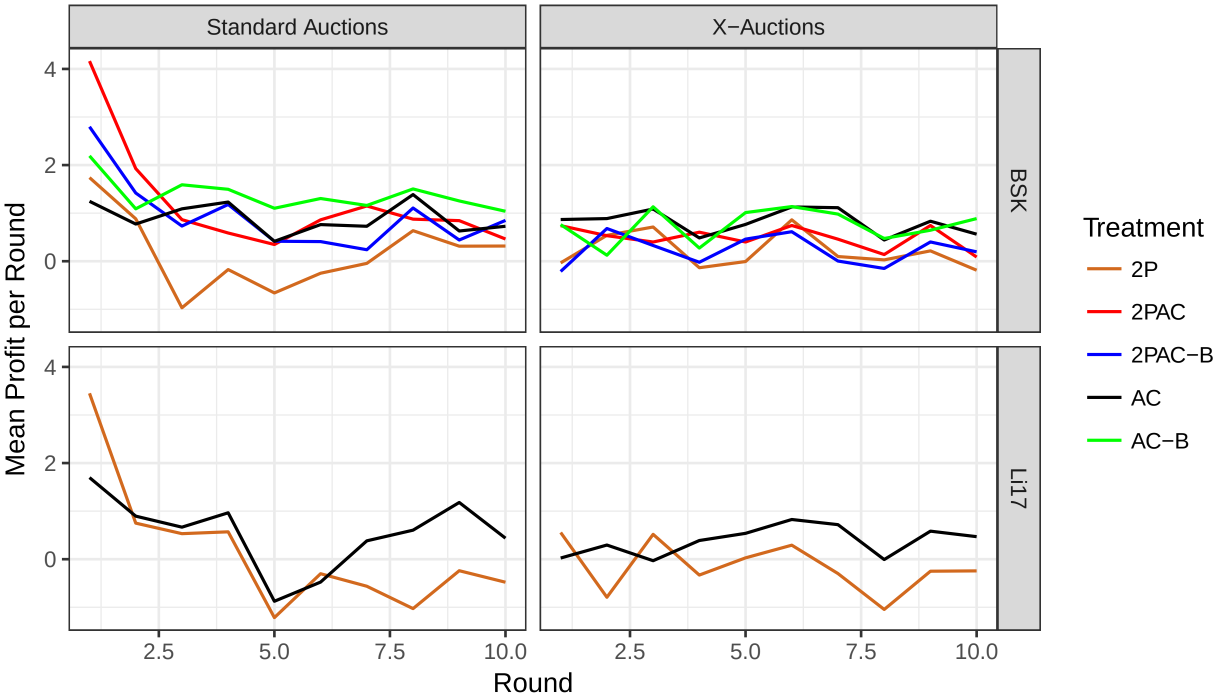
(a) BSK: Relative frequency of $b - v < -1$

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	0.54	0.22	0.12	–	0	0	0	0.033	–	0	0.03	0.131	0.001
2PAC-B	0.86	0.52	0.34	0	–	0.143	0	0.002	0	–	0.14	0	0.797
2PAC	0.78	0.43	0.25	0	0.143	–	0	0.077	0.03	0.14	–	0	0.246
AC	0.3	0.07	0.05	0	0	0	–	0	0.131	0	0	–	0
AC-B	0.69	0.49	0.33	0.033	0.002	0.077	0	–	0.001	0.797	0.246	0	–
X-2P	0.2	0.17	0.1	–	0	0.005	0.007	0	–	0.003	0.003	0.027	0
X-2PAC-B	0.48	0.35	0.29	0	–	0.35	0.316	0.46	0.003	–	0.866	0.393	0.275
X-2PAC	0.41	0.34	0.29	0.005	0.35	–	0.938	0.095	0.003	0.866	–	0.31	0.39
X-AC	0.39	0.24	0.22	0.007	0.316	0.938	–	0.084	0.027	0.393	0.31	–	0.05
X-AC-B	0.52	0.44	0.36	0	0.46	0.095	0.084	–	0	0.275	0.39	0.05	–

(b) Li17: Relative frequency of $b - v < -1$

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	0.65	0.31	0.2	–	0	–	0.089
AC	0.3	0.13	0.1	0	–	0.089	–
X-2P	0.31	0.22	0.18	–	0.168	–	0.724
X-AC	0.2	0.14	0.15	0.168	–	0.724	–

Figure 14: Mean Profit per Round



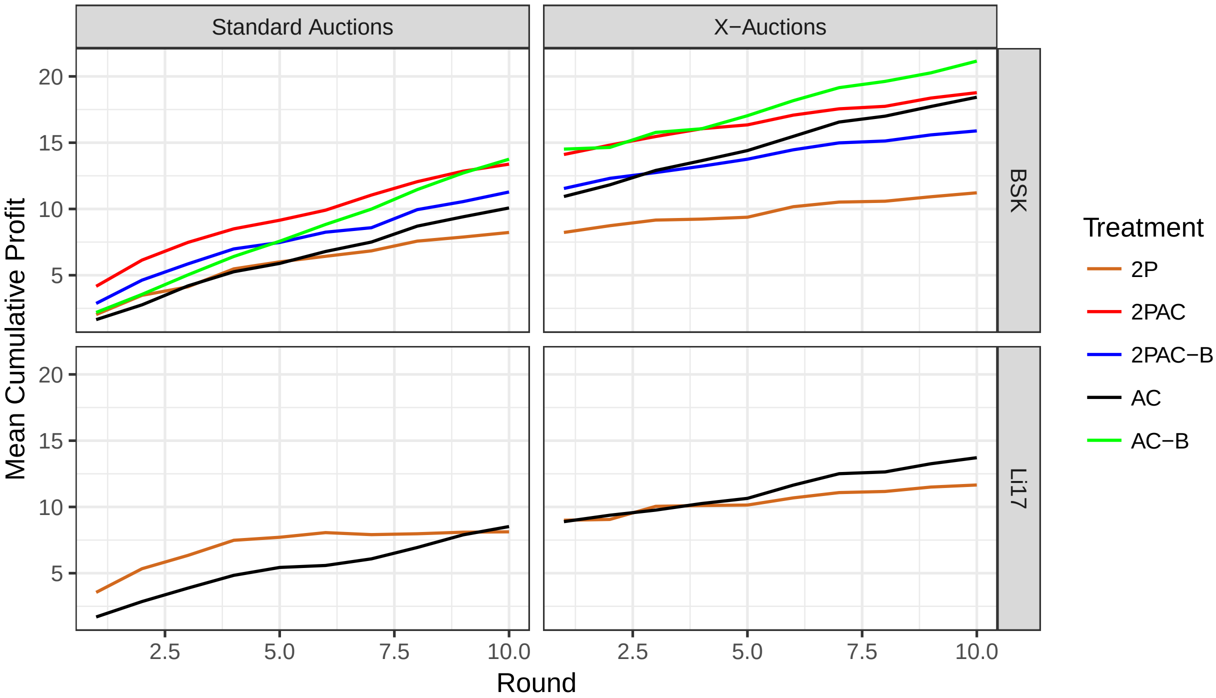
(a) BSK: Mean profits

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	0.55	-0.36	0.3	–	0.116	0.018	0.476	0.122	–	0.228	0.038	0.03	0
2PAC-B	1.65	0.67	0.66	0.116	–	0.273	0.221	0.96	0.228	–	0.488	0.406	0.024
2PAC	2.32	0.6	0.83	0.018	0.273	–	0.029	0.246	0.038	0.488	–	0.886	0.104
AC	1.04	0.8	0.87	0.476	0.221	0.029	–	0.216	0.03	0.406	0.886	–	0.131
AC-B	1.62	1.3	1.24	0.122	0.96	0.246	0.216	–	0	0.024	0.104	0.131	–
X-2P	0.41	0.24	0.04	–	0.496	0.481	0.008	0.185	–	0.86	0.125	0.002	0
X-2PAC-B	0.26	0.35	0.11	0.496	–	0.252	0.005	0.083	0.86	–	0.332	0.02	0.014
X-2PAC	0.56	0.58	0.36	0.481	0.252	–	0.108	0.603	0.125	0.332	–	0.076	0.04
X-AC	0.95	0.79	0.74	0.008	0.005	0.108	–	0.238	0.002	0.02	0.076	–	0.719
X-AC-B	0.67	0.81	0.75	0.185	0.083	0.603	0.238	–	0	0.014	0.04	0.719	–

(b) Li17: Mean profits

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	1.58	-0.32	-0.58	–	0.373	–	0
AC	1.09	-0.13	0.65	0.373	–	0	–
X-2P	0.09	0	-0.46	–	0.995	–	0.002
X-AC	0.1	0.58	0.44	0.995	–	0.002	–

Figure 15: Mean Cumulative Profit



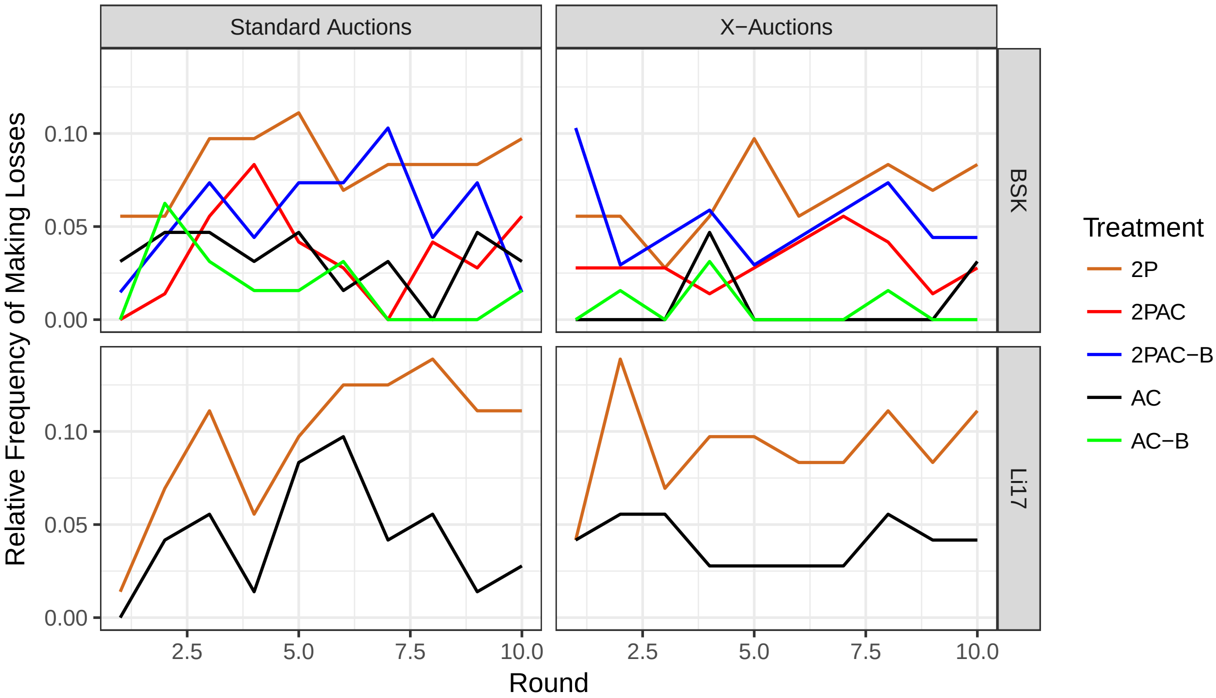
(a) BSK: Mean cumulative profits

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	3.21	5.97	7.63	–	0.285	0.057	0.723	0.734	–	0.221	0.025	0.462	0.021
2PAC-B	4.45	7.57	10.09	0.285	–	0.319	0.129	0.462	0.221	–	0.291	0.49	0.337
2PAC	5.93	9.19	12.33	0.057	0.319	–	0.027	0.113	0.025	0.291	–	0.082	0.87
AC	2.87	5.98	8.92	0.723	0.129	0.027	–	0.461	0.462	0.49	0.082	–	0.073
AC-B	3.59	7.6	11.98	0.734	0.462	0.113	0.461	–	0.021	0.337	0.87	0.073	–
X-2P	8.71	9.59	10.81	–	0.109	0.007	0.091	0.003	–	0.069	0.003	0.003	0
X-2PAC-B	12.2	13.81	15.4	0.109	–	0.269	0.869	0.204	0.069	–	0.296	0.356	0.07
X-2PAC	14.8	16.49	18.11	0.007	0.269	–	0.176	0.939	0.003	0.296	–	0.779	0.457
X-AC	11.89	14.51	17.43	0.091	0.869	0.176	–	0.108	0.003	0.356	0.779	–	0.255
X-AC-B	14.98	17.08	20.05	0.003	0.204	0.939	0.108	–	0	0.07	0.457	0.255	–

(b) Li17: Mean cumulative profits

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	5.08	7.75	8.03	–	0.062	–	0.692
AC	2.81	5.29	7.36	0.062	–	0.692	–
X-2P	9.37	10.31	11.35	–	0.989	–	0.43
X-AC	9.34	10.85	13.03	0.989	–	0.43	–

Figure 16: Relative Frequency of Making Losses



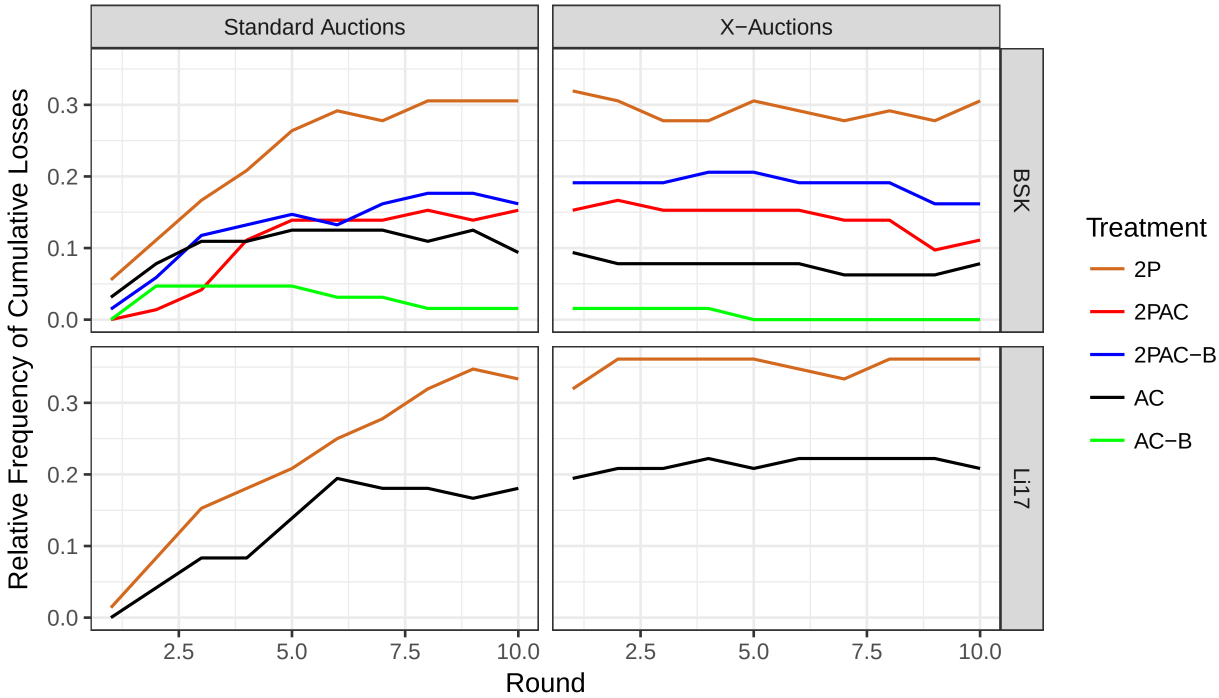
(a) BSK: Relative frequency of profits below zero

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	0.07	0.09	0.09	–	0.3	0.038	0.243	0.097	–	0.302	0.013	0.011	0
2PAC-B	0.04	0.06	0.06	0.3	–	0.283	0.907	0.519	0.302	–	0.151	0.112	0.001
2PAC	0.02	0.05	0.03	0.038	0.283	–	0.32	0.638	0.013	0.151	–	0.805	0.024
AC	0.04	0.03	0.03	0.243	0.907	0.32	–	0.588	0.011	0.112	0.805	–	0.052
AC-B	0.03	0.02	0	0.097	0.519	0.638	0.588	–	0	0.001	0.024	0.052	–
X-2P	0.05	0.07	0.08	–	0.58	0.324	0.003	0.011	–	0.429	0.049	0.001	0
X-2PAC-B	0.06	0.04	0.06	0.58	–	0.164	0.002	0.007	0.429	–	0.373	0.029	0.017
X-2PAC	0.03	0.03	0.03	0.324	0.164	–	0.036	0.111	0.049	0.373	–	0.044	0.016
X-AC	0	0.02	0.01	0.003	0.002	0.036	–	0.319	0.001	0.029	0.044	–	0.564
X-AC-B	0.01	0.01	0	0.011	0.007	0.111	0.319	–	0	0.017	0.016	0.564	–

(b) Li17: Relative frequency of profits below zero

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	0.06	0.09	0.12	–	0.137	–	0.001
AC	0.03	0.06	0.03	0.137	–	0.001	–
X-2P	0.08	0.09	0.1	–	0.241	–	0.019
X-AC	0.05	0.03	0.04	0.241	–	0.019	–

Figure 17: Relative Frequency of Cumulative Losses



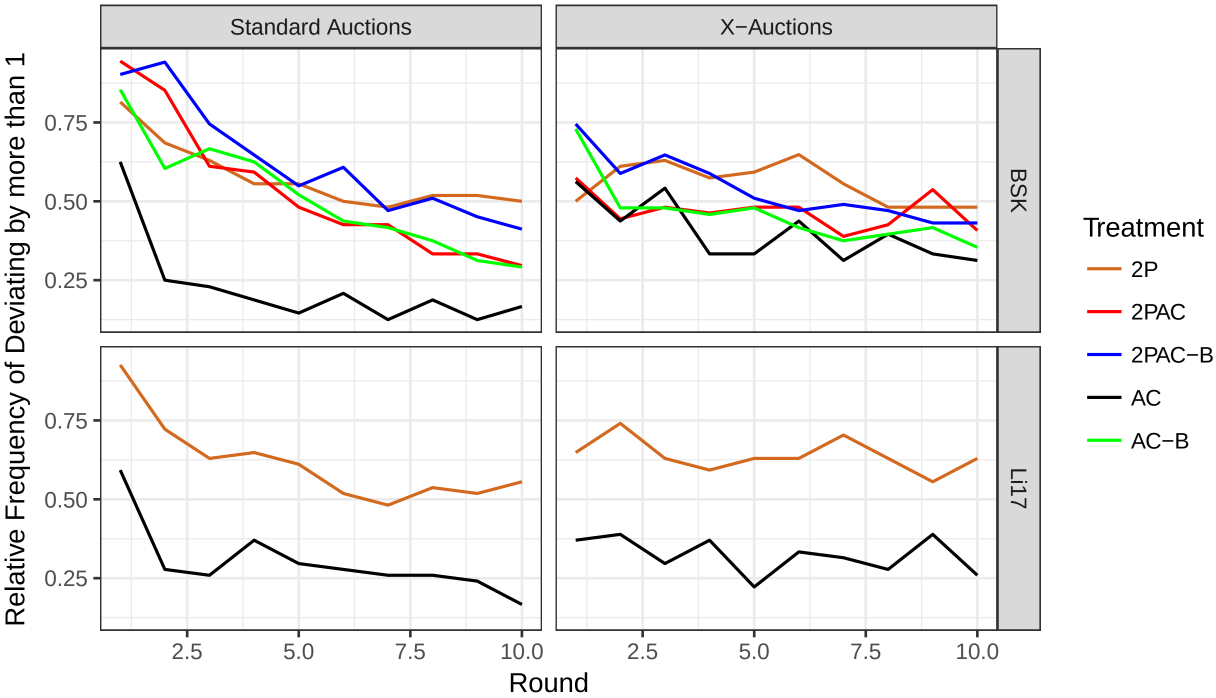
(a) BSK: Relative frequency of cumulative profits below zero

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	0.11	0.25	0.3	–	0.242	0.008	0.368	0.035	–	0.076	0.022	0.006	0
2PAC-B	0.06	0.14	0.17	0.242	–	0.079	0.794	0.261	0.076	–	0.692	0.336	0.002
2PAC	0.02	0.13	0.15	0.008	0.079	–	0.052	0.511	0.022	0.692	–	0.55	0.005
AC	0.07	0.12	0.11	0.368	0.794	0.052	–	0.227	0.006	0.336	0.55	–	0.027
AC-B	0.03	0.04	0.02	0.035	0.261	0.511	0.227	–	0	0.002	0.005	0.027	–
X-2P	0.3	0.29	0.29	–	0.149	0.035	0.001	0	–	0.13	0.007	0	0
X-2PAC-B	0.19	0.2	0.18	0.149	–	0.597	0.071	0	0.13	–	0.344	0.047	0
X-2PAC	0.16	0.15	0.12	0.035	0.597	–	0.181	0.003	0.007	0.344	–	0.232	0.001
X-AC	0.08	0.08	0.07	0.001	0.071	0.181	–	0.098	0	0.047	0.232	–	0.034
X-AC-B	0.02	0.01	0	0	0	0.003	0.098	–	0	0	0.001	0.034	–

(b) Li17: Relative frequency of cumulative profits below zero

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	0.08	0.21	0.32	–	0.177	–	0.04
AC	0.04	0.14	0.18	0.177	–	0.04	–
X-2P	0.35	0.36	0.35	–	0.051	–	0.066
X-AC	0.2	0.22	0.22	0.051	–	0.066	–

Figure 18: Relative Frequency of Misbidding by more than 1 Currency Unit



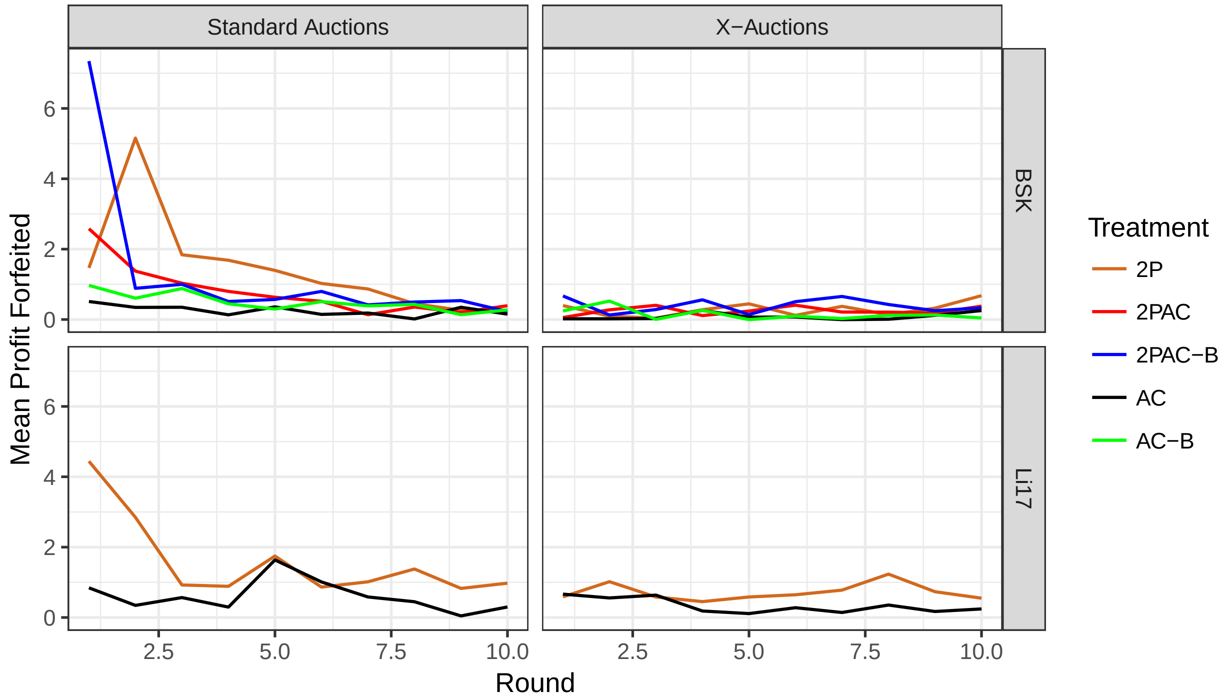
(a) BSK: Relative frequency of $|b - v| > 1$

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	0.71	0.54	0.5	–	0.004	0.083	0	0.844	–	0.636	0.012	0	0.014
2PAC-B	0.86	0.6	0.46	0.004	–	0.21	0	0.003	0.636	–	0.043	0	0.042
2PAC	0.8	0.5	0.35	0.083	0.21	–	0	0.059	0.012	0.043	–	0.003	0.876
AC	0.37	0.18	0.15	0	0	0	–	0	0	0	0.003	–	0.006
AC-B	0.71	0.53	0.35	0.844	0.003	0.059	0	–	0.014	0.042	0.876	0.006	–
X-2P	0.58	0.6	0.5	–	0.236	0.296	0.36	0.882	–	0.364	0.153	0.004	0.019
X-2PAC-B	0.66	0.52	0.46	0.236	–	0.027	0.037	0.21	0.364	–	0.6	0.059	0.216
X-2PAC	0.5	0.48	0.44	0.296	0.027	–	0.887	0.376	0.153	0.6	–	0.155	0.344
X-AC	0.51	0.37	0.34	0.36	0.037	0.887	–	0.46	0.004	0.059	0.155	–	0.65
X-AC-B	0.56	0.45	0.39	0.882	0.21	0.376	0.46	–	0.019	0.216	0.344	0.65	–

(b) Li17: Relative frequency of $|b - v| > 1$

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	0.76	0.59	0.52	–	0	–	0
AC	0.38	0.31	0.23	0	–	0	–
X-2P	0.67	0.62	0.63	–	0	–	0
X-AC	0.35	0.31	0.31	0	–	0	–

Figure 19: Mean Profit Forfeited



(a) BSK: Mean profit forfeited by sub-optimal bidding

	Observations			Tests 1–3					Tests 7–10				
	1–3	4–6	7–10	2P	2PAC-B	2PAC	AC	AC-B	2P	2PAC-B	2PAC	AC	AC-B
2P	2.82	1.37	0.46	–	0.858	0.153	0.003	0.013	–	0.811	0.292	0.101	0.399
2PAC-B	3.08	0.63	0.42	0.858	–	0.117	0.002	0.007	0.811	–	0.291	0.045	0.422
2PAC	1.66	0.65	0.27	0.153	0.117	–	0	0.014	0.292	0.291	–	0.429	0.855
AC	0.4	0.21	0.17	0.003	0.002	0	–	0.074	0.101	0.045	0.429	–	0.237
AC-B	0.82	0.42	0.31	0.013	0.007	0.014	0.074	–	0.399	0.422	0.855	0.237	–
X-2P	0.18	0.28	0.38	–	0.157	0.629	0.03	0.495	–	0.872	0.399	0.06	0.028
X-2PAC-B	0.36	0.4	0.42	0.157	–	0.496	0.005	0.499	0.872	–	0.43	0.13	0.103
X-2PAC	0.24	0.25	0.25	0.629	0.496	–	0.099	0.94	0.399	0.43	–	0.204	0.123
X-AC	0.02	0.13	0.09	0.029	0.005	0.099	–	0.023	0.06	0.13	0.204	–	0.836
X-AC-B	0.26	0.12	0.08	0.495	0.499	0.94	0.023	–	0.028	0.103	0.123	0.836	–

(b) Li17: Mean profit forfeited by sub-optimal bidding

	Observations			Tests 1–3		Tests 7–10	
	1–3	4–6	7–10	2P	AC	2P	AC
2P	2.74	1.17	1.05	–	0	–	0.011
AC	0.58	0.98	0.34	0	–	0.011	–
X-2P	0.73	0.56	0.82	–	0.731	–	0.018
X-AC	0.62	0.19	0.23	0.731	–	0.018	–

C Experimental instructions and screenshots

In order to maximize comparability with Li (2017), our instructions for the AC and 2P treatments are literal translations of Li's instructions using the following translations for frequently used terms.

Table 4: Translation of key terms

Original formulation	German translation
game	Spiel
round	Periode
group	Gruppe
people	Personen
auction	Auktion
bidder	Bieter
bid	Gebot
common value	Gruppenwert
private adjustment	individuelle Anpassung
confirm bid	Gebot bestätigen
stop bidding	Bieten beenden
current price (OSP)	aktueller Preis
stopped bidding at	Bieten beendet bei
prices where bidders stopped	Preise bei denen die Bieter ausstiegen
winning bidder's profits	Gewinn des erfolgreichen Bieters
tie for the highest bidder	Gleichstand beim Höchstgebot
bidders left in the auction	verbliebene Bieter in der Auktion
active bidders	Aktive Bieter
overall bidders	Auktionsteilnehmer
(submit bids) privately	(geben Gebote) verdeckt (ab)
click button	[auf der] Schaltfläche klicken
money prize	Geldpreis
(different) dollar value for the prize	(unterschiedlicher) Wert für den Geldpreis
value for the prize	Wert für den Geldpreis
total value for the prize	Wert für den Geldpreis insgesamt
price	Bietpreis

Our instructions for the remaining treatments make minimal adaptations of the AC and 2P instructions. First, the AC-B uses the same verbal instructions as AC (i.e., only the screenshot is adapted), which was feasible since Li's AC instructions did not announce that the subjects would see the number of active bidders.

Second, the 2PAC (resp., 2PAC-B) instructions are a simple blend of the 2P and AC (resp., AC-B) instructions: The 2P instructions are used up to the instruction on submitting bids, following which the AC (resp., AC-B) instructions on the clock auction are used, with the adaptation that a bidder drops out if the current price reaches the bidder's predetermined bid, instead of dropping out by clicking "Stop Bidding".

WILLKOMMEN

Dies ist eine Studie zur Entscheidungsfindung. Vielen Dank für Ihre Teilnahme. Im Rahmen dieser Studie können Sie Geld verdienen, das Ihnen am Ende des Experiments in bar ausgezahlt wird. Das Experiment dauert ungefähr 90 Minuten.

Sie erhalten € 5 für Ihr pünktliches Erscheinen. Zusätzlich werden Ihnen Ihre Einnahmen aus dem Experiment ausgezahlt. Falls Sie Entscheidungen fällen, die zu Verlusten führen, werden wir diese von Ihrer Gesamtauszahlung abziehen. Ihre Gesamtauszahlung einschließlich der zuvor genannten 5 Euro an fixen Zahlungen wird jedoch immer mindestens € 5 sein.

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Wir geben Ihnen Instruktionen zu den Spielen jeweils direkt bevor sie erstmals gespielt werden. Ihre Entscheidungen in einem Spiel beeinflussen nicht, was in anderen Spielen geschehen wird.

Es gibt keine Irreführung in diesem Experiment. Jedes Spiel wird genau so gespielt, wie es in den Instruktionen spezifiziert ist. Abweichungen hiervon würden die Regeln des Labors verletzen, in dem das Experiment durchgeführt wird.

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SPIEL 1

In diesem Spiel bieten Sie in einer Auktion auf einen Geldpreis. Der Geldpreis kann unterschiedliche Werte für jede Person in Ihrer Gruppe haben. Sie werden dieses Spiel zehn Mal spielen, also in 10 Perioden. Alle Geldbeträge in diesem Spiel werden in Schritten von 25 Cent angegeben.

Zu Beginn jeder Periode zeigen wir Ihnen Ihren Wert für den Geldpreis dieser Periode. Falls Sie den Geldpreis gewinnen, werden Sie diesen Geldbetrag verdienen, abzüglich Ihrer Zahlung im Rahmen der Auktion.

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1. Für jede Gruppe bestimmen wir zufällig einen **Gruppenwert**, der zwischen € 10,00 und € 100,00 liegen wird. Jede Zahl zwischen € 10,00 und € 100,00 wird mit gleicher Wahrscheinlichkeit gezogen.

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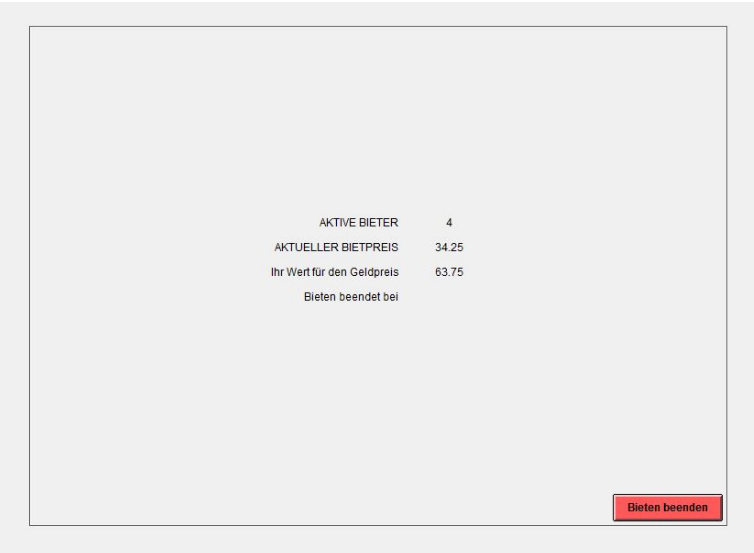
In jeder Periode ist Ihr **Wert für den Geldpreis** gleich dem **Gruppenwert** plus Ihrer **individuellen Anpassung**. Zu Beginn jeder Periode erfahren Sie Ihren Wert für den Geldpreis insgesamt, jedoch nicht den Gruppenwert oder die individuelle Anpassung.

Das bedeutet, dass jede Person in Ihrer Gruppe einen anderen Wert für den Geldpreis haben kann. Allerdings gilt, dass wenn Sie einen hohen Wert haben, es wahrscheinlicher ist, dass andere Personen in Ihrer Gruppe einen hohen Wert haben.

Die Auktion läuft wie folgt ab: Zuerst erfahren Sie Ihren Wert für den Geldpreis. Dann beginnt die Auktion. Wir zeigen allen in Ihrer Gruppe einen **Bietpreis**, der niedrig startet und dann in 25-Cent-Schritten aufwärts zählt, bis zu einem Maximum von € 150,00. Zu jedem Zeitpunkt können Sie entscheiden, aus der Auktion auszusteigen, durch Klicken auf der Schaltfläche “Bieten beenden”.



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Bietpreis ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Nach Beendigung jeder Auktion zeigen wir Ihnen jeweils die Bietpreise, bei welchen Bieter ausgestiegen sind und den Gewinn des erfolgreichen Bieters. Bei einem Gleichstand für das Höchstgebot gewinnt kein Bieter den Geldpreis.

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Die Regeln der Auktion lauten etwas anders, und zwar wie folgt:

Der **Bietpreis** zählt wieder in 25-Cent Schritten von einem niedrigen Startwert aufwärts, und Sie können zu jedem Zeitpunkt aus der Auktion aussteigen, durch Klicken auf der Schaltfläche “Bieten beenden”. Sobald nur noch ein Bieter in der Auktion übrig ist, wird nun aber der Bietpreis noch **um weitere X Euro steigen**, und dann eingefroren.

Falls der letzte Bieter **in der Auktion verbleibt, bis der Bietpreis eingefroren wird**, dann gewinnt er den Geldpreis zum **eingefrorenen Bietpreis**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon den **eingefrorenen Bietpreis** ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

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X wird **zwischen €0,00 und €3,00** liegen, wobei alle Zahlen in Schritten von 25 Cent mit der gleichen Wahrscheinlichkeit gezogen werden. Sie werden zu Beginn jeder Periode Ihren Wert für den Geldpreis erfahren, nicht jedoch **X**. Nach Abschluss der jeweiligen Periode erfahren Sie dann den Wert von **X**.

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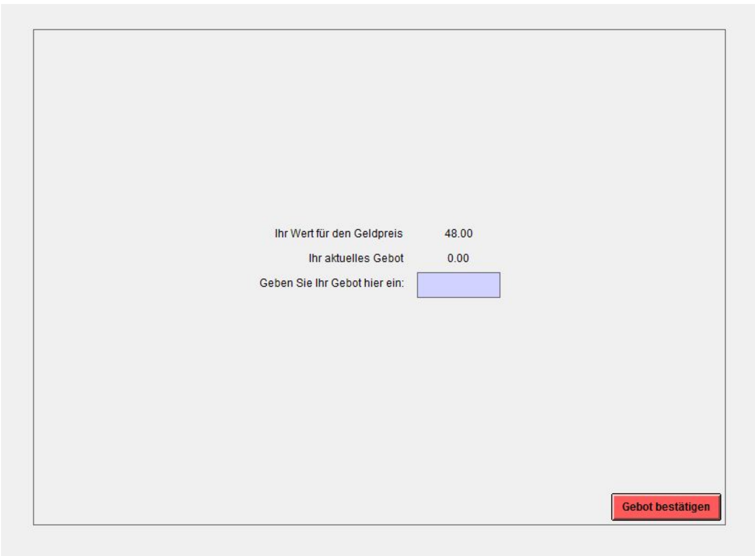

Alle Gebote müssen zwischen € 0,00 und € 150,00 liegen, in Schritten von 25 Cent.

Der Höchstbieter gewinnt den Geldpreis und zahlt dafür einen Betrag gleich dem **zweithöchsten Gebot**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon das **zweithöchste Gebot** ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Nach Beendigung jeder Auktion zeigen wir Ihnen jeweils alle Gebote, geordnet vom höchsten zum niedrigsten, und den Gewinn des erfolgreichen Bieters. Bei einem Gleichstand für das Höchstgebot gewinnt kein Bieter den Geldpreis.

SPIEL 2

In diesem Spiel bieten Sie in einer Auktion auf einen Geldpreis. Sie werden dieses Spiel zehn Mal spielen, also in 10 Perioden.

Ihr **Wert für den Geldpreis** wird generiert wie zuvor.

Allerdings ziehen wir nun in jeder Periode noch eine weitere Zahl, **X**, für jede Gruppe.

Die Regeln der Auktion lauten etwas anders, und zwar wie folgt:

Alle Bieter geben Ihre Gebote verdeckt und gleichzeitig ab. Jedoch gewinnt der Höchstbieter den Geldpreis nur dann, wenn das Gebot das **zweithöchste Gebot um mehr als X übertrifft**.

Falls der Höchstbieter den Geldpreis gewinnt, zahlt er einen Betrag gleich dem **zweithöchsten Gebot plus X**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon das **zweithöchste Gebot plus X** ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Falls das höchste Gebot das zweithöchste Gebot nicht um **mehr als X** übertrifft, gewinnt kein Bieter den Geldpreis. In diesem Fall bleiben die Einnahmen aller Bieter unverändert.

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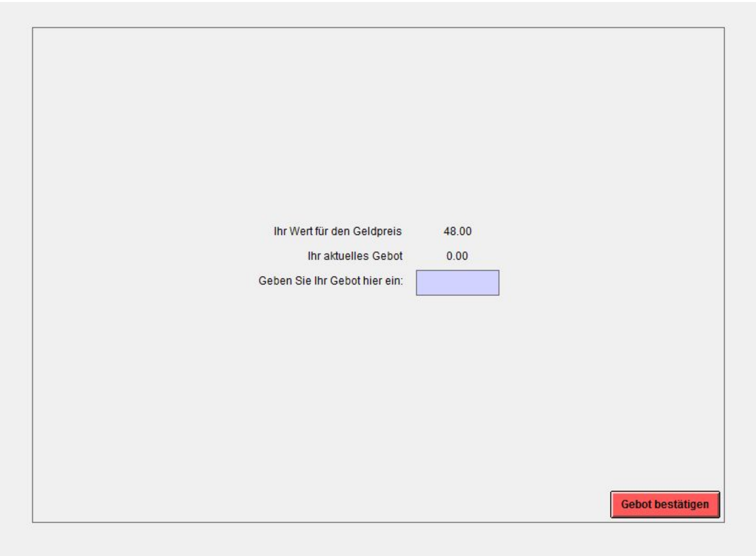

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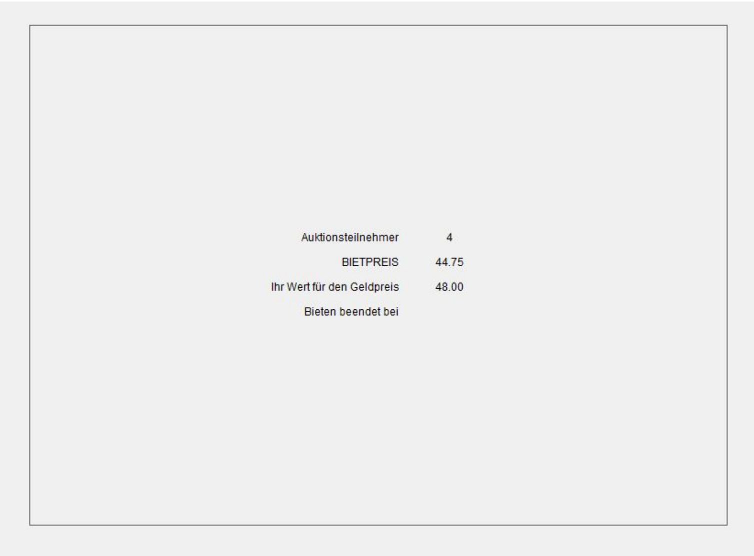

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Nach Beendigung jeder Auktion zeigen wir Ihnen jeweils die Bietpreise, bei welchen Bieter ausgestiegen sind und den Gewinn des erfolgreichen Bieters. Bei einem Gleichstand für das Höchstgebot gewinnt kein Bieter den Geldpreis.

SPIEL 2

In diesem Spiel bieten Sie in einer Auktion auf einen Geldpreis. Sie werden dieses Spiel zehn Mal spielen, also in 10 Perioden.

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Allerdings ziehen wir nun in jeder Periode noch eine weitere Zahl, **X**, für jede Gruppe.

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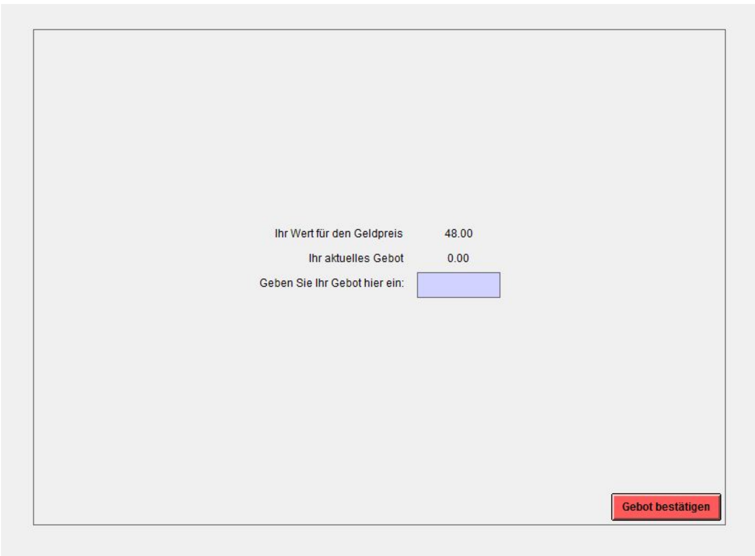

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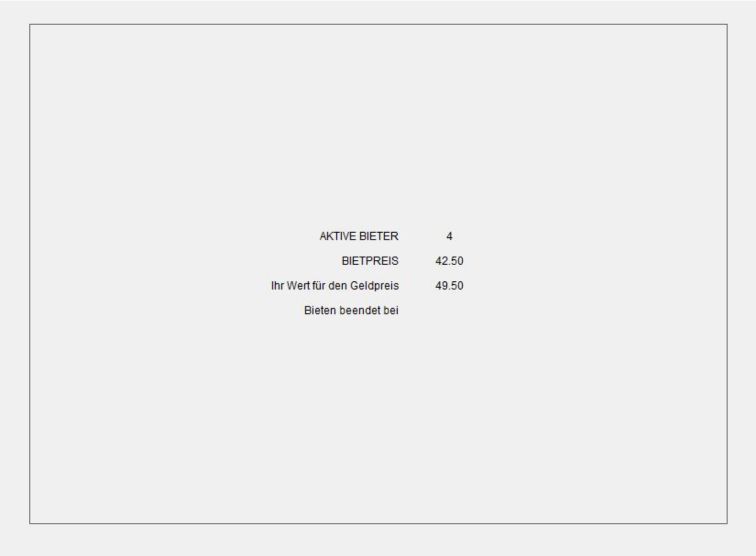


Sobald nur noch ein Bieter in der Auktion übrig ist, gewinnt dieser Bieter den Geldpreis zum **aktuellen Bietpreis**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon den **aktuellen Bietpreis** ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Nach Beendigung jeder Auktion zeigen wir Ihnen jeweils die Bietpreise, bei welchen Bieter ausgestiegen sind und den Gewinn des erfolgreichen Bieters. Bei einem Gleichstand für das Höchstgebot gewinnt kein Bieter den Geldpreis.

SPIEL 2

In diesem Spiel bieten Sie in einer Auktion auf einen Geldpreis. Sie werden dieses Spiel zehn Mal spielen, also in 10 Perioden.

Ihr **Wert für den Geldpreis** wird generiert wie zuvor.

Allerdings ziehen wir nun in jeder Periode noch eine weitere Zahl, **X**, für jede Gruppe.

Die Regeln der Auktion lauten etwas anders, und zwar wie folgt:

Alle Bieter geben Ihre Gebote verdeckt und gleichzeitig ab. Der **Bietpreis** zählt wieder in 25-Cent Schritten von einem niedrigen Startwert aufwärts, und Sie verbleiben in der Auktion, solange Ihr vorher festgelegtes Gebot über dem Bietpreis liegt, andernfalls steigen Sie aus. Sobald nur noch ein Bieter in der Auktion übrig ist, wird nun aber der Bietpreis noch **um weitere X Euro steigen**, und dann eingefroren.

Falls der letzte Bieter **in der Auktion verbleibt, bis der Bietpreis eingefroren wird**, dann gewinnt er den Geldpreis zum **eingefrorenen Bietpreis**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon den **eingefrorenen Bietpreis** ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Falls der letzte Bieter aus der Auktion aussteigt, bevor der Preis eingefroren wird, gewinnt kein Bieter den Geldpreis. In diesem Fall bleiben die Einnahmen aller Bieter unverändert.

X wird **zwischen €0,00 und €3,00** liegen, wobei alle Zahlen in Schritten von 25 Cent mit der gleichen Wahrscheinlichkeit gezogen werden. Sie werden zu Beginn jeder Periode Ihren Wert für den Geldpreis erfahren, nicht jedoch **X**. Nach Abschluss der jeweiligen Periode erfahren Sie dann den Wert von **X**.

WILLKOMMEN

Dies ist eine Studie zur Entscheidungsfindung. Vielen Dank für Ihre Teilnahme. Im Rahmen dieser Studie können Sie Geld verdienen, das Ihnen am Ende des Experiments in bar ausgezahlt wird. Das Experiment dauert ungefähr 90 Minuten.

Sie erhalten €5 für Ihr pünktliches Erscheinen. Zusätzlich werden Ihnen Ihre Einnahmen aus dem Experiment ausgezahlt. Falls Sie Entscheidungen fällen, die zu Verlusten führen, werden wir diese von Ihrer Gesamtauszahlung abziehen. Ihre Gesamtauszahlung einschließlich der zuvor genannten 5 Euro an fixen Zahlungen wird jedoch immer mindestens €5 sein.

Sie wurden zufällig in Vierergruppen eingeteilt. Dieses Experiment umfasst zwei Spiele, in denen es um "echtes" Geld geht. Sie werden jedes Spiel zehn Mal mit den anderen Personen in Ihrer Gruppe spielen.

Wir geben Ihnen Instruktionen zu den Spielen jeweils direkt bevor sie erstmals gespielt werden. Ihre Entscheidungen in einem Spiel beeinflussen nicht, was in anderen Spielen geschehen wird.

Es gibt keine Irreführung in diesem Experiment. Jedes Spiel wird genau so gespielt, wie es in den Instruktionen spezifiziert ist. Abweichungen hiervon würden die Regeln des Labors verletzen, in dem das Experiment durchgeführt wird.

Bitte nutzen Sie während dieser Studie keine elektronischen Geräte und reden Sie nicht mit anderen Teilnehmern. Wenn wir die Nutzung elektronischer Geräte oder Gespräche mit anderen Teilnehmern beobachten, verlangen es die Regeln dieser Studie von uns, €10 von Ihren Einnahmen abzuziehen.

Falls Sie zu irgendeinem Zeitpunkt Fragen haben, heben Sie bitte Ihre Hand und wir werden Ihre Fragen diskret beantworten.

SPIEL 1

In diesem Spiel bieten Sie in einer Auktion auf einen Geldpreis. Der Geldpreis kann unterschiedliche Werte für jede Person in Ihrer Gruppe haben. Sie werden dieses Spiel zehn Mal spielen, also in 10 Perioden. Alle Geldbeträge in diesem Spiel werden in Schritten von 25 Cent angegeben.

Zu Beginn jeder Periode zeigen wir Ihnen Ihren Wert für den Geldpreis dieser Periode. Falls Sie den Geldpreis gewinnen, werden Sie diesen Geldbetrag verdienen, abzüglich Ihrer Zahlung im Rahmen der Auktion.

Ihr **Wert für den Geldpreis** wird folgendermaßen bestimmt:

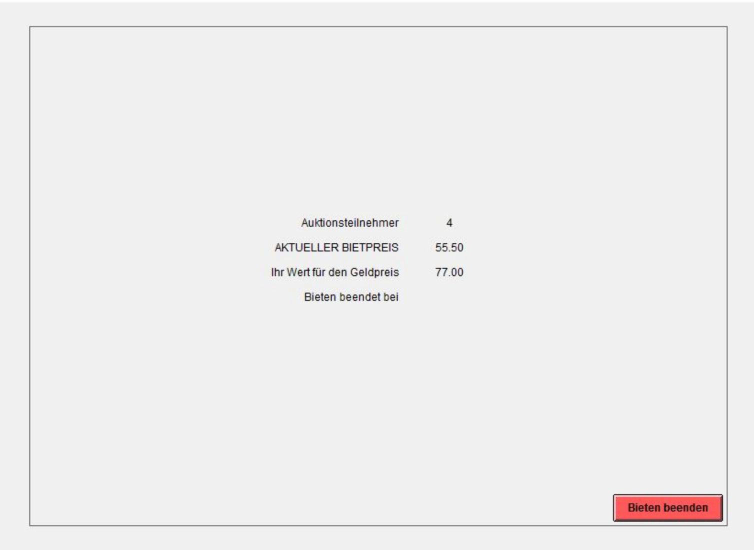
1. Für jede Gruppe bestimmen wir zufällig einen **Gruppenwert**, der zwischen € 10,00 und € 100,00 liegen wird. Jede Zahl zwischen € 10,00 und € 100,00 wird mit gleicher Wahrscheinlichkeit gezogen.

2. Für jede Person bestimmen wir zufällig eine **individuelle Anpassung**, die zwischen € 0,00 und € 20,00 liegen wird. Jede Zahl zwischen € 0,00 und € 20,00 wird mit gleicher Wahrscheinlichkeit gezogen.

In jeder Periode ist Ihr **Wert für den Geldpreis** gleich dem **Gruppenwert** plus Ihrer **individuellen Anpassung**. Zu Beginn jeder Periode erfahren Sie Ihren Wert für den Geldpreis insgesamt, jedoch nicht den Gruppenwert oder die individuelle Anpassung.

Das bedeutet, dass jede Person in Ihrer Gruppe einen anderen Wert für den Geldpreis haben kann. Allerdings gilt, dass wenn Sie einen hohen Wert haben, es wahrscheinlicher ist, dass andere Personen in Ihrer Gruppe einen hohen Wert haben.

Die Auktion läuft wie folgt ab: Zuerst erfahren Sie Ihren Wert für den Geldpreis. Dann beginnt die Auktion. Wir zeigen allen in Ihrer Gruppe einen **Bietpreis**, der niedrig startet und dann in 25-Cent-Schritten aufwärts zählt, bis zu einem Maximum von € 150,00. Zu jedem Zeitpunkt können Sie entscheiden, aus der Auktion auszusteigen, durch Klicken auf der Schaltfläche "Bieten beenden".



Sobald nur noch ein Bieter in der Auktion übrig ist, gewinnt dieser Bieter den Geldpreis zum **aktuellen Bietpreis**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon den **aktuellen**

Bietpreis ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Nach Beendigung jeder Auktion zeigen wir Ihnen jeweils die Bietpreise, bei welchen Bieter ausgestiegen sind und den Gewinn des erfolgreichen Bieters. Bei einem Gleichstand für das Höchstgebot gewinnt kein Bieter den Geldpreis.

SPIEL 2

In diesem Spiel bieten Sie in einer Auktion auf einen Geldpreis. Sie werden dieses Spiel zehn Mal spielen, also in 10 Perioden.

Ihr **Wert für den Geldpreis** wird generiert wie zuvor.

Allerdings ziehen wir nun in jeder Periode noch eine weitere Zahl, **X**, für jede Gruppe.

Die Regeln der Auktion lauten etwas anders, und zwar wie folgt:

Der **Bietpreis** zählt wieder in 25-Cent Schritten von einem niedrigen Startwert aufwärts, und Sie können zu jedem Zeitpunkt aus der Auktion aussteigen, durch Klicken auf der Schaltfläche "Bieten beenden". Sobald nur noch ein Bieter in der Auktion übrig ist, wird nun aber der Bietpreis noch **um weitere X Euro steigen**, und dann eingefroren.

Falls der letzte Bieter **in der Auktion verbleibt, bis der Bietpreis eingefroren wird**, dann gewinnt er den Geldpreis zum **eingefrorenen Bietpreis**. Das bedeutet, wir fügen seinen bisherigen Einnahmen aus dem Experiment seinen **Wert für den Geldpreis** hinzu und ziehen davon den **eingefrorenen Bietpreis** ab. Die Einnahmen aller anderen Bieter bleiben unverändert.

Falls der letzte Bieter aus der Auktion aussteigt, bevor der Preis eingefroren wird, gewinnt kein Bieter den Geldpreis. In diesem Fall bleiben die Einnahmen aller Bieter unverändert.

X wird **zwischen € 0,00 und € 3,00** liegen, wobei alle Zahlen in Schritten von 25 Cent mit der gleichen Wahrscheinlichkeit gezogen werden. Sie werden zu Beginn jeder Periode Ihren Wert für den Geldpreis erfahren, nicht jedoch **X**. Nach Abschluss der jeweiligen Periode erfahren Sie dann den Wert von **X**.

WELCOME

This is a study about decision-making. Money earned will be paid to you in cash at the end of the experiment. This study is about 90 minutes long.

We will pay you €5 for showing up, and €15 for completing the experiment. Additionally, you will be paid in cash your earnings from the experiment. If you make choices in this experiment that lose money, we will deduct this from your total payment. However, your total payment (including your show-up payment and completion payment) will always be at least €20. You have been randomly assigned into groups of 4. This experiment involves 3 games played for real money. You will play each game 10 times with the other people in your group.

We will give you instructions about each game just before you begin to play it. Your choices in one game will not affect what happens in other games. There is no deception in this experiment. Every game will be exactly as specified in the instructions. Anything else would violate the IRB protocol under which we run this study. (IRB Protocol 34876)

Please do not use electronic devices or talk with other volunteers during this study. If we do find you using electronic devices or talking with other volunteers, the rules of the study require us to deduct €20 from your earnings.

If you have questions at any point, please raise your hand and we will answer your questions privately.

GAME 1

In this game, you will bid in an auction for a money prize. The prize may have a different dollar value for each person in your group. You will play this game for 10 rounds. All dollar amounts in this game are in 25 cent increments. At the start of each round, we display your value for this round's prize. If you win the prize, you will earn the value of the prize, minus any payments from the auction.

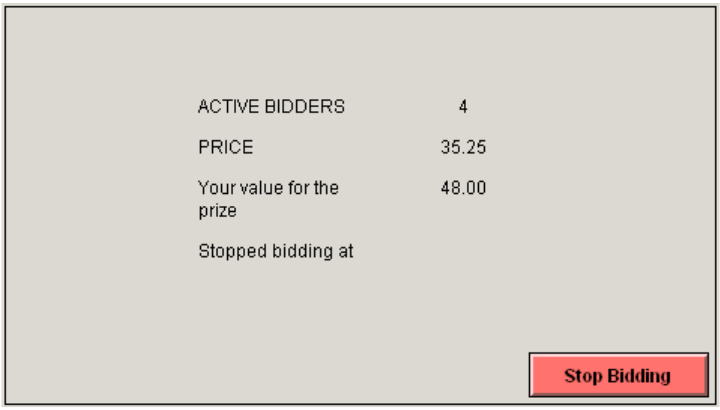
Your **value for the prize** will be calculated as follows:

1. For each group we will draw a **common value**, which will be between € 10,00 and € 100,00. Every number between € 10,00 and € 100,00 is equally likely to be drawn.
2. For each person, we will also draw a **private adjustment**, which will be between € 0,00 and € 20,00. Every number between € 0,00 and € 20,00 is equally likely to be drawn.

In each round, your **value for the prize** is equal to the **common value** plus your **private adjustment**. At the start of each round, you will learn your total value for the prize, but not the common value or the private adjustment.

This means that each person in your group may have a different value for the prize. However, when you have a high value, it is more likely that other people in your group have a high value.

The auction proceeds as follows: First, you will learn your value for the prize. Then, the auction will start. We will display a price to everyone in your group, that starts low and counts upwards in 25 cent increments, up to a maximum of € 150,00. At any point, you can choose to leave the auction, by clicking the button that says "Stop Bidding".



When there is only one bidder left in the auction, that bidder will win the prize at **the current price**. This means that we will **add** to her earnings her **value for the prize**, and **subtract** from her earnings **the current price**. All other bidders' earnings will not change.

At the end of each auction, we will show you the prices where bidders stopped, and the winning bidder's profits. If there is a tie for the highest bidder, no bidder will win the object.

GAME 2

In this game, you will bid in an auction for a money prize. You will play this game for 10 rounds.

Your **value for the prize** will be generated as before.

However, each round, we will also draw a new number, **X**, for each group.

The rules of the auction are different, as follows:

The price will count up from a low value, and you can choose to leave the auction at any point, by clicking the button that says "Stop Bidding". When there is only one bidder left in the auction, the price will **continue to rise for another X dollars**, and then freeze.

If the last bidder **stays in the auction until the price freezes**, then she will win the prize at the **final price**. This means that we will **add** to her earnings her **value for the prize**, and subtract from her earnings **the final price**. All other bidders' earnings will not change.

If the last bidder stops bidding before the price freezes, then no bidder will win the prize. In that case, no bidder's earnings will change.

X will be **between € 0,00 and € 3,00**, with every 25 cent increment equally likely to be drawn. You will be told your value for the prize at the start of each round, but will not be told **X**. At the end of each round, we will tell you the value of **X**.

WELCOME

This is a study about decision-making. Money earned will be paid to you in cash at the end of the experiment. This study is about 90 minutes long.

We will pay you € 5 for showing up, and € 15 for completing the experiment. Additionally, you will be paid in cash your earnings from the experiment. If you make choices in this experiment that lose money, we will deduct this from your total payment. However, your total payment (including your show-up payment and completion payment) will always be at least € 20.

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We will give you instructions about each game just before you begin to play it. Your choices in one game will not affect what happens in other games.

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GAME 1

In this game, you will bid in an auction for a money prize. The prize may have a different dollar value for each person in your group. You will play this game for 10 rounds. All dollar amounts in this game are in 25 cent increments.

At the start of each round, we display your value for this round's prize. If you win the prize, you will earn the value of the prize, minus any payments from the auction.

Your **value for the prize** will be calculated as follows:

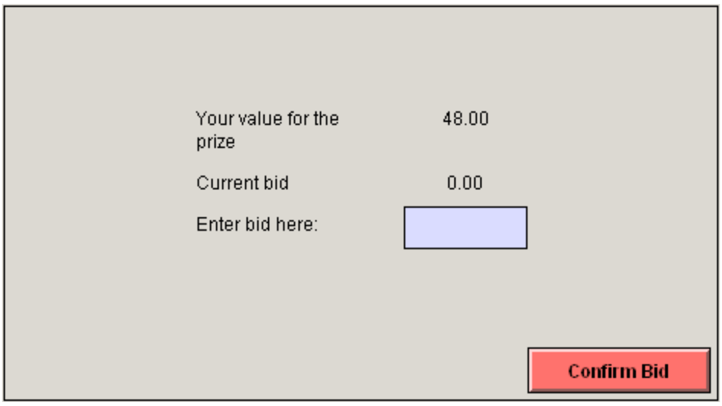
1. For each group we will draw a **common value**, which will be between € 10,00 and € 100,00. Every number between € 10,00 and € 100,00 is equally likely to be drawn.

2. For each person, we will also draw a **private adjustment**, which will be between € 0,00 and € 20,00. Every number between € 0,00 and € 20,00 is equally likely to be drawn.

In each round, your **value for the prize** is equal to the **common value** plus your **private adjustment**. At the start of each round, you will learn your total value for the prize, but not the common value or the private adjustment.

This means that each person in your group may have a different value for the prize. However, when you have a high value, it is more likely that other people in your group have a high value.

The auction proceeds as follows: First, you will learn your value for the prize. Then you can choose a bid in the auction. Each person in your group will submit their bids privately and at the same time. You do this by typing your bid into a text box and clicking "confirm bid". You will have 90 seconds to make your decision, and can revise your bid as many times as you like. At the end of 90 seconds, your final bid will be the one that counts.



All bids must be between € 0,00 and € 150,00, and in 25 cent increments.

The highest bidder will win the prize, and make a payment equal to **the second-highest bid**. This means that we will **add** to her earnings her **value for the prize**, and **subtract** from her earnings **the second-highest bid**. All other bidders' earnings will not change.

At the end of each auction, we will show you the bids, ranked from highest to lowest, and the winning bidder's profits. If there is a tie for the highest bidder, no bidder will win the object.

GAME 2

In this game, you will bid in an auction for a money prize. You will play this game for 10 rounds.

Your **value for the prize** will be generated as before.

However, each round, we will also draw a new number, **X**, for each group.

The rules of the auction are different, as follows:

All bidders will submit their bids privately and at once. However, the highest bidder will win the prize if and only if their bid **exceeds the second-highest bid by more than X**.

If the highest bidder wins the prize, she will make a payment equal to **the second-highest bid plus X**. This means that we will **add** to her earnings her **value for the prize**, and **subtract** from her earnings **the second-highest bid plus X**. All other bidders' earnings will not change.

If the highest bid does not exceed the second-highest bid by **more than X**, then no bidder will win the prize. In that case, no bidder's earnings will change.

X will be **between €0,00** and **€3,00**, with every 25 cent increment equally likely to be drawn. You will be told your value for the prize at the start of each round, but will not be told **X**. At the end of each round, we will tell you the value of **X**.

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