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Abstract

This study investigates experimentally information disclosure in settings with and without seller competition. Sellers often choose to report information selectively and buyers account for this—even though not fully—by bidding skeptically. As expected, competition increases sellers' information disclosure but leads, surprisingly and replicably, to more buyer naïvety, offsetting the welfare benefits from improved disclosure. A framing effect generates this result: merely describing a situation as competitive rather than monopolistic alters buyer behavior. Akin to the so-called *Peltzman effect*, buyers seemingly perceive competition as a safer environment to which they behaviorally adapt by abandoning their skepticism. Consequently, consumer benefits hinge on perceived competitiveness—a vulnerability firms may leverage to their advantage.

Keywords: Disclosure, verifiable information, competition, Peltzman effect

JEL Classification: D40, D83

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1 Introduction

Many times per day, consumers are confronted in media advertisements and on product packages with verifiable product information that firms choose to convey. This might be a microwave oven's "very good" result in an independent test or the certified absence of a chemical in a plastic container. The presence and in particular the absence of such information is more or less useful for the consumer depending on her ability to understand how firms' interests shape the selection of disclosed information.

In contrast to cheap talk, verifiable information provides a clear link between information and the underlying state of the world. The resulting "persuasion games" can feature full information revelation. However, for this to work, the seminal theoretical treatments emphasize the important role of all buyers' sophistication in the form of "skepticism" in the inference process (Grossman, 1981; Milgrom and Roberts, 1986; Milgrom, 2008). Recent experimental evidence (Jin, Luca and Martin, 2021b) indicates that consumers' limited sophistication often prevents full information revelation, leading to incorrect inferences and giving firms the opportunity to mislead consumers by withholding or partially disclosing information. For example, restaurants may abstain from voluntarily disclosing health department ratings (Jin and Leslie 2003; Bederson et al. 2018); online stores may hide shipping charges (Brown, Hossain and Morgan 2010); health maintenance organizations (HMOs) can choose whether to disclose quality by submitting to independent accreditation (Jin 2005); hedge funds may only selectively report past earnings (Malkiel and Saha 2005); and film distributors might withhold movie previews from critics to boost earnings (Brown, Camerer and Lovallo 2012, 2013).

A likely aid for the unsophisticated consumer, as well as a constraint on firms' ability to mislead consumers, is competition between sellers. For example, Milgrom and Roberts (1986) show that competition among parties with strongly opposed interests re-establishes full information revelation with naïve consumers. In order to better understand sellers' disclosure behavior and buyers' inferences under competition, we set up a rich experimental framework. In practice, variations in market structure are often intertwined with other factors such as consumer demographics or firm characteristics, whereas our design enables the identification of the causal effect of competition by directly manipulating competitive conditions while keeping constant other variables that might confound the relationship.

Our setup involves an intuitive product market frame, a gradual spectrum of information disclosure and the typical conflict of interest that arises from sellers' incentives to increase buyers' willingness to pay and buyers' incentives to evaluate the product correctly. Sellers offer a good whose true value is known only to them. They have 10 pieces of noisy evidence about the product quality and, for each piece of evidence, can decide whether to reveal it. The bidding mechanism gives buyers incentives to correctly evaluate the product. In this setup, we implement two conditions varying the market structure: a monopoly with one seller and competition with four sellers.

Across the two settings, we find that sellers frequently report only a selected set of evidences that predominantly contains the most favorable evidences. In general, buyers compensate for that selection fairly well, but—in accordance with the literature—we find some indication of consumer naïvety. Although buyers' accurate inferences could impose at least an equality of payoffs, sellers gain more than buyers across both games. As predicted, competition increases disclosure throughout, including the frequency of full disclosure. There is, however, a significant and replicable influence of the competitive setting on the buyers' inferences. Compared to the monopolistic setting, buyers compensate surprisingly little for the selection in the competitive settings. Indeed, this "competition naïvety" has the consequence that buyers do not earn significantly more when having the choice of seller despite sellers' increased disclosure.

In two extensions, we investigate a number of potential reasons for the lack of buyers' skepticism in the competitive setting. We hypothesize that buyers' actions might be influenced by social preferences, an illusion of control, distorted beliefs, the complexity of the choice situation and the idea that competition might legitimize incomplete disclosure. By manipulating these factors in our two extensions, we can rule out most explanations and, surprisingly, can conclude that a framing effect generates the diverging levels of skepticism between competition and monopoly. Letting subjects play essentially the same modified version of our game, once with a competition frame and once with a monopoly frame, shows that the former inhibits skepticism. Importantly, this highlights that consumer benefits may not simply relate to the underlying market structure but critically depend on how competitive consumers *perceive* a market to be and on how they react to that.

Subjects' perception of the situation seems to be fundamentally changed in the competition frame: buyers behaviorally adapt to what they perceive as an arguably more favorable environment. Buyers' adaptation, however, works to their detriment, nullifying any gains from an increased information disclosure. Such patterns of misguided behavioral adaptations are well known in the social sciences and are referred to as the *Peltzman effect* (Peltzman, 1975): Bicycle helmets or seat belts may help to avoid injuries but may also lead some cyclists or motorists to ride less cautiously. Similarly, face masks or vaccines may help to slow the spread of a pandemic but could lead to less compliance with other anti-COVID-19 regulations.[1] In behavioral economics, the effec-

[1] In the original study, the offset was actually complete. Later studies, however, found that—at least

tiveness of some nudges suffers from such adaptations (Carrol et al., 2009; Caplin and Martin, 2017). Consistent with a behavioral adaptation of buyers, we find heterogeneity in individual behavior. The estimation of a simple mixture model with two behavioral types – naïve and skeptical buyers – reveals that the fraction of naïve buyers is about 1/3 in monopoly while it rises to about 2/3 in competition.

Ultimately, our study shows that a competitive environment, where buyer skepticism remains essential, may offer consumers no greater benefits than a monopolistic one. Moreover, consumer outcomes may not simply relate to the underlying market structure but critically depend on how competitive they perceive a market to be and on how they adapt to that—a vulnerability firms may leverage to their advantage. Indeed, in marketing science, dating back to Bauer (1960), the concept of *consumer-perceived* risk, along with risk-reduction strategies and their connection to other consumer behavior concepts, has garnered significant attention (for reviews see Mitchell, 1999; Maziriri and Chuchu, 2017). Risk is commonly seen as an antecedent of consumer trust and involvement, as building consumer trust entails mitigating perceived transactional or relational risk. However, the impact of competition remains relatively unexplored in this literature. Our findings suggest that competition plays a role in reducing perceived risk and possibly increasing trust, albeit unwarrantedly and with potential negative consequences for consumers.

Competition has long been praised to benefit the consumer by keeping prices low and the quality of goods and services high.[2] With respect to information revelation, a long tradition in political, legal and economic thought has argued that competition increases the amount of information revealed (Milgrom and Roberts, 1986; Battaglini, 2002; Gentzkow and Shapiro, 2008). While previous theoretical studies have advanced arguments that this may not always be the case (Gentzkow and Kamenica, 2016; Milgrom, 2008; Board and Lu, 2018), our findings are different. In our setting, competition indeed leads to increased information disclosure but fails to benefit consumers as it erodes their skepticism. In other words, our results provide a cautionary tale, that a cornerstone of consumer welfare, namely competition, may invoke consumer reactions that in the worst case nullify its fundamental benefits.

in the case of seat belts—the Peltzman effect may fade away over time (Cohen and Einav, 2003; Lv et al., 2015). In other domains, this is still under discussion. Peltzman effects have also been found (and disputed) for airbags and anti-lock brakes (Lardelli-Claret et al., 2003; Phillips et al., 2011; Janssen, 1994; Winston et al., 2006), for condoms or PrEP (pre-exposure prophylaxis) and risky sexual behavior (Shelton, 2007; Wilson et al., 2013; Holt, 2018).

[2]As an example, 86% of people across the EU agree with the idea that competition allows for more choice, 85% (74%) think that it lowers prices (leads to higher quality goods) (Eurobarometer 2015).

Related Literature In economics, the understanding of the negative consequences of information asymmetries motivates the investigation of information disclosure (Akerlof, 1970; Viscusi, 1978). The prediction of full voluntary disclosure of verifiable information due to unravelling is useful, but also dependent on assumptions such as negligible disclosure costs, sufficient competition, or sophistication of the consumer (Milgrom, 1981; Milgrom and Roberts, 1986).[3] In an influential study, Jin et al. (2021b) experimentally implemented a stylized disclosure setting: a sender privately observes the true state of the world and decides whether to fully disclose this information to a receiver, who then estimates the state of the world, knowing that interests are not aligned. The authors find that many players are not sufficiently skeptical about non-disclosure, preventing complete unraveling—a finding supported in the literature. Hagenbach and Perez-Richet (2015) and Li and Schipper (2020) observed buyer naïvety in disclosure and persuasion games, though the extent varies. Nevertheless, receiver naïvety remains a robust phenomenon. For instance, introducing communication or obfuscation typically reduces skepticism (Montero and Sheth, 2021; Jin et al., 2021a; Deversi et al., 2021).

Relative to this literature, our flexible product market framework of information disclosure is less stylized than the typical sender-receiver framework, naturally capturing differing degrees of disclosure instead of a binary disclosure choice. Experiencing all types of disclosure—none, limited, moderate, substantial—may facilitate understanding the dangers of limited or no disclosure. Similarly, Farina et al. (2024) examine a setting where partial disclosure is possible but focus on *exogenously* varying its potential selectivity. Their design, which limits the number of disclosed evidences from a large pool, leads to widespread full, but selective disclosure in some treatments—contrary to prior findings. Our design also allows us to explore how selectivity affects buyer inference but imposes no such restrictions. As a result, we continue to observe that the unraveling logic does not hold.

Experimental studies investigating competition are still scarce.[4] Benndorf, Kübler and Normann (2015) study information disclosure in competitive labor markets with a focus on privacy but cannot address receiver naïvety as the receiver side is simulated.

[3]The large theoretical and experimental literature on cheap talk games considers situations in which statements are not bound to relate to the true seller type (Crawford and Sobel, 1982; Cai and Wang, 2006; Wang, Spezio and Camerer, 2010). In addition, since their establishment, persuasion games have continuously received attention from theorists (e.g. Glazer and Rubinstein, 2001, 2004, 2006; Kamenica and Gentzkow, 2011; Gentzkow and Kamenica, 2016)

[4]Empirical studies support the view that competition matters, although it is not clear in which direction. Jin (2005) studies health organizations and shows that competition gives stronger incentives to differentiate in disclosure decisions. Burks, Cuny, Gerakos and Granja (2018) find an increase in competition between banks to raise the level of voluntary disclosure and in particular of negative information.

Building on Jin, Luca and Martin (2021b), Sheth (2021) shows that, in a stylized binary disclosure setting with two competing senders, receiver preference for disclosing senders creates competitive pressure, significantly enhancing information disclosure. This increased disclosure leads to greater unraveling and improved receiver welfare.

In contrast to this binary disclosure environment, our less stylized setting, outlined in the next section, features *varying degrees of disclosure*. Competitive pressure may hence lead to increased, but not necessarily complete disclosure, requiring receivers to remain skeptical to a degree even with improved but partial disclosure. In this arguably more realistic setting, our novel finding of *competition naïvety* emerges and implies that receivers might not benefit from competition.

2 Experimental games

We investigate the effects of competition by implementing two market structures that we call "monopoly" (M) and "competition" (C). These games involve information in the form of 10 evidences and are therefore denoted as M10 and C10.

Monopoly (M10) Our basic game features one seller and one buyer. The seller offers a good of value v, with v uniform-randomly drawn from $V = [200, 1000]$. While this distribution is common knowledge, the realization of v is private information to the seller. The seller cannot communicate the true value of the good to the buyer, but he receives a disclosable set E of 10 informative but noisy evidences e_i. The evidences are normally distributed with a standard deviation of $\sigma = 100$ and a mean of $\mu = v$. The number and distribution of evidences is common knowledge. The seller decides which of those evidences, if any, to report to the buyer and thus determines a message $M \subseteq E$. Due to the verifiability of information, the seller cannot change or manipulate the evidences' values. Undisclosed evidences remain his private information.

The buyer observes M and bids $b \in [0, 1200]$ to buy one unit of the seller's good. The design of the price mechanism is equivalent to a Becker, DeGroot and Marschak (1964, BDM) mechanism: The price for one unit of the good, p, is uniform-randomly drawn from the interval $[v - 200, v + 200]$. p is disclosed only after the buyer places her bid. If $b < p$, the transaction does not take place, leaving both parties with a payoff of 0. If $b \geq p$, the transaction takes place and the buyer gets the value v for the price p. The seller obtains the bid b and incurs costs of $c = v - 50$ upon production of one unit. This implies that a seller does not per se benefit from a high value as it is associated with higher production cost.[5]

[5]For examples of possible outcomes see Appendix A.2. Notice that due to the production cost, in

In summary, the seller's payoff is

$$\Pi_S(b, p, v) = \begin{cases} b - (v - 50) & \text{if } b \geq p, \\ 0 & \text{otherwise.} \end{cases}$$

and the buyer's payoff is

$$\Pi_B(b, p, v) = \begin{cases} v - p & \text{if } b \geq p, \\ 0 & \text{otherwise.} \end{cases}$$

In general, the purpose of the BDM mechanism is to give the buyer the incentive to bid what she believes to be the true value of the good.[6] Therefore, this setup implements the natural conflict of interest between the seller that wants the buyer's bid to be as high as possible and the buyer that wants her evaluation of the good to be as accurate as possible.

Competition (C10) The competition game features four sellers and four buyers in each market. Sellers are indexed by j. Each seller offers a good with a value v_j that is independently drawn and private information as before. All aspects of E remain the same. The decisions of each seller to determine $M \subseteq E$ are made simultaneously. Subsequently, the buyers as well as the sellers observe the published evidences of all four sellers.

In contrast to before, buyers choose from which seller j^* to buy before they place their bid b for the chosen seller's product. The prices p_j and payoffs are determined in the same fashion as before. There is no competition among the buyers because a seller can sell up to four units of his good.

theory, situations may occur in which sellers try to avoid a transaction if they expect bids to be very low. Bids could be depressed by (i) not disclosing any evidence at all or (ii) consistently sending the lowest evidence(s) while not disclosing higher evidences. Empirically, these two types of behavior appear to be very infrequent, occurring in about 0.5% and 1.0% of cases, respectively.

[6]Traditionally, the Becker-deGroot-Marschak mechanism is an incentive-compatible mechanism used to elicit willingness-to-pay (WTP). If a buyer knows her WTP, the mechanism gives incentives to reveal it truthfully. In our case, the true value v is subject to uncertainty from the point of view of the buyer, hence risk-averse buyers are expected to underbid relative to their expected value, and vice versa for risk-lovers (Kaas and Ruprecht, 2006). This way, risk attitudes influence bidding behavior. Comparisons of behavior between games will still reveal differences due to game structure. In table B.7 and B.8 in the online appendix, we find some evidence consistent with the idea that some players may prefer a high v. Notably, this analysis also reveals that such inclinations would actually bias against observing some of our main results.

2.1 Some remarks on the design

We create a multifaceted and easily extendable framework in order to reflect naturally occurring structures of information disclosure that are intuitive for participants. This implies that not all features of existing simple frameworks extend to our setting. For example, we have evidences with normally distributed errors because those naturally reflect the noise in test results. With such noise, even full disclosure—revealing all available evidence—will not perfectly reveal the true value. We start with 10 pieces of evidence to create a relatively fine spectrum of gradual disclosure and to make unselected full disclosure a good approximation of the true value.[7] We have four competing sellers to give buyers ample choice and leave no doubt about the competitive nature of the market. Still, we think that many of the intuitions about voluntary information disclosure hold to be true here. Four aspects of the implemented games are more unusual and deserve particular mentioning.

First, we work with an exogenous price level to avoid the introduction of any strategic consideration via a mechanism of endogenous price determination. In our setup, the buyer simply wants her bid to reflect the value of the good as accurately as possible.[8]

Second, while any price mechanism levels the playing field between sellers with different v—a fairly priced low-v product can be more attractive than an overpriced high-v product—our exogenous price mechanism makes this feature particularly apparent, as different v's induce different price ranges. In contrast to some theoretical models, the absolute level of v becomes irrelevant because it is evaluated by seller and buyer relative to price or cost, respectively. Put differently, our mechanism makes the situation strategically symmetric between the sellers[9] and puts an emphasis on the amount of information available.

Third, like any experimental design, this setup is not without its share of artificiality compared to a naturally occurring market environment. Here, the fact that the buyer pays a price p while the seller receives the bid $b \geq p$ may be odd on first sight. If, however, this is viewed as a reduced-form model of situations in which the difference between the bid—or willingness to pay—and the price is not effectively paid in one transaction but over a longer customer relationship or in which some monetary benefits to the seller are non-monetary to the customer, such as customer data, it is certainly

[7] Empirically, we find that the average of all signals is only 1.5 points different from the true value. However, selectively revealing all evidence when the realizations are particularly favorable will imply a higher publication bias.

[8] We instruct buyers that they benefit most from bidding according to their beliefs, a procedure which generates fewer false reports than a reliance on the subject's quantitative understanding (Danz et al., 2022).

[9] The interval boundaries of V provide information that prevents complete symmetry between sellers with different v. In theory, this favors the inference from low-v sellers but does not show in the data.

tenable.

Fourth, it is noteworthy that we implement a competitive setting which does not exhibit "strongly opposed interests" as modelled in Milgrom and Roberts (1986) via an additional pricing stage. All disclosure incentives are derived from the relative amount of disclosure, there is no possibility to disclose information about other products. The information environment is not "Blackwell-connected" in the sense of Gentzkow and Kamenica (2016). Intuitively, our setting is closer to the commonly observed nature of competition with companies not disclosing verifiable information about competitors' products and allows us to observe the effectiveness of competition when disclosure incentives are smaller.

2.2 Theoretical considerations and hypotheses

Although we will not be able to present a full characterization of equilibria in the implemented, less stylized games, a few theoretical considerations will be useful to derive hypotheses and facilitate the interpretation of the observed behavior.

2.2.1 Seller strategies

In all games, the seller is informed about the true value v of the good and the 10 available evidences E before he specifies the message $M \subseteq E$. Consider the product space $V \times \mathcal{E}$, where $\mathcal{E}$ is the 10-dimensional integer space of possible evidence realizations, $\bigtimes_{n=1}^{10} \mathbb{Z}^n$. For each value and evidence realization, the seller's strategy $\sigma(v, E)$ gives a choice of M.

Among the large number of possible disclosure strategies, let us consider strategies that have previously received attention in the literature and their analogues in our setting. First, the fully revealing equilibrium strategy as discussed in Milgrom and Roberts (1986) corresponds most closely to the full revelation of all evidences $M = E$. If this strategy was chosen irrespective of the realization of v and E, the expected value $\mathbb{E}(v|E)$ would simply be derived from ex ante probabilities of v updated with E by Bayes' rule. In games without uncertainty about the number of evidences available to the seller—as in our setting—the literature has established that such a strategy can be sustained in equilibrium under skeptical beliefs of the buyer (Milgrom and Roberts, 1986; Milgrom, 2008).[10]

Analogue to Shin (1994, 2003), it is useful in our setting to consider the "naïve sanitization strategy" (NS) of disclosing an evidence e_i if and only if it provides evidence of at least the good's value, $e_i \geq v$. This establishes a benchmark strategy in which

[10]Whether such an equilibrium is unique depends on the precise assumptions on players' utility.

the realization of evidences matters greatly for their disclosure, establishing a natural selection into more or less disclosure. The resulting message M always induces a *positive* "publication bias" of $\bar{e}^M - v$, where $\bar{e}^M$ is the mean of the disclosed evidences, $\frac{1}{\#M}\sum_{i:e_i \in M} e_i$. Under this strategy and given the data generation process of E, full disclosure is observed very rarely but would—due to the selection—be associated with a considerable publication bias.[11]

2.2.2 Buyer types

From the buyer's perspective, the strategy of the seller $\sigma(v, E)$ and the observed message $M \subseteq E$ induce a conditional probability distribution over the product value, $f(v|M, \sigma)$, and an expected value $\mathbb{E}(v|M, \sigma)$. It is instructive to consider two different types of buyers, a *skeptical* buyer and a *naïve* buyer, with respect to the evaluation of these entities.

A naïve buyer evaluates the message M as an unselected account of the seller's disclosable evidence. Irrespective of any strategy σ, such a buyer uses each evidence in M as an additional, independent observation that helps to more precisely infer $\mathbb{E}(v|M)$. The naïve buyer would thus form an expected value $\mathbb{E}(v|M) = \bar{e}^M$ that is the mean of disclosed evidences. While this buyer is unconscious of the fact that publishing a limited number of evidences might imply a publication bias, when given the choice, she still prefers more to fewer evidences as she has an incentive to judge the product as precisely as possible. Even when abstracting from any potential publication bias, 10 available evidences enable estimating the product value with a smaller confidence interval than fewer evidences and thereby help avoiding losses due to over- and underbidding.[12]

A skeptical buyer is aware of the possible selection of evidence due to the seller's incentives to generate a high bid. Such a selection—as featured for example by a naïve sanitization strategy—implies that only the $\#M$ highest evidences are disclosed and that disclosure might be driven by high realizations in E. The skeptical buyer forms the expected value corresponding to the seller's strategy $\mathbb{E}(v|M, \sigma)$ and bids accordingly. Compared to the naïve bid, the skeptical bid features a skepticism-induced markdown $S(M) = \mathbb{E}(v|M) - \mathbb{E}(v|M, \sigma)$. When given a choice, the skeptical buyer also prefers more evidences as it allows for judging the true value more precisely.

[11] In our full sample of 1600 realized sets of E, this would have occurred once with a publication bias of 71.9 (see panel (a) of table 2 on page 16).

[12] In principle, naïve buyers could be ignorant both to a potential publication bias and to the advantages of more evidences but we conjectured the first aspect to be a more severe problem. Indeed, we will find that naïve buyers choose their sellers almost as well as skeptical ones but bid sub-optimally.

2.2.3 Hypotheses

As is established in the literature, the interaction between sellers and buyers is shaped by the skepticism of the buyers. Being confronted with skeptical buyers implies for sellers that their ability to misrepresent the true value of the good is limited. For example, a naïve sanitization strategy turns out to be quite informative for buyers that expect such behavior. Therefore, in equilibrium, the skepticism will effectively limit the profit of sellers. In contrast, naïve bidding in the face of strategies that lead to positive publication bias generates sizable profits for the seller. Publishing only the highest evidences implies that naïve buyers on average overbid the product value substantially. Certainly, a share of naïve buyers as considerable as found by Jin et al. (2021b) makes strategies with higher publication biases attractive for the sellers. This leads us to hypothesize as follows on behavior in M10.

Hypothesis 1 (M10) *The seller's disclosure strategy exhibits a positive publication bias and the average buyer's bidding strategy compensates for that incompletely.*

The incomplete compensation of the buyer's bidding strategy is entirely driven by naïve buyers. In the competitive settings, the four sellers are a priori homogeneous from the buyers' perspective. Since both sophisticated and naïve buyers have incentives to judge the product as precisely as possible and, thus, prefer more evidence compared to less evidence when given a choice, sellers compete with each other to offer the most information about the product.[13]

Hypothesis 2 (C10) *Compared to M10, competition on the seller side increases the number of disclosed evidences and reduces the publication bias. This allows the buyer to judge the product more precisely and requires a lower markdown, implying a higher payoff.*

Buyers' preference for disclosure provides sellers incentives to disclose more. Furthermore, naïve buyers will, in particular, benefit from disclosure because the lower markdown requirement reduces their overbidding. Due to the lower publication bias, the seller benefits less from naïve buyers and obtains a lower payoff.

[13] Whether the competitive forces are strong enough to lead to full disclosure depends, of course, on the relative incentives: providing one evidence more than all other sellers may ensure that all buyers bid for a seller's product but it also reduces the publication bias and, thus, the expected profit per (naïve) buyer.

2.3 Experimental procedures

We conducted the experiments in the Experimental Economics Laboratory at the University of Mannheim (mLab).[14] Across eight sessions, 96 students participated.[15] We recruited all students from the general student population of the University of Mannheim.

At the beginning of each session, the instructions are read out aloud and explained to all participants. Subsequently, subjects have three attempts to complete an understanding test of four comprehension questions about the payoff structure. Those who fail get an individual short briefing on the points they did not understand. Finally, subjects have three minutes to generate sets of 10 draws from normal distributions with standard deviation of 100 in order to get familiar with the kind of draws occurring in the experiment.[16]

In a session, participants play two games sequentially. To control for order effects, we vary the order of the games between sessions, see table A.1. Each game is played for 10 rounds with two unpaid practice rounds before game 1. Participants are randomly matched into markets and randomly assigned the role of seller or buyer. While their market counterparts change randomly in each round, participants keep their role of seller or buyer for 5 rounds. In order to increase learning, for the rounds 6-10, roles are switched and maintained until the end, while market counterparts again keep changing.

At the end of each round, the feedback in M10 consists of the true item value, the price, the bid, the realization of a transaction and own payoffs. In C10, sellers are further informed about the published messages of all other sellers, their own number of realized transactions and the total amount bid for their good.[17]

Participants are compensated based on the outcomes in the 20 paid rounds. Individual payoffs are in points and are converted to cash at an exchange rate of 1 EUR for 60 points.[18] The average payoff per subject was 10 EUR. Since payoffs can be zero and even negative in a given round, we established a minimum payoff for the session of 2 EUR which was not binding for any participant.

[14]The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007) and subjects were recruited with ORSEE (Greiner, 2004).

[15]Note that the experiment implemented a second treatment variation as discussed in section 3.4. Overall, the experiment had 10 sessions and 160 participants, see table A.1 in appendix A.1.

[16]Appendix A.4 provides screenshots of the games and of this tool.

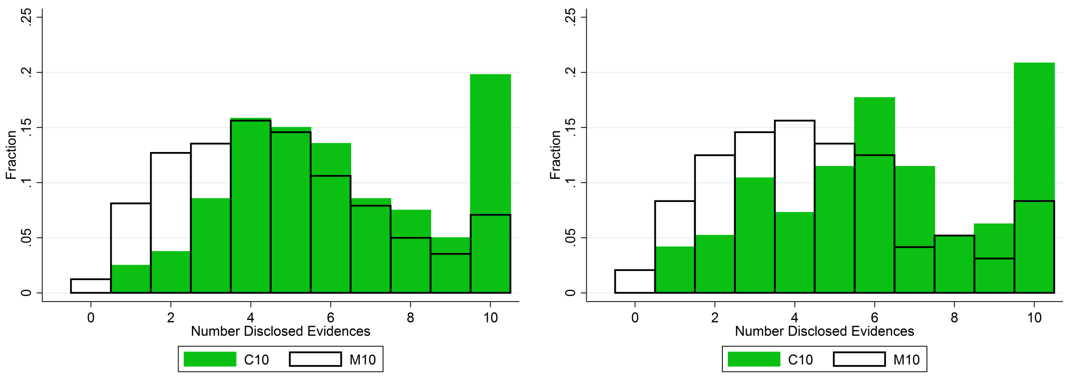
[17]In the two initial sessions, sellers in C10 were not able to see what the other sellers decided to report.

[18]All values are restricted to integers and given in experimental points.

3 Results

3.1 Seller behavior

A basic indicator of the effect of competition is the number of evidences that sellers disclose in their message to buyers, $\#M$. Figure 1a shows that this number is fairly symmetrically distributed around 4 and 5 and shifts slightly to the right in competition, with a pronounced spike developing at the full disclosure of 10 evidences. Figure 1b shows a slightly stronger difference in the last round of being a seller, suggesting that the competitive pressure leads to even more disclosure over time. By additionally showing the 5th and the 10th round data, we account for each participant only once in each role, at a point when some experience has been accumulated.



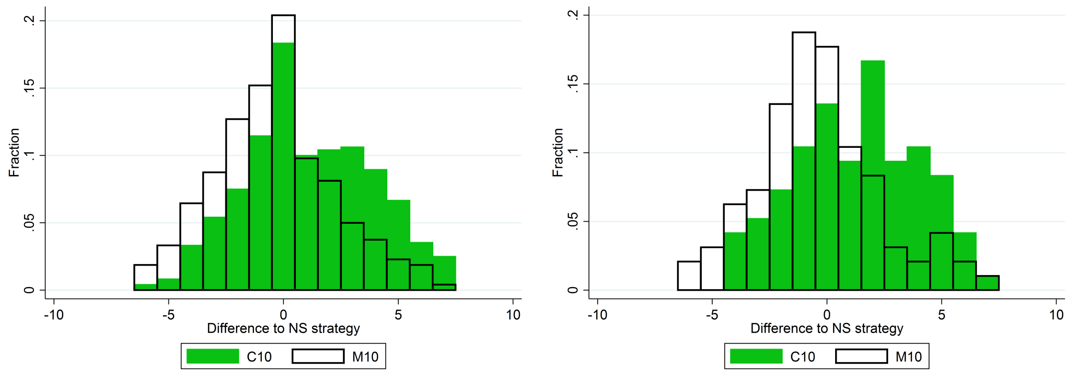
(a) All rounds.

(b) Rounds 5&10.

Figure 1: Number of disclosed evidences, $\#M$, in X10.

Note that a naïve sanitization (NS) strategy of disclosing only evidences that are weakly higher than the true value of the good generates a hump-shaped, symmetric-around-5 distribution of the number of disclosed evidences similar to the central part of the distribution in figure 1a. It turns out to be illuminating to look at the data from the perspective of such a strategy. We therefore simulate the NS strategy for each realized set of evidences E and calculate the difference in the number of disclosed evidences from a NS strategy, $\#M - \#M_{NS}$. In figure 2 the mode at zero shows that most disclosure behavior exhibits exactly the number of evidences of a NS strategy. The fact that many sellers fully disclose their evidences in C10—as reflected by the spike at $\#M = 10$ in figure 1—clearly translates to higher fractions of positive differences from the NS strategy in figure 2.

Throughout games and rounds, panel (a) of table 1 shows significant differences in the average number of disclosed evidences between M and C. In C10, over all rounds,



(a) All rounds.

(b) Rounds 5&10.

Figure 2: Difference in disclosed evidences to a naïve sanitization strategy, $\#M - \#M_{NS}$.

1.47 pieces of evidence more are disclosed compared to M10. The difference is 1.60 in the last rounds of being a seller.

The selection of evidences leads to a positive "publication bias" of $\bar{e}^M - v$ if the mean of the disclosed evidences exceeds the true value of the good. Panel (b) of table 1 shows that the increased disclosure in C is associated with a lower publication bias than in M, as expected when sellers publish their highest evidences. This difference is significant across games and even more pronounced in the last rounds between M10 and C10. Overall, the data show that sellers' disclosure improves significantly due to competition. We present robustness checks and evidence from a regression analysis that controls for order effects and role-switching in section B of the online appendix.[19]

	All rounds			Rounds 5&10		
	M10		C10	M10		C10
(a) Nr Published evidences $\#M$	4.71	***	6.18	4.65	***	6.25
(b) Publication bias $\bar{e}^M - v$	75.05	***	45.61	82.83	***	42.21
N	480		480	96		96

Notes: t-tests for equal means, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table 1: Means of number of disclosed evidences and publication bias.

The publication bias and the sellers' disclosure decisions can be analyzed in further

[19] Among other things, we reproduce table 1 in the online appendix with non-parametric tests that only make use of statistically-independent session-level data, providing a very conservative test.

detail when reporting results by the number of published evidences $\#M$ as in table 2. In panel (a), we report the results of simulations, in which we draw 10 evidences 10000 times. The first line indicates the mean difference to the true value for the kth ranked draw, $e_k - v$. While the highest signal (1) on average is 153.7 points higher than the true value, the lowest signal (10) is 154.2 points lower. The second line indicates the mean publication bias for the k highest draws, $\frac{1}{k}\sum_{i\in\{1,\ldots,k\}} e_i - v$, if $k = \#M$ is chosen irrespective of the realization of E. Naturally, a top-1 strategy features a large empirical publication bias of 153.7 while a top-10 strategy features a bias of zero. The last two lines of panel (a) indicate the publication bias and frequencies of message sizes $\#M$ under a naïve sanitization strategy. As expected, the publication bias is positive and substantial throughout and the number of observations are hump-shaped in $\#M$ and centered around five.

Panel (b) reports the observed publication bias split up between M10 and C10. Across games, for $k \leq 6$, the magnitudes of the publication bias are below the levels theoretically expected under the rigid benchmark of a "top k evidences" disclosure strategy, and above for $k > 6$. As expected, participants seem to select evidences dependent on their realization, as also a naïve sanitization strategy would suggest. Without such selection, for example, a publication bias of 13.2 for $k = 10$ in C10 cannot be explained by the evidence realizations alone. Critically, in contrast to a naïve sanitization strategy, some players seem however not to select the best k evidences above the true value, lowering the publication bias.

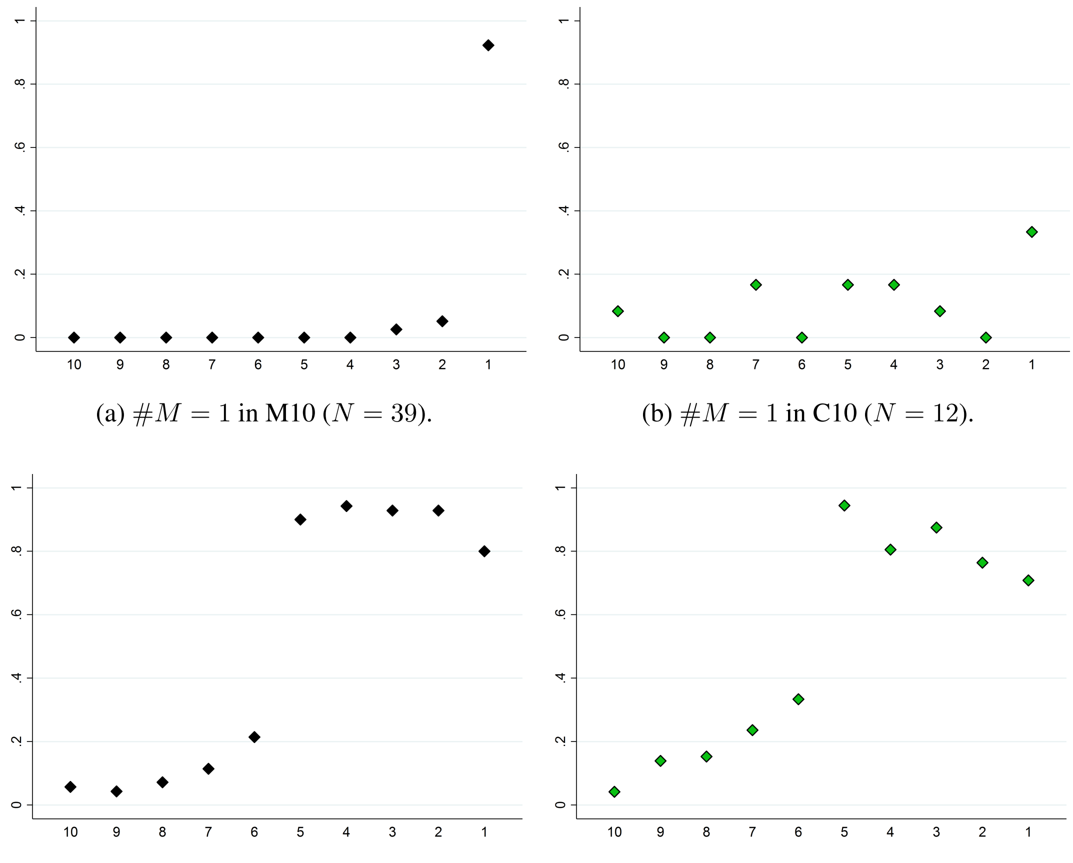
In order to understand better the sellers' strategies, we can take a closer, exemplary look behind these averages. For the case of the very limited disclosure of only one evidence, $\#M = 1$, consider the publication bias of 141.3 in M10 and the significantly lower value of only 40.6 in C10. Figure 3 shows the fraction of disclosure by the ranked pieces of evidence. For $\#M = 1$, figure 3a shows that almost all of the 39 sellers in M10 disclose their highest evidence (rank 1), as expected from a top-k strategy, while less than 40% of only 12 sellers do so in C10 (3b). The other strategies of those sellers seem to mostly consist of disclosing the single evidence that is closest to (or slightly above) v. In other words, the comparison between M10 and C10 suggests that competition not only leads to disclosure of more evidences but it also increases the incidences of unusual strategies.

		#M / Rank k										
		10	9	8	7	6	5	4	3	2	1	0
(a)	k	-154.2	-100.4	-65.8	-37.8	-12.5	12.0	37.5	65.5	100.1	153.7	
Bench-	Top k	-0.2	16.9	31.6	45.5	59.4	73.8	89.2	106.4	126.9	153.7	
mark	naïve San.	71.9	85.1	78.7	82.7	80.0	80.8	80.8	80.8	89.6	23.8	
means	N	1	7	42	136	198	241	188	106	33	8	0
(b)	M10	⁝10.4	⁝31.6	⁝53.2	⁝57.1	52.4⁝	65.0⁝	72.7⁝	90.2⁝	117.8·	141.3	
Pub.	N	34	17	24	38	51	70	75	65	61	39	6
bias	C10	⁝13.2	21.7	⁚37.3	44.6	42.5⁝	54.9⁝	69.0⁝	67.3⁝	96.5⁝	40.6⁝	
	N	95	24	36	41	65	72	76	41	18	12	0
(c)	M10	39.8	16.8	29.5	35.1	45.3	40.2	59.8	75.1	116.2	170.4	
Mark-	N	34	17	24	38	51	70	75	65	61	39	6
down	C10	7.0	10.5	16.7	28.8	25.4	19.6	16.1	11.0	-57.8	1.8	
	N	174	21	35	47	53	60	58	22	8	2	0
(d)	C10 Chosen	9.8	26.4	40.4	44.2	51.2	56.1	73.5	64.0	140.1	45.3	
Pub. bias	sellers	174	21	35	47	53	60	58	22	8	2	0

Notes: Average publication bias being ⁝above (below⁝) the simulated confidence interval of N Top k draws is denoted with Maya numerals ⁝ (99%), ⁚ (95%), and · (90%) to the left (right) of the average.

Table 2: Summary by number #M and rank k of disclosed evidences (X10 games).

Similarly, figures 3c and 3d show for an intermediate level of disclosure, $\#M = 5$, that strategies feature a top-k nature (i.e. involve disclosure of all top-k strategies) and that deviations from that are slightly more numerous in C10. Indeed, a perfect top-5 strategy would imply that evidences ranked 1-5 have a disclosure probability of 1 while evidences ranked 6-10 have a zero probability. Of course, abstracting from nuanced differences, most $\#M = 5$ sellers seem to try to mislead buyers about the true value of the good both in M and C. More generally, when sellers disclose exactly k pieces of evidence, they pick the top-k with 82% chance under monopoly and 76% under competition.



(a) $\#M = 1$ in M10 ($N = 39$).

(b) $\#M = 1$ in C10 ($N = 12$).

(c) $\#M = 5$ in M10 ($N = 70$).

(d) $\#M = 5$ in C10 ($N = 72$).

Notes: A 'top-k' strategy would involve disclosing all top-k ranked evidences and none of the other.

Figure 3: Fraction of disclosure for k-ranked evidences.

3.2 Buyer behavior

The buyer observes the disclosed evidences with a mean of $\bar{e}^M$ and—in order to bid approximately the true value v—should on average account for the selection by bidding

	All rounds			Rounds 5&10		
	M10		C10	M10		C10
(a) Markdown $\bar{e}^M - b$	67.30	***	13.78	67.68	***	9.60
$\neq 0$	***		***	***		[0.33]
(b) Discrepancy $b - v$	5.63	***	25.22	8.13	[0.22]	26.43
$\neq 0$	[0.32]		***	[0.45]		***

Notes: t-tests for equal means, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table 3: Buyers' markdown and bidding discrepancy.

this mean minus a "markdown", $\bar{e}^M - b$. For correct inferences of v, the buyer's markdown and the seller's publication bias should be the same. In C, we always refer in the following to the evidences of the seller j^* chosen by the buyer for a transaction.

Panel (a) of table 3 reveals a considerable markdown in M10. We can judge the appropriateness of the markdown by calculating the "discrepancy" of the bid, $b-v$. The more precisely buyers judge the product, the closer the discrepancy is to zero. Panel (b) of table 3 shows some overbidding in M10, but it is small and not significant.

Result 1 (M10) *In line with Hyp. 1, the seller's disclosure strategy exhibits a considerable publication bias and the average buyer's bidding compensates for this bias, albeit incompletely. The discrepancy is, however, not significantly different from zero.*

As expected, panel (a) of table 3 shows that the markdown is lower in competition than in monopoly. Surprisingly, the magnitude of the markdown in competition is very small: in C10 it is only 30% of the publication bias while it is 90% in M10, suggesting that buyers are much less skeptical than in monopoly. Panel (b) of table 3 shows that overbidding in C10 is almost five times stronger than in M10. This difference remains roughly stable until rounds 5&10 and the overbidding in C remains significantly different from zero.[20]

This difference is maintained when conditioning on the observed number of evidences. Panel (c) of table 2 shows the markdown by the number of disclosed evidences and for M10 and C10. Panel (d) reports the publication bias of sellers j^* chosen by the buyers. The markdown is generally smaller in magnitude than the publication bias. In

[20]In the M10 game, the markdown and the discrepancy do not precisely add up to the publication bias due to 6 sellers that do not disclose any evidence and for which publication bias and markdown – in contrast to the discrepancy – cannot be calculated.

	All rounds
$\#M_j$	1.40***
$\bar{e}^M$	49.17***
N	480
	Rounds 5&10
$\#M_j$	2.21***
$\bar{e}^M$	47.77
N	96

Notes: t-tests for means equal to 0, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table 4: Mean characteristics for chosen seller relative to other three sellers (C10).

particular, the minor markdowns even for small $\#M$ are surprising and contribute to the high discrepancy in C10. Reassuringly, table 2 also shows that the buyers' markdown approximates the sellers' publication bias very well in C10 under full disclosure ($\#M = 10$). This suggests that buyers really have a problem with biased information and are not impeded too much from other issues such as e.g. computing mean values.

In addition to the bidding behavior, in C the buyers' choice of seller can discipline the latter. Table 4 reports the difference in disclosure characteristics x_j between the chosen seller j^* and the average of the other 3 sellers, $\frac{1}{3}\sum_{j \neq j^*} x_j$. The chosen seller on average reports 1.40 pieces of evidence more than the average competing seller, which is significantly more than zero. Notably, 2/3 of buyers choose a seller who offers the highest number of evidences, indicating that in our setting choosing well is less difficult than bidding well.[21] Buyers choices, thus, provide a reason why sellers provide more evidences in C10 even though buyers bidding is less skeptical than in M10.

A higher number of disclosed evidences thus indeed increases the seller's probability of being selected. The mean of the disclosed evidence of the chosen seller is relatively higher in C10, reflecting that their nominal magnitude potentially has a positive effect on the buyer's choice of the seller. This effect is, however, not robust across rounds.

3.3 Payoffs

The interaction between seller and buyer can be summarized from the perspective of the payoffs earned. In order to get familiar with possible payoffs, consider the hypothetical benchmark case of the buyer always bidding the true value v. In that case, she would

[21] In Section 5, we will classify buyers as either naïve or skeptical with the help of a mixture model. Notably, while naïve buyers may *bid* worse than skeptical buyers they seem not to *choose* substantially worse. On average their chosen sellers offer 1.38 pieces of evidences more while the chosen sellers of the skeptical buyers offer 1.55 pieces of evidence more.

make no profit if the price realizes below v and on average a profit of 100 points otherwise, leading to an overall expected payoff of 50 per transaction. Accordingly, the seller would make no profit or make a profit of 50 above the cost, respectively, leading to an expected payoff of 25. The asymmetry in this case is deliberately chosen since buyers are not likely to reach this level and—in the face of naïve buyers—sellers are likely to exceed this level.

Panel (a) of table 5 shows that the buyer payoffs do not significantly increase between M10 (40.49) and C10 (42.70). In order to see how the buyer's naïveté influences her payoffs, in panel (b) we simulate payoffs under an improved buyer strategy of bidding the average of the evidences minus the average publication bias for the given number of disclosed evidences (which can be found in table 2). Under this strategy, the buyers' payoffs could be almost 50, implying that the would buyer be better off.[22] Rather than 25, across all treatments, the seller obtains a payoff higher than 25 thanks to the naïveté of the buyer. Notably, seller payoffs do not significantly decrease in C10 compared to M10. The simulation shows that their payoffs in C would be much lower with less naïve buyers.

	(a) Payoffs			(b) Simulated payoffs	
	M10		C10	M10	C10
Buyers (Benchmark: 50)	40.49	[0.65]	42.70	48.11	49.68
Sellers (Benchmark: 25)	58.73	[0.94]	59.26	31.04	27.84
N	480		480	480	480

Notes: t-tests for equal means, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$]. In the simulation, the buyers adjust their bids according to the average publication bias of the sellers conditional on the number of disclosed evidences.

Table 5: Payoffs and simulated payoffs.

Result 2 (C10) *Compared to M10, competition on the seller side increases the number of disclosed evidences—as buyers prefer to choose sellers with a high number of revealed evidences—and reduces the publication bias. Buyers, however, are judging the product less precisely and do not earn a significantly higher payoff. Despite the competition, sellers' payoffs are on a similarly high level as in M10.*

Overall, sellers behave broadly in line with our predictions. In M10, they bias their disclosure to mislead buyers. In C10, in response to buyers choosing sellers revealing

[22] Since the seller was obviously not faced with the simulated strategy and could have adapted to the original strategy rather than this alternative strategy, the payoffs under the simulation might overstate the usefulness of the simulated strategy. At the same time, since the simulated payoffs are below 50, they are all in the range that the buyer can guarantee herself by bidding precisely.

more evidence, sellers disclose more and reduce their bias, surprisingly being able to maintain their high payoff share. In our context-rich product market environment, buyers are—at least on average[23]—reasonably skeptical in M10. Their skepticism, however, erodes under competition, nullifying any potential gains.

3.4 10+5 Evidences (M10+5 and C10+5)

In the reported experiment, games were implemented that enable to investigate whether and how another institutional change influences skepticism: uncertainty about available evidences (see appendix A.1). Because the mechanisms underlying the effects of competition and uncertainty appear to be distinct and motivated running several extension treatments for separate in-depth analyses, the detailed results concerning uncertainty are presented in Koch et al. (2025).

In the so-called 10+5 games, the possibility of purchasing additional evidences implies that sellers can – in addition to the initial set of 10 evidences – purchase 5 more evidences. The additional evidences are independently distributed in the same way as the initial evidences. Sellers make the purchasing decision after they observe their initial set of 10 evidences. Buyers know that sellers have the possibility to purchase these additional 5 evidences but cannot observe a seller's purchasing decision. Apart from a small fee paid for the extra evidences, the payoff structure is the same as above.

While Koch et al. (2025) find that the uncertainty about evidences actually awakens skepticism among buyers, the comparison between the analogue monopolistic (M10+5) and competitive (C10+5) settings confirms and strengthens our competition naïvety result. See tables B.5 and B.6 in appendix B.2.

4 Causes of lack of skepticism in competition

The results so far raise the question as to what causes the lack of skepticism in competition. In the following, we investigate five possible explanations. First, fairness concerns might motivate buyers to reciprocate the usually more generous information provision of the chosen seller by reducing their bid less (Rabin, 1993; Fehr and Gächter, 2000). Second, the active choosing of one seller might give buyers an illusion of control (or invoke motivated beliefs) and make them optimistic about the selection of evidence that they face (Langer, 1975; Burdea et al., 2018). Third, the knowledge of the competitive pressure might lead to a belief that the selection of published evidence not only

[23] In section 5, we will show that a simple mixture-model estimation suggests that a non-negligible fraction of naïve buyers exists even in M10, providing sellers with an incentive to bias their disclosure.

is less biased than in M but also less biased than it actually is. Fourth, a competitive environment may legitimize problematic strategies such as not publishing all available evidences (Bartling et al., 2017) whereas such behavior provokes 'punishment' in a one-to-one setting. Fifth, the additional complexity of facing four instead of only one seller may imply that fewer cognitive resources are available for the sophistication required to counterbalance the publication bias (Drichoutis and Nayga, 2020).

We investigate these explanations with the help of two extension experiments, each consisting of three further games. The **C** extension is based on the competition setting, modifies the C10 game in details, and confronts buyers with data that had been generated by sellers in the previous sessions 1-10. First, in "C10B", buyers face markets of 4 sellers and their disclosed evidences that arose in C10 games of *previous* sessions but their bids only have the usual monetary consequences for them but none for any seller or any other participant. Avoiding consequences for other participants removes any relevance of social preferences and reciprocity. Second, and almost identical to C10B, "C10B-NC" (No Choice) not only confronts buyers with markets and disclosed evidences, but also imposes the buyer's choice of a seller in the original C10 market. By removing the buyer's choice, no illusion of control can arise that could make a buyer deem himself—for the same amount of evidences—better informed facing competition than monopoly. Finally, "C10B-4M" (4 Monopolists) confronts buyers with markets that are made up of four monopolists' products and their disclosed evidences as previously observed in M10. Since originally the behavior of those monopolists was not shaped by competitive pressure there is no ground to believe the evidences to be less selected than in monopoly.

The **M** extension is based on the monopoly setting, modifies the M10 game in details, and again confronts buyers with seller data from previous sessions. First, in "M10B", buyers face one seller of a previous M10 session. Similar to C10B, monetary consequences for sellers or any other participant are avoided, implying that buyers cannot 'punish' sellers for not disclosing all evidences. Effectively, this game serves as an instrument check for our **C** extension games by showing whether the removal of an active seller already leads to less strategic sophistication and skepticism than in M10. Second, in "M10B-CS" (Chosen Sellers) and in "M10B-AS" (Avoided Sellers), buyers are confronted with *one* previous seller and his disclosed evidences but also see additional information displayed, namely the disclosed evidences of three other sellers for which they cannot bid. All **M** extension games use the same seller data as C10B-4M. In M10B-CS, buyers are matched with a 'chosen seller', i.e. a seller previously chosen by a buyer in C10B-4M. In M10B-AS, they are matched with an 'avoided seller', i.e. a seller avoided by buyers in C10B-4M.

In summary, the **C** extension games depart from the competition setting and include monopoly elements to reestablish skepticism. Similarly, the **M** extension games depart from the monopoly setting and include competition elements to extinguish skepticism. Critically, being described from two different starting points, games C10B-NC and M10B-CS are, except for controllable features, identical and allow us—even though we did not design them for that purpose—to test for framing effects.

In both games, participants received initial instructions, spanning 3-4 pages which exposed them to our general setting. The situation is either described as one in which buyers have a choice between four sellers (C10) or not (M10): we put participants either in a competitive or a monopolistic frame of mind. Afterwards, participants read further but shorter instructions, highlighting simple modifications of this general setting valid for the next part of the experiment. These modifications revoke the element of choice in C10B-NC and indicate that further information about three additional sellers will be made available, without allowing subjects to choose among them in M10B-CS. In effect, participants play the same game but have been introduced to it in different ways. In table 6, we indicate the key differences between the instructions of the two games.

4.1 Experimental procedures

For both **C** and the **M** extensions, experimental sessions were conducted in Mannheim (MA) and in the AWI-laboratory of the University of Heidelberg (HD). While the **C** extension was implemented as a normal lab experiment in 2017 (sessions 11-16), the **M** extension was, due to Covid-19, conducted as an online experiment in 2021 (sessions 21-26), using z-Tree unleashed (Duch et al., 2020). To assure comparability, we ran 4 sessions of the M10/C10 games ('M10on', 'C10on', sessions 17-20) to replicate our original results in an online setting. These sessions used evidence data generated in sessions 1, 2, 3, and 9.

All extension sessions feature a sequence of 3 games with each game occurring once in each sequence position following a latin square design, see table 7. Each game features 5 rounds, using data from the first 5 rounds of specific games in sessions 1-10.

As opposed to the eight sellers and eight buyers in sessions 1-10 and 17-20, each session now includes 16 buyers. C10B and C10B-NC uses data from previous C10 sessions. C10B-4M, M10B, M10B-CS, M10B-AS uses seller data from previous M10 sessions. Due to a few no-shows, 93 subjects participated in the **C** extension while 91 subjects participated in the **M** extension. 64 subjects participated in the replication exercise.

<table>
<tr><th colspan="2">Initial Instructions (spanning 3-4 pages)</th></tr>
<tr><th>C10B-NC</th><th>M10B-CS</th></tr>
<tr><td>In THIS EXPERIMENT, <u>FOUR SELLERS</u> AND FOUR BUYERS FORM A MARKET.
[...] After the Sellers' choice, the Buyers will see the reports of <u>all four sellers.</u>
[...] Before the Buyers place their bid, they have to choose from which Seller to buy.
For the chosen Seller's good, the Buyer places a Bid.</td><td>IN THIS EXPERIMENT, <u>ONE SELLER</u> AND ONE BUYER FORM A MARKET.
[...] After the Seller's choice, the Buyer observes only the Evidences that <u>the Seller</u> chose to report.
S/He then places a Bid for the good.</td></tr>
<tr><th colspan="2">Modification Instructions (spanning 1-2 paragraphs)</th></tr>
<tr><th>C10B-NC</th><th>M10B-CS</th></tr>
<tr><td>Notably [...] you will <u>not be able to choose</u> which Seller's product to bid for in this part, but you will be bidding for one Seller out of the four that is given to you by the computer.</td><td>Notably [...] you will receive some additional information: Specifically, you will be provided with information of <u>three additional sellers</u>.
[...] Importantly, while you will be able to see this additional information of three other sellers, you will <u>only be able to bid</u> for the product of <u>your own seller</u> but not the products of the other sellers.</td></tr>
<tr><td colspan="2">Notes: Highlights are specific to this table and do not occur in the original instructions.
CAPS refer to the MARKET STRUCTURE, Italics to buyer's choice and
BOLD fonts to the buyer's information. <u>Underlining</u> indicates differences between games.</td></tr>
</table>

Table 6: Differences in instructions between C10B-NC and M10B-CS

4.2 Results

In order to start the analysis, table 8a (**C** extension) reports benchmark averages from M10 and C10 for rounds 1-5. Similarly, table 8b (**M** extension) reports benchmark averages for rounds 1-5 from our online replication (M10on and C10on). While smaller deviations exist, the online treatments broadly replicate our original sessions.[24] In table 8a, due to the replication of seller data, the numbers in the first two rows of C10 can

[24]Comparing M10/C10 with M10on/C10on in table 8, we are unable to establish any significant difference apart from the fact that the publication bias is slightly higher in M10 than in M10on ($p = 0.019$) and consequently this may also be true for the markdown ($p = 0.114$). More importantly, we observe very similar qualitative differences between competition and monopoly.

(a) **C** extension

Session	Sequence position		
MA, HD	1	2	3
11, 14	C10B	C10B-4M	C10B-NC
12, 15	C10B-NC	C10B	C10B-4M
13, 16	C10B-4M	C10B-NC	C10B

(b) **M** extension

Session	Sequence position		
MA, HD	1	2	3
21, 24	M10B	M10B-CS	M10B-AS
22, 25	M10B-AS	M10B	M10B-CS
23, 26	M10B-CS	M10B-AS	M10B

Table 7: **C** and **M** extension: Games in extension sessions 11-16 and 21-26.

be found again in C10B and C10B-NC. Similarly, the numbers in the first two rows of M10 can be found again in C10B-4M. In table 8b, the seller data in brackets in the last two columns coincide with C10B-4M/M10 (in table 8a), on which these sessions were based.[25] Not in brackets, we actually report the averages for the 'matched' sellers which excludes the data of the three merely displayed sellers.

Strikingly, the markdowns in all C10B games (table 8) are—if anything—less pronounced than in C10, significantly lower than in M10 (for all comparisions $p < 0.01$) and furthermore do not differ from each other (for all $p > 0.25$).[26] This remains true when we break down game results by sequence position as in table A.2a. The similar behavior in C10 and all C10B games suggests that reciprocity is not behind the non-skeptical attitudes in C10. Further, the similar buyer behavior in C10B and C10B-NC suggests that the own choice of a seller has relatively little influence on her skepticism and does not explain the comparatively large skepticism in M10. Finally, the similar behavior in C10B and C10B-4M suggests that the competition does not influence the beliefs about the selection of the evidences or about any unusual strategies (see figure 3).[27]

Similarly strikingly, table 8 reports the markdown in all **M** extension games to be

[25] The small differences that can be noted are due to 9 no-show subjects who prevent a complete replay of the seller data. In addition, we excluded the data from a subject that did not pass the comprehension test and with whom subsequently online communication could not be established.

[26] Qualitatively similar statistical results emerge with respect to buyers' discrepancy, with the exception that the discrepancy is significantly worse ($p < 0.05$) in C10B-4M compared to other treatments.

[27] Note, of course, that the NC and 4M manipulations are implemented on top of C10B. Hence, these games differ in two respects from C10. The M10B game is useful to show that the B manipulation in itself does not explain the lack of skepticism.

(a) **C** extension					
	M10	C10	C10B	C10B-NC	C10B-4M
Nr Published evidences	4.90	5.81	5.77	5.80	4.90
Publication bias	75.09	46.69	47.13	46.55	74.45
Markdown	56.59	4.23	0.91	1.83	0.57
Discrepancy	17.45	35.62	38.61	37.56	55.76
N	240	240	465	465	465

(b) **M** extension					
	M10on	C10on	M10B	M10B-CS	M10B-AS
Nr Published evidences	4.99	5.29	4.90	6.57 (4.92)	2.87 (4.88)
Publication bias	62.01	47.72	74.70	56.26 (74.34)	98.97 (75.44)
Markdown	39.55	4.54	41.88	29.96	82.07
Discrepancy	20.33	44.13	33.80	26.30	21.32
N	155	155	455	455	455

Table 8: Buyers' markdown and bidding discrepancy in **C** and **M** extension.

clearly different from zero and positive (for all $p < 0.01$) as well as different from C10on (for all $p < 0.05$). The similarity of the markdown of M10on and M10B ($p = 0.78$) suggests that running sessions with seller data from previous sessions does not strongly inhibit strategic sophistication and skepticism, ruling out that the **C** extension results are an artifact of eliminating an active seller.[28] Further, skepticism being alive and well in M10B shows that it does not result from a wish to punish an active seller. Increasing the complexity of the monopoly environment by adding additional information in M10B-CS and M10B-AS seems not to have a negative effect on skepticism. Indeed, buyers seem fairly capable to tailor their markdown to the differing publication bias associated with chosen and avoided sellers in the two latter treatments.

The results of these additional games therefore allow us to rule out some explanations for the difference in skepticism. The diverging markdowns between C10B-NC

[28]Similarly, the markdown is not different between M10B-CS and M10on ($p > 0.25$) but between the latter and M10B-AS ($p < 0.01$). Regarding the discrepancy, we usually (apart from M10B-CS) fail to establish significant differences both from M10on and C10on, indicating that the discrepancy of the new treatments is somewhere in-between those former treatments.

and M10B-CS ($p < 0.01$) are surprising and informative. Conceptually these treatments are very similar: both provide buyers with information of 4 sellers and allows them to only bid for one given seller. While C10B-NC uses C10 seller data, M10B-CS uses M10 seller data. But note that C10B and C10B-4M featured the same difference and did not generate differences in buyer skepticism. Ultimately, the remaining difference between these games is that in C10B-NC the setup was introduced to subjects as a competition environment with added monopoly features (i.e., no choice). In M10B-CS the setup was presented as a monopoly environment, adding competition features (i.e., information about three other sellers). We can conclude that a *framing effect* produces the diverging differences in skepticism between the monopoly and competition environment: while players are put in different frames from the outset in C10B-NC and M10B-CS, they ultimately play the fundamentally same game.

Buyers seem to behaviorally adapt to the competition frame, abandon their skepticism and almost 'blindly trust' the competitive market to their detriment. Similar misguided 'behavioral adaptations' have been observed in other domains and this phenomenon is generally referred to as the *Peltzman effect* (following Peltzman, 1975) in the social science. Consumers' reactions to potential safety improvements (such as a bicycle helmet or face masks and vaccines that can reduce the risk of brain injuries or slow the spread of a pandemic, respectively) undermine at least in part the beneficial effects of the latter. Of course, competition has long been claimed to be favorable to consumers and, intuitively, facing four instead of one seller in the competition frame seems to put oneself—as buyer—in a better position. In other words, buyers seem less careful in an environment that they, arguably, perceive as more favorable to them. In our original sessions, this behavioral adaptation undermines any gains from the informational improvement that competition provides.

Notably, our data is not only consistent with buyers' misguided behavioral adaptation to competition on the aggregate level but we also observe a related heterogeneity in individual behavior. We estimate a simple mixture model—the details of which can be found in the online appendix—that allows us to classify our buyer population as either 'skeptical' (i.e., aware of sellers' publication bias) or 'naïve', following our theoretical analysis. Looking at data from our original sessions, the model finds that the proportion of naïve buyers is only 28.9% (30.8%) in M10 (M10+5). In contrast, this proportion is estimated to be 68.1% (88.4%) in C10 (C10+5). We find further evidence in line with a misguided behavioral adaptation of buyers when looking only at sessions in which subjects play both M10 and C10. We observe that no subject is classified as naïve in M10 but skeptical in C10, but 40 (out of 64) subjects are classified as skeptical in M10 and naïve in C10. Finally, for the extensions we find the proportion of naïve buyers to be estimated as only 36.9% in our **M** extension, but 81.1% in our **C** extension.

Result 3 ('Framing, perception and behavioral adaptation') *We rule out a number of potential explanations for the observed lack of skepticism in competition (among them social preferences, an illusion of control and the increased complexity associated with competition). We find that a mere framing effect generates our treatment differences: In line with the so-called Peltzman effect, buyers perceptions seem to change. They behaviorally adapt to the competition frame, i.e., they abandon their skepticism in the competitive environment to their own detriment.*

5 Conclusion

In this study, we establish a rich experimental market framework to study disclosure of verifiable information by interested sellers and inference by buyers. The setup implements the natural conflict of interest inherent in any firm-consumer interaction and enables a direct observation of consumers' inference and skepticism. We study the effect of competition. In our experiment, sellers predominantly bias the information they provide. Even though buyers show some signs of naïvety—as observed in previous studies—they are able to account for the sellers' selection to a reasonable degree in the monopolistic setting. Competitive forces predicted to favor buyers lead to more disclosed evidences but fail to increase the buyers payoffs as they corrode the buyers' skepticism. We investigate the buyers' inability to tailor their skepticism to the competitive environment in two additional extension experiments. We can rule out many plausible confounds such as social preferences, distorted beliefs or an illusion of control. Introducing participants to our setup with a competition framing, in other words, putting them in a competition frame of mind, already suffices to generate the effect. Buyers simply seem to behave less careful in an environment they arguably perceive as safer and more in their favor. This result is hence in line with the *Peltzman effect*: Consumers seem to adapt their behavior to the differing perceived levels of safety between competition and monopoly and precisely this behavioral adaptation erodes the gains from competition for them.

Ultimately, our study therefore shows that a competitive environment, in which buyers' skepticism is still a necessary ingredient, may not be more beneficial than a monopolistic environment. Moreover, our results highlight that consumer benefits may not simply relate to the underlying market structure but also depend on how competitive consumers perceive a market to be and on how they adapt to that. While market structures may be complex and at times difficult to penetrate, consumer perceptions are likely guided by simple cues and, for more hotly debated issues, by the public discourse. Similar to our experiment, it may, for example, be relatively easy for consumers to observe how many providers of a particular good exist. Similarly, they may take the fierceness with which different producers of a good engage in combative advertisement campaigns ('competitive interference' – Allen 1994) as a proxy for a market's level of competitiveness. For topics such as gasoline prices and competition between petrol stations, public debate is likely to influence perceptions about competition. We anticipate consumer perceptions of industry competitiveness deviate from objective measures, though they will likely be correlated to an extent—–a question for future research. That consumer behavior will depend in some part on perceived competitiveness creates an opening for firms to mislead, thus potentially lending support to mandatory disclosure policies, particularly those challenged on the grounds that competition adequately addresses information asymmetry.

Our framework can be fruitfully extended in order to explore additional fundamental structures as well as to further investigate the link between competition and consumer naïvety. Building on the present structure, Koch et al. (2025) make a first step in investigating uncertainty about the number of available evidences. Letting sellers purchase additional evidences varies the information structure and provides an interesting contrast to the manipulation of the market structure. In addition to replicating competition naïvety, they find that buyers are able to tailor their skepticism quite well to the increased uncertainty resulting from the sellers' option to purchase evidences. This shows that buyers' behavioral adaptations might well be useful. On the other hand, Johnson et al. (2019) study the context of refinancing a mortgage, in which skepticism is too strong. Overall, we see that the right calibration of skepticism is necessary and miscalibrations can turn out to be costly for consumers.

Some *Peltzman effects* (e.g. for seat belts) have been shown to fade away over time while others have not. Regarding competition naïvety, one could study what elements of a competitive environment may facilitate an appropriate calibration of consumers' 'safety perceptions' over time. Or, in addition, one could explore whether providing consumers with the correct information about firms' bias in disclosure even under competition will help them with this calibration. Without doubt, further research is necessary to determine whether the message of praise for competition in the public discourse should usefully give way to a more nuanced assessment of competition that makes consumers aware of its intricacies.

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A Appendix: Additional Figures and Tables

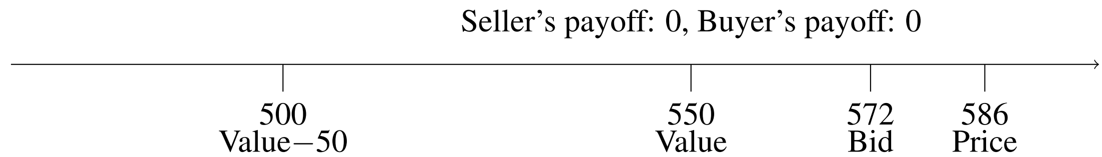
A.1 Experimental Sessions

Session	Game 1	Game 2	Session	Game 1	Game 2
1	M10	C10	6	C10	C10+5
2	C10	M10	7	M10	M10+5
3	C10	M10	8	C10+5	C10
4	M10+5	C10+5	9	M10	C10
5	C10+5	M10+5	10	M10+5	M10

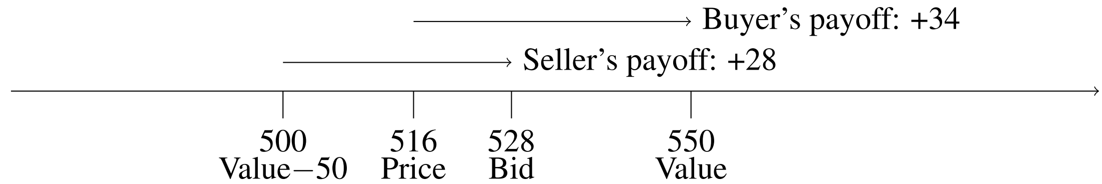
Table A.1: Games in 10 sessions.

Apart from the initial sessions 1 and 2 involving exclusively X10 games, each game is played twice as game 1 and game 2 of the session, respectively, following a latin square design.

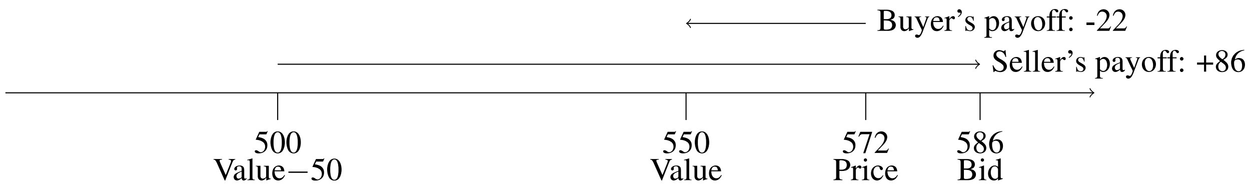
A.2 Examples of Possible Outcomes



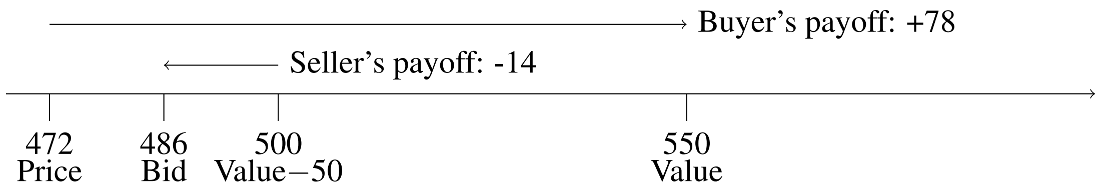
(a) Price>Bid: No transaction. Payoffs=0.



(b) Transaction scenario 1.



(c) Transaction scenario 2.



(d) Transaction scenario 3.

Figure A.1: Transaction scenarios.

A.3 Additional figures and tables

		C10B	C10B-NC	C10B-4M
(a)	Seq. position 1	2.33	1.25	-0.75
	N	155	160	150
	Seq. position 2	2.91	2.05	0.24
	N	160	150	155
	Seq. position 3	3.53	2.22	2.12
	N	150	155	160
(b)	Mannheim	5.65	7.22	3.67
	N	230	230	230
	Heidelberg	-3.73	-3.44	-2.47
	N	235	235	235

Notes: t-tests indicate that none of the markdowns is significantly different from zero, neither over all 5 rounds nor in round 5.

Table A.2: Buyers' markdown and bidding discrepancy over rounds.

		M10-B	M10B-CS	M10B-AS
(a)	Seq. position 1	36.41	31.14	112.36
	N	160	140	150
	Seq. position 2	39.03	22.71	68.98
	N	155	160	135
	Seq. position 3	51.26	36.39	63.61
	N	140	155	160
(b)	Mannheim	35.41	33.58	68.28
	N	215	215	215
	Heidelberg	47.69	26.72	94.45
	N	240	240	240

Notes: t-tests indicate that all of the markdowns are significantly different from zero, both over all 5 rounds or in round 5.

Table A.3: Buyers' markdown and bidding discrepancy over rounds.

A.4 Screenshots

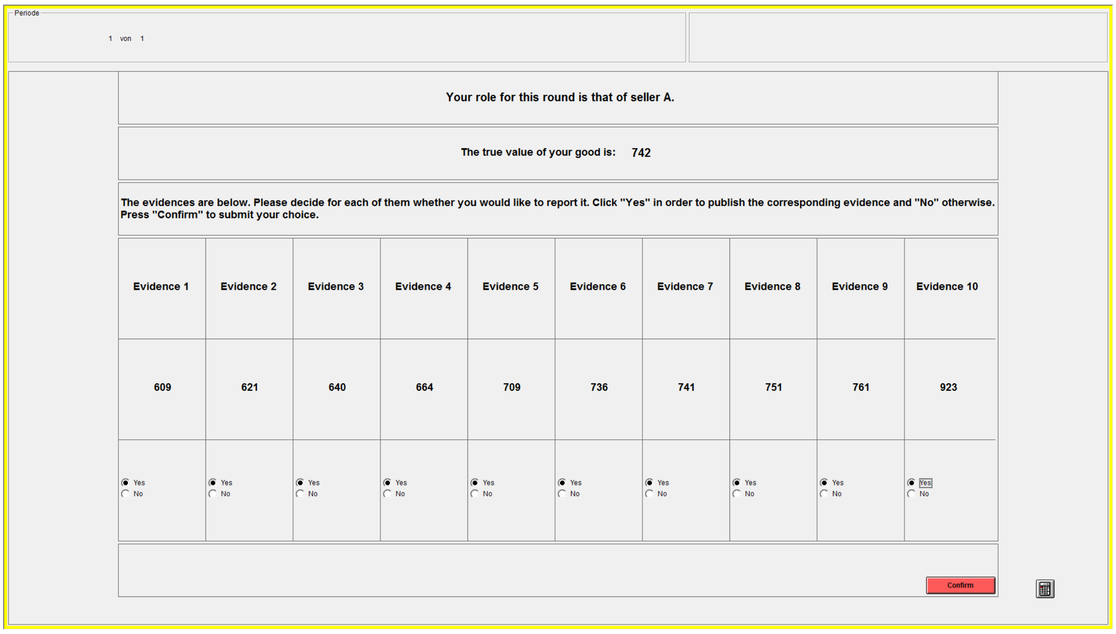


Figure A.2: Sellers' screen in C10.

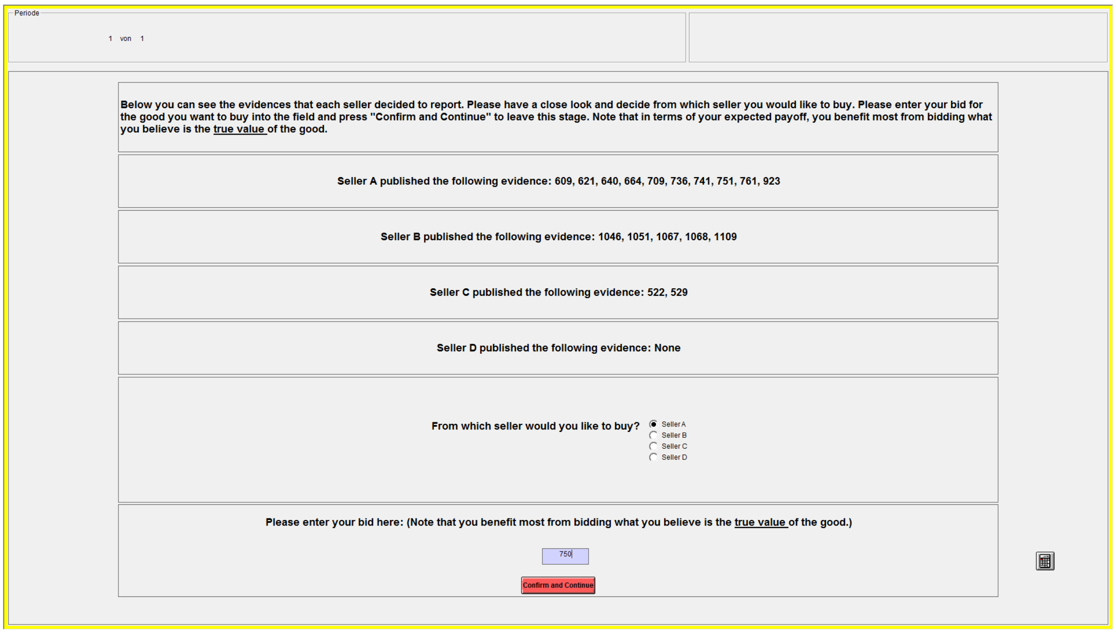


Figure A.3: Buyers' screen in C10.

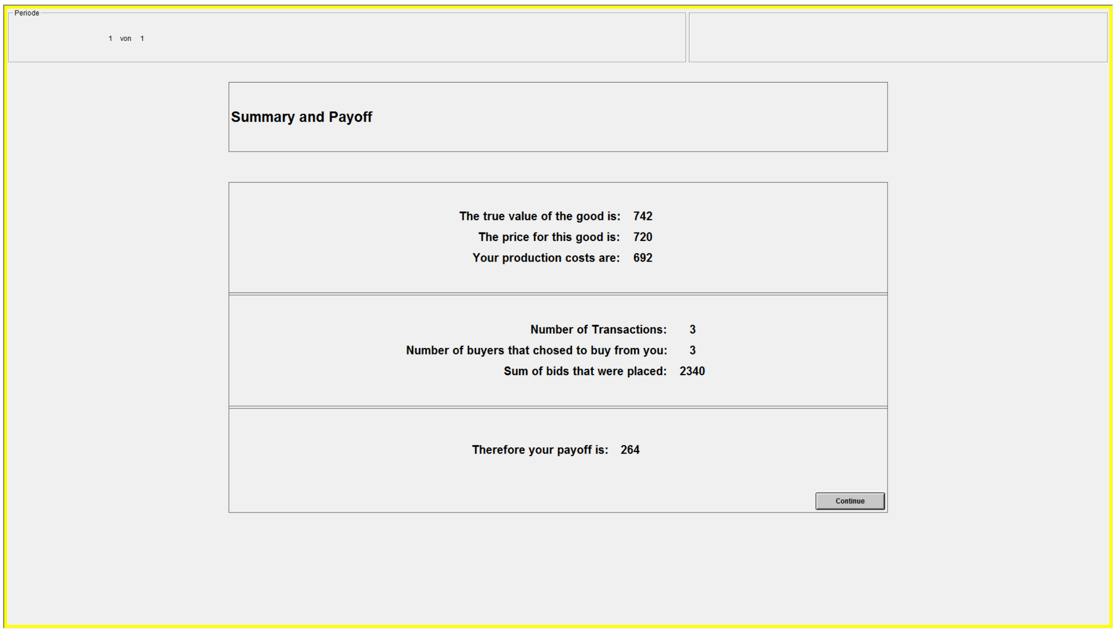


Figure A.4: Sellers' feedback screen in C10.

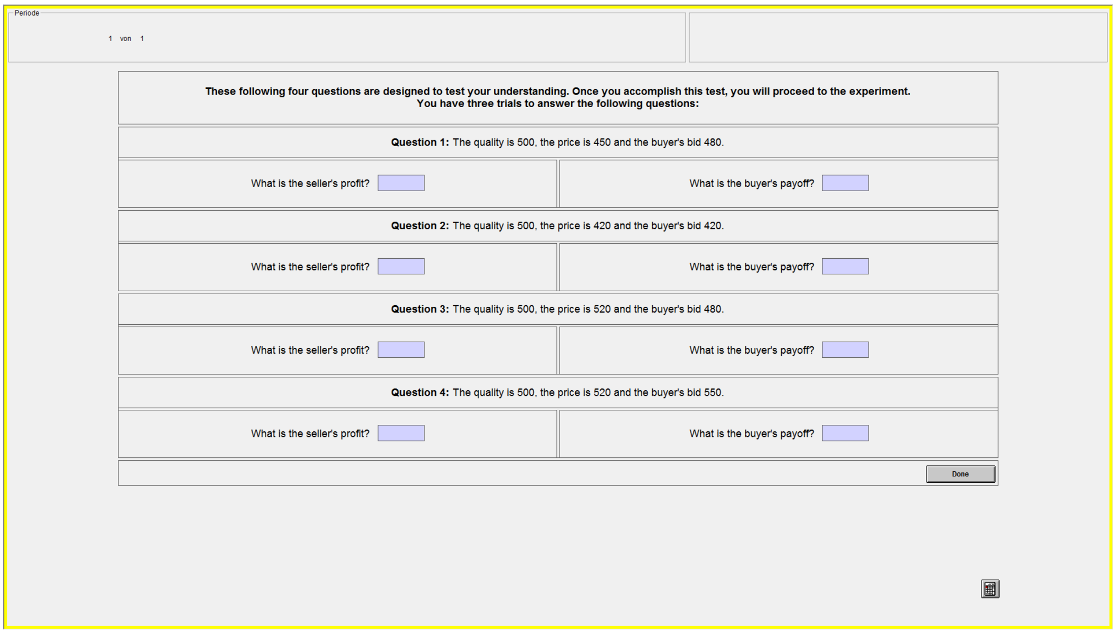


Figure A.5: Screen of the understanding test.

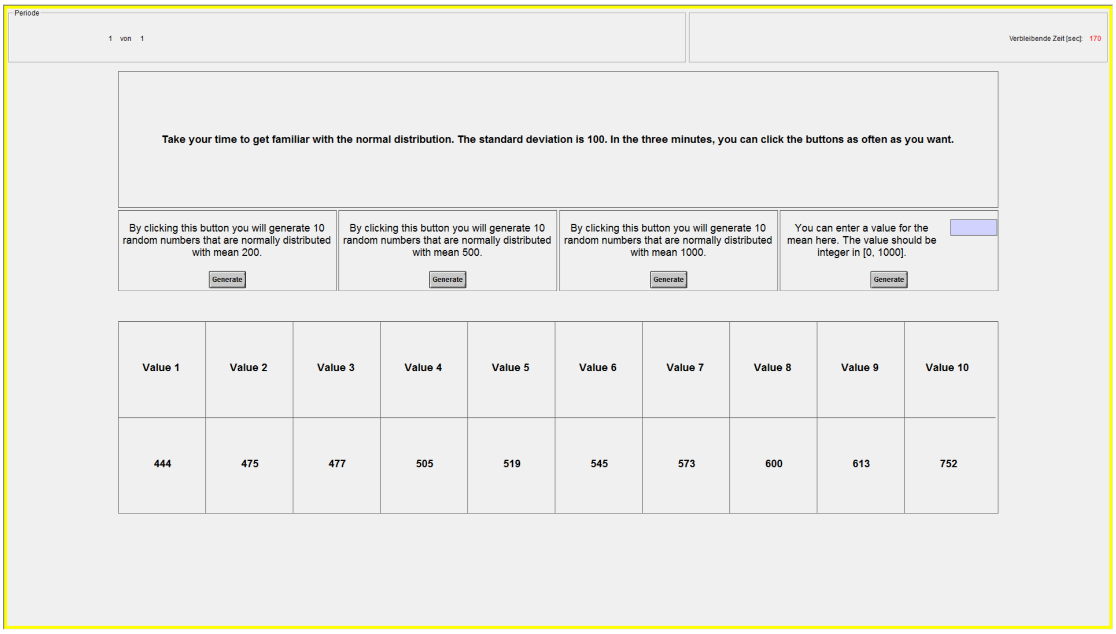


Figure A.6: Tool to generate 10 draws from a normal distribution $N(v, 100)$, $v \in [0, 1000]$. Here, $v = 500$.

B Appendix: Robustness

In this section we provide some robustness analyses of our core results and tables. First, we replicate our core tables with non-parametric tests that only use session level observations. Notably, this provides a very hard test as it reduces the number of observations to 6 observations for M10/C10. Afterwards, we provide a regression analysis that controls for session-level variation by clustering at this level.

B.1 Non-parametric tests

In the main text, our statistical analysis is based on individual level data. For "Rounds 5&10", this already implies that the t-tests are only based on one observation per participant. In this section, we, however, replicate our core tables with non-parametric tests that only make use of session-level data, providing a very conservative test. Table B.1 and B.2 reproduce table 1 and 3, respectively. While the level of significance is sometimes slightly reduced, we generally replicate our previous findings. There is, however, an exceptions. We are unable to establish that there is a significant difference in discrepancy between M and C. Regarding this aspect, we can, however, establish that the discrepancy is significantly different from zero in C but not in M. Hence, even if we test with as few as 4 session-level observations per treatment, we still find (indirect) evidence that the discrepancy in C is actually worse than in M. This conclusion is further supported by the regression analysis in the next section.

Table B.3 reproduces table 4. Similar as in the main text, chosen sellers seem to be characterized by the fact that they report significantly more evidences. The difference in terms of the mean of the disclosed evidences, however, seems not be robustly different from zero (as indicated before). Further, table B.4 reproduces table 5 with largely similar results, namely that we are not able to establish a lot of differences in payoffs.

	All rounds			Rounds 5&10		
	M10		C10	M10		C10
(a) Nr Published evidences $\#M$	4.71	**	6.18	4.65	**	6.25
(b) Publication bias $\bar{e}^M - v$	75.05	**	45.61	82.83	**	42.21
N	480		480	96		96

Notes: Wilcoxon rank-sum and signed rank tests on the session level, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table B.1: Sellers - means of number of disclosed evidence and publication bias

	All rounds			Rounds 5&10		
	M10		C10	M10		C10
(a) Markdown $\bar{e}^M - b$	67.30	***	13.78	67.68	***	9.60
$\neq 0$	**		[0.11]	**		[0.24]
(b) Discrepancy $b - v$	5.63	[0.52]	25.22	8.13	[0.35]	26.43
$\neq 0$	[0.34]		**	[0.46]		[0.11]

Notes: Wilcoxon rank-sum and signed rank tests on the session level, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table B.2: Buyers' markdown and bidding discrepancy

	All rounds
$\#M_j$	1.40**
$\bar{e}^M$	49.17
N	480
	Rounds 5&10
$\#M_j$	2.21**
$\bar{e}^M$	47.77
N	96

Notes: Wilcoxon signed-rank tests on the session level, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table B.3: Mean characteristics for chosen seller relative to other three sellers (C10).

	(a) Payoffs			(b) Simulated payoffs	
	M		C	M	C
Buyers (Benchmark: 50)	40.49	[0.33]	42.70	48.11	49.68
Sellers (Benchmark: 25)	58.73	[0.87]	59.26	31.04	27.84
N	480		480	480	480

Notes: Wilcoxon rank-sum tests on the session level, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$]. In the simulation, the buyers adjust their bids according to the average publication buyers of the sellers conditional on the number of disclosed evidences.

Table B.4: Payoffs and simulated payoffs.

B.2 Competition naïvety in C10+5

		All rounds			Rounds 5&10		
		M		C	M		C
(a)	10	4.71	***	6.18	4.65	***	6.25
Nr Published evidences		***		***	[0.24]		**
$\#M$	10+5	5.31	***	6.91	5.16	***	7.38
(b)	10	75.05	***	45.61	82.83	***	42.21
Publication bias		[0.94]		[0.49]	[0.59]		[0.49]
$\bar{e}^M - v$	10+5	75.40	***	43.15	77.35	***	47.24
N	10	480		480	96		96
	10+5	320		320	64		64

Notes: t-tests for equal means, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table B.5: Means of number of disclosed evidences and publication bias.

		All rounds			Rounds 5&10		
		M		C	M		C
(a)	10	67.30	***	13.78	67.68	***	9.60
Markdown		[0.46]		[0.42]	[0.77]		[0.63]
$\bar{e}^M - b$	10+5	73.23	***	19.28	72.67	***	16.44
$\neq 0$	10	***		***	***		[0.33]
	10+5	***		***	***		*
(b)	10	5.63	***	25.22	8.13	[0.22]	26.43
Discrepancy		[0.63]		[0.41]	[0.71]		[0.90]
$b - v$	10+5	1.63	**	19.14	1.30	[0.15]	28.31
$\neq 0$	10	[0.32]		***	[0.45]		***
	10+5	[0.79]		***	[0.93]		**

Notes: t-tests for equal means, significance level indicated by: *** $p < 0.01$, ** $p < 0.05$, * $p \leq 0.1$, [$p > 0.1$].

Table B.6: Buyers' markdown and bidding discrepancy.

B.3 Regression analysis

In table B.7 and B.8, we present fixed-effects panel regressions with standard errors clustered at the session level, taking into account variation at the session level.[29] These regression summarize how the behavior of sellers (table B.7) and buyers (table B.8) depends on game characteristics.

On the seller side, competition has a significantly positive effect on the number of disclosed evidences (1)/(2)/(3) and a significantly negative effect on the publication bias (4)/(5)/(6) in table B.7. On the buyer side, competition reduces the markdown (1)/(2)/(3) and increases/worsens the discrepancy (4)/(5), confirming previous results. The decrease in markdown due to competition in (1) of table B.8 is significantly larger than the decrease of the publication bias in (4)/(5)/(6) of B.7.

Since our original implementation included sessions featuring both M/C10 and M/C 10+5 games, we use the full dataset in our analysis while controlling for the option to purchase additional evidence. Importantly, an analysis restricted to sessions without this option yields qualitatively identical results regarding competition and the other variables. The effect of the option to purchase additional evidences is positive but not significant on $\#M$ in (1) of table B.7. Regressions (2)/(3) and (5)/(6) control for the purchase of additional evidences, which has a significantly positive effect on both $\#M$ and the publication bias. Since this strongly drives disclosure behavior, the effect of X10+5 on $\#M$ turn negative, even though not significantly so.

The effect of subjects' role-switching ($Rounds\ 6-10$) is negligible for sellers but some improvement in terms of markdown and discrepancy is seen for the buyers in rounds 6-10 in (2)/(5) of table B.8. Specifications (3)/(6) of both tables evaluate whether this learning within a game differs between different games. We only find a significant effect in (3) of table B.7, indicating that sellers seem to disclose more evidences over time in competition, at least compared to the monopoly case. In addition, specification (6) of table B.8 indicates that the improvement in discrepancy due to learning seems not take place in competition.[30] Comparing the regression coefficients in (5) and (6) of that table shows that the negative impact of competition on discrepancy materialize in part due to a lack of learning in this environment (however, $Competition + Round\ 6-10 \times Comp.$ is significant $p = 0.001$). Notably, the order of the two games seems not to play a large role for buyers but has a moderate effect for sellers. While the markdown (discrepancy) is somewhat higher (lower) for the second game, these effects are not significant. Sellers offer significantly more evidences in the second game, presumably

[29] Regressions that cluster at the individual level lead to very similar results.

[30] While the interaction coefficient is not statistically different from zero, the joint estimate ($Rounds\ 6-10 + Round\ 6-10 \times Comp$) is statistically indistinguishable from zero.

due to learning across games. The publication bias is not significantly different between first and second game. Finally, we do not see a lot of evidence that subjects learn over rounds.[31]

Further, the characteristics of the set of 10 evidences E are relevant. A higher mean of the evidences relative to the value increases significantly the number of reported evidences. The fact that this higher mean also leads to an increased publication bias reflects the selection into more disclosure when evidences happen to realize relatively high. A high spread in terms of the standard deviation of the evidences E reduces the number of disclosed evidences but increases the publication bias due to disclosed high outliers. As expected, the number of disclosed evidences reduces the markdown significantly (as more disclosure leads to less publication bias) but has no effect on the discrepancy.[32]

Regressions (2)/(3)/(5)/(6) of table B.7 further show that a higher value v increases the number of disclosed evidences significantly and reduces the publication bias. Regression (2)/(3)/(5)/(6) of table B.8 show that a higher value leads to a higher markdown and a better discrepancy, consistent with the findings for the sellers. These observations are consistent with the idea that higher numbers might be perceived as more attractive (implying that lower numbers are hidden more). Along those lines, buyers implicitly tend to prefer/choose sellers with an above average value (600) in C10. Among all sellers, the chance to have a value above 600 is 50% while it is 57% for chosen sellers. Importantly, the observed pattern biases against observing a difference between competition and monopoly as it improves the discrepancy in competition relative to the monopoly case.

Taken our non-parametric tests and the regression analysis together, let us conclude that our main findings are robust. Competition affects both seller and buyer behavior and, in both analyses, there is at least some evidence that buyers become less skeptical in a competition environment. While we cannot confirm that more evidences are disclosed in the X+5 treatments, we, at least, find clear effects along those lines for those who purchase evidences in these treatments.

[31]More precisely, the $Round$ dummy captures whether subjects learn over the 5 rounds they play a particular game in a particular role. As indicated above, switching roles after round 5 and moving to the second game after round 10 is captured by separate dummies.

[32]Neither gender nor an economics-related field of study have a significant effect on any of these dependent variables.

	$\#M$ (1)	$\#M$ (2)	$\#M$ (3)	Pub. bias (4)	Pub. bias (5)	Pub. bias (6)
Competition	0.98***	0.97***	0.63***	-24.47***	-22.87***	-20.21***
dummy	(0.30)	(0.24)	(0.24)	(4.11)	(5.02)	(4.31)
X10+5	0.31	-0.32	-0.25	4.36	2.63	2.44
dummy	(0.37)	(0.28)	(0.29)	(6.16)	(4.98)	(6.54)
Value v		0.00***	0.00***		-0.01***	-0.01***
		(0.00)	(0.00)		(0.00)	(0.00)
Mean $E - v$		0.01***	0.01***		0.68***	0.68***
		(0.00)	(0.00)		(0.05)	(0.05)
SE E		-0.01*	-0.01*		0.59***	0.59***
		(0.00)	(0.00)		(0.06)	(0.06)
Purchase +5		2.01***	1.97***		14.00***	14.25***
dummy		(0.28)	(0.28)		(4.47)	(4.64)
Rounds 6-10		0.01	-0.31		-0.63	2.15
dummy		(0.18)	(0.28)		(2.66)	(5.25)
Round 6-10 $\times$ Comp.			0.67**			-5.27
			(0.26)			(4.48)
Round 6-10 $\times$ X10+5			-0.12			0.28
			(0.25)			(6.31)
Game 2		0.41**	0.41**		-5.24	-5.23
		(0.19)	(0.19)		(3.76)	(3.76)
Round		0.02	0.02		1.46*	1.46*
		(0.05)	(0.05)		(0.82)	(0.82)
Constant	5.10***	4.97***	5.14***	70.33***	13.60***	12.14**
	(0.32)	(0.33)	(0.36)	(4.83)	(5.01)	(5.77)
N	1600	1600	1600	1592	1592	1592
Subjects	160	160	160	160	160	160
R^2 overall	0.09	0.20	0.20	0.07	0.27	0.27

Notes: Panel fixed-effects regressions. Standard errors clustered at the session level are provided in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level. In 8 instances, sellers choose $\#M = 0$ and do not feature in (4)-(6).

Table B.7: Determinants of seller behavior

	Markdown (1)	Markdown (2)	Markdown (3)	Discrepancy (4)	Discrepancy (5)	Discrepancy (6)
Competition	-42.12***	-27.27***	-22.98***	13.83**	17.19***	10.01
	(8.09)	(6.27)	(8.36)	(6.13)	(5.08)	(7.70)
X10+5	2.45	8.49	0.64	3.18	1.34	12.41
	(13.56)	(13.14)	(12.89)	(13.84)	(13.79)	(13.13)
Value v		0.09***	0.09***		-0.10***	-0.10***
		(0.02)	(0.02)		(0.01)	(0.01)
$\#M$		-8.32***	-8.28***		0.02	-0.07
		(2.19)	(2.18)		(1.48)	(1.46)
Rounds 6-10		12.71***	10.80		-20.56***	-19.18*
		(4.44)	(9.59)		(5.38)	(10.57)
Rounds 6-10 $\times$ Comp.			-8.77			14.73
			(10.64)			(11.21)
Rounds 6-10 $\times$ X10+5			15.65			-21.98
			(11.38)			(13.52)
Game 2		11.76	11.73		-12.66	-12.63
		(8.43)	(8.43)		(8.48)	(8.47)
Round		-0.35	-0.36		2.30	2.30
		(1.43)	(1.43)		(1.74)	(1.75)
Constant	62.96***	42.25*	42.98*	5.22	71.94***	71.71***
	(10.86)	(24.06)	(24.58)	(10.35)	(19.57)	(19.41)
N	1592	1592	1592	1600	1600	1600
Subjects	160	160	160	160	160	160
R^2 overall	0.06	0.13	0.13	0.01	0.06	0.06

Notes: Panel fixed-effects regressions. Standard errors clustered at the session level are provided in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level. In 8 instances, sellers choose $\#M = 0$ and do not feature in (1)-(3).

Table B.8: Determinants of buyer behavior

C Appendix: Mixture Model

A mixture model is a probabilistic model that takes into account the presence of different types within a population (Bardsley and Moffatt, 2007; Cappelen et al., 2007; Moffatt, 2016). Based on a player's behavior, the model identifies which type she most likely belongs to. At the same time, the mixture model cannot identify the underlying types by itself but relies on externally given types. Theoretical considerations as outlined in Section 3 guided the selection of types in our model. Notably, by estimating our mixture model, we try to further illuminate the lack of skepticism in our competition treatments compared to the monopoly sessions. As the sellers' behavior is roughly in line with our predictions (i.e. sellers offer more evidences in competition and reduce their publication bias) but the buyers' actions are not (i.e., they become less skeptical), we focus on the buyer side.

In the main text, we report results for a simple specification of our mixture model that closely follows our theoretical considerations. As indicated below, we have run more complex models that deviate somewhat from our theoretical outline but still lead to fundamentally similar results. In our preferred and simple specification, buyers are either naïve or skeptical. A naïve buyer's bid is described by a random draw from $N(\mu_n, \sigma)$ where μ_n is the mean of the disclosed evidences: $\mathbb{E}(v|M) = \bar{e}^M$. In other words, naïve buyers maximize their utility Π_B unaware that sellers bias their evidences and, thus, bid according to these available evidences. A skeptical buyer's bid is described by a random draw from $N(\mu_s, \sigma)$ where μ_s is the skeptical bid that features a skepticism-induced markdown $\mathbb{E}(v|M) - \mathbb{E}(v|M, \sigma)$. In other words, skeptical buyers maximize their utility Π_B and are aware that sellers potentially bias their evidences. In our main specification, we simply assume that the skeptical buyer manages to bid the true value correctly. Alternatively assuming that the skeptical buyer's bid features the average empirical markdown as outlined in Table 2, however, leads to very similar results.[33]

Given these behavioral assumptions, for each observed bid b_i, we can compute the likelihood of this choice being made by each of the two types. The contribution of this observation to the likelihood function to be optimized is then given by the likelihood for each type weighted by its frequency in the population (denoted by P_n, P_s). The objective function of the maximum likelihood estimation is therefore given by

[33]We also assume that the behavior of the naïve and skeptical buyers feature the same standard deviation. This effectively implies that substantial underbidding will be classified as skeptical and substantial overbidding will be classified as naïve behavior, which is broadly in line with our theoretical considerations. But even when assuming differing standard deviations, we would still observe a substantial difference in naïve and skeptical behavior between monopoly and competition.

$$LL(x, \sigma, P_n) = P_n \cdot LL_n(x, \sigma, \mu_n) + (1 - P_n) \cdot LL_s(x, \sigma, \mu_s). \tag{C.1}$$

As indicated in the main text, we estimate the frequency of types for different treatments, utilizing the fact that players make five (buyer) choices for each treatment. In particular for M10 and C10, we estimate the proportion of naïve buyers to be 28.9% in M10 (with a standard deviation of choices of 98) but 68.1% in C10 (with a standard deviation of choices of 83). Figure C.1 reports the posterior probabilities of being skeptical against subjects' discrepancy both for monopoly and competition. In our simple model, choices will be classified as naïve with the remaining probability. Naturally, the estimation procedure not only depends on how precise a bid is but also how far away it is from the naïve choice (which cannot be seen in Figure C.1). A lot of bids cannot be attributed to naïvety or skepticism with certainty. This seems plausible as the skeptical choice can be very close to the naïve one in many cases, in particular when a lot of evidences are made available. Of course, this happens especially frequently in C10, where many bids are made under full disclosure. We see, however, a very clear distinction between competition and monopoly. While in the latter case many choices have a high probability of being skeptical, most choices have a lower probability in the monopoly case.

Notably, we have run other specifications that lead to qualitatively very similar results. As our extension treatments already indicate that social preferences do not play a major role and previous work (Jin et al., 2021b,a) does not find substantial effects related to both risk and social preferences, we have focused on introducing decision noise.[34] In one specification, we have added a third type that makes random decisions (to better capture 'outlier' bidding behavior). In another, naïve and skeptical buyers sometimes tremble, i.e., make a random decision with a certain probability. While a likelihood ratio test indicates that the simple two-type model we present in the main text is nested in our three-type specification (which is in addition preferable to a two-type model with tremble), we still report the simpler model in the main text as the qualitative conclusions remain the same. With naïve, skeptical and random buyers, we estimate 61% of the population to be naïve, 30% to be skeptical and 9% to play randomly in C10. Similar to the two-type model, only 31% of decisions are naïve while 57% are skeptical (and 12% are random) in M10.[35]

In the main text, we also look at those subjects that play both M10 and C10. We

[34] Of course, even if e.g. risk attitudes generally play a role they should affect levels but not necessarily the difference between monopoly and competition.

[35] With only two types – naïve and skeptical buyer that tremble – we estimate the proportion of naïve buyers to be 34% in M10 but 65.2% in C10.

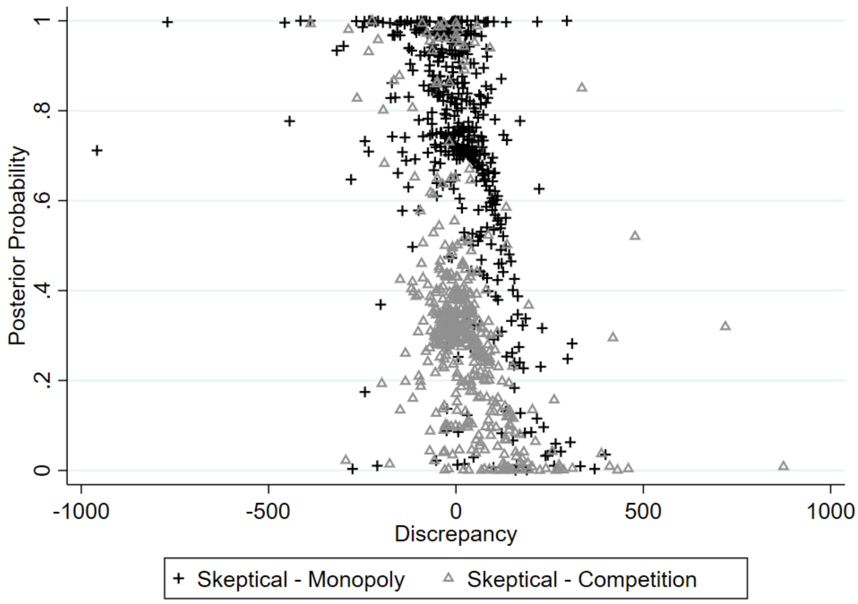


Figure C.1: Discrepancy of subjects' choices and likelihood of being skeptical - Monopoly vs. Competition

classify a subject as naïve (skeptical) in M10/C10 if their five choices in M10/C10 have an average likelihood of coming from the naïve type of more (less) than 50%. In line with the idea that buyers behavioral adaption to competition is misguided, we find that many subjects that are skeptical in M10 become naïve in C10. No subject, however, that is skeptical in C10 becomes naïve in M10.

Finally, to illustrate that the shifts in strategic sophistication (i.e. the change in the percentages of naïve/skeptical buyers) observed in this section matter, assume that we would have the same proportion of skeptical buyers in C10 as in M10. Instead of the observed discrepancy of 25.22, this would imply a discrepancy of only 10.92 in C10, much closer to zero and M10's average discrepancy of 5.63.

D Appendix: Instructions

D.1 Instructions for C10 - M10

You are about to participate in an experiment in a market setting. You may earn a considerable amount of money. The amount you earn will depend on your decisions and the decisions of others, so please follow the instructions carefully. All that you earn is yours to keep, and will be paid to you in private, in cash, at the end of today's session.

During the experiment your payoffs are denominated in points. Your point earnings will be converted to cash at the end of today's session at an exchange rate of 60 points = 1 Euro. No matter what your payoffs are in the experiment, you will be paid at least 2 Euro.

It is important to us that you remain silent and do not look at other people's screens. If you have any questions or need assistance of any kind, please raise your hand, and an experimenter will come to you. If you talk, exclaim out loud, etc., you will be kindly asked to leave.

The experiment consists of two parts (**Part I**, **Part II**) which are independent of each other. Each of these parts, in turn, consists of up to 12 rounds.

Part I

In this part of the experiment, four Sellers and four Buyers form a market. The Seller knows the Value (in points) of his/her good but cannot report it to the Buyers. The Seller has requested 10 external test institutes to officially evaluate the Value of the good and can indeed report these 10 official "Evidences" about the Value to the Buyers. The external Evidences are informative about the good's Value but noisy. In particular, they follow a normal (Gaussian) distribution around the Value with a constant standard deviation of 100. The standard deviation measures the dispersion of the Evidences, how far away they are from the Value. Figure E.1 shows that Evidences are more likely to be close to the Value than far away. You will be able to get familiar with this distribution later.

At the beginning of a round, each Seller is informed about the true Value of her/his good. The true Value lies between 200 and 1000 points, each Value level in this interval is equally likely. As we said, the Seller cannot report the true Value to the Buyers, but she/he can choose for each of the 10 Evidences whether to report it to the Buyers or not. The Seller cannot change or manipulate the Evidences in any way.

After the Sellers' choice, the Buyers will see the reports of all 4 Sellers. Sellers as well will be informed about the reports of the other Sellers. Before the Buyers place

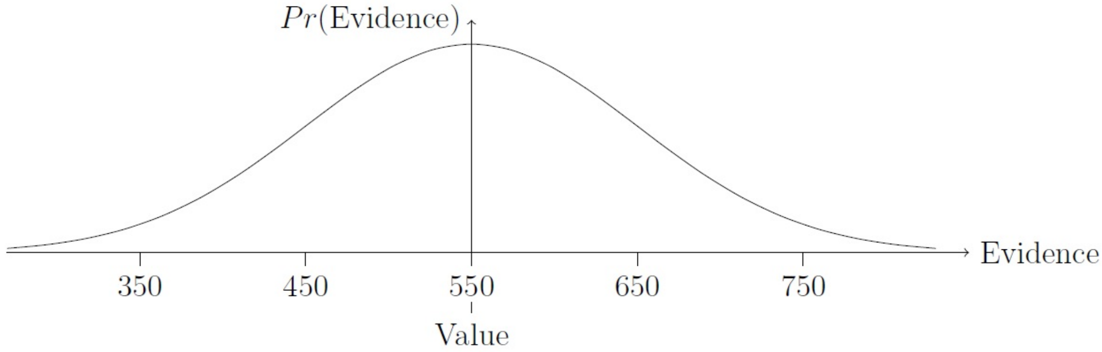


Figure E.1: Normal probability distribution with a Value of 550 and a standard deviation of 100, indicating the probability of Evidences.

their bid, they have to chose from which Seller to buy. For the chosen Seller's good, the Buyer places a Bid. The Bid has to be an integer value between 0 and 1200.

When does the transaction take place? The computer generates randomly a Price of the Seller's good. Neither Seller nor Buyer will be informed about the Price when they take their decisions. The Price takes integer values between the Value minus 200 and the Value plus 200, with each Price level being equally likely (figure E.2). The transaction takes place whenever the Buyer's Bid is greater than or equal to the Price.

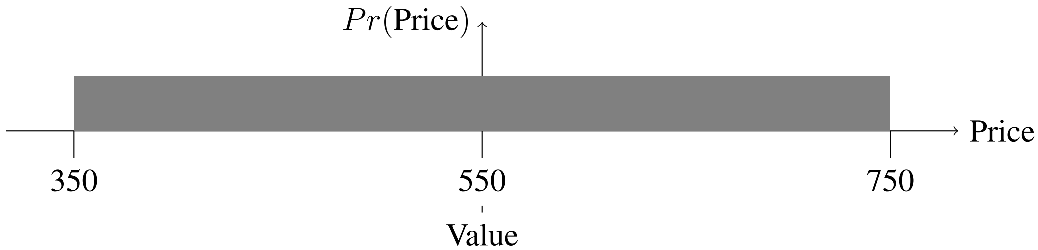


Figure E.2: Price distribution around the Value of 550.

So, in our example, a Bid of

1. 349 will never lead to a transaction, since the Price is certainly above.
2. 750 will always lead to a transaction since the Price is certainly below or the same.
3. 550 corresponding to the Value will lead to a transaction with 50% chance.

How are the Seller's and the Buyer's payoff determined? First, if no transaction takes place, both Seller and Buyer get a payoff of 0 points. If a transaction takes place, as we said depending on the Buyer's Bid and the random Price, the Seller produces the good at the cost of the Value minus 50. The Seller sells one item of their good to each Buyer whose Bid exceeds the Price. A Seller might sell to none, one, two, three, or four Buyers. The Seller's payoff per transaction at the end of the round will be the Bid placed by the Buyer minus the cost:

$$\textbf{Payoff}_{\textbf{Seller}} = \textbf{Bid}_{\textbf{Buyer}} - \textbf{(Value} - \textbf{50).}$$

The Buyer's payoff is the true Value of the good minus the Price:

$$\textbf{Payoff}_{\textbf{Buyer}} = \textbf{Value} - \textbf{Price.}$$

Notice that a Bid equal to the Value will ensure the Buyer to never make losses. If the Price was higher than the Bid=Value, the transaction would not take place. Recall from 3. that under a Bid=Value, the transaction does not take place half the times. Further, note that in terms of expected payoff, the Buyer benefits most from bidding what he believes the Value of the item is. The bids are limited to be between 0 and 1200. Figures E.3a to E.3d present various scenarios.

Part I of this experiment consists of 2 practice rounds and 2 blocks of 5 experiment rounds. At the beginning of each round, you will be informed about the randomly chosen role (Seller or Buyer) that you take. You keep this role in the first block of 5 rounds, and take on the other role in the second block. You will keep the same role within a block, but you will face randomly chosen market counterparts. Throughout, four Sellers and four Buyers will form a market.

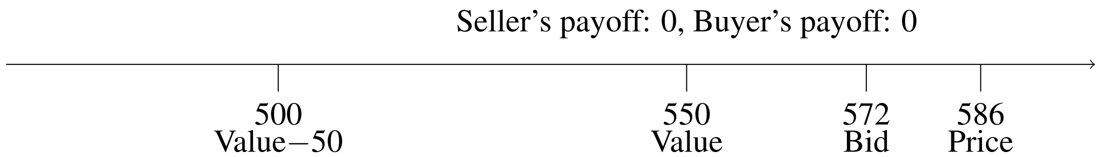
In order to participate in the experiment, you will go through a brief understanding test. Here and throughout the experiment, you can access a calculator via a button in the right bottom corner of your screen. Once everybody accomplishes this test, you can get more familiar with the normal distribution with standard deviation of 100. For that purpose, you will have three minutes to simulate as often as you want the process of generating 10 Evidences for different Values. The experiment will start with the 2 practice rounds that are not paid. Finally, you proceed to the paid rounds.

Are there any questions? If not, please turn to your screens and follow carefully the instructions there.

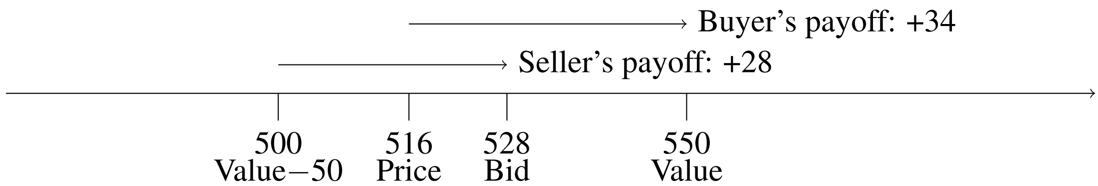
Part II

You are about to start Part II of the experiment, which consists of no practice rounds and 2 blocks of 5 experiment rounds. Like before, in each block of 5 rounds you will take the same role, and you will face randomly chosen market counterparts.

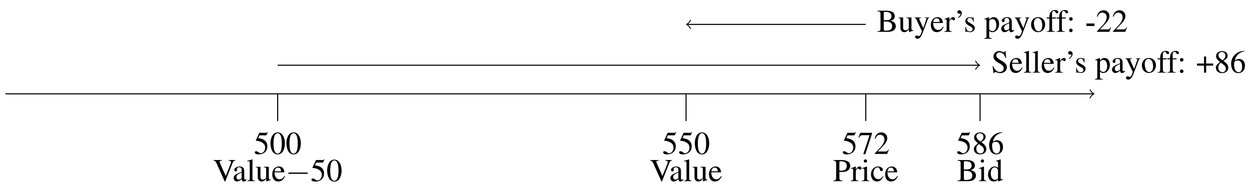
In this part, one market will consist of one Seller and one Buyer. Just like before, the Seller first chooses the Evidences s/he wants to report. The Buyer will observe only the Evidences that the Seller chose to report. S/He then places a bid for the good. The Buyer's and the Seller's payoffs are determined in the same fashion as before.



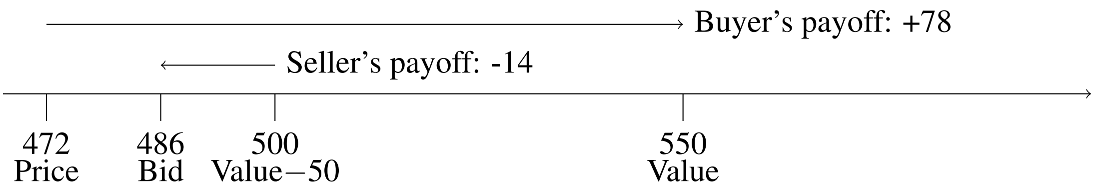
(a) Price>Bid: No transaction. Payoffs=0.



(b) Transaction scenario 1.



(c) Transaction scenario 2.



(d) Transaction scenario 3.

Figure E.3: Transaction scenarios.

D.2 Instructions for M10+5 - C10+5

You are about to participate in an experiment in the economics of decision making in a market setting. You may earn a considerable amount of money. The amount you earn will depend on your decisions and the decisions of others, so please follow the instructions carefully. All that you earn is yours to keep, and will be paid to you in private, in cash, at the end of today's session.

During the experiment your payoffs are denominated in points. Your point earnings will be converted to cash at the end of today's session at an exchange rate of 60 points = 1 Euro. No matter what your payoffs are in the experiment, you will be paid at least 2 Euro.

It is important to us that you remain silent and do not look at other people's screens. If you have any questions or need assistance of any kind, please raise your hand, and an experimenter will come to you. If you talk, exclaim out loud, etc., you will be kindly asked to leave.

The experiment consists of two parts (**Part I**, **Part II**) which are independent of each other. Each of these parts, in turn, consists of up to 12 rounds.

Part I

In this part of the experiment, one Seller and one Buyer form a market. The Seller knows the Value (in points) of his/her good but cannot report it to the Buyer. The Seller has requested 10 external test institutes to officially evaluate the Value of the good and can indeed report these 10 official "Evidences" about the Value to the buyer. Additionally, the Seller has the possibility to ask 5 more test institutes to evaluate the Value of his/her good at a package price of 15 points. These 5 additional Evidences can also be reported to the Buyer. The external Evidences are informative about the good's Value but noisy. In particular, they follow a normal (Gaussian) distribution around the Value with a constant standard deviation of 100. The standard deviation measures the dispersion of the Evidences, how far away they are from the Value. Figure E.4 shows that Evidences are more likely to be close to the Value than far away. You will be able to get familiar with this distribution later.

At the beginning of a round, the Seller is informed about the true Value of her/his good. The true Value lies between 200 and 1000 points, each Value level in this interval is equally likely. After observing the initial 10 Evidences, the Seller has the opportunity to get 5 additional Evidences for a price of 15 points. As we said, the Seller cannot report the true Value to the Buyer, but s/he can choose for each of the 10 (or 15) Evidences whether to report it to the Buyer or not. The Seller cannot change or manipulate

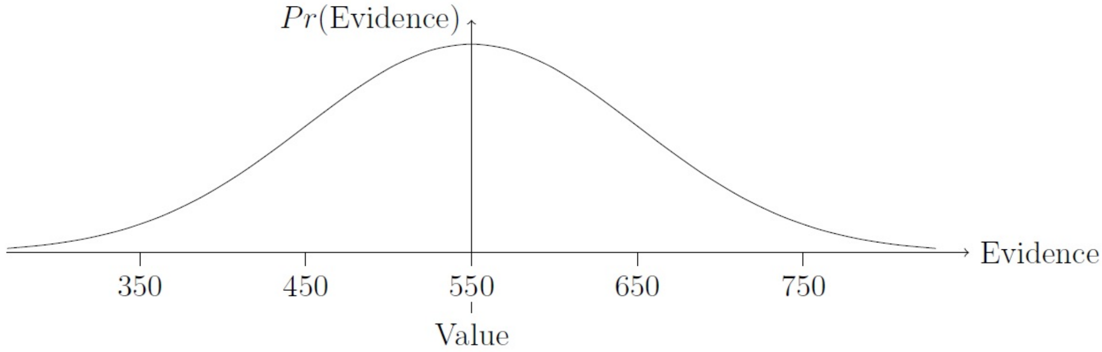


Figure E.4: Normal probability distribution with a Value of 550 and a standard deviation of 100, indicating the probability of Evidences.

the Evidences in any way.

After the Seller's choice, the Buyer observes only the Evidences that the Seller chose to report. S/He then places a Bid for the good. The Bid has to be an integer value between 0 and 1200.

When does the transaction take place? The computer generates randomly a Price of the Seller's good. Neither Seller nor Buyer will be informed about the Price when they take their decisions. The Price takes integer values between the Value minus 200 and the Value plus 200, with each Price level being equally likely (figure E.5). The transaction takes place whenever the Buyer's Bid is greater than or equal to the Price.

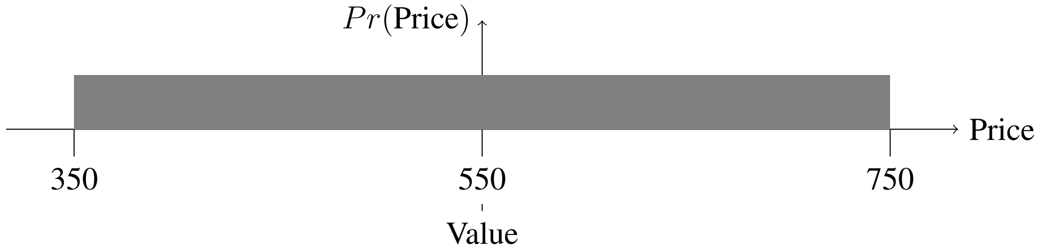


Figure E.5: Price distribution around the Value of 550.

So, in our example, a Bid of

1. 349 will never lead to a transaction, since the Price is certainly above.
2. 750 will always lead to a transaction since the Price is certainly below or the same.
3. 550 corresponding to the Value will lead to a transaction with 50% chance.

How are the Seller's and the Buyer's payoff determined? First, if no transaction takes place, the Buyer gets a payoff of 0 points. In case the Seller didn't purchase additional Evidences her/his payoff is 0 points as well. Otherwise her/his payoff is -15 points. If the transaction takes place, as we said depending on the Buyer's Bid and the random Price, the Seller produces the good at the cost of the Value minus 50. The Seller's payoff at the end of the round will be the Bid placed by the Buyer minus the cost (production cost and possibly cost from purchasing 5 additional Evidences):

$$\textbf{Payoff}_{\textbf{Seller}} = \begin{cases} \textbf{Bid}_{\textbf{Buyer}} - \textbf{(Value} - \textbf{50)} & \text{without purchase of additional Evidences} \\ \textbf{Bid}_{\textbf{Buyer}} - \textbf{(Value} - \textbf{50)} - \textbf{15} & \text{with purchase of additional Evidences} \end{cases}$$

The Buyer's payoff is the true Value of the good minus the Price:

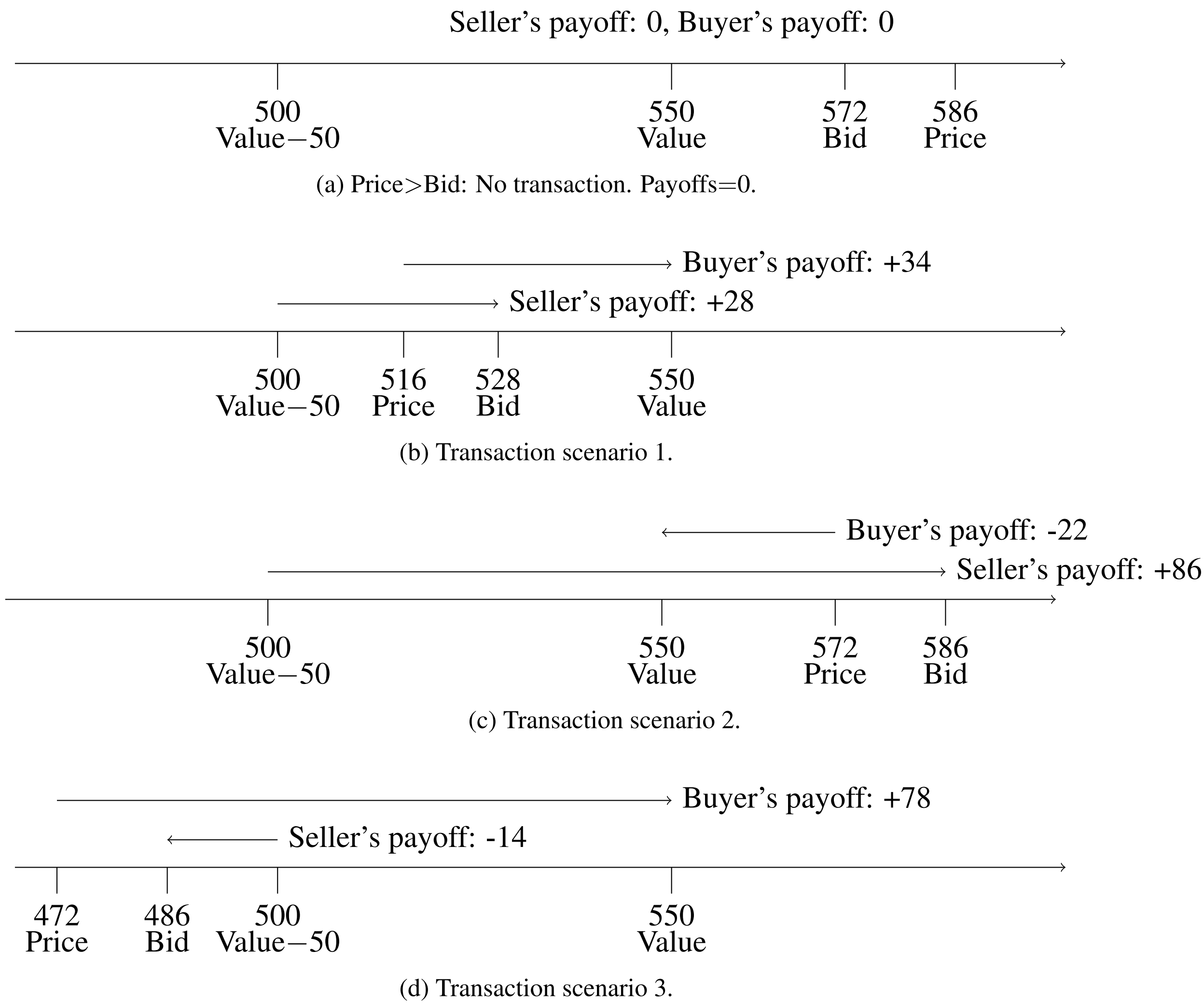
$$\textbf{Payoff}_{\textbf{Buyer}} \textbf{ = Value} - \textbf{Price.}$$

Notice that a Bid equal to the Value will ensure the Buyer to never make losses. If the Price was higher than the Bid=Value, the transaction would not take place. Recall from 3. that under a Bid=Value, the transaction does not take place half the times. Further, note that in terms of expected payoff, the Buyer benefits most from bidding what he believes the Value of the item is. The bids are limited to be between 0 and 1200. Figures E.6a to E.6d present various scenarios. In these scenarios the purchasing of additional Evidences is not considered.

Part I of this experiment consists of 2 practice rounds and 2 blocks of 5 experiment rounds. At the beginning of each round, you will be informed about the randomly chosen role (Seller or Buyer) that you take. You keep this role in the first block of 5 rounds, and take on the other role in the second block. You will keep the same role within a block, but you will face randomly chosen market counterparts. Throughout, one seller and one buyer will form a market.

In order to participate in the experiment, you will go through a brief understanding test. Here and throughout the experiment, you can access a calculator via a button in the right bottom corner of your screen. Once everybody accomplishes this test, you can get more familiar with the normal distribution with standard deviation of 100. For that purpose, you will have three minutes to simulate as often as you want the process of generating 10 Evidences for different Values. The experiment will start with the 2 practice rounds that are not paid. Finally, you proceed to the paid rounds.

Are there any questions? If not, please turn to your screens and follow carefully the instructions there.



(a) Price>Bid: No transaction. Payoffs=0.

(b) Transaction scenario 1.

(c) Transaction scenario 2.

(d) Transaction scenario 3.

Figure E.6: Transaction scenarios.

Part II

You are about to start Part II of the experiment, which consists of no practice rounds and 2 blocks of 5 experiment rounds. Like before, in each block of 5 rounds you will take the same role, and you will face randomly chosen market counterparts.

In this part, one market will consist of 4 Sellers and 4 Buyers. Just like before, the Sellers first decide whether they want to purchase 5 additional Evidences. Then they choose the Evidences they want to report. The Buyers will see the reports of all 4 Sellers. Sellers as well will be informed about the reports of the other Sellers. Before the Buyers place their bid, they have to chose from which Seller to buy. For the chosen Seller's good, the Buyer places a bid and the Buyer's payoff is determined in the same fashion as before. The payoff of the Sellers is as well determined in the same fashion as before. The Seller sells one item of their good to each Buyer whose Bid exceeds the

Price. A Seller might sell to none, one, two, three, or four Buyers.

D.3 C and M extension experiments

Below we present the instructions for the extension experiments with the following sequence: C10B (Part 1), C10B-4M (Part 2), C10B-NC (Part 3) and M10B (Part 1), M10B-CS (Part 2), M10B-AS (Part 3). Other sequences are available upon request. An aspect particular to M (C) is marked "[M: ...]" ("[C: ...]"). While the ***C*** *extension was implemented in the lab, the* ***M*** *extension was done online:*

You are about to participate in an experiment in the economics of decision making in a market setting. You may earn a considerable amount of money. The amount you earn will depend on your decisions and the decisions of others, so please follow the instructions carefully. All that you earn is yours to keep, and will be paid to you [M: either via bank transfer or paypal after today's session.][C: in private, in cash, at the end of today's session]

During the experiment your payoffs are denominated in points. Your point earnings will be converted to cash at the end of today's session at an exchange rate of 60 points = 1 Euro. No matter what your payoffs are in the experiment, you will be paid at least 5 Euro.

[M: While you cannot speak directly to us, you can contact the experimenter via private chat in case you have a question. We may ask you to turn on your microphone in case this is necessary.][C: It is important to us that you remain silent and do not look at other people's screens. If you have any questions or need assistance of any kind, please raise your hand, and an experimenter will come to you. If you talk, exclaim out loud, etc., you will be kindly asked to leave.]

The experiment consists of three parts (**Part I**, **Part II**, **Part III**) which are independent of each other. Each of these parts, in turn, consists of up to 7 rounds.

General Setting

[M: In this experiment, one Seller and one Buyer form a market.][C: In this experiment, four Sellers and four Buyers form a market.] The Seller knows the Value (in points) of his/her good but cannot report it to the [M: Buyer][C: Buyers]. The Seller has requested 10 external test institutes to officially evaluate the Value of the good and can indeed report these 10 official "Evidences" about the Value to the [M: buyer][C: buyers]. The external Evidences are informative about the good's Value but noisy. In particular, they follow a normal (Gaussian) distribution around the Value with a constant standard deviation of 100. The standard deviation measures the dispersion of the Evidences, how far away they are from the Value. Figure E.7 shows that Evidences are more likely to be close to the Value than far away. You will be able to get familiar with this distribution later.

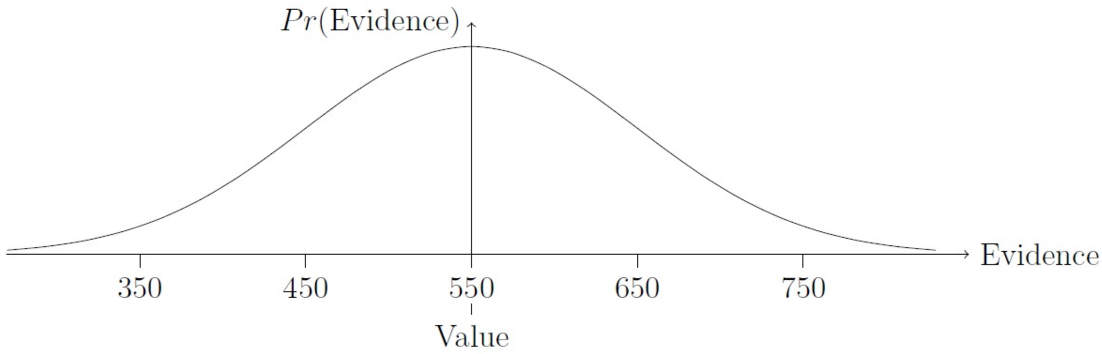


Figure E.7: Normal probability distribution with a Value of 550 and a standard deviation of 100, indicating the probability of Evidences.

At the beginning of a round, [M: the][C: each] Seller is informed about the true Value of her/his good. The true Value lies between 200 and 1000 points, each Value level in this interval is equally likely. As we said, the Seller cannot report the true Value to the [M: Buyer][C: Buyers], but she/he can choose for each of the 10 Evidences whether to report it to the [M: Buyer][C: Buyers] or not. The Seller cannot change or manipulate the Evidences in any way.

[M: After the Seller's choice, the Buyer observes only the Evidences that the Seller chose to report. S/He then places a Bid for the good. The Bid has to be an integer value between 0 and 1200.]

[C: After the Sellers' choice, the Buyers will see the reports of all 4 Sellers. Sellers as well will be informed about the reports of the other Sellers. Before the Buyers place their bid, they have to chose from which Seller to buy. For the chosen Seller's good, the Buyer places a Bid. The Bid has to be an integer value between 0 and 1200.]

When does the transaction take place? The computer generates randomly a Price of the Seller's good. Neither seller nor buyer will be informed about the Price when they take their decisions. The Price takes integer values between the Value minus 200 and the Value plus 200, with each Price level being equally likely (figure E.8). The transaction takes place whenever the Buyer's Bid is greater than or equal to the Price.

So, in our example, a Bid of

1. 349 will never lead to a transaction, since the Price is certainly above.

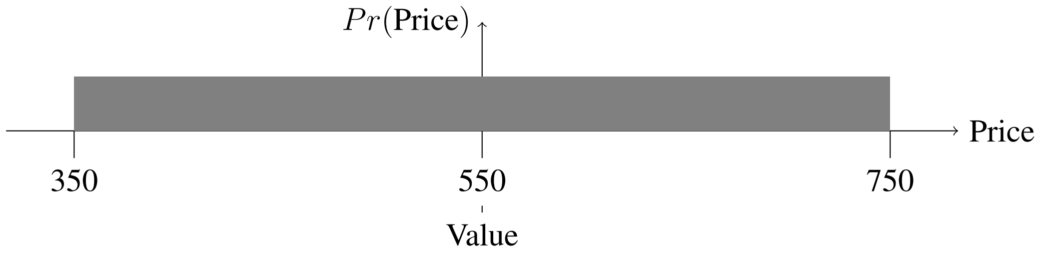


Figure E.8: Price distribution around the Value of 550.

2. 750 will always lead to a transaction since the Price is certainly below or the same.

3. 550 corresponding to the Value will lead to a transaction with 50% chance.

How are the Seller's and the Buyer's payoff determined? First, if no transaction takes place, both Seller and Buyer get a payoff of 0 points. If the transaction takes place, as we said depending on the Buyer's Bid and the random Price, the Seller produces the good at the cost of the Value minus 50. [C: The Seller sells one item of their good to each Buyer whose Bid exceeds the Price. A Seller might sell to none, one, two, three, or four Buyers.] The Seller's payoff [C: per transaction] at the end of the round will be the Bid placed by the Buyer minus the cost:

$$\textbf{Payoff}_{\textbf{Seller}} = \textbf{Bid}_{\textbf{Buyer}} - \textbf{(Value} - \textbf{50).}$$

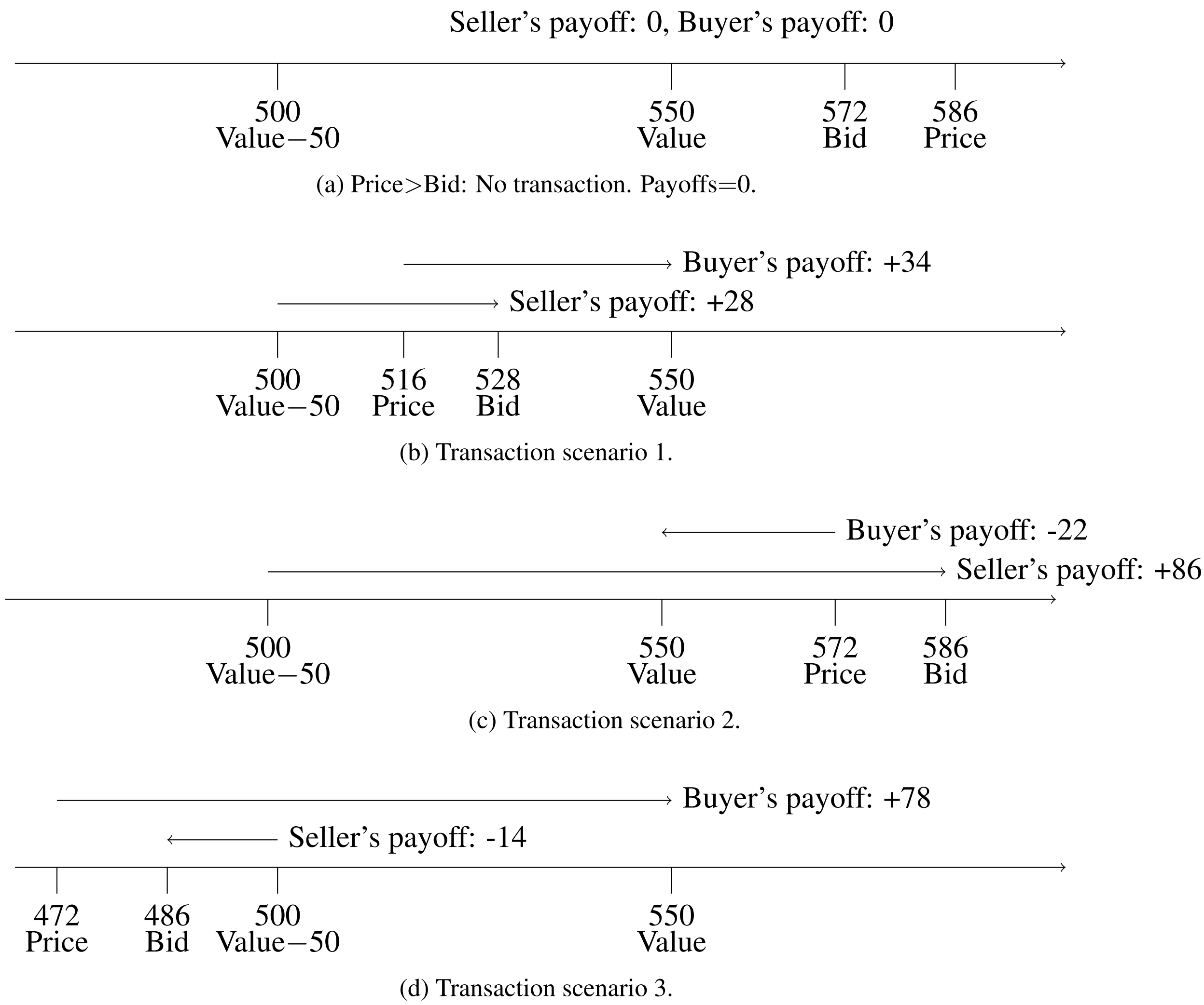
The Buyer's payoff is the true Value of the good minus the Price:

$$\textbf{Payoff}_{\textbf{Buyer}} = \textbf{Value} - \textbf{Price.}$$

Notice that a Bid equal to the Value will ensure the Buyer to never make losses. If the Price was higher than the Bid=Value, the transaction would not take place. Recall from 3. that under a Bid=Value, the transaction does not take place half the times. Further, note that in terms of expected payoff, the Buyer benefits most from bidding what he believes the Value of the item is. The bids are limited to be between 0 and 1200. Figures E.9a to E.9d present various scenarios.

Part I

Part I of this experiment consists of 2 practice rounds and 5 experiment rounds. **In today's session, everybody will be in the role of a Buyer. [M: Throughout, one Seller and Buyer will form a market.][C: Throughout, four Sellers and four Buyers will form a market.] You will be confronted with [M: a good][C: goods] and information of [M: a Seller who was][C: four Sellers who were] in the situation described**



(a) Price>Bid: No transaction. Payoffs=0.

(b) Transaction scenario 1.

(c) Transaction scenario 2.

(d) Transaction scenario 3.

Figure E.9: Transaction scenarios.

in the General Setting in a previous experimental session. As described above, your payoff as a Buyer depends on the Value, the Price and your Bid. Nobody will receive the Seller payoff.

In order to participate in the experiment, you will go through a brief understanding test. Here and throughout the experiment, you can, of course, access your computer's calculator. Once everybody accomplishes this test, you can get more familiar with the normal distribution with standvard deviation of 100. For that purpose, you will have three minutes to simulate as often as you want the process of generating 10 Evidences for different Values. The experiment will start with the 2 practice rounds that are not paid. Finally, you proceed to the paid rounds.

Are there any questions? If not, please turn to [M: your browser window with the experiment][C: your screens] and follow carefully the instructions there.

Part II

You are about to start Part II of the experiment, which consists of no practice rounds and 5 experiment rounds. **Like before, everybody will be in the role of a Buyer.** Again, you will be confronted with [M: a good][C: goods] and information of [M: a Seller who was in the situation described in the General Setting in a previous experimental session.][C: four Sellers from a previous experimental session.][M: Notably, unlike in Part I, you will receive some additional information: Specifically, you will be provided with information of three additional sellers each of whom was in the situation described in the General Setting in a previous experimental session. Importantly, while you will be able to see this additional information of three other sellers, you will only be able to bid for the product of your own seller but not the products of the other sellers whose products/Values are also different from your own seller.] [C: Notably, unlike in Part 1, the setup in this previous session was identical to the one here with one exception: The Sellers were in markets with one Seller and one Buyer in which the Buyer could only bid for this one Seller's product.] As before, your payoff as a Buyer depends on the Value, the Price and your Bid. Nobody will receive the Seller payoff.

Part III

You are about to start Part III of the experiment, which consists of no practice rounds and 5 experiment rounds. **Like before, everybody will be in the role of a Buyer.** Again, you will be confronted with [M: a good][C: goods] and information of [M: a Seller who was][C: four Sellers who were] in the situation described in the General Setting [C: (four Sellers, four Buyers)] in a previous experimental session. [M: Notably, the situation you are facing will be the same as in Part II. Unlike in Part I, you will receive some additional information: Specifically, you will be provided with information of three additional sellers each of whom was in the situation described in the General Setting in a previous experimental session. Importantly, while you will be able to see this additional information of three other sellers, you will only be able to bid for the product of your own seller but not the product of the other sellers whose products/Values are also different from your own seller.][C: Notably, unlike in Part 1 or 2, you will not be able to choose which Seller's product to bid for in this part, but you will be bidding for one Seller out of the four that is given to you by the computer.] As before, your payoff as a Buyer depends on the Value, the Price and your Bid. Nobody will receive the Seller payoff.