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Abstract

When sellers disclose verifiable information, buyers must exercise sufficient skepticism to account for potentially selective disclosure, yet previous evidence suggests they often fail to do so. We experimentally examine how buyers adapt their skepticism in response to *uncertainty* about the pool of available verifiable information. Contrary to previous findings on institutional manipulations in the literature, we discover that buyers adapt to our institutional change quite effectively, even—if anything—enhancing their skepticism. These results suggest that buyers' skepticism may adjust appropriately, or not, depending on the specific context, implying that consumer naivety may be less frequent, at least when real-world features prompt scrutiny.

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 studies suggest that the level of sophistication is too low to generate full revelation and often leads to harmful inferences on the consumer side (Benndorf, Kübler and Normann, 2015; Jin, Luca and Martin, 2021b). This phenomenon of naïvety seems remarkably resilient, as diverse institutional changes——intended either to aid or hinder consumers through mechanisms like communication, obfuscation, or competition (Montero and Sheth, 2021; Jin et al., 2021a; Deversi et al., 2021; Penczynski et al., 2025)——fail to increase skepticism.

In this study, we experimentally examine an important yet previously unexplored form of institutional change——specifically, a variation in the information environment. We investigate how uncertainty about the pool of verifiable information available to sellers affects buyer inference. For instance, buyers may be aware that a given product has been tested by multiple independent evaluators but remain uncertain about how many have tested it. Intuitively, in such cases, the correct bid calibration is more demanding since buyers cannot clearly determine what information is available for disclosure to the seller (Jovanovic, 1982; Shin, 1994, 2003).

We build on the flexible product market framework of information disclosure proposed by Penczynski et al. (2025), which allows for varying degrees of disclosure by enabling sellers to selectively reveal verifiable evidence from a pool of available evidence. A key advantage of this framework is that the number of available evidences can be extended naturally—sellers in our model can purchase them——in contrast to more stylized sender-receiver models with binary disclosure, where one signal fully reveals the state of the world. Similar to more stylized settings, Penczynski et al. (2025) find that the unraveling logic fails due to buyers' limited sophistication. Theoretically Jovanovic (1982) demonstrates that a seller's ability to generate verifiable test results can lead to inefficiently high levels of information production. In our setting, sellers are expected to exploit this ability to take advantage of naive consumers. Consumer sophistication might therefore be even more limited than already observed in the laboratory and might thus align with field evidence (Jin and Leslie 2003; Bederson et al. 2018; Jin 2005; Brown et al. 2012, 2013).

Our setup features an intuitive product market framework, a gradual spectrum of information disclosure, and the inherent conflict of interest between sellers—–who aim to maximize buyers' willingness to pay—–and buyers—–who seek to accurately assess the product's value. Sellers offer a product whose true quality is known only to them. They possess 10 pieces of noisy evidence about the product and can choose whether to disclose each piece. The bidding mechanism incentivizes buyers to evaluate the product correctly. Using this setup as a benchmark, we introduce a variation in the information structure: in addition to the initial 10 pieces of evidence, sellers have the option to purchase an additional set of 5 evidences for a small fee. While buyers are aware of this option, they cannot observe whether the seller has exercised it. As a robustness check, we extend this variation beyond a monopoly setting with a single seller to a competitive market with four sellers.

We find that the option to purchase additional information leads on average to more evidences being published. This increase is mostly due to a larger set of evidences to select from and does not reduce the bias in the published evidences. Surprisingly, in the monopolistic setting, sellers earn significantly less when the purchase option is available, as buyer skepticism—–contrary to predictions—–tends, if anything, to increase. Consequently, we observe an average bid that almost coincides with the product value. Similarly, in the competitive setting with such an option, the skepticism increases somewhat compared to the setting without it. This slightly but insignificantly reduces the discrepancy between the bid and the true product value. Overall, with minimal buyer exploitation, our results indicate that consumers are able to adapt to an institutional change in the information environment, despite predictions that the change would harm consumers.

To determine whether our findings are specific to an environment where gradual disclosure inherently fosters greater skepticism than previously documented in the literature, we test our variation within a more stylized sender-receiver framework (Jin et al., 2021b), modeled after the classic setup in Milgrom and Roberts (1986). In this model, a sender (e.g., a firm) privately learns the true state of the world (e.g., the firm's product value on a scale from 1 to 5) and decides whether to disclose this information to a receiver. The receiver then estimates the state of the world, knowing that the sender's and receiver's interests are not fully aligned. To introduce uncertainty about what senders know regarding disclosable information, we modify the original game: initially, senders are unaware of their true value but have the option to purchase this information for a small cost. However, whether they choose to purchase it remains undisclosed to receivers. This simplified setting yields a clear-cut prediction——consistent with our original intuition——that senders who acquire and selectively disclose information can exploit naïve receivers. As a result, receivers are predicted to overestimate under non-disclosure, ultimately harming their welfare in equilibrium. Critically, however, we replicate our previous finding: rather than being disadvantaged by uncertainty, receivers, if anything, demonstrate an improved ability to navigate the situation.

The variation in information structure offers an interesting contrast to previously studied forms of institutional manipulation, such as seller-side competition or obfuscation. More broadly, prior research has shown that consumers often lack sophistication in stylized sender-receiver settings. In contrast, we find that buyers adjust their skepticism effectively in response to the increased uncertainty introduced by sellers' ability to purchase additional evidence. While real-world consumer behavior likely still allows room for strategic firm influence, simply recognizing, for instance, that multiple testing institutes exist——and that one may not know which have evaluated a given product——could encourage caution when interpreting a single published test result. In practice, concerns over fake reviews or tests may further heighten skepticism. Overall, our results suggest that buyers' skepticism adjusts contextually, or not, depending on the specific context, implying that consumer naivety may be less prevalent, particularly when real-world cues prompt scrutiny.

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).[1] In an influential study, Jin et al. (2021b) experimentally implemented a stylized disclosure setting and 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

[1]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)

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) and even seller competition may not increase skepticism (Sheth, 2021)

While previous studies focus on stylized sender-receiver settings with binary disclosure, our companion paper Penczynski et al. (2025) introduces a more realistic product market framework where disclosure varies in degree. 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.

In Penczynski et al. (2025), we investigate the effect of seller competition—a mechanism often assumed to benefit consumers——on buyer inference and reveal a surprising result: competition increases buyer naïveté, counteracting the welfare gains from improved disclosure. Through two extensions, we establish that a framing effect generates this phenomenon. Similar to the Peltzman effect, buyers appear to perceive competition as a safer environment, leading them to lower their skepticism.

In this paper, we examine how sellers can *endogenously* expand the set of available evidence, introducing uncertainty for buyers regarding the pool of verifiable information and potentially shaping their inference. This connects our study to the literature on strategic experimentation and the trade-off between benefits from exploration and exploitation in bandit problems (Rothschild, 1974; Aghion, Bolton, Harris and Jullien, 1991). Here, the trade-off is simpler since the seller only has to weigh the fixed costs of additional evidence against the information and selection benefits of more evidences. In the context of persuasion games, Jovanovic (1982) shows that a seller's ability to generate verifiable test results can therefore lead to inefficiently high information generation, whose cost is only incurred in order to move economic rent from the buyer to the seller in a zero-sum fashion.

To the best of our knowledge, we are the first to study this feature experimentally and in our setting the inefficiency enters when additional evidences are purchased in order to increase the publication bias whilst trying to appear transparent yet hiding the purchase.

2 Experimental games

We investigate the effects of additional disclosable information in a 2×2 experimental design as shown in table 1. In the first dimension, we implement two information structures, without uncertainty (X10) and with uncertainty (X10+5) about the number of available disclosable evidences. The second dimension is spanned by the variation analysed in Penczynski et al. (2025), the two market structures that we call “monopoly” (M) and “competition” (C). We will thus be able to see whether the uncertainty has similar effects across market structures.

		Monopoly M	Competition C
10 Evidences	X10	M10	C10
10+5 Evidences	X10+5	M10+5	C10+5

Table 1: 2×2 experimental design.

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.[2]

[2]For examples of possible outcomes see Appendix A.1. Notice that due to the production cost, in theory, situations may occur in which sellers try to avoid a transaction if they expect bids to be very low.

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.[3] 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.

10+5 Evidences (X10+5) In the X10+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 for a total price of $P_e = 15$ points.[4] The additional evidences are independently distributed in the same way as the initial evidences. Sellers simultaneously make the purchasing decision after they observe their initial set of 10 evidences. They do not learn other sellers' decisions. Buyers know that sellers have the possibility to purchase these additional 5 evidences but cannot observe a seller's purchasing decision in a particular round, providing sellers with an additional opportunity to mislead buyers over the true value. Apart from the price P_e which sellers pay upon purchase independently of realized transactions, the payoff structure is the same as before.

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, in particular avoiding those higher than the true value. Empirically, such behavior appears to be very infrequent, occurring in about 0.5% and 1.0% of cases, respectively.

[3]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.6 and B.7 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.

[4]The fee was chosen to be smaller than the expected publication bias of choosing the highest 10 out of 15 evidences (51.8, see panel (a) of table 6).

Competition (C10 and C10+5) The competition games feature 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. 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.

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. Some 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. Second, the 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. 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 $b \geq p$ 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.

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 or 15 available evidences E before he specifies the message $M \subseteq E$. Consider the product space $V \times \mathcal{E}$, where $\mathcal{E}$ is the $\#E$-dimensional integer space of possible evidence realizations, $\bigtimes_{n=1}^{\#E} \mathbb{Z}^n$. 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 (X10 games) 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).[5]

Shin (1994, 2003) show that full revelation is not necessarily sustainable in equilibrium in games with uncertainty about the number of evidences available to the seller.[6] Of course, with or without additional evidences (X10+5 vs. X10), full revelation will not be sustainable in equilibrium when buyers lack sophistication. Shin alternatively considers "sanitization strategies" in which exclusively "good" evidences are disclosed but "bad" evidences are not (see also Deversi et al. 2021). It is difficult to pin down the analogous strategy in our setting because we have no analogue to his binary "successful" and "failed" realizations and because such strategy depends very much on buyers' beliefs. Shin (2003, p. 110) discusses the "difficulty of tying down beliefs in sequential games of incomplete information" and in the light of counterexamples to sanitization strategies emphasizes the simplicity and intuitive force of the sanitization strategy.

In our setting, it is instructive 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 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

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

[6] Notably, unlike in Shin (1994, 2003), sellers strategically choose to expand the set of evidences in our X10+5 games and, at least when purchasing evidences is costless, buyers can infer that disclosing less than 15 evidences is driven by strategic motives in such a setting.

rarely but would – due to the selection – be associated with a considerable publication bias.[7]

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 $\mathbb{E}(v|M, \sigma)$. As already discussed in kthe stylized setting of section 4, 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.[8]

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.

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

[7] 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 6 on page 18).

[8] 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.

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.

As a start, we anticipate that the unraveling result will not hold in M10, consistent with previous findings in the literature (and as discussed in more detail in Penczynski et al. 2025). Given the substantial share of naive buyers reported, for instance, by Jin et al. (2016), we expect the seller's disclosure strategy in M10 to exhibit a positive publication bias, while the average buyer's bidding strategy only partially offsets this effect—entirely due to the influence of naive buyers.

In this paper, we are mainly interested in the effect of uncertainty about the number of available disclosable evidences. The difference between M10 and M10+5 is that the seller can purchase five further pieces of evidence without the buyer being informed about the purchase. A larger set E allows him to generate a higher publication bias for a given number $\#M$ of disclosed evidences. The skeptical buyer is aware of the potential selection from 15 evidences. It is thus reasonable to expect that skeptical buyers once more limit the profits that sellers can make off them but that naïve buyers can be exploited. By the nature of their inference, the naïve buyers' bidding strategy is not influenced by the size of the set of available evidences. In the face of naïve buyers, sellers can therefore – compared to M10 – maintain the level of $\#M$ and benefit from a higher publication bias. Alternatively, if sellers expect more favourable bidding from an increase of $\#M$, such an increase can be implemented at a maintained level of the publication bias. Given the positive purchasing fee, the seller will in equilibrium only purchase the additional evidences if naïve buyers' bids can be sufficiently increased due to an increased publication bias or larger $\#M$.

Hypothesis 1 (M10+5) *Sellers engage in the purchase of additional evidence and the publication bias, the number of disclosed evidences, or both will increase. While the seller's payoff (weakly) increases with purchasing, the buyer's profit decreases.*

Notably, we expect heterogeneity in seller behavior as the profitability of purchasing additional evidences, of course, depends on the level of buyer naïvety that individual sellers expect.

Intuitively, in the competitive settings, the buyers' search for precisely judgeable products leads competing sellers to increase the number of disclosed evidences.[9] In this paper, we are mainly interested in the effect of uncertainty, i.e. the difference between C10 and C10+5. Similar to M10+5, the increase in disclosed evidences can be achieved

[9]We discuss this aspect in more detail in Penczynski et al. (2025).

with purchases of additional evidences and might not result in a smaller publication bias. Competition in the 10+5 context will further increase the propensity to purchase additional evidences. From the outset, it is not clear whether the competitive pressure suffices to generate disclosure of more than 10 evidences.

Hypothesis 2 (C10+5) *Compared to M10+5, competition on the seller side will increase the number of disclosed evidences and increase the propensity to purchase additional evidences.*

Disclosing more evidences out of a larger set of evidences might or might not lead to a smaller publication bias, but the sellers' payoff will be at least as high as the profit after no purchase.

2.3 Experimental procedures

We conducted the experiments in the Experimental Economics Laboratory at the University of Mannheim (mLab).[10] Across ten sessions, 160 students participated. 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.[11]

In a session, participants play two of the four different games sequentially in each session as shown in table 2. To control for order effects, we vary the order of the games between 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.

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.

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

[11]Appendix A.3 provides screenshots of the games and of this tool.

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 2: Games in 10 sessions.

At the end of each round, the feedback in M games consists of the true item value, the price, the bid, the realization of a transaction and own payoffs. In C games, 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.[12]

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.[13] 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.

3 Results

3.1 Benchmarks

The literature, particularly Jin et al. 2016, has shown that in settings like ours, sellers tend to mislead buyers who only partially account for the selection of evidence. We discuss our benchmark case of M10 (and, for robustness, C10) in greater detail in Penczynski et al. (2025). Here, we provide an overview of the benchmark case to set the stage for our focus on the effect of uncertainty—specifically, sellers purchasing additional information.

On the seller side, a basic indicator of the scope of disclosure is the number of evidences included in the seller's message to buyers, $\#M$. In M10, this number is fairly symmetrically distributed around 4 and 5, with a spike at the full disclosure of 10 evidences (see figure 1). On average 4.71 pieces of evidence are disclosed in M10, leading to a publication bias of 75.05 on average (see table 4). This implies that the average disclosed evidences of a seller are about 75 points higher than the true value. In C10, more evidences are disclosed (6.18) and as a consequence the publication bias is

[12] In the two initial sessions, sellers in C10 were not able to see what the other sellers decided to report. In X10+5, sellers were never able to see whether other sellers bought additional evidences.

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

smaller (45.61). Overall, this supports the view, consistent with previous findings in the literature, that sellers selectively disclose information. Full unraveling does not occur in either the monopoly or competition cases, although disclosure is higher in the latter.

Buyers mark down their bids from the average of the disclosed evidences, as shown in panel (a) of table 5. In both M10 and C10 we observe a remaining discrepancy (M: 5.63 and C: 25.22) between bid and true value, although this is only significant in C10. Our companion paper elaborates on the fact that we see more sophistication in M10 than in C10, adding to the naivety results of the literature (Penczynski et al., 2025). The key takeaway here is that buyers' adjustments for the selective disclosure by sellers remain incomplete.

3.2 Purchasing

When sellers have the option to purchase additional evidences in the X10+5 games, they do so roughly one-third of the time as panel (a) of table 3 shows. The realizations of the original 10 evidences are predictive of the purchasing. For sellers that decide to purchase, the top-5 pieces of the original evidences result in a publication bias of 37.44, versus 78.91 for non-purchasers ($p = 0.001$).

Competitive pressure in C10+5 increases the fraction of purchasing slightly, but not significantly. In the last rounds, this fraction increases, an effect that can be attributed to the payoff benefits that purchasing brings.

Within X10+5, panel (b) of table 3 shows that – over all rounds – purchasing ("+5") leads to a higher payoff than not purchasing further evidences ("+0"). In C10+5, this difference is significant for all rounds and increases until the final rounds 5 and 10. In contrast, the difference in M10+5 is not significant and decreases to almost zero in the final rounds.

3.3 Disclosure

Panel (a) of table 4 shows that the average numbers of disclosed evidences in the X10+5 games are roughly 0.5 higher than in the benchmark X10 games. This increase is driven by those buying new evidences. Interestingly, this happens at a maintained level of publication bias. Figure 1 shows that the distributions of disclosed evidences in X10+5 are similar to the ones in X10. A systematic difference is, however, a more pronounced fraction of disclosure of 10 evidences in X10+5. Interestingly in X10+5, very few sellers reveal that they made a purchase by disclosing more than 10 evidences. The previously observed differences between monopoly and competition in X10 are approximately replicated in X10+5.

			X10+5	M10+5	C10+5
(a)	All rounds		33.13	31.88	34.38
				[0.56]	
Purchase +5	N		640	320	320
(%)	Rounds 5&10		39.84	35.94	43.75
				[0.47]	
	N		128	64	64
(b)	All rounds	+5	62.16	53.99	69.74
			***	[0.28]	**
Payoff		+0	41.44	43.04	39.78
	Rounds 5&10	+5	72.04	47.00	90.79
			*	[0.96]	**
		+0	38.19	45.76	29.58

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: Sellers' purchasing decisions and average payoffs.

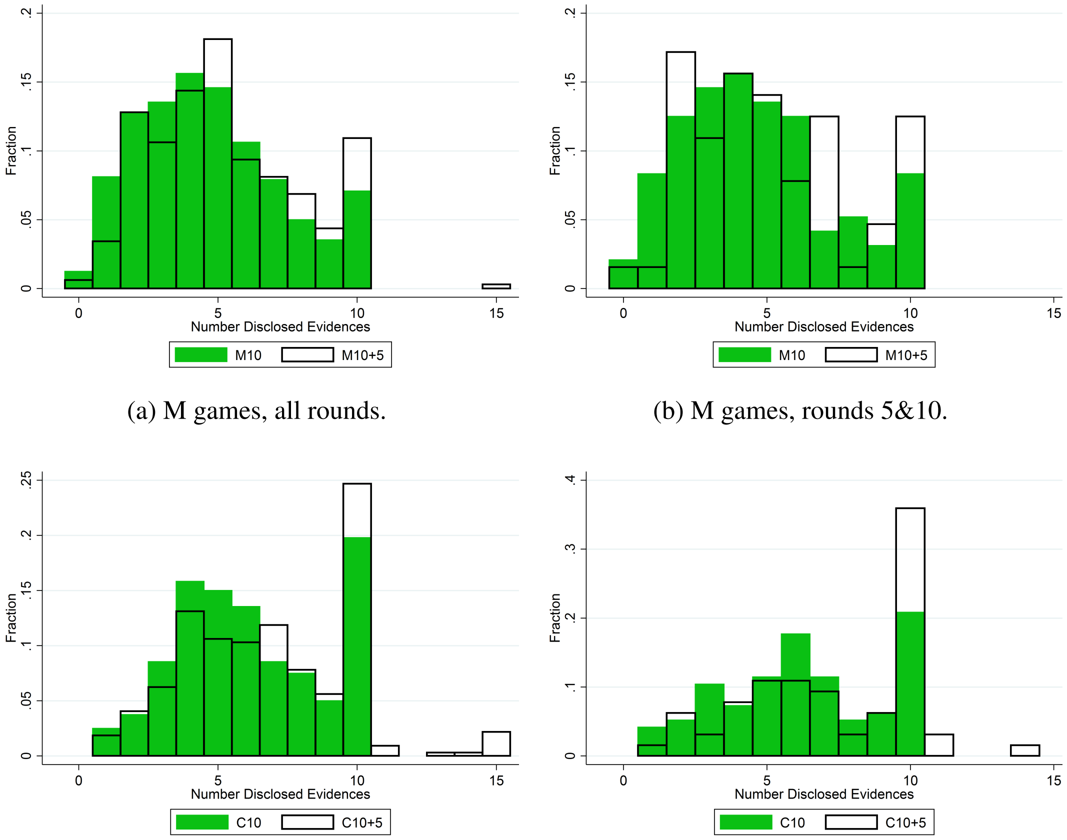
		All rounds			Rounds 5&10		
		M		C	M		C
(a)	10	4.71	***	6.18	4.65	***	6.25
Nr Published evidences		***		***	[0.24]		**
$\#M$	10+5	5.31	***	6.91	5.16	***	7.38
(b)	10	75.05	***	45.61	82.83	***	42.21
Publication bias		[0.94]		[0.49]	[0.59]		[0.49]
$\bar{e}^M - v$	10+5	75.40	***	43.15	77.35	***	47.24
N	10	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 4: Means of number of disclosed evidences and publication bias.

The results so far suggest that purchases are made mostly to maintain the publication bias with a larger number of disclosed evidences. Figure 2 supports this view graphically and illustrates disclosure as a function of the purchasing decision. Among purchasers, disclosing 10 evidences is most common in M10+5 and by far so in C10+5. This way, sellers leave the buyer in the unknown about their purchasing decision and try to appear transparent while still selecting evidences.

Table 6 shows the publication bias by the number of disclosed evidences for purchasers (Panel c) and non-purchasers (d) in M10+5 and for M10 (e). Most notably, the publication bias among purchasers is higher for almost all numbers $\#M$ compared to both non-purchasers and M10 sellers.



(a) M games, all rounds.

(b) M games, rounds 5&10.

(c) C games, all rounds.

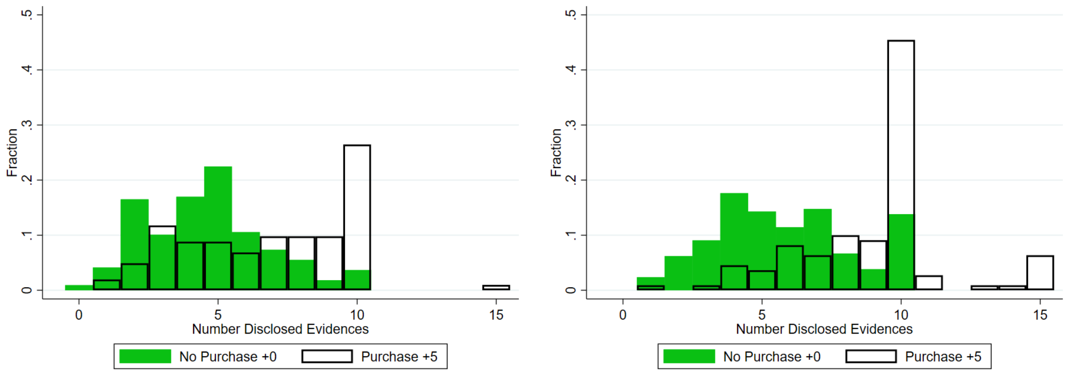
(d) C games, rounds 5&10.

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

3.4 Bidding

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 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.

The uncertainty about the seller's pool of evidences makes buyers more skeptical. The costly purchasing option might make the strategicness of the disclosure more salient and highlight the seller's expanded selection possibility. Despite the fact that more evidences are offered, buyers at least maintain or, if anything, improve the magnitude of their markdown compared to the benchmark X10 settings, as table 5 shows. While this leads on average to a discrepancy of almost zero in the M10+5 game, the slight markdown change in C10+5 is not sufficient to improve the discrepancy by much. In the last rounds, the discrepancy increases because the markdown – in contrast to the



(a) M10+5.

(b) C10+5.

Figure 2: Number of disclosed evidences $\#M$ by purchasing decision.

		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 5: Buyers' markdown and bidding discrepancy.

publication bias – is even smaller in absolute terms.

Panel (f) of table 6 shows that the markdown is often sufficient for the publication bias of non-purchasing sellers, but largely insufficient for the large publication bias of purchasing sellers.

		#M / Rank k															
		15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0
(a)	k	-173.5	-124.7	-94.7	-71.4	-51.4	-33.4	-16.3	0.2	16.7	33.7	51.7	71.7	95.0	125.1	173.8	
Bench-	Top k	0.2	12.6	23.1	33.0	42.4	51.8	61.3	71.0	81.1	91.8	103.5	116.4	131.3	149.5	173.8	
mark 15	naïve San.			101.0	82.9	77.1	81.9	78.1	85.3	79.5	72.7	69.3	83.0	81.3	78.2		
	N	0	0	1	3	8	22	23	35	48	39	21	6	5	1	0	0
(b)	k						-154.2	-100.4	-65.8	-37.8	-12.5	12.0	37.5	65.5	100.1	153.7	
Benchmark 10	Top k						-0.2	16.9	31.6	45.5	59.4	73.8	89.2	106.4	126.9	153.7	
(c) P. bias	M10+5	∶20.2					⁝59.8	49.2⁝	59.6⁝	79.2	93.4	95.6	108.4	138.7	125.6	143.8	
Purchase	N	1	0	0	0	0	27	10	10	10	7	9	9	12	5	2	
(d) P. bias	M10+5						-9.5·	24.4	21.3∶	40.4	61.6	61.1⁝	79.2∶	78.8⁝	111.5∶	153.4	
No Purchase	N						8	4	12	16	23	49	37	22	36	9	2
(e)	M10						⁝10.4	⁝31.6	⁝53.2	⁝57.1	52.4⁝	65.0⁝	72.7⁝	90.2⁝	117.8·	141.3	
P. bias	N						34	17	24	38	51	70	75	65	61	39	6
(f)	M10+5	-0.9					26.1	34.2	19.8	73.6	49.7	74.7	80.0	95.7	120.9	166.7	
Markdown	N	1	0	0	0	0	35	14	22	26	30	58	46	34	41	11	2

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 6: Summary by number #M and rank k of disclosed evidences (M10 and M10+5 games).

3.5 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 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 7 shows that the overall payoffs of sellers are reduced in X10+5 compared to X10, significantly so in the monopolistic environment. This is true in terms of the shown net payoff accounting for the fee as well as gross payoff without the fee.

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 (see table 6). Under this strategy, the buyers' payoffs could be almost 50, implying that the would buyer be better off.[14] Rather than 25, across all treatments, the seller obtains a payoff higher than 25 thanks to the naïveté of the buyer.

In M10+5, the more moderate payoff decrease in the simulation shows that the reduction in payoff is mainly due to the buyers' improved bidding discrepancy. In C10+5, sellers are benefiting again strongly from buyers' low markdown albeit less than in C10. Unpredicted, buyers succeed in maintaining their payoff level despite the given possibilities of the sellers and the higher uncertainty.

Overall, sellers act broadly in line with our hypotheses in M10+5 and C10+5. In case they buy additional evidences, they use it to increase the number of published evidences, carefully avoiding to publish more than 10 evidences most of the time. In other words, they prevent revealing that they actually bought more evidences. Contrary to our predictions, buyers appropriately react to the institutional change and appear to understand the potential risk from additional evidences that may have been acquired without their knowledge. Hence, their discrepancy on average does not deteriorate and they avoid a decrease in payoffs. Notably, the moderate but insignificant decrease in discrepancy is insufficient to overcome 'competition naïvety' in C10+5 (see Penczynski

[14]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.

		(a) Payoffs			(b) Simulated payoffs	
		M		C	M	C
	10	40.49	[0.65]	42.70	48.11	49.68
Buyers		[0.76]		[0.86]		
(Benchmark: 50)	10+5	42.08	[0.95]	41.75	49.90	46.99
	10	58.73	[0.94]	59.26	31.04	27.84
Sellers		**		[0.28]		
(Benchmark: 25)	10+5	46.53	[0.63]	50.08	28.49	24.65
N	10	480		480	480	480
	10+5	320		320	320	320

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 7: Payoffs and simulated payoffs.

et al., 2025).

Result 1 (M10+5) *One-third of sellers engage in the purchasing of additional evidence. The number of disclosed evidences increases while the level of the publication bias is unchanged. The seller's payoff decreases mainly due to the reduced discrepancy of the buyers. Against our prediction, buyers can maintain the same level of payoff as in M10. Purchasers do not earn significantly more than non-purchasers.*

Result 2 (C10+5) *Compared to M10+5, competition increases the number of disclosed evidences at a maintained level of publication bias (compared to C10). The propensity to buy additional evidence is, however, not significantly higher than in M10+5. Purchasers earn significantly more than non-purchasers.*

4 Our variation in a stylized setting

Our setup naturally extends to include additional pieces of evidence, enabling us to study the purchase of additional information and the resulting uncertainty. For robustness, it is instructive to consider our variation also in the simpler, more stylized setting from the literature, namely in the setting of Jin et al. (2021b). Although incorporating information purchases and the resulting uncertainty is less intuitive in a setting where a single signal fully reveals the state, we propose an implementation in this section and report on its results, which reinforce the findings of the previous section.

Jin et al. (2021b) experimentally implement a stylized disclosure game akin to the setup in Milgrom and Roberts (1986): A sender (e.g., the firm) privately learns the true state of the world (e.g., the firm's true product value from 1 to 5) and decides whether to disclose this information to a receiver. We label this game as M1, “monopoly with one evidence”. The receiver then estimates the state of the world, knowing that the sender and receiver do not have aligned interests. Skeptical consumers treat all nondisclosing senders the same, prompting the best senders among those that do not disclose to separate themselves through disclosure. Applied iteratively, this unraveling process leads to a unique sequential equilibrium where senders always reveal their information. However, Jin et al. (2021b) find that many players are not sufficiently skeptical about non-disclosure, which prevents complete unraveling and may suggest the need for mandatory disclosure policies.

Uncertainty about the disclosable information may provide a challenge to receivers and has not yet been studied in the environment of Jin et al. (2021b). We experimentally implemented three games to illuminate the issue (see details in appendix C). Apart from replicating the original M1 game, we construct, as a counterpoint, M0 (“monopoly with no signal”), where senders are uninformed about their true value but still decide whether to unbeknownly disclose it to receivers. In contrast to M1, senders cannot selectively disclose their value, implying that both skeptical and naïve players are predicted to behave optimally, bidding either the disclosed value or, absent disclosure, the unbiased average of 3. Based on M0, we introduce the possibility of uncertainty about the disclosable information and selective sender disclosure in M+1: while senders are initially unaware of their true value, they can purchase this information for a small cost, with the actual decision to purchase remaining undisclosed to receivers. Uncertainty produces predictions contrary to those of competition: Only skeptical receivers remain unaffected, whereas purchasing and selectively disclosing information allows senders to exploit naïve receivers, who overestimate the product value under non-disclosure.

Table 8 displays empirical behavior both under disclosure and non-disclosure. We replicate Jin et al. (2021b). In M1, unraveling does not occur; a value of 2 is reported only 24% of the time, rather than the predicted 100%, while higher values are frequently reported. Critically, senders bias their non-disclosure ($2.05 < 3$, $p < 0.001$; all tests: two-sided t-tests) and (naïve) receivers – overestimating the value – account for this bias incompletely. In contrast and as expected, non-disclosure is unbiased in M0 ($3.12 \sim 3$, $p = 0.172$) and generates no overestimation. In M0, disclosure cannot be conditioned on the true value, which explains why, unlike in M1, no clear pattern emerges in the frequency of disclosure across different values. In M+1, sellers choose to purchase additional information in 60% of cases, shaping their disclosure patterns to closely resemble those in M1. These sellers also exhibit a bias in their non-disclosure behavior—again, similar to M1—whereas the remaining sellers, like in M0, lack the ability to do so. From the receivers’ perspective, non-disclosure exhibits notable bias

($2.57 < 3$, $p = 0.001$), although less pronounced than in M1. They tend to account for the bias, albeit somewhat incompletely, slightly overestimating the value. However, they seem to be alerted to the risks of uncertainty, as their estimate is lower than in M0 ($2.78 < 2.96$, $p = 0.016$) and their overestimate is smaller than in M1 ($0.64 > 0.21$, $p = 0.026$). Even in relative terms, the overestimation is approximately two-thirds of the bias in M1 but reduced to only one half in M+1. These observations suggest that receivers are, if anything, better able to manage the uncertain situation.

Consequently, similar to our full setting above, naive receivers may not be as adversely affected by uncertainty as predicted since the option to purchase appears to underscore potential risks.

	M1	M+1		M0
		Purchase	No purchase	
Sender		*60%*	*40%*	
Disclosure (value = 1)	0.05	0.05	0.31	0.53
Disclosure (value = 2)	0.24	0.12	0.19	0.44
Disclosure (value = 3)	0.61	0.46	0.13	0.34
Disclosure (value = 4)	0.84	0.92	0.22	0.23
Disclosure (value = 5)	0.90	0.96	0.32	0.38
Value under non-disclosure	2.05	2.00	3.13	3.12
		2.57		
Receiver				
Estimate (disclosure = 1)	1.00	1.25		1.43
Estimate (disclosure = 2)	2.00	2.14		2.09
Estimate (disclosure = 3)	3.10	3.23		3.31
Estimate (disclosure = 4)	4.00	4.02		3.86
Estimate (disclosure = 5)	4.68	4.87		5.00
Estimate under non-disclosure	2.69	2.78		2.96
Overestimate	0.64	0.21		-0.18
Overestimate $\neq 0$ (p-value)	0.001	0.086		0.172

Table 8: Sender and receiver behavior – M1, M+1, M0

5 Conclusion

In this paper, we explore how uncertainty in the information environment——specifically, sellers' ability to purchase additional evidence——affects buyer skepticism and market outcomes. We utilize both a flexible product market framework of information disclosure and a stylized sender-receiver setting. We demonstrate that the option to acquire extra evidence increases the disclosed evidences information available. Surprisingly, rather than undermining buyer inference, this institutional change leads, if anything, to enhanced skepticism. In both monopolistic and competitive settings, buyers adjust their behavior in ways that largely prevent the exploitation predicted by traditional models, bringing average bids fairly close to the true product value.

These findings suggest that buyers' behavioral adaptations to institutional changes can sometimes be beneficial rather than detrimental. By triggering real-world cues that promote cautious evaluation, consumers may overcome inherent naivety and avoid being misled by selective disclosure. Expanding the range of possible outcomes, Johnson et al. (2015) demonstrate in the context of mortgage refinancing that excessive skepticism can be costly for consumers, highlighting that the optimal level of skepticism is context-dependent. Our results underscore the importance of considering the institutional environment in assessing consumer behavior, suggesting that under certain conditions, adaptive skepticism can serve as a valuable defense against potential exploitation.

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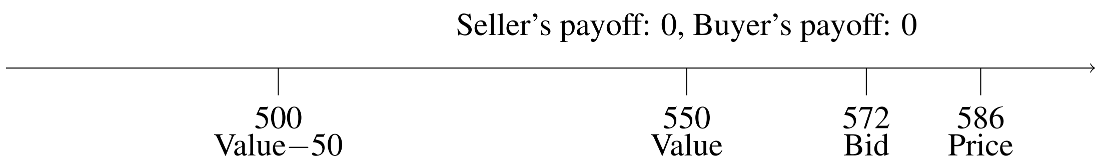
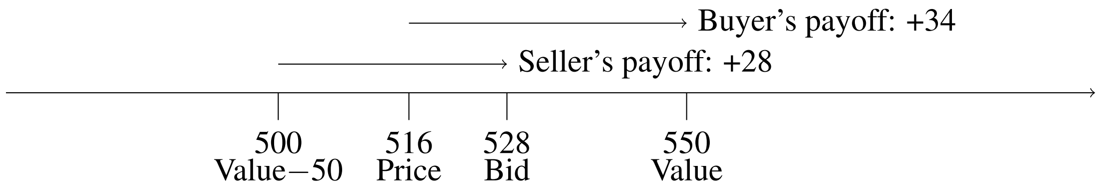
A Appendix: Additional Figures and Tables

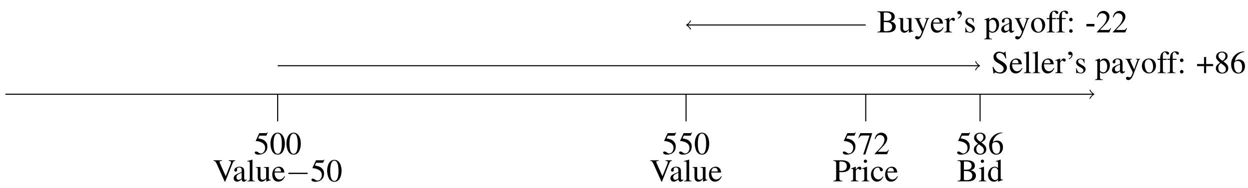
A.1 Examples of Possible Outcomes



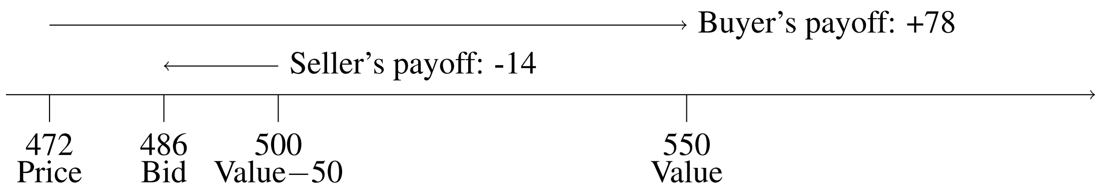
(a) Price>Bid: No transaction. Payoffs=0.



(b) Transaction scenario 1.



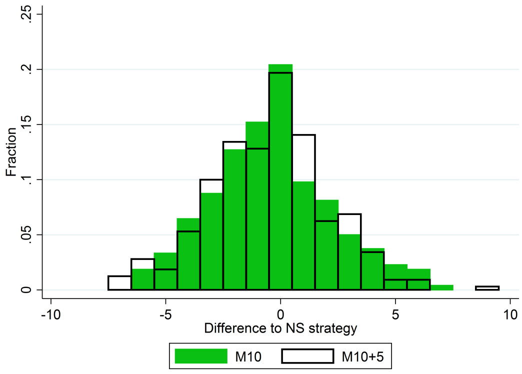
(c) Transaction scenario 2.



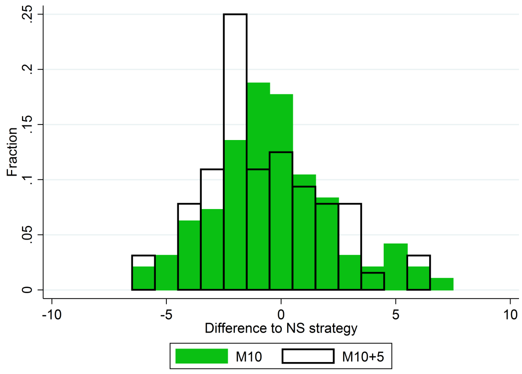
(d) Transaction scenario 3.

Figure A.1: Transaction scenarios.

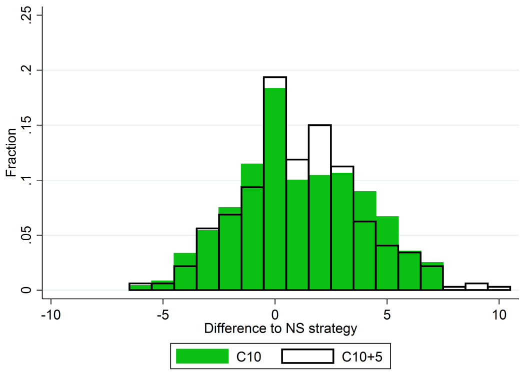
A.2 Additional figures and tables



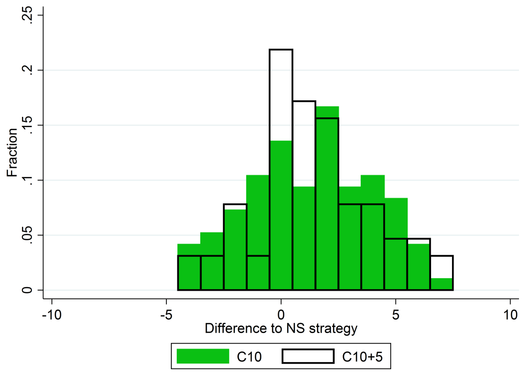
(a) M games, all rounds.



(b) M games, rounds 5&10.



(c) C games, all rounds.



(d) C games, rounds 5&10.

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

A.3 Screenshots

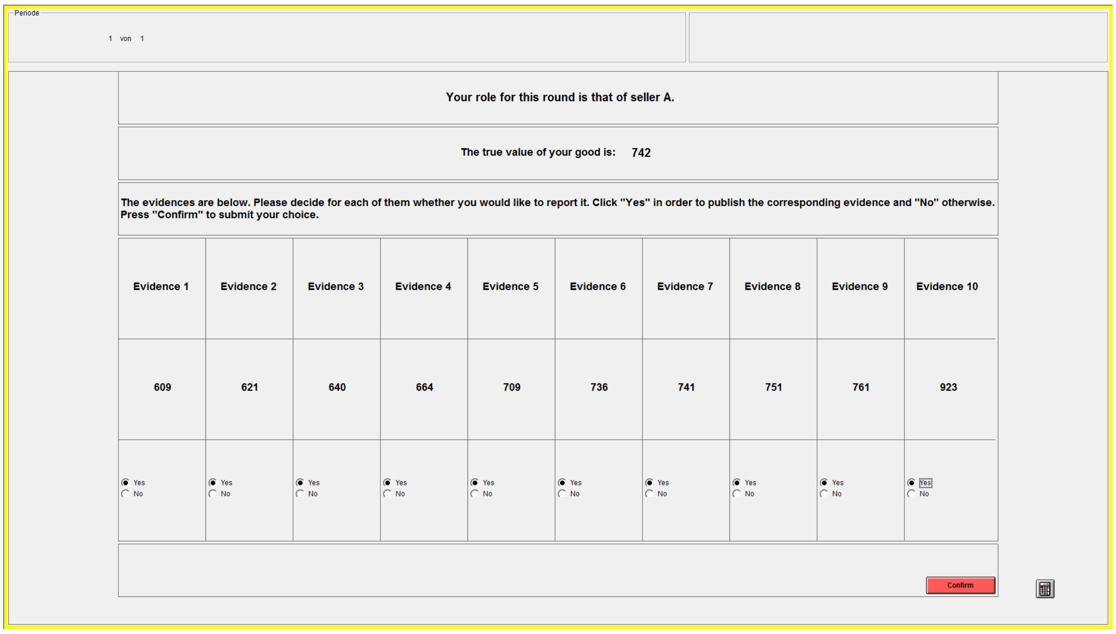


Figure A.3: Sellers' screen in C10.

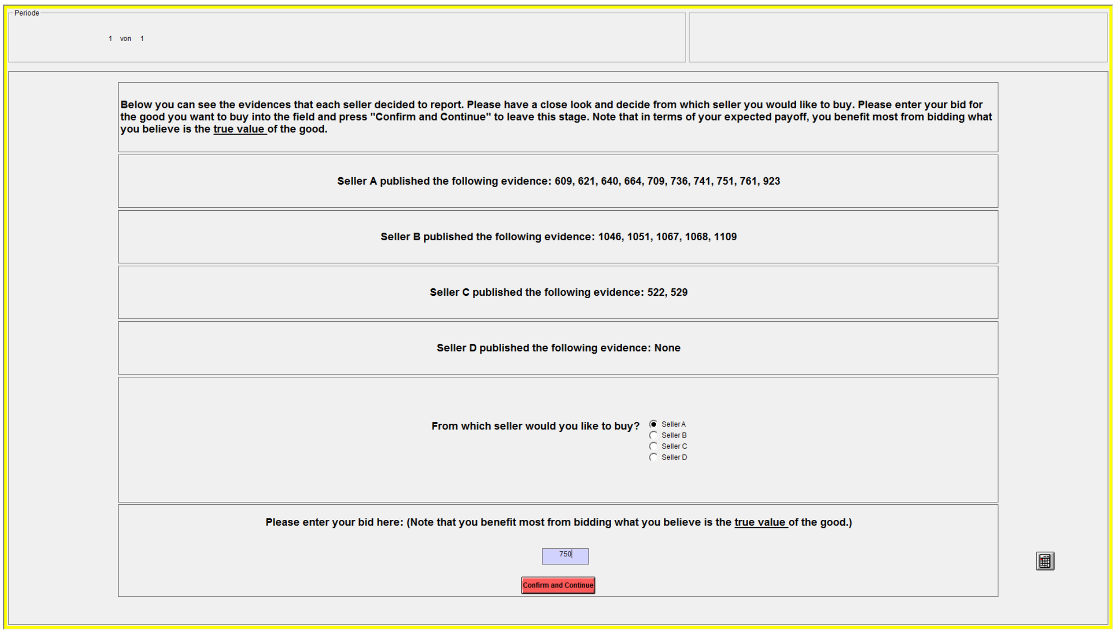


Figure A.4: Buyers' screen in C10.

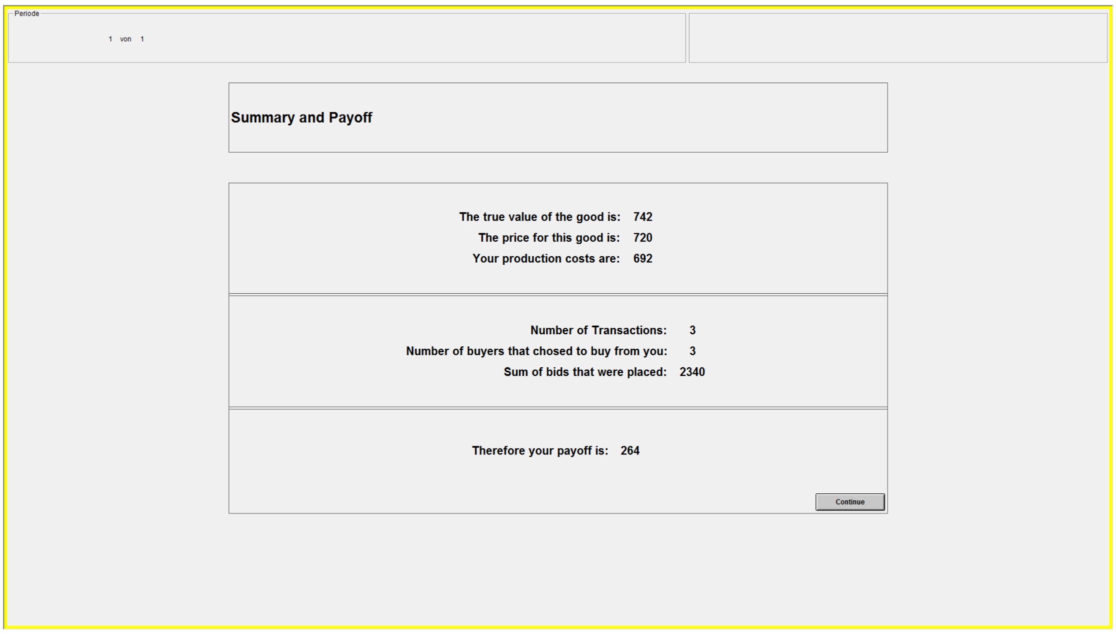


Figure A.5: Sellers' feedback screen in C10.

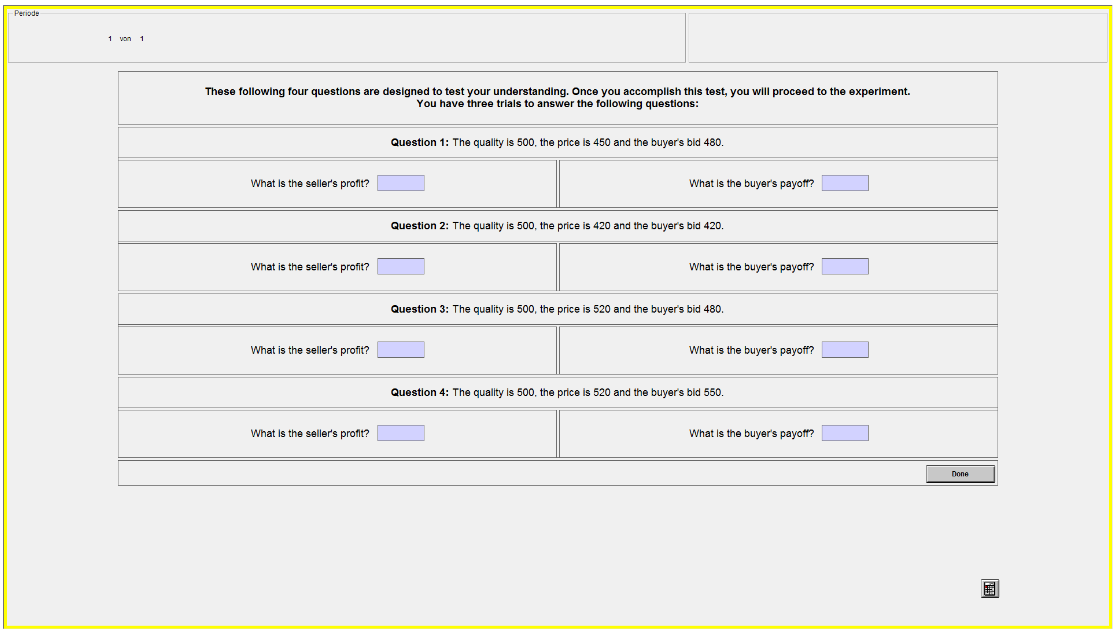


Figure A.6: Screen of the understanding test.

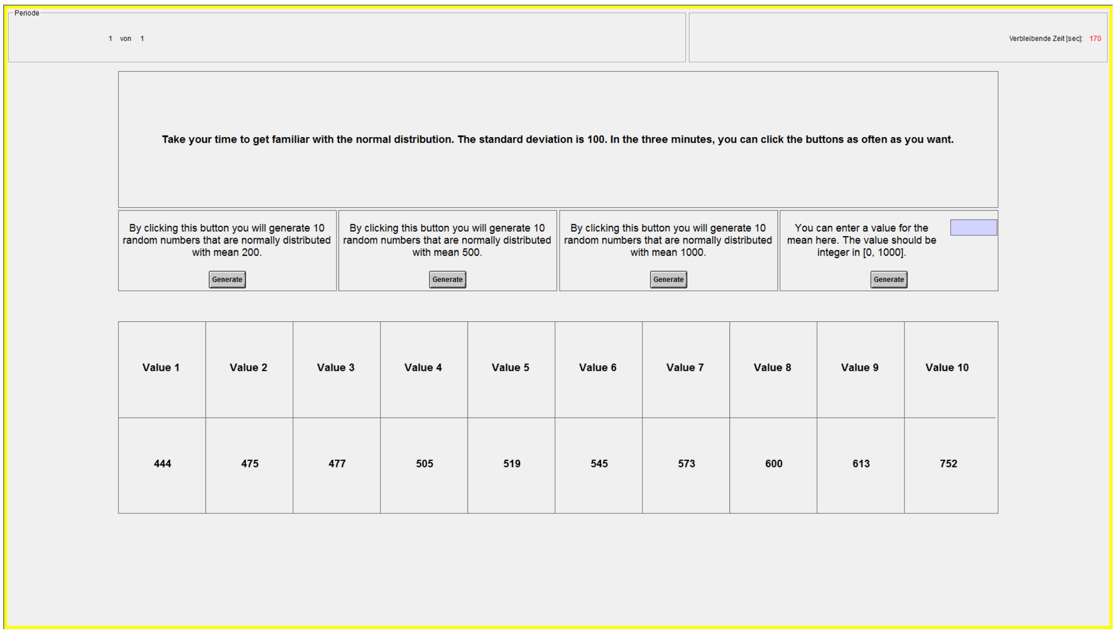


Figure A.7: Tool to generate 10 draws from a normal distribution $N(v, 100)$, $v \in [0, 1000]$. Here, $v = 500$.

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 (or 4) observations for M10/C10 (M10+5/C10+5). 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 4 and 5, respectively. While the level of significance is sometimes slightly reduced in particular for the X+5 treatments as there are only 4 session-level observations for theses treatments, we generally replicate our previous findings. There are, however, two exceptions. First, we are unable to establish that the the number of published evidences, $\#M$, increases from X to X+5 and that there is a significant difference in discrepancy between M and C. Regarding the latter 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 **??**. 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 7 with largely similar results, namely that we are not able to establish a lot of differences in payoffs. Finally, table B.5 reproduces table 3 again with qualitatively largely similar results, albeit with a slight reduction in the level of significance.

		All rounds			Rounds 5&10		
		M		C	M		C
(a)	10	4.71	**	6.18	4.65	**	6.25
Nr Published evidences		[1.00]		[0.45]	[0.91]		[0.16]
$\#M$	10+5	5.31	*	6.91	5.16	**	7.38
(b)	10	75.05	**	45.61	82.83	**	42.21
Publication bias		[1.00]		[0.83]	[0.83]		[0.39]
$\bar{e}^M - v$	10+5	75.40	**	43.15	77.35	*	47.24
N	10	480		480	96		96
	10+5	320		320	64		64

Notes: 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		
		M		C	M		C
(a)	10	67.30	***	13.78	67.68	***	9.60
Markdown		[0.67]		[0.52]	[0.67]		[1.00]
$\bar{e}^M - b$	10+5	73.23	*	19.28	72.67	**	16.44
$\neq 0$	10	**		[0.11]	**		[0.24]
	10+5	*		[0.14]	**		*
(b)	10	5.63	[0.52]	25.22	8.13	[0.35]	26.43
Discrepancy		[0.67]		[0.67]	[1.00]		[1.00]
$b - v$	10+5	1.63	[0.14]	19.14	1.30	[0.24]	28.31
$\neq 0$	10	[0.34]		**	[0.46]		[0.11]
	10+5	[0.71]		*	[1.00]		*

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

Characteristic x	C	C10	C10+5
		All rounds	
$\#M_j$	1.51***	1.40**	1.67*
$\bar{e}^M$	32.24	49.17	6.83
N	800	480	320
		Rounds 5&10	
$\#M_j$	2.04***	2.21**	1.79*
$\bar{e}^M$	64.99**	47.77	90.83*
N	160	96	64

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.

		(a) Payoffs			(b) Simulated payoffs	
		M		C	M	C
	10	40.49	[0.33]	42.70	48.11	49.68
Buyers		[0.66]		[0.32]		
(Benchmark: 50)	10+5	42.08	[1.00]	41.75	49.90	46.99
	10	58.73	[0.87]	59.26	31.04	27.84
Sellers		[0.13]		[0.28]		
(Benchmark: 25)	10+5	46.53	[1.00]	50.08	28.49	24.65
N	10	480		480	480	480
	10+5	320		320	320	320

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.

			X10+5	M10+5	C10+5
(a)	All rounds		33.13	31.88	34.38
				[0.67]	
Purchase +5	N		640	320	320
(%)	Rounds 5&10		39.84	35.94	43.75
				[0.72]	
	N		128	64	64
(b)	All rounds	+5	62.16	53.99	69.74
			**	[0.24]	**
Payoff		+0	41.44	43.04	39.78
	Rounds 5&10	+5	72.04	47.00	90.79
			*	[1.00]	**
		+0	38.19	45.76	29.58

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

Table B.5: Sellers' purchasing decisions and average payoffs.

B.2 Regression analysis

In table B.6 and B.7, we present fixed-effects panel regressions with standard errors clustered at the session level, taking into account variation at the session level.[15] These regression summarize how the behavior of sellers (table B.6) and buyers (table B.7) 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.6. 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.7 is significantly larger than the decrease of the publication bias in (4)/(5)/(6) of B.6.

The effect of the option to purchase additional evidences is positive but not significant on $\#M$ in (1) of table B.6. 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.7. 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.6, 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.7 indicates that the improvement in discrepancy due to learning seems not take place in competition.[16] 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 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.[17]

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

[16]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.

[17]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

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.[18]

Regressions (2)/(3)/(5)/(6) of table B.6 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.7 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.

second game after round 10 is captured by separate dummies.

[18]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.6: 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.7: Determinants of buyer behavior

C Comparison with stylized setting

In this section, we discuss in more detail the comparison with the simple, more stylized setting of Jin et al. (2021b). First, we elaborate on the relevant games studied (Section C.1). Second, we outline the related theoretical considerations (Section C.2). Third, we present the experimental procedures (Section C.3). Finally, we discuss some additional results (Section C.4).

C.1 Disclosure game (Jin et al. 2021b), variations and parameters

Jin et al. (2021b) study a disclosure game involving an information sender and an information receiver.[19] Their original setting is presented as Monopoly (M1) below. On top of that, we also study the effects of competition (C1) and of uncertainty about what the sender knows about the true state of the world (M0 & M+1).

Monopoly (M1) Our baseline game (M1) follows the parametrization of the original experimental game of Jin et al. (2021b). It considers a monopoly – i.e. a bilateral sender-receiver relationship – with one signal, the true value. More specifically, for each sender-receiver pair, the sender's true product value v (type) was drawn from $V = \{1, 2, 3, 4, 5\}$ with equal probability. While senders are aware of their value, receivers are not, at least ex-ante. Senders then decide whether to verifiably disclose their value to the receiver or not. Regardless of whether the value was disclosed to them or not, receivers then bid/guess b the value, with $b \in B = \{1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5\}$. This action is interpreted as the receiver's estimate of the sender's value. The true value and the receiver's bid determine the payoffs for both parties. The receiver payoff is $\Pi_R = 110 - 20|v - b|^{1.4}$. This payoff decreases strictly as the bid moves further from the value in either direction. The sender payoff is $\Pi_S = 110 - 20|5 - b|^{1.4}$. This payoff is independent of the value and increases strictly with the receiver bid. Because there are just a small number of states and actions, the payoffs are shown to participants in a table similar to table D.1, so that subjects do not need to know or interpret these functional forms. With these payoff functions, there is a clear misalignment of interests between senders and receivers.

Purchasing additional information (M0 and M+1) To study uncertainty about what the sender knows about the true state of the world in the simple environment, we propose two novel games. First, as a counter-point to M1, we study a game without any ev-

[19] We adjust their notation somewhat to align with our setting (e.g. value v instead of type b) but keep their framing as a sender-receiver game.

PAYOFFS:		Receiver's bid								
S, R		1	1.5	2	2.5	3	3.5	4	4.5	5
	1	-29, 110	-6, 102	17, 90	38, 75	57, 57	75, 38	90, 17	102, -6	110, -29
	2	-29, 90	-6, 102	17, 110	38, 102	57, 90	75, 75	90, 57	102, 38	110, 17
Sender value	3	-29, 57	-6, 75	17, 90	38, 102	57, 110	75, 102	90, 90	102, 75	110, 57
	4	-29, 17	-6, 38	17, 57	38, 75	57, 90	75, 102	90, 110	102, 102	110, 90
	5	-29, -29	-6, -6	17, 17	38, 38	57, 57	75, 75	90, 90	102, 102	110, 110

Table D.1: Payoff Table

idences (M0). In this game, as in M1, receivers face no uncertainty about what senders know, similar to our X10 games. In contrast to M1, senders are, however, not informed about the true state of the world. Consequently, while senders still decide whether to unbeknownly 'disclose' the true value to the receiver or not, their disclosure cannot depend on their value, as they are unaware of it. Hence, unlike in M1, senders cannot bias their disclosure. Taking M0 as a starting point, we then introduce uncertainty about the senders' knowledge (as well as the possibility of bias) in our last game M+1, similar to our X10+5 games. In M+1, senders are ex-ante unaware of their value but can purchase this information at a small cost of 10.[20] Importantly, to create uncertainty, as in our complex X10+5 games, whether senders indeed purchase this information or not is not communicated to receivers.

C.2 Theoretical considerations

Sender strategies and receiver types

In M1, the sender is informed about the true value v of the good whereas, ex ante, the receiver knows only the distribution of possible states. The sender has two possible actions, and the receiver is aware that these are the only two actions available to the sender. The sender can either report the state to the receiver or make no report. Thus, following the notation of section 2.2, the set of reports M available to a sender of value v is just $M(v) = \{v, null\}$. For every value v, the sender's strategy $\sigma(v)$ gives a choice of M.

Regardless of whether or not they receive a report from the sender, the receiver estimates (bids) $b \in B$ the value of the sender. The true value and the receiver's bid determine the payoffs for the two parties. Payoffs are such that the receiver benefits more from selecting an action that is closer to the true state, while the sender benefits the most when the receiver's action is as high as possible. There is a strong conflict of interest when the state is low. From the perspective of a receiver, disclosure $M = v$ provides perfect information of the state of the world. For non-disclosure, the sender's strategy

[20]The fee was chosen to be non-negligible but at the same time to be smaller than potential gains from selective non-disclosure with respect to unsophisticated receivers, see below.

$\sigma(v)$ induces a conditional probability distribution over the true value $f(v|M, \sigma)$ and an expected value $E(v|M, \sigma)$. With respect to the evaluation of these entities, we consider two different types of receivers, a *skeptical* receiver and a *naïve* receiver.

With disclosure, both *skeptical* and *naïve* receivers know the true state of the world and can bid it accordingly. However, a naïve receiver evaluates non-disclosure as an unselected, random representation of the possible true values. Irrespective of any strategy σ, such a receiver forms the expectation $\mathbb{E}(v|M = null) = 3$, the average of the randomly distributed value $v \in \{1, 2, 3, 4, 5\}$. This receiver is, thus, unconscious of the fact that non-disclosure may be selective on particular v values and, given the payoff structure, simply bids $b = 3$. Skeptical receivers are aware of a potential bias. They form the expected value corresponding to the sender's strategy $\mathbb{E}(v|M = null, \sigma)$ and bid accordingly, with $\mathbb{E}(v|M = null, \sigma) = 1 = b$ when the receiver e.g. beliefs that non-disclosure only happens for the lowest value. Compared to the naïve bid, the skeptical bid may thus feature a skepticism-induced markdown $S(M) = \mathbb{E}(v|M = null) - \mathbb{E}(v|M = null, \sigma)$.

Hypotheses

As shown before in Jin et al. (2021b), for M1, in every sequential equilibrium of the M1 game, the sender always reports the true state of the world unless it is the minimum element, and if there is no report, the receiver bids the minimum: senders disclose $v \in \{2, 3, 4, 5\}$, are indifferent for $v = 1$ and receivers match disclosed values and bid $b = 1$ for non-disclosure. The rationale behind this result is simple: receivers will treat all nondisclosing senders the same, so among those that do not disclose the one with the best v has an incentive to separate themselves through disclosure. Applied iteratively, this logic produces unraveling.

However, unraveling or always disclosing the information (with the potential exception of the lowest value), is not a best response towards naïve receivers, unaware of any selectivity in non-disclosure. More precisely, as long as $v < 3 = \mathbb{E}(v|M = null)$, non-disclosure will (strictly) benefit senders when interacting with naïve receivers whose 'optimal' strategy is bidding the unselected average value of 3 under non-disclosure. Hence: senders disclose $v \in \{4, 5\}$, are indifferent for $v = 3$ and do not disclose $v \in \{1, 2\}$ while receivers match disclosed values and bid $b = 3$ under non-disclosure.

We expect a mixture of *skeptical* and *naïve* receivers. Senders will, however, not disclose $v = 2$ (in addition to $v = 1$) when sufficiently many naïve receivers are present. Selective non-disclosure helps the sender to exploit the naïve receivers, eliciting a bid of $b = 3$ and ensuring comparably high payoffs ($\Pi_S = 57$), while disclosing $v = 2$ only moderately improves payoffs with skeptical receivers.[21]

Importantly, in M0, senders are unable to selectively disclose values und thus to bias their non-disclosure, simply because they are ex-ante unaware of their v. Here, $\mathbb{E}(v|M = null, \sigma)$ and $\mathbb{E}(v|M = null)$ coincide by design. Hence, both skeptical and naïve receivers are able to play a best response to non-disclosure, bidding the unselected average of 3. Given the payoff structure, where an average bid of 3 leads to slightly higher expected sender payoffs than when receivers, under disclosure, bid the values occurring with equal probability ($\Pi_S = 57$ vs. $\Pi_S = 49$), senders should not disclose in M0.[22] Based on the literature, we assume the presence of sufficiently many naïve receivers:

Hypothesis 3 (M1 & M0) *In M1, sender's non-disclosure exhibits bias and the average receiver's bidding strategy compensates for that incompletely. In M0, no such sender bias exists and receivers bid the average true value under non-disclosure more precisely.*

In M+1, the senders can effectively choose to play either M0 or M1, the latter being associated with a small cost c. While receivers are unaware which of the two games is actually played, their skepticism can still discipline senders, maintaining unraveling as an equilibrium outcome and removing incentives for senders to purchase information. Receivers match any disclosed value and bid skeptically $b = 1$ in case of non-disclosure, while senders disclose their information both when they know their value (strictly for $v \in \{2, 3, 4, 5\}$ while being indifferent for $v = 1$) and when not (where they cannot discriminate between v's).

Indeed, with skeptical non-disclosure bidding, it is optimal for senders to disclose with and without information (apart from being indifferent for the lowest value in case it is known to senders). This is true as receivers match any disclosed values, leading to strictly higher bids than $b = 1$ (apart from $v = 1$). In turn, under an unraveling disclosure strategy, skeptical beliefs about non-disclosure are correct/optimal. As either playing M1 or M0 lead to the same unraveling result, there is actually no benefit for the sender in purchasing information.[23]

[21] More precisely, disclosing elicits $b = 2$ instead of $b = 1.5$ from skeptical receivers, implying only a moderate increase in payoff: $\Delta\Pi_S = 17 - (-6) = 23$.

[22] Notably, disclosing values leads to higher overall payoffs as receiver can bid the value precisely. Social preferences could also make this behavior optimal from the sender perspective.

[23] Indeed, purchasing information would only make sense for senders if they are able to disclose their acquired information only selectively to regain the cost of purchase. Sophisticated receivers, however, will anticipate selective disclosure. Similarly, non-disclosure without purchase and bidding the average cannot be part of an equilibrium strategy, as it creates incentives to purchase and selectively disclose.

With only naïve receivers, senders can exploit these receivers by purchasing information as long as the cost is not too high. Specifically, receivers match any disclosed value and bid $b = 3$ in case of non-disclosure, while senders disclose their information selectively (they strictly disclose only $v \in \{4, 5\}$) under purchase, do not disclose under no purchase, but actually purchase the information.

Indeed, with acquired information, selective non-disclosure (specifically not disclosing $v = 1$ and $v = 2$ and eliciting a bid of $b = 3$ while eliciting a matching bid otherwise) leads to an average sender gross payoff of $\Pi_S = 75.4$[24] at a purchasing cost of $c = 10$. Without information, the best average payoff is just $\Pi_S = 57$ (eliciting bidding the average of 3 in response to non-disclosure). Thus, acquiring information and selectively disclosing them to naïve receivers is profitable under the parameters of our experiment.

When considering a mixture of naïve and skeptical receivers, purchasing information can only be optimal when the fraction of exploitable naïve receivers is high enough. Skeptical receivers understand the induced bias in non-disclosure, bid accordingly and thus void any potential gains when senders interact with them.[25] This is similar to the scenario discussed for M1. Assuming sufficiently many naïve receivers to justify the purchase decision:

Hypothesis 4 (M+1) *In M+1, senders have an incentive to purchase the information about their value. Compared to M0, a bias is reintroduced in non-disclosures and (naïve) receivers are harmed as they overestimate the true value under non-disclosure (similar to M1).*

Strictly speaking, with sufficiently many naïve receivers, senders should always purchase the information about the true value. Consequently, behavior in M+1 should closely mirror behavior in M1. However, due to the uncertainty regarding the number of naïve receivers and the limited number of rounds, we might expect a lower frequency of information purchases. This would imply a weaker bias, making it interesting to consider the relative overestimation – specifically, how much receivers overestimate the true value under non-disclosure relative to the bias induced by senders.

C.3 Experimental procedure

For the comparison with the simple setting, we run sessions with a protocol similar to the M-extension: Sessions were conducted as an online experiment in Mannheim

[24] $\Pi_S = (57 + 57 + 57 + 90 + 110)/5 = 75.4$.

[25] In this case the gross payoff is $\Pi_S = (-6 - 6 + 57 + 90 + 110)/5 = 49$.

(MA) in April 2024. 89 subjects participated.[26] All sessions featured a sequence of 3 games with each game occurring once in each sequence position following a latin square design, see table D.2. Instructions are provided in Appendix D.4. As Sheth (2021) already implemented competition, our sessions focused on the two relevant games about uncertainty (M0 and M+1) as well as the replication of the original sessions (M1). Each game features 5 rounds. While Jin et al. (2021b) implemented much longer interaction, our aim was to create behavior in an environment similar to our complex setting.

Comparison Sessions			
Session	Sequence position		
MA	1	2	3
27	M1	M+1	M0
28	M0	M1	M+1
29	M+1	M0	M1

Table D.2: **Comparison** sessions: Games in comparison sessions 27-29.

C.4 Extended results

	M1	M+1		M0
		Purchase	No purchase	
Sender		*60%*	*40%*	
Disclosure (value = 1)	0.05	0.05	0.31	0.53
Disclosure (value = 2)	0.24	0.12	0.19	0.44
Disclosure (value = 3)	0.61	0.46	0.13	0.34
Disclosure (value = 4)	0.84	0.92	0.22	0.23
Disclosure (value = 5)	0.90	0.96	0.32	0.38
Value under non-disclosure	2.05	2.00	3.13	3.12
		2.57		
Receiver				
Estimate (disclosure = 1)	1.00	1.25		1.43
Estimate (disclosure = 2)	2.00	2.14		2.09
Estimate (disclosure = 3)	3.10	3.23		3.31
Estimate (disclosure = 4)	4.00	4.02		3.86
Estimate (disclosure = 5)	4.68	4.87		5.00
Estimate under non-disclosure	2.69	2.78		2.96
Overestimate	0.64	0.21		-0.18
Overestimate $\neq 0$ (p-value)	0.001	0.086		0.172

Table D.3: Sender and receiver behavior – M1, M+1, M0

[26] We scheduled 3 sessions comprising 32 subjects but experienced a few no-shows. In addition, we exclude one subject due to connection issues.

Table D.3 replicates table 8. Behavior in the original setting, M1, resembles the findings of Jin et al. (2021b) fairly closely: Unraveling does not occur; a value of 2 is reported only 24% of the time, rather than the predicted 100%, while higher values are frequently reported. This implies that senders bias their non-disclosure (average $2.05 < 3$, $p < 0.001$; all tests are two-sided t-tests). Receivers reasonably match the reported values, but significantly overestimate by 0.64 ($p = 0.001$) when no report is provided, indicating that they do not fully account for the sender's bias. In other words, receiver naivety offers an explanation for why senders avoid disclosure. In M0, senders tend to disclose less frequently than in M1 (38% vs. 53%). However, if they disclose, their disclosure is by design largely unbiased ($3.12 \sim 3$, $p = 0.172$) and generates no overestimation (-0.18, $p = 0.172$). This implies that without the possibility of biased disclosure, receivers behave largely as predicted.

In M+1, in approximately 60% of the cases senders choose to purchase, influencing their disclosure patterns to closely resemble those in M1. Indeed, purchasing senders' bias their non-disclosure (similar to M1) while others cannot (as in M0). From the receivers' perspective, who are unaware of whether the information was purchased, an undisclosed value implies a notable bias ($2.57 < 3$, $p = 0.001$), although it is less pronounced than in M1 ($p = 0.001$). Receiver tend to account for this bias, albeit somewhat incompletely, resulting in a slight overestimation of the value by 0.21 ($p = 0.086$). They seem to be alerted to the risks of uncertainty, as their estimate is lower than in M0 ($2.78 < 2.96$, $p = 0.016$). In addition, their overestimation is lower than in M1 ($0.64 > 0.21$, $p = 0.026$) and this appears even to be true relative to the induced bias ($M + 1 = 0.43$, $M1 = 0.95$). Specifically, the overestimation is approximately only half of the bias in M+1 but two-thirds of the bias in M1. These observations suggest that receivers are, if anything, better able to manage the situation. Their alertness is further supported by the fact that receivers believe that purchases happen 53% of the time, signaling that they do not have a perfect but an adequate understanding of the fact that purchases are a non-negligible phenomenon. Further, receivers earnings are also not significantly lower ($p = 0.262$) in M+1 (89.67) when uncertainty is introduced compared to M0 (93.50) (or to M1: 86.91, $p = 0.116$).[27]

All these evidences provide a first indication that naive receivers may not be harmed as much by uncertainty as predicted because introducing the sender's opportunity to purchase may highlight potential risks.

[27] Unsurprisingly, senders earn somewhat less in M+1 (53.03) compared to M1 (60.40, $p = 0.027$). Interestingly, however, this difference seems to be mainly driven by the lack of selection of those not purchasing (47.15) compared to those incurring the cost of purchasing (57.03). Finally senders in M+1 earn more than those in M0 (49.80, $p = 0.002$).

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

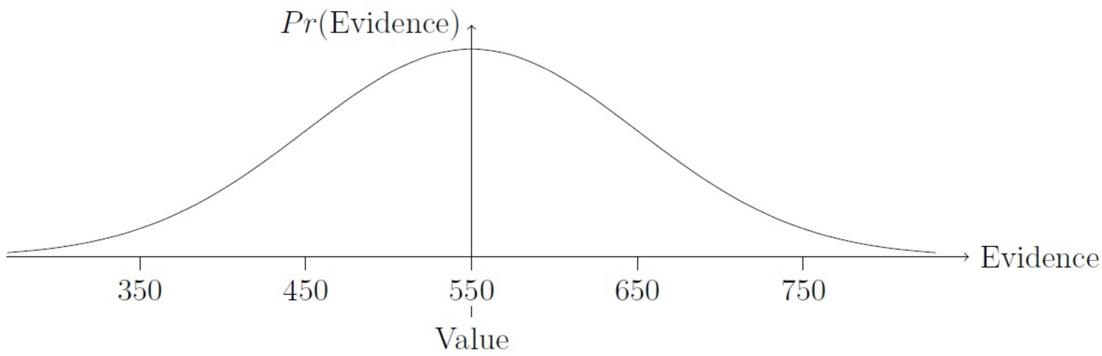


Figure E.1: Normal probability distribution with a Value of 550 and a standard deviation of 100, indicating the probability of Evidences.

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.

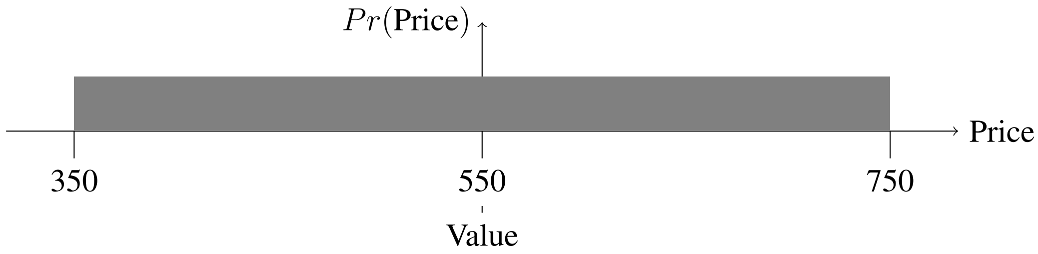


Figure E.2: Price distribution around the Value of 550.

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.

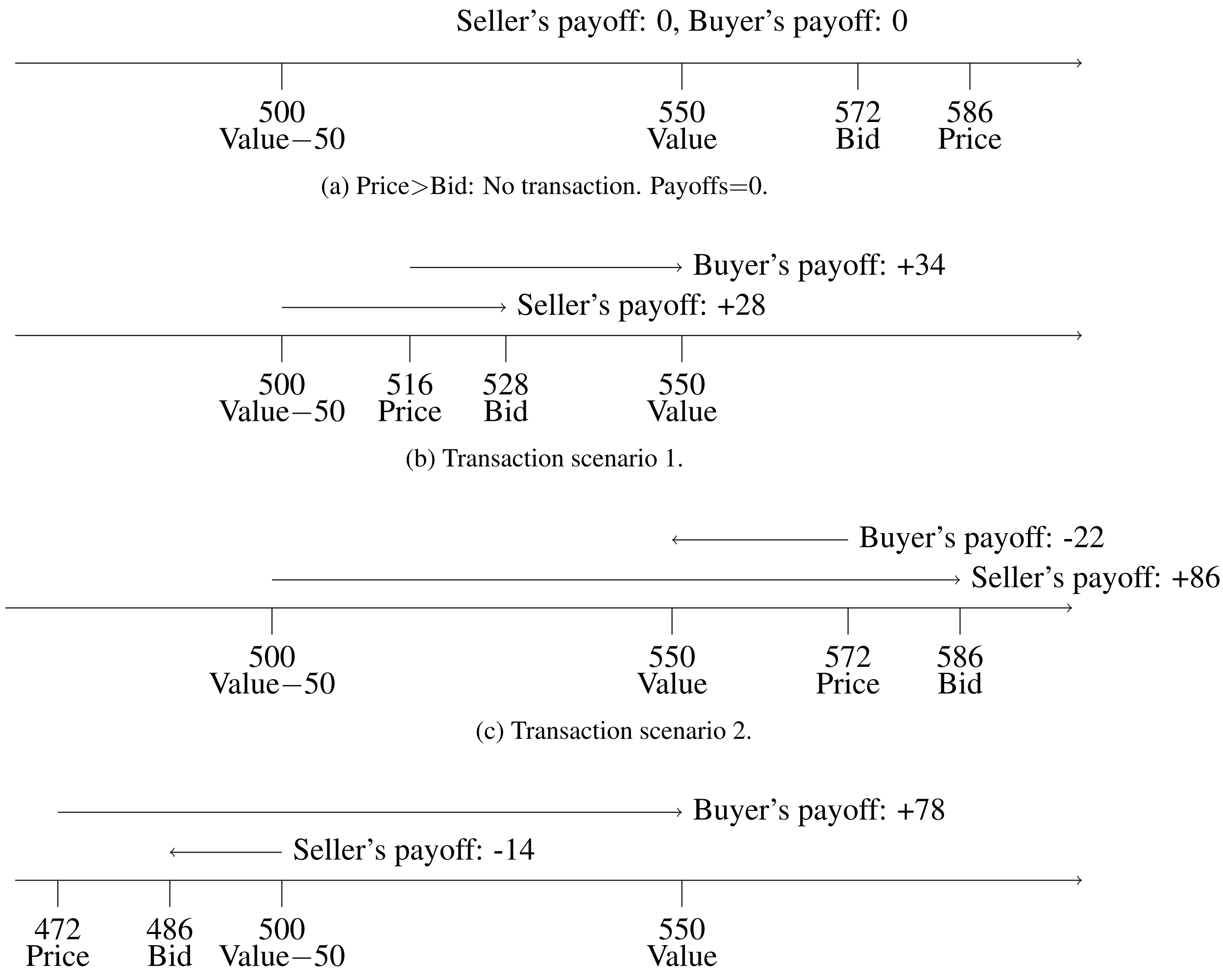
In order to participate in the experiment, you will go through a brief understanding test. Here and throughout the experiment, you can access a calculator via a button in the right bottom corner of your screen. Once everybody accomplishes this test, you can get more familiar with the normal distribution 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

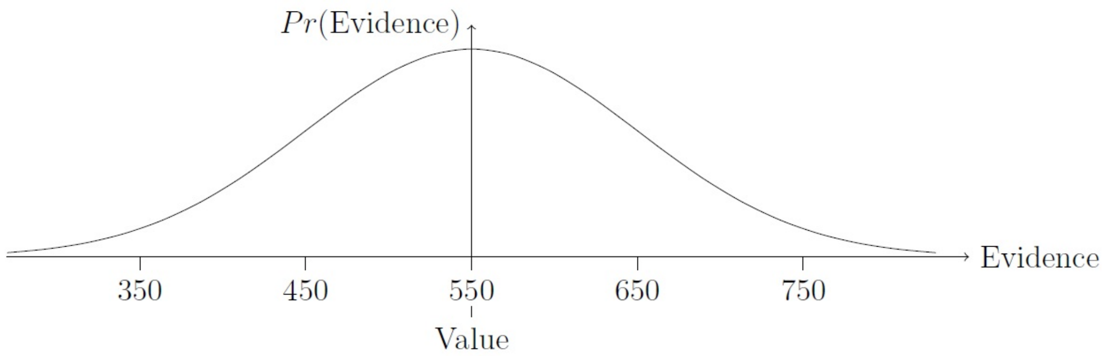


Figure E.4: Normal probability distribution with a Value of 550 and a standard deviation of 100, indicating the probability of Evidences.

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.

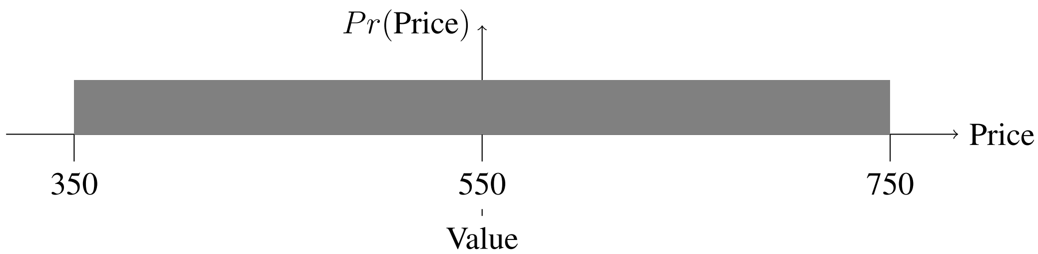


Figure E.5: Price distribution around the Value of 550.

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:

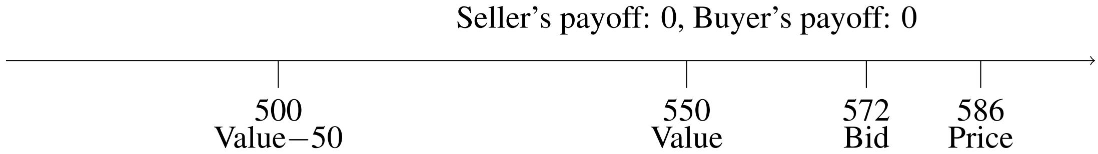
$$\textbf{Payoff}_{\textbf{Buyer}} \textbf{ = Value} - \textbf{Price.}$$

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.

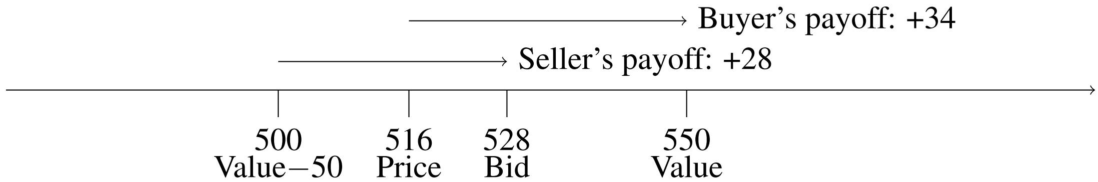
Part I of this experiment consists of 2 practice rounds and 2 blocks of 5 experiment rounds. At the beginning of each round, you will be informed about the randomly chosen role (Seller or Buyer) that you take. You keep this role in the first block of 5 rounds, and take on the other role in the second 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.

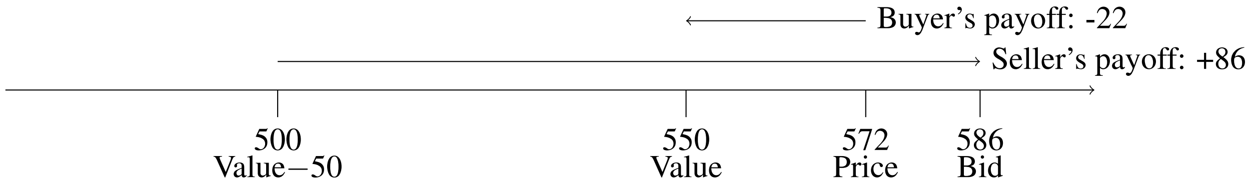
Are there any questions? If not, please turn to your screens and follow carefully the instructions there.



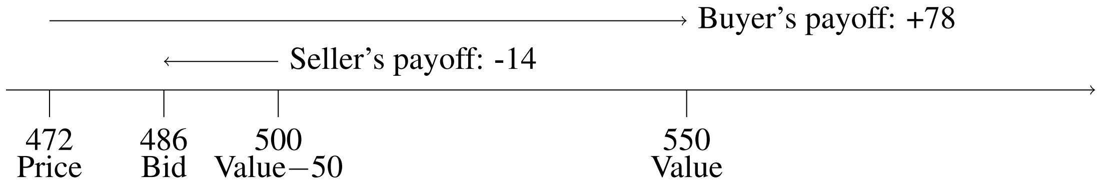
(a) Price>Bid: No transaction. Payoffs=0.



(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.

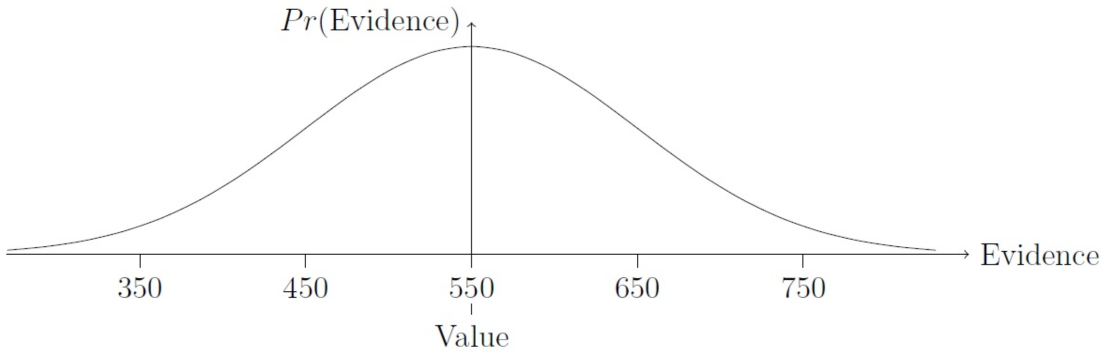


Figure E.7: Normal probability distribution with a Value of 550 and a standard deviation of 100, indicating the probability of Evidences.

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.

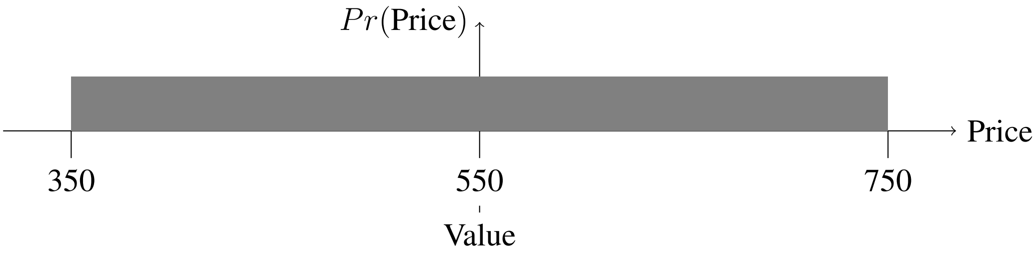


Figure E.8: Price distribution around the Value of 550.

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:

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

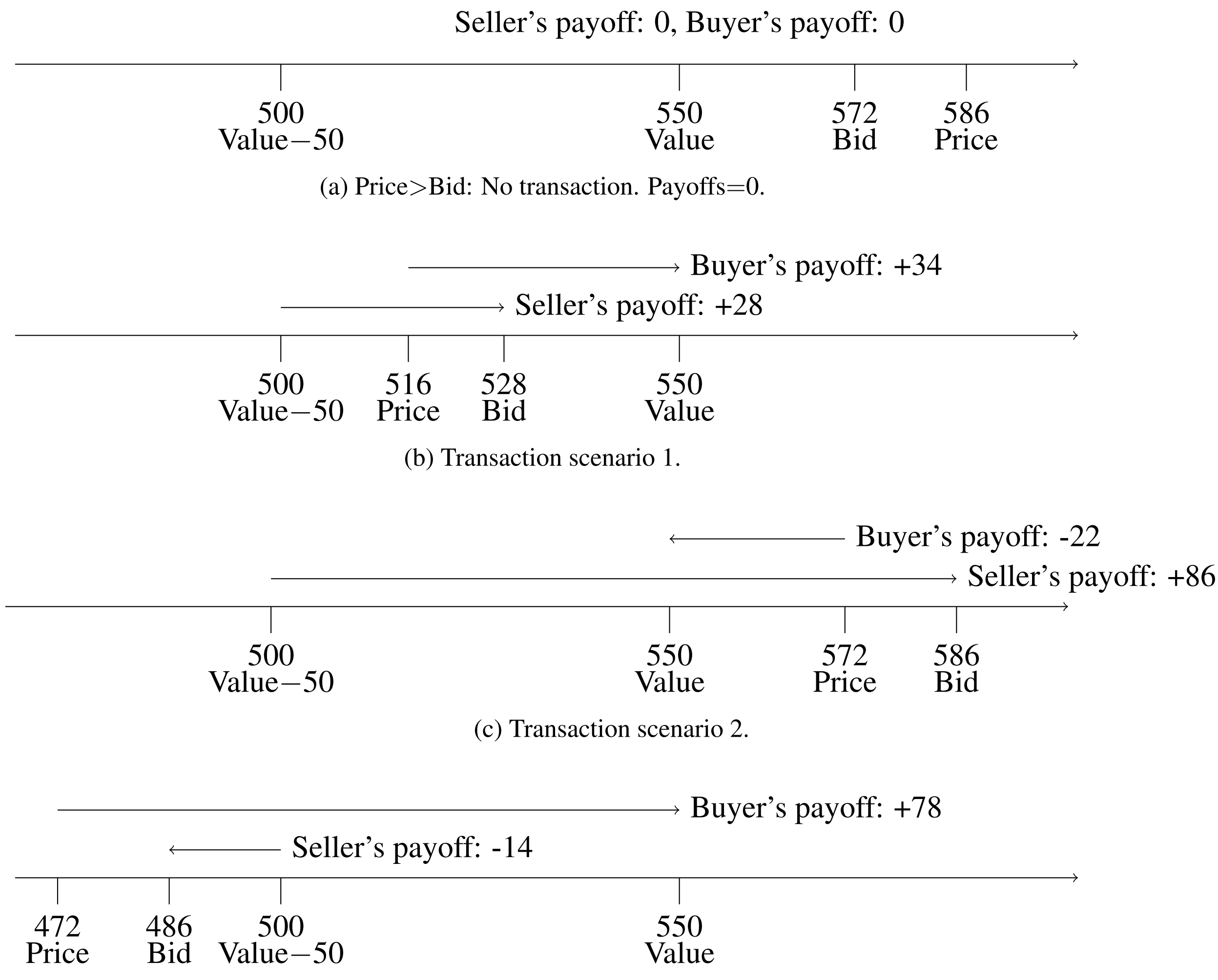
The Buyer's payoff is the true Value of the good minus the Price:

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

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 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 [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.

D.4 Instructions for M1, M+1, M0

Below we present the instructions for following sequence: M1, M+1 and M0. The table to which the instructions refer is basically the same as table D.1.

You are about to participate in an experiment in the economics of decision making. 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 after today's session either via bank transfer or paypal.

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 100 points = 1 Euro. But no matter what your payoffs are in the experiment, you will be paid at least 5 Euro (if you continue until the end).

During the experiment, you will at times have to wait for the action of others. Please stay in the experiment until the end, as we will not be able to make a payoff to you otherwise.

The experiment consists of **three parts** (Part I, Part II, Part III and a final questionnaire) which are independent of each other. Each of these parts, in turn, consists of up to 7 rounds. Please click "next" to read the instructions for the first part.

Part I

At the beginning of the experiment, some of you will be randomly assigned to be the S Player and the others to be the R Player. You will remain in these roles for the entire duration of this experiment. In each round, an R Player will be randomly matched with an S Player and in each new round, the R Player will be matched with a different S Player.

In each round and for every pair, the computer program will generate a **secret number** that is randomly drawn from the set $\{1, 2, 3, 4, 5\}$. The computer will then disclose this secret number to the S Player. After receiving this number, the S Player will have to **choose whether or not** the secret number is also *revealed* to the R Player. If the S Player chooses to reveal the number, the R Player will receive this message: "The secret number is" followed by the actual secret number. Otherwise, the R Player will receive a message that "The secret number was not revealed".

After seeing either of the two messages, the R Player will guess the value of the secret number. The earnings of both players depend on the value of the **secret number** and the R Player's **guess**. The specific earnings are shown in the table below, which is **displayed again** before the S Player and R Player make their choices. In each cell of the table, the payoff for the S Player is on the left, and the payoff for the R Player is on the right. As you can see from the table, the S Player earns more when the R Player makes a **higher guess**, and the R Player earns more when their guess is **closer** to the secret number.

Part I consists of *2 practice rounds* and *5 experiment rounds* that are relevant for your payoff. Please click "Next" to continue (the table below will be displayed again).

Part II

In Part II, you will be confronted with a situation very similar to the one before. Notably, however, there will be one **important deviation**. The secret number will NOT be automatically disclosed to S Players. Instead, S Players can decide whether to **purchase this information** at a small cost of 10 points or not. S players will still have to **choose whether or not** the secret number is *revealed* to the R Player (by the computer). However, they can only make this decision based on what the **secret number actually** is if they opt to purchase this information. Otherwise, S Players will have to make this decision "blindly" and will discover the secret number at the end of the period. (Notably, the R Player will NOT learn whether the information was purchased or not.)

As before, the R Player will guess the value of the secret number (after seeing either a message that reveals the secret number or one that does not). Again, the S Player earns more when the R Player makes a **higher guess**, and the R Player earns more when their guess is **closer** to the secret number. Part II consist of *5 experiment rounds*. Please click "Next" to continue.

Part III

In Part III, you will be confronted with a situation very similar to the one before. Notably, however, there will be one **important deviation**. The secret number will NEITHER be automatically disclosed to S Players NOR will there be an opportunity to purchase this information. S Players will still have to **choose whether or not** the secret number is *revealed* to the R Player. However, S Players have to make the decision "blindly" and will only discover the secret number themselves at the period's end, once all decisions have been made.

As before, the R Player will guess the value of the secret number (after seeing either a message that reveals the secret number or one that does not). Again, the S Player earns more when the R Player makes a **higher guess**, and the R Player earns more when their guess is **closer** to the secret number. Part III consist of *5 experiment rounds*. Please click “Next” to continue.