

Vacuous information affects bargaining

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Abstract

We introduce vacuous information into buyer-seller ultimatum exchanges and provide evidence that it can affect bargaining outcomes. Notably bargaining behavior is affected asymmetrically in a way that leads allocative efficiency to respond negatively. Our results are consistent with self-serving bias that commands players to interpret vacuous information in a self-serving way.

JEL codes: C90 (Design of Experiments: Group Behavior), C78 (Bargaining)

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1 Introduction

The commonly known context of many real-life bargaining situations provides vacuous information that might favorably bias the bargaining outcome. Collective bargaining agreements of labor and trade unions in some industry are one case in point. The bargaining outcome in a situation of prolonged wage cuts observed in other sectors of the economy might differ from an agreement reached when having witnessed a series of wage increases. Bargaining about a no-bailout policy of the European Union is another example. It seems very different in the context of a few of its member countries being on the brink of insolvency as suggested by casually comparing the bargaining in the wake of the 1992 Maastricht Treaty to that in the years 2010 and 2011 with Greece, Ireland, and Portugal in financial troubles. Similarly, but standing out less, vacuous information regarding the circumstances of sellers and buyers in individual trade conversations, e.g. the availability of outdated price-tags, might change the bargaining outcome.

The purpose of this paper is to introduce the notion of vacuous information and to systematically explore if vacuous information that derives from the pure context of a bargaining situation has the power to shape bargaining outcomes. We define commonly known information to be ‘vacuous’ if its variation neither changes the strategy set nor the payoffs of any bargaining party. This notion of vacuous information differs from cheap talk since the latter is actively communicated while the former is common knowledge. Further, cheap talk about vacuous information is excluded since it requires the strategy set to change. Formally, vacuous information only adds to the set of common knowledge of the original game and we think of vacuous information as additional information about a game’s context.

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To generate data of bargaining situations identical except for the instance of vacuous information, we conduct controlled experiments. Specifically, we utilize buyer-seller ultimatum exchanges. There are two advantages to using a variant of the ultimatum game. Firstly, it allows to compare our results to a multitude of available studies of the differential aspects of bargaining widely studied using this game. For a survey of factors affecting ultimatum bargaining see Handgraaf, Van Dijk, and De Cremer (2003).¹ Secondly, it provides a reasonable test of the power of vacuous information due to the importance of strategic considerations as a primary decision motive in the ultimatum game: e.g., Charness and Gneezy (2008) vary social distance by revealing the last name of bargaining partners and report that proposer behavior in the ultimatum game is robust to this variation while changing in the non-strategic dictator game.

Related research embedded the stylized bargaining situation in a broader strategic environment that provides context of the bargaining situation. In one study Schotter, Weiss, and Zapatero (1996) turned an otherwise standard ultimatum game into a survival stage for proposers. Only these proposers with earnings above the median had the opportunity of playing another ultimatum game with a new set of responders. The study finds that the need of proposers to survive one bargaining game with a high payoff to have the opportunity of participating in another bargaining game seems to serve as a justification for proposers to make higher demands. In another study, Gächter and Riedl (2005) introduce bargaining-with-claims experiments and find claims inherited from an earlier experimental stage that were foregone subsequently by chance to affect freeform bargaining. This evidence shows that context derived from another part of the game, either already played at the time of bargaining or anticipated before being played, can matter for bargaining behavior. Importantly, the observed bargaining behavior in these studies may respond to factors related to some part of the game that is actually played either before or after encountering the bargaining situation. This provides subjects with either game history and particular experiences of playing or with additional strategic effects tied to the bargaining action. As a result it is difficult to disentangle pure context effects from factors connected to preceding or anticipating actual play. In contrast, in our research we intend to explore if the provision of pure context, i.e. vacuous information that is not derived from actual play, is sufficient to affect bargaining.

For incorporating vacuous information into the ultimatum exchange we use outdated price-tags that are exogenous at any point in the experiment and do not affect the payment of any subject. The usage of outdated price-tags offers the advantage of providing vacuous information that relates to the encountered bargaining situation of fixing a price in a direct and concise way. Importantly it easily lends itself to the systematic variation of the content of vacuous information by simply manipulating the outdated price-tag's value. This manipulation can be viewed as a way of 'anchoring' the bargaining situation. Ariely, Loewenstein, and Prelec (2003) reported that subjects' willingness to accept hearing an unpleasant noise changes with an anchoring price. While Ariely, Loewenstein, and Prelec (2003) investigate the effects of anchoring for individual choice situations, we study a two-person game. To this end, we provide evidence on how anchoring effects can extend to situations of choice with strategic interaction; e.g., would anchoring effects apply to all players in a similar way?

Since vacuous information augments the description of a game, it qualifies as framing in the sense of Camerer (1997, p.172) who broadly refers to a behavioral effect as a framing effect "when a difference in description does cause behavior to vary." For pure coordination games, van Huyck, Gillette, and

¹More recently Grimm and Mengel (2011) and Sutter, Kocher, and Strauß (2003) provide evidence that delay and time-pressure influence bargaining. Fellner and Güth (2003) study threat power in an ultimatum experiment. Oosterbeek, Sloof, and van de Kuilen (2004) explore cultural differences in a meta-study of ultimatum games. A gender effect is reported by Eckel and Grossman (2001), see also Sutter, Bosman, Kocher, and van Winden (2009).

Battalio (1992) have shown that vacuous information can serve as an important cue for coordinating play.² More generally there is evidence that framing can change behavior and beliefs in situations of strategic interaction beyond coordination games.³ There is, however, also evidence that framing does not always affect behavior,⁴ so that it is a priori not obvious if the manipulation of vacuous information yields behavioral effects or how these would look like.

Anticipating results we find that outdated price-tags affect bargaining behavior in an asymmetric way. While some proposers behave as if legitimating higher asking prices by higher outdated price-tags, strikingly, the behavior of responders does not respond at all. As a result allocative efficiency responds strongly to the asymmetric change in behavior. Specifically efficiency drops from 65.3% with a low outdated price-tag to 46.0% with a high one.

The paper proceeds as follows: Section 2 describes our experimental design and section 3 presents our results. Finally, in section 4 we summarize our findings and discuss them.

2 Experimental design

Implemented game: We introduce an outdated price-tag into a variant of the ultimatum game that is presented in a market context as a buyer-seller exchange similar to Hoffman et al. (1994). In any pair of subjects, one subject is assigned the role of a seller with a reservation price of €0 for some fictitious object. The other subject is assigned the role of a buyer with a reservation price of €10. First, the seller suggests a price $p \in \{\text{€}0, \text{€}1, \text{€}2, \dots, \text{€}10\}$ for which he is willing to sell the object. Second, the buyer either accepts or rejects the price. If the buyer accepts it, there is a trade and the accepted price determines the monetary payoff distribution. The seller's payoff is p and the buyer's payoff is $10 - p$. If the buyer rejects, there is no trade and the monetary payoff of either party is zero.

Strategy method: To obtain more data about responder behavior we used the strategy method, specifically, responders had to indicate the largest price they were willing to accept. In general the strategy method may or may not affect behavior.⁵ In a large study on the ultimatum game, however, Oxoby and McLeish (2004) find no significant differences when comparing data generated with the strategy method to data collected with sequential decision making where the responder is informed about the particular offer of the proposer before accepting or rejecting the offer.

Treatments: For introducing vacuous information into the market exchange, we focus on the exogenous outdated price-tag ρ as the main treatment variable. Since we also want to study the extent to which content variations of vacuous information affects behavior, we investigate different levels of ρ . In one treatment we set $\rho = \text{€}5$ to represent the most common surplus distribution observed in standard ultimatum games where the mode of offers typically is at or around the fair split. In two other treatments, the values of ρ were set to represent the polar surplus distributions such that either party would have obtained maximum surplus while the other one would have obtained strictly

²Van Huyck (1992) provide their subjects with written recommendations of playing a strategy profile assigned by the experimenter in 3x3 two-player coordination games.

³E.g., Dufwenberg, Gächter, and Hennig-Schmidt (forthcoming) study framing in a public good game and show that contributions as well as beliefs about the contributions of other players and the beliefs about the beliefs of others contributing to the public good can be affected by framing. Hoffman, McCabe, Shachat, and Smith (1994) found that implementing the ultimatum game as an allocation equivalent market exchange significantly reduces offers to second-movers while Larrick and Blount (1997) report that second-movers significantly accept more often lower offers when first-movers and second-offers sequentially announce individual claims from the available common resource.

⁴Abbink and Hennig-Schmidt (2006) compared the behavioral effects from using loaded instructions instead of neutral instructions in a bribery game and found that framing did not affect behavior.

⁵E.g., Brandts and Charness (2000) do not find significant differences when varying the elicitation method, while Brosig, Weimann, and Yang (2003) report differences in another experiment; Schotter, Weigelt, and Wilson (1994) show that the presentation of a game in either normal form or extensive form can affect choice behavior. Roth (1995, pp.322-323) briefly discusses the strategy method. For a recent survey see Brandts and Charness (2011).

positive minimum surplus, i.e. $\rho = \text{€}1$ or $\rho = \text{€}9$. In short, we investigated three levels of the treatment variable— $\rho \in \{\text{€}1, \text{€}5, \text{€}9\}$ —one per treatment. These three treatments differ only in the exogenous outdated price-tag ρ at which the seller acquired the object before subjects were assigned roles. The instructions⁶ include a comment about the circumstance of the fictitious object for sale that was irrelevant to the monetary payoff: “It is a stamp that he (the seller) bought for ρ a while ago.”⁷ Importantly, the instructions inform subjects about the determination of individual monetary payoffs, and in these ρ does not affect payoffs. We used the commodity of stamps instead of an abstract fictitious commodity to make the experimental situation more tangible and, hence, accessible to our subjects as these were first-year, first-semester undergraduate students.

We ran two standard ultimatum games as controls that allow us to assess the internal validity of our experimental design. The subject pool used for control treatment UG06 was independent of the one used for control treatment UG07. The composition of both subject pools is similar as they represent two student cohorts during the first semester of their undergraduate studies. Since the experiment was integrated into a first-year course, any student of the cohort of 2007 could participate in the control treatment UG07 (almost everyone did) and in one of the outdated-price treatments conducted four weeks later. It was not possible to exclude subjects based on prior experience. In particular, 85%, 87%, or 89% of the subjects in the control treatment UG07 participated in outdated-price treatment A, B, or C, respectively. We check for experience effects by including a dummy in any of the regression specifications reported below. Anticipating results, however, none of the dummy coefficient’s estimates is significant in any of the regressions except on one occasion, so that there seem to be no substantial behavioral effects related to experience.

Subjects: We vary treatment conditions between-subjects. Each subject participated in a treatment with outdated price-tags only once, either as a buyer or as a seller. Similar to Fellner and Güth (2003) we implemented the experiment in a classroom setting to obtain a very large amount of data. In total, 923 students participated in the experiment. It was integrated into the first-year, first-semester undergraduate course “Economics and Business” as a classroom experiment at Maastricht University. Data were collected in 37 tutorial group classes on October 31, 2007 (UG07, 453 students) and November 30, 2007 (outdated price-tag treatments, 355 students). Data for the control treatment UG06 was collected in 39 groups with 423 students on October 31, 2006. No class size exceeded 14 students. In any class, the teaching assistant read aloud the same set of instructions that was also projected to the screen. All displayed instruction slides were the same across classes except for the value of the outdated price-tag. Participation in the experiment was voluntary, however, there was no student declining to participate. Table 1 provides details on the number of independent observations by treatment and role.

treatment	outdated price-tag ρ	presentation	# of sellers (proposers)	# of buyers (responders)
Low	€1	market UG	63	58
Med	€5	market UG	63	64
High	€9	market UG	54	53
UG06	none	standard UG	219	204
UG07	none	standard UG	234	219

Table 1: Treatment information and independent observations by role

⁶See appendix A for the instructions and appendix B for the record sheets used in the experiment.

⁷In the original instructions, the particular treatment value substitutes ρ .

Payment: For each treatment, one seller form and one buyer form were randomly drawn from all collected forms during the lecture. If the buyer accepted the suggested price, the corresponding students were paid out their payoffs in cash. Those students that earned a cash payoff could choose to either immediately and publicly receive the payment in the lecture or to privately collect the payment later at the chair’s office.

3 Experimental results

It is possible that the data generated in our experiment is affected by a few unusual aspects of our design as compared to standard laboratory data; e.g., subjects faced rather low monetary incentives, the experiment is conducted in a classroom setting (with not more than fourteen non-communicating students per classroom, however), or the procedural rule that payment takes place in public in the lecture one week later. We check for the internal validity of our experiment in two ways. Firstly, we verify that our experimental design allows for non-arbitrary and stable data generation in a well studied setting. For that we collect data for the standard ultimatum game in one year (UG2006) and then repeat it with new subjects one year later (UG2007) where the experimental procedures are identical to those used for our main treatments. Secondly, we compare the standard ultimatum game data obtained with our experimental protocol to these of Roth et al. (1991) to see if our design introduced any sort of bias. Only after we have addressed these two concerns on stability and potential bias of the generated data, we present our results.

3.1 Internal validity

To verify that our experimental design allows for the generation of data in a stable way in our setting of the ultimatum game, we compare the data of two standard ultimatum game experiments collected for two different cohorts of students. The top panel of Figure 1 shows the first-mover data of both control experiments. As can be seen from the figure, the frequency distributions seem rather similar suggesting that our experimental procedures allow to measure proposer offers in a meaningful way. This assessment is supported by data analysis. Neither the Kolmogorov-Smirnov-test (two-tailed, $p = 0.981$), the t -test (two-tailed, $p = 0.196$), nor the Mann-Whitney- U -test (two-tailed, $p = 0.383$) reveal any statistical difference. Similar results of insignificant differences emerge for the distribution of second-mover data on the lowest accepted offer (KS: $p = 0.882$; t -test: $p = 0.490$, MWU: $p = 0.487$).

Next we check if our experimental procedures yield patterns of data that are different from those typically observed in ultimatum game experiments with standard laboratory procedures. As benchmark data we use the US data with a pie size of \$10 of Roth et al. (1991). These data were also used by Costa-Gomes and Zauner (2001) who provide discretized frequency distributions of the Roth et al. (1991) data.⁸ One advantage of this dataset is the large number of observations, $n = 270$, as compared to, e.g., only $n = 24$ in Forsythe et al. (1994). A downside of the Roth et al. (1991) data is that it does not allow us to perform any check of responder behavior since Roth et al. (1991) uses the direct response method for responders whereas we employ the strategy method. Hence we have to restrict data comparisons to first-mover behavior.

Although there is a number of differences between the experiment of Roth et al. (1991) – e.g., offers in multiples of 5 cents, direct response method for responders, non-public payment, standard monetary incentives, different subject pool –, a comparison of these data to our ultimatum game data suggests the absence of any fundamental difference, see Figure 1. Arguably the US proposer data

⁸See Table 1 in Costa-Gomes (2001, p.244).

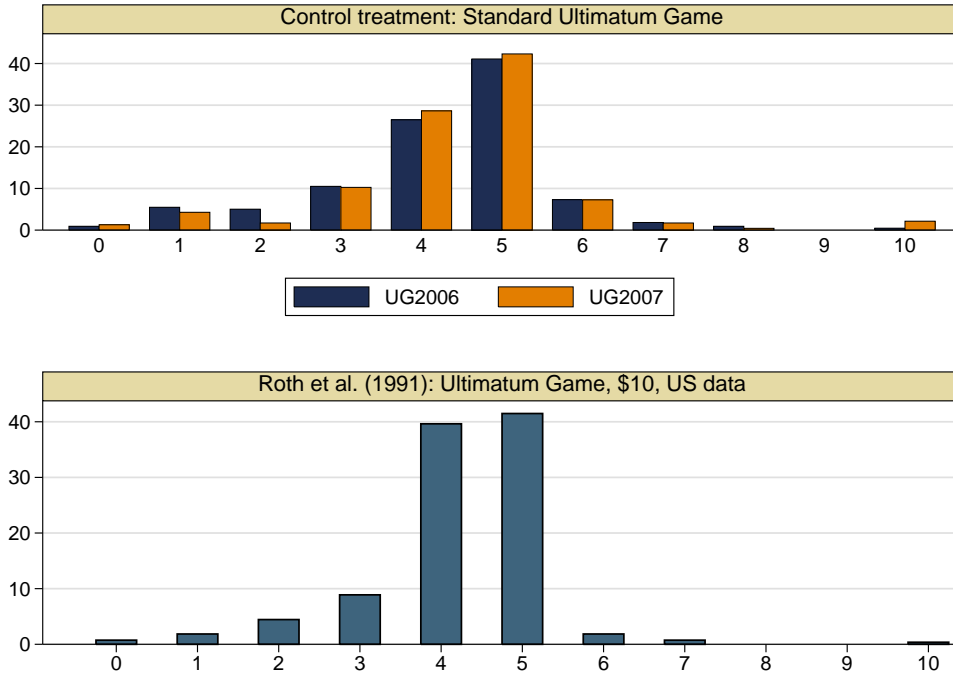


Figure 1: Frequency distributions of proposer offers in the Ultimatum Game in percent.

seems slightly different as reflected in somewhat more weight, roughly 10%, on the offer of \$4 and less weight on the offers of \$1 and \$6. Pooling the data from our two control experiments UG2006 and UG2007 leads to 453 proposer offers that we compare to the 270 proposer offers of Roth et al. (1991). Comparing the distributions does not reveal any significant difference (two-tailed, $p = 0.193$). Similarly, there is no significant difference in the average offer according to the t -test (two-tailed, $p = 0.214$). Checking for a difference between the central locations of the distributions using the Mann-Whitney- U -test reveals, however, significant differences, (two-tailed, $p = 0.057$). Nevertheless, in light of the other two test results and the shape of the entire distribution, we conclude that our experimental procedures lead to data that is consistent with data reported for the ultimatum game.

Taken together we have confirmed that our experimental procedures allow for the collection of meaningful and stable data in the setting of the ultimatum game. Likewise the obtained data seem in line with the ultimatum game data that is typically reported. Therefore we are confident that also our experimental treatment conditions for the market exchange variant of the ultimatum game yield reliable data despite unorthodox methods so that we can proceed to reporting the results for our main experimental manipulations.

3.2 Seller behavior with outdated price-tags

We begin to report on seller, i.e. first-mover, behavior. Figure 2 provides the distributions of prices asked by sellers. It is easy to see that the number of observations on the right tail increases in the outdated price-tag. We define a seller’s price as a “high price” if it either equals €9 or €10 (no

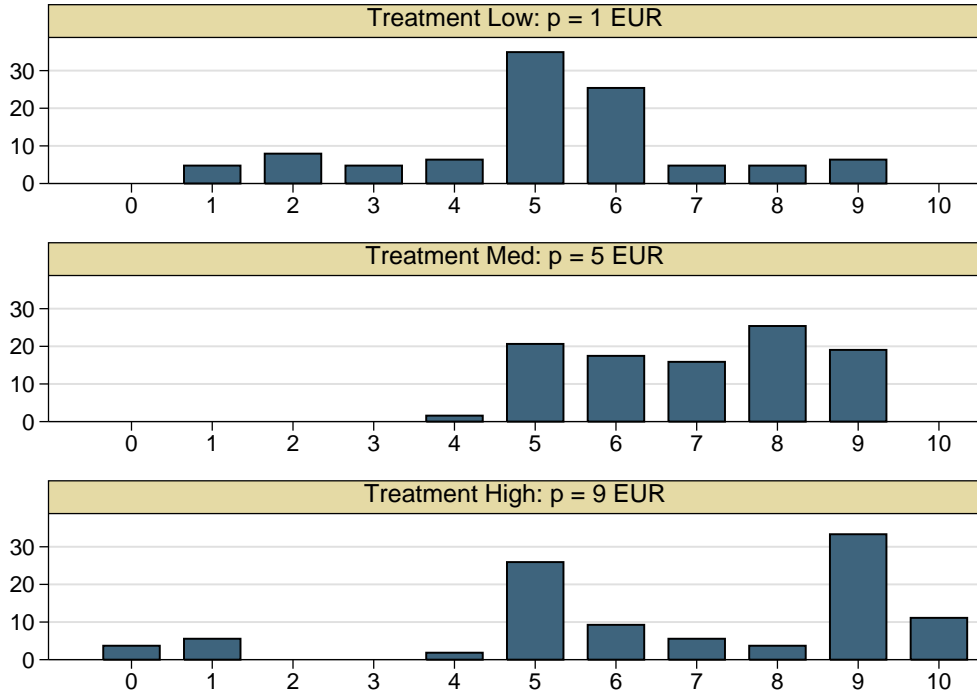


Figure 2: Frequency distributions of prices asked by proposers in percent by treatment.

other price is supported by any subgame-perfect Nash equilibrium with self-regarding preferences.⁹ Figure 2 reveals that the share of high prices is under 10% in treatment Low ($\rho = \text{€}1$); importantly, it increases to roughly 20% in treatment Med ($\rho = \text{€}5$), and is above 40% in treatment High ($\rho = \text{€}9$). Therefore, some sellers appear to use higher outdated price-tags as a legitimization to offer higher prices, i.e., to ask for a larger share of the surplus. A probit regression on indicator variables that indicate if the outdated price-tags was at least $\text{€}5$ or $\text{€}9$ confirms this impression, see table 2. The likelihood

explanatory variable	coefficient $\hat{\beta}$	σ_{β}	z	p -value
constant	-1.726	0.378	-4.57	< 0.001
outdated price-tag $\rho \geq \text{€}5$	0.647	0.308	2.10	0.036
outdated price-tag $\rho \geq \text{€}9$	0.725	0.250	2.90	0.004
experienced UG07	0.240	0.333	0.72	0.472

Table 2: Probit regression of sellers asking for a high price

that a seller asks for a high price is significantly larger if the outdated price-tag is at least $\text{€}5$. It increases further if the outdated price-tag is higher.

To see how the asked prices respond to variations of the outdated price-tag on average, we regress them on outdated price-tags. The results show that the relation between the price asked by sellers and the outdated price-tag is strictly positive, see table 3. On average, sellers ask for $\text{€}4.71$ of the surplus with a hypothetical outdated price-tag of $\text{€}0$ which conforms to standard findings for proposers in ultimatum games. They require an additional $\text{€}0.20$ from buyers for each increase of the outdated

⁹See Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991, p.1069) for the equilibrium predictions in the standard ultimatum game.

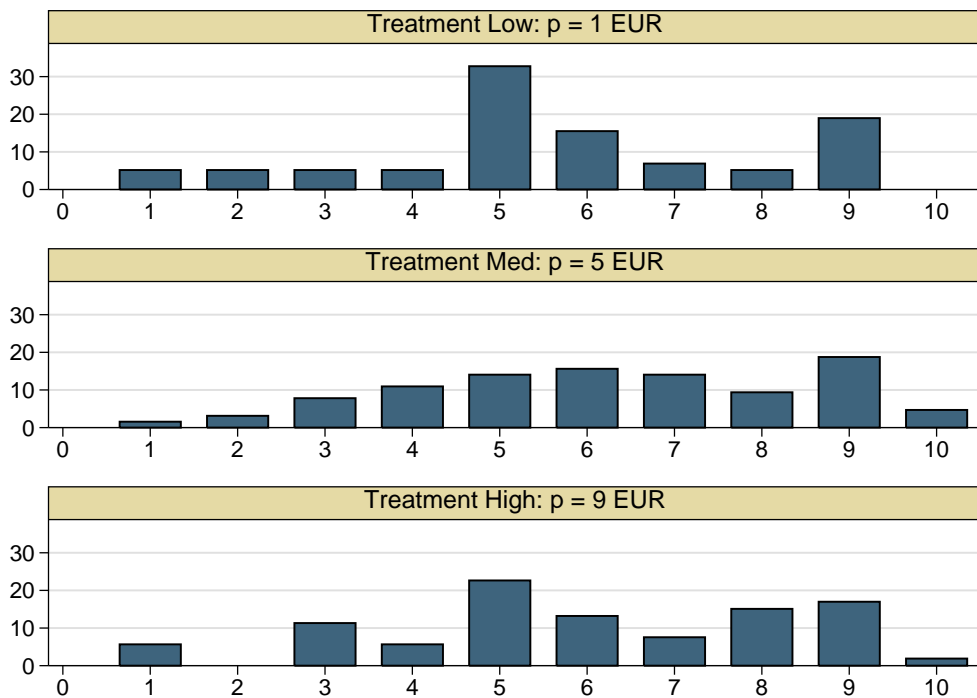


Figure 3: Frequency distributions of highest prices that buyers are accepting in percent by treatment.

price-tag by €1. The dummy variable indicating if subjects experienced the standard ultimatum game earlier is weakly significant and shows that the more experienced subjects tended to ask for prices that are €0.75 higher on average. For robustness we also test the hypothesis that a higher

explanatory variable	coefficient $\hat{\beta}$	σ_{β}	t	p -value
constant	4.708	0.438	10.75	< 0.001
outdated price-tag ρ	0.199	0.049	4.02	< 0.001
experienced UG07	0.747	0.433	1.73	0.086

Table 3: OLS regression of prices asked by sellers on outdated price-tags

outdated price-tag leads to higher asked prices using non-parametric methods. In particular, we appeal to the Jonckheere-Terpstra test on ranks for ordered alternatives. We find significant evidence ($p < 0.001$) for ascendingly ordered alternatives, so that the ranks of price asks for our treatment data are systematically varying with the treatment; specifically the ranks of price asks in treatments with a higher outdated price-tag are greater than the ranks of price asks in treatments with lower outdated price-tag. We summarize these findings on seller behavior as follows:

Finding 1 *The distribution of asked prices is not independent of the exogenous outdated price-tag as theoretically predicted with self-regarding preferences. In particular, the average of asked prices and the probability of observing a “high price” is positively correlated with the outdated price-tag.*

3.3 Buyer behavior with outdated price-tags

Our experimental results show that sellers behave as if using higher outdated price-tags to legitimate higher asked prices. Here we investigate if also buyer behavior responds to exogenous outdated price-tags. Figure 3 provides the distributions of the highest prices that buyers are willing to accept for any treatment with outdated price-tags. A comparison across treatments shows no clear effect of the outdated price-tag on the distributions of buyers' reservation prices. The most blatant difference appears to be the amount of maximum prices around the equal split of €5. In treatment Low, 53% of responders accept a maximum price of €4, €5, or €6. In treatments Med (41%) and High (42%), this number is somewhat smaller and the amount of maximum prices is less concentrated on the fair split of €5. This impression is, however, statistically insignificant according to a probit regression of observing a price of €4, €5, or €6 on treatment dummy variables, see table 4.

explanatory variable	coefficient $\hat{\beta}$	σ_{β}	z	p -value
constant	-0.198	0.343	-0.58	0.565
treatment Med	-0.306	0.229	-1.33	0.182
treatment High	-0.290	0.240	-1.21	0.228
experienced UG07	0.305	0.322	0.95	0.344

Table 4: Probit regression of buyers selecting a maximum acceptance price of €4, €5, or €6

If buyers were responding to outdated price-tags consistent with seller behavior, then they would be willing to accept higher asked prices when facing higher outdated price-tags. In contrast, the number of buyers accepting high prices is independent of the treatment.¹⁰ Specifically, the percentage of buyers accepting a high price is rather stable across treatments (Low: 19%, Med: 23%, High: 19%). A probit regression shows that the outdated price-tag does not significantly affect the probability that

explanatory variable	coefficient $\hat{\beta}$	σ_{β}	z	p -value
constant	-0.832	0.380	-2.19	0.029
outdated price-tag $\rho \geq \text{€}5$	0.151	0.258	0.59	0.557
outdated price-tag $\rho \geq \text{€}9$	-0.158	0.263	-0.60	0.548
experienced UG07	-0.050	0.350	-0.14	0.886

Table 5: Probit regression of buyers accepting a high price

buyers accept high prices, see table 5.

To see if the average buyer responds to outdated price-tags at all, we regress the highest prices accepted by buyers on outdated price-tags. The regression results reveal no significant treatment differences, see table 6. Responders are willing to accept prices of roughly €6.30 on average while the outdated price-tag has no clear effect on their behavior; its coefficient is not significantly different from zero.

Taken together we do not find any statistical difference of second-mover behavior across the outdated-prices treatments, so that we summarize our findings on buyer behavior as follows:

Finding 2 *The distribution of the largest price accepted by the buyer is independent of the exogenous outdated price-tag.*

¹⁰Again, we refer to a price as 'high' if it is greater than or equal to €9.

explanatory variable	coefficient $\hat{\beta}$	σ_{β}	t	p -value
constant	6.296	0.616	10.22	< 0.001
outdated price-tag ρ	0.038	0.055	0.69	0.491
experienced UG07	-0.534	0.571	-0.94	0.351

Table 6: OLS regression of highest asked prices accepted by buyers on outdated price-tags

3.4 Allocative efficiency

In this section, we report the devastating effects of higher outdated price-tags on allocative efficiency. Given that sellers ask for higher prices with higher outdated prices while the distribution of maximum prices that buyers accept is immune to variations of the outdated price-tag, it is straightforward that increasing outdated price-tags reduce allocative efficiency.

We use the share of feasible trades out of all potential trades in our experiment to measure efficiency; a trade is feasible whenever the buyer would accept the price asked by the seller. For any treatment we match each proposer with any responder. Table 7 summarizes efficiency results by treatment. As expected a larger outdated price-tag decreases allocative efficiency. For example, if the outdated price-tag is €1, the share of trades is 65.3% out of all possible seller-buyer encounters. With an outdated price-tag of €9 it drops to just 46.0%.

treatment	outdated price-tag	share of trades	#matchings
Low	$\rho = 1$	65.3%	3654
Med	$\rho = 5$	48.3%	4032
High	$\rho = 9$	46.0%	2862
UG06		70.2%	44676
UG07		71.8%	51246

Table 7: Allocative efficiency by treatment

Finding 3 *Allocative efficiency is negatively correlated with the outdated price-tag.*

4 Discussion

In this paper we have introduced the notion of vacuous information that embeds a game within some context that does not derive from game history. Our evidence shows that vacuous information about the bargaining situations can strongly shape the bargaining outcome and reduces allocative efficiency substantially. Specifically outdated-price tags are taken into account by first-movers but ignored by second-movers. These results show that anchoring effects as reported for individual choice by Ariely, Loewenstein, and Prelec (2003) can extend to situations of strategic interaction. Anchoring effects, however, seem to apply much more subtle in games as suggested by the asymmetric response of our players to vacuous information.

The finding that second-mover behavior is immune to variations of vacuous information though first-mover behavior suggests that it would respond to it seems surprising at first sight. The experimental literature, however, documents that the variation of a first-mover’s circumstances can affect the choices of first-movers while leaving unaffected these of second-movers in bargaining games. In particular, Brandts, Güth, and Stiehler (2006), Cox, Servatka, and Vadovic (2010), and Dufwenberg and Gneezy (2000) investigate behavior in lost wallet games and find that decreasing the value of the

first-mover's outside option increases the frequency that first-movers grant second-movers the possibility to dictate the allocation. Importantly and puzzlingly, they also find that the allocation dictated by second-movers does not respond to the first-mover's opportunity cost of, arguably kindly, foregoing the outside option.

Why did second-movers not respond to outdated price-tags? One possible explanation is that the asymmetry finding is simply an artefact of the experiment as it utilizes the strategy method. This explanation is, however, inconsistent with other ultimatum game experiments that identify changes in second-mover behavior while implementing the strategy method; e.g., Fellner and Güth (2003) apply the strategy method and find differences in second-mover behavior when varying threat points in the ultimatum game and Larrick and Blount (1997) report that second-mover behavior significantly responds to framing the ultimatum game as a social dilemma with employing the strategy method.

Another possible explanation relates to a potential asymmetry of reference points. In our experiment, the outdated price-tag may affect the reference point of a seller directly while the reference point of a buyer - who can resell the stamp to the experimenter always at 10 EUR - might have been constant across treatments. Hence the notion of differently perceived reference points potentially explains the asymmetry finding. In our experimental design the assumption of asymmetric reference points is, however, hard to believe since - according to the experimental protocol - subjects were informed about the instructions before roles were assigned. Thus, if there would have been some effect on a reference point, then it would have been symmetric at the beginning of the experiment and it would have then changed for the subjects in the role of responders after the assignment of roles.

In contrast, our results are consistent with the hypothesis of self-serving bias on the part of players when interpreting vacuous information. While proposers seem to use the opportunity of higher outdated price-tags to improve their case for demanding higher payoffs, second-movers seem to ignore this manipulation of first-mover circumstances altogether.

We have shown that the context of bargaining situations constitute an important factor of bargaining. Our research contributes to the bargaining literature by showing that pure context in the sense of vacuous information is already sufficient to affect bargaining behavior and allocative efficiency. Moreover, it suggests that a primary factor of how vacuous information can translate into behavioral effects is self-serving interpretations of the context of the bargaining situation that can be inconsistent with the interpretation of other players.

A Instructions

The following instructions were projected to the screen and read aloud in class. Altogether there were four slides:

Slide 1: Instructions

- For this experiment we divided all classrooms into three groups. You are in the first [LOW: second; HIGH: third] group.
- In this experiment, two individuals in this classroom and any other E+B-classroom of the same group are selected **at random**. Let's call them Steve and Martin.
- Imagine that Steve found a special stamp in his drawer. It is a stamp that he bought for **5 EUR** [LOW: 1 EUR; HIGH: 9 EUR] a while ago. Unfortunately the stamp was valid only for a short time, and cannot be used any more.
- Martin is a stamp trader, and he knows of a collector, who buys the stamp for 10 EUR.

Slide 2: Instructions

- Now, Steve suggests stamp trader Martin a price at which he may purchase the stamp.
- Martin can accept or reject the price.
- If Martin accepts the price, Steve receives an amount of money equal to the price, and Martin receives 10 EUR minus the price (Imagine that he immediately sells the stamp to the collector).
- If Martin rejects the price, then no one receives anything: Steve's stamp crumbles to dust and for Martin, there is no other possibility to obtain a stamp that can be sold to the collector.

Slide 3: Instructions

- In a minute, you receive a sheet of paper that informs you about the role that you assume (Steve or Martin)
- Please fill in your student number, so that we can later identify you to pay your winnings in cash.
- If you assume the role of Steve, then you indicate for which price between 0 and 10 EUR you would sell the stamp for which you once paid **5 EUR** [LOW: 1 EUR; HIGH: 9 EUR] to Martin.
- If you assume the role of Martin, then you indicate the **highest** price you are willing to pay for the stamp.
- If there is an agreement, Steve receives the price he offered, and Martin receives 10 EUR minus this price.

Slide 4: Instructions

- During the lecture, the Bargaining-Game is played with **REAL MONEY!**
- There is 1x10 EUR: Out of all properly filled in and collected sheets, 1 Steve sheet and 1 Martin sheet of your group is selected at random.
- Then, the bargaining game is played according to the rules and the data on the sheets. The corresponding students will receive the resulting payoffs **DURING THE LECTURE** in **CASH!**

B Record sheets

B.1 Seller record sheet

Welcome to the classroom experiment!

Please fill in your student number: i-.....

You assume the role of STEVE. This means that you suggest the price at which you sell the stamp that you once bought for **5 EUR** [LOW: 1 EUR; HIGH: 9 EUR] to MARTIN if he accepts your suggested price. If he doesn't accept it, you receive nothing. If he accepts, you receive the amount equivalent to the price and MARTIN receives an amount equivalent to 10 EUR minus the price.

Please notice that one pair (Steve/Martin) is randomly matched in the lecture. The result is paid to the corresponding Steve and Martin in **REAL MONEY** in class if the offer is accepted (this is why we need your student number: to identify the lucky ones to give them their money!)

Please check the appropriate checkbox: (check only once!)

I offer MARTIN to buy the stamp for:

- 0€
- 1€
- 2€
- 3€
- 4€
- 5€
- 6€
- 7€
- 8€
- 9€
- 10€

B.2 Buyer record sheet

Welcome to the classroom experiment!

Please fill in your student number: i-.....

You assume the role of MARTIN. This means that you decide about the highest price at which you would buy the stamp from STEVE that he once bought for **5 EUR** [LOW: 1 EUR; HIGH: 9 EUR].

If STEVE suggests a price that is lower or equal to the highest price that you accept, then the transaction is implemented at STEVE's suggested price. Then, STEVE receives the amount equivalent to the price he offered and you receive the amount of 10 EUR minus the price STEVE offered. If STEVE suggests a higher price, then you reject the offer and you both receive nothing.

Please notice that one pair (Steve/Martin) is randomly matched in the lecture. The result is paid to the corresponding Steve and Martin in **REAL MONEY** in class if the offer is accepted (this is why we need your student number: to identify the lucky ones to give them their money!)

Please check the appropriate checkbox: (check only once!)

The highest price I would pay to STEVE is:

- 0€
- 1€
- 2€
- 3€
- 4€
- 5€
- 6€
- 7€
- 8€
- 9€
- 10€

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