Telling the Other What One Knows? Strategic Lying in a Modified Acquiring-a-Company Experiment with Two-sided Private Information

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Abstract

Lying for a strategic advantage is to be expected in commercial interactions. But would this be more or less obvious when lying could come from either party and question mutually profitable exchange? To explore this we modify the Acquiring-a-Company game (Samuelson and Bazerman, 1985) by letting both, buyer and seller, be privately informed. Specifically, the value of the company for the buyer is known only by the seller whereas only the buyer is aware by which proportion the sellers evaluation is lower than the one of the buyer. Before bargaining, both parties can reveal what they know via cheap-talk numerical messages. Game theoretically, the pooling equilibrium may or may not allow for trade depending on the commonly known expected evaluation discrepancy. By mutually revealing what one knows one could boost trade and efficiency. Although strategic misreporting prevails quite generally, it is higher for sellers throughout the experiment. Regarding gender, women misreport less, especially as sellers, and offer higher prices.

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1 Introduction

Lying for strategic advantages in commercial interactions where one side has private information[1], involves mostly (market) situations in which the parties could gain from trade but have at least partly conflicting interests. Successful deception usually benefits the deceiver and harms the deceived.[2] But what happens when both parties have private information regarding the value of the good to be traded? How will the possibility to mutually lie to each other affect lying behavior, as well as profitable trade?

To investigate this we modified the seminal Acquiring a Company game[3] (AaC in the following) which allows only for private information about the linearly affiliated valuations of the company and behaviorally for the winner's curse: the buyer's and the seller's valuation of the company are linearly linked via an undervaluation coefficient for the seller which is commonly known, but the valuation of the company is known only by the seller. Our modified version of the game (which we refer to as MAC in the following) substitutes one-sided with two-sided private information, and allows both parties to send numerical cheap-talk messages regarding what they privately know before bargaining: the real value of the company in case of the seller, and the seller's undervaluation coefficient in case of the buyer. After simultaneously exchanging messages, the buyer proposes a price which the seller can accept or reject. While the sequential game structure of MAC allows signaling, only pooling equilibria can be confirmed (game) theoretically. Most importantly, it allows us to study whether lying behavior is role dependent, i.e. whether sellers lie more or less frequently than buyers, as well as whether lying on either side of the trade is gender dependent, i.e. whether women are more reliable in what they report and more other-regarding in bargaining than men and if so, whether price proposals and acceptance decisions differ across gender.

MAC substitutes the commonly known valuation discrepancy of AaC by a commonly known expected valuation discrepancy on which it depends whether there should always be trade or

[1]See Akerlof (1978).

[2]This situation differs from lying to an experimenter, on which recently many researchers focussed their attention (see Fischbacher and Foellmi-Heusi, 2013), since it implies lying and harming another participant.

[3]See Samuelson and Bazerman (1985).

no trade at all. For risk and loss-neutral parties efficiency-enhancing trade would only prevail when the commonly known (expected) evaluation discrepancy is large, i.e. when trade is very efficient. By honestly telling each other what one knows, however, one may trade even when this is not predicted. In MAC, seller participants only know the buyer's message but not their own true undervaluation of the company. So they can not be certain whether acceptance of buyer's price yields a profit or not. Therefore, now, not only the buyer but also the seller has to expect gains and losses with positive probability when trade is (game) theoretically predicted. Altogether MAC seems more balanced than AaC what could crowd-in and strengthen reciprocity concerns and behaviorally weaken the winner's curse.

This design offers another justification of not lying in addition to intrinsic lying aversion, namely that "one does not lie because (one expects that) the other does not lie". We do not predict-but also do not exclude-intrinsic lying aversion and intrinsic reciprocity concerns. Mutual lying could, of course, also weaken intrinsic lying aversion (see Barr and Michailidou, 2017; Kocher et al., 2018; Weisel and Shalvi, 2015). In our setup such complicity could result in conflict, i.e. in no trade. More troublesome, in our view, is that seller participants in similar games, when deciding whether to accept the buyer's price proposal, nearly always behave opportunistically: they accept when it yields a profit and reject otherwise (e.g. Samuelson and Bazerman, 1985 and Di Cagno et al., 2016). There is hardly any evidence that seller participants care for the buyers. Expecting mutual private information sharing to trigger stronger and sufficient intrinsic concerns, however, may limit the opportunism of seller participants.

Appreciating the mutual gains from information sharing trade could definitely be enhanced by looking at the situation from both perspectives in line with the "Theory of mind" (ToM, see Köhler 1925; Premack, 2014). Rather than speculating on ToM we have tried to induce and actually test such "taking the other's perspective into account" by two between-subjects treatments, one implementing constant roles across rounds, the other implementing random role assignment for each round. We have expected but do not confirm that experiencing both roles across rounds boosts mutual information revelation and strengthens reciprocity concerns (Brandts and Charness, 2003, also explore role reversals).

Other regarding concerns, often narrowly defined as social preferences (see Cooper and Kagel, 2016, for a review), are often based on only special experimental evidence, mostly of deterministic interaction with complete information (compare this, for instance, with the review of auction experiments by Kagel and Levine, 2016, which usually involve privately informed bidder participants). There is a little, partly (like for AaC-experiments) even no evidence of equality or reciprocity concerns. In AaC this may be due to only one party commanding private information what allows for moral wiggle room (see Dana et al., 2007) only on one side of the bargaining table. This is avoided by MAC which could induce mutual information sharing and encourage trade when mutually trusting each other. On the other hand MAC is stochastically even more complex than AaC which might crowd out other regarding concerns since the effect of own behavior is much more difficult to predict.

When trying to hide what they privately know, sellers will likely over-report the actual value of the company whereas buyers will likely under-report the seller's undervaluation coefficient. Such mutual misreporting may render both messages uninformative (see Crawford, 1998); also Croson, Boles and Murnighan, 2003, report how that mutual misreporting nevertheless affects bargaining outcomes. The repeated structure of the game (with random partners matching) also allows us to analyse the existence of learning in strategic lying and to check if there is a faster convergence to strategic lying for constant than for random roles due to less possibility to learn in the latter case.

Lying has long been discussed in the ethics and philosophy literature (Stegmuller, 1977) before recently becoming fashionable in behavioral and experimental economics (e.g. Rosenbaum, Billinger and Stiglitz, 2014, and Arbel et al., 2014). Specifically, the economic literature has added no-trade predictions when trading credence goods whose quality customers are unable to assess directly (for experimental studies see Dulleck, Kerschbamer and Sutter, 2011, and Beck, Kerschbamer and Sutter, 2013).

The recent literature on lying and, more generally, on the "dark side" of human behavior mostly relies on reporting tasks without market embedding.[4] Actually, self-serving strategic lies in markets may seem ethically less objectionable than individual misreporting (see Boles, Croson

[4]See as examples Fischbacher and Föllmi-Heusi (2013), using the "die throw-reporting task" and Gneezy, Kajackaite and Sobel (2018) using a variation of the cheating game.

and Murnighan, 2000, for a discussion). The former is so common that economic policy tries to limit misreporting or not revealing private information on markets (e.g. the European Union Directives "Transparency" and "Takeover Bid", 2004 a and b). MAC data inform about strategic misreporting in a bargaining framework and allows to mimic, for instance, real world "Merger and Acquisition" activities in large commercial enterprises whose regulation is the focus of such directives.

Whether and how gender affects the propensity to lie is still a controversial topic in the literature.[5] While Dreber and Johannesson (2008) find women to lie less than men, Childs (2012, 2013) and Gylfason, Arnardottir and Kristinsson (2013) observe no gender effect in lying even when varying the amount at stake. On the other hand, Erat and Gneezy (2012) highlight the interplay between lying behavior and other-regarding concerns and find that lying behavior is sensitive to how it affects others: women are less likely lie when this harms others and lie more likely when this benefits others. As Di Cagno et al. (2017) and Galliera (2018), we find strategic misreporting to be role and gender dependent with sellers and men being more likely to lie strategically. Constant role assignment does not speed up neither lying nor thruthfully reporting.

Section 2 formally describes MAC and derives its pooling equilibrium. Section 3 introduces the experimental protocol. Section 4 describes and statistically analyzes the experimental data before section 5 concludes.

2 The Setup

We first present the original AaC-model (Samuelson and Bazerman, 1985) where the undervaluation coefficient q is known by both the seller S and the buyer B. The decision process of that game proceeds as follows: the value $v \in (0, 1)$ is drawn, according to the uniform density concentrated on (0,1), and revealed only to seller S. Being aware that S knows v and how v is randomly selected, buyer B proposes a price $p \in [0, 1]$ which the seller S accepts, $d = 1$, or rejects, $d = 0$.

The resulting payoff is $d(p - qv)$ for S and $d(v - p)$ for B. No trade, $d = 0$, yields 0-payoffs whereas trade, $d = 1$ realizes the positive surplus $(1 - q)v$ due to $q < 1$ and $v > 0$. Backward

[5] See Capraro (2018) for a review of experimental studies using the sender-receiver game.

induction yields $d^* = 1$ for $p \geq qv$ when the seller is opportunistic (only concerned about own payoff). Anticipating this, the risk and loss neutral buyer B with price p expects to earn:

$$\int_0^{p/q} (v - p)dv = \frac{p^2}{q}\left(\frac{1}{2q} - 1\right) \tag{1}$$

Profit expectation (1) increases with p only for $\frac{1}{2} > q$. Thus the optimal price is $p^* = q$ for q$\leq \frac{1}{2}$ and $p^* = 0$ for $q > \frac{1}{2}$, i.e. $q > \frac{1}{2}$ yields a no-trade result, similarly to Akerlof (1978).

MAC instead assumes that both v and q are randomly determined via uniform densities, based on $0 < v < 1$ and $0 < q \leq \bar{q} < 1$. We denote the commonly known expectation of q by $E(q)$ and assume $E(q) < \frac{1}{2}$. Whereas only S learns about v, only B is informed about q, i.e. private information is mutual. After being privately informed about v, respectively q, both S and B independently send a numerical message which can truly or falsely report what one privately knows: S sends a value message $\hat{v}$ to B, with $0 < \hat{v} < 1$, and B sends a q-message $\hat{q}$ to S, with $0 < \hat{q} \leq \bar{q}$. After that, MAC-play proceeds in the same way as for AaC.

We extend the assumption of risk and loss neutrality for buyer B, on which the AaC-benchmark analysis is based, to both parties, B and S. Game theoretically, the message exchange qualifies as cheap talk. Since S expects to earn $p - vE(q)$ when accepting ($d = 1$), the expected profit of B is $\int\limits_0^{p/E(q)} (v - p)dv = \frac{p^2}{E(q)}(\frac{1}{2E(q)} - 1)$, which implies a pooling equilibrium with $d^* = 1$ only if $p \leq vE(q)$ and $p^* = E(q)$ for $E(q) \leq 1/2$ and $p^* = 0$ for $E(q) > 1/2$. Due to $E(q) = 0.4$ in the experiment the benchmark solution always prescribes trade. Regarding reporting behavior, it seems beneficial for sellers to over-report the value of the firm (i.e. $\hat{v} > v$) and for buyers to under-report the disparity coefficient (i.e. $\hat{q} < q$). We refer to this as strategic lying.

AaC and MAC mainly differ in that the commonly known expectation $E(q)$ of MAC substitutes the commonly known q of AaC and that risk and loss neutrality of both parties is common(ly known).

3 Experimental design

The experiment consists of two treatments: a random-role treatment, R, in which participants are randomly assigned the role of a seller or of a buyer at the beginning of each period, and a constant-role treatment, C, in which the role of seller or buyer is randomly assigned at the beginning of the first period and then maintained for the entirety of the experiment. Treatment R (116 participants in 5 sessions) allows participants to experience both sides of the trade and both types of private information while in treatment C (132 participants in 7 sessions) participants experience only one side of the trade and one type of private information. In our view, both conditions have external validity.[6] Since we are also interested in gender differences, we included nearly as many female as male participants (48.4% and 51.6% of our sample, respectively). Behaviorally the cognitive load when facing both roles is more demanding, what could slow down role-specific learning in Treatment R.

Each participant plays the MAC game for 24 successive periods with end-of-period feedback (own individual payoff for the round). The crucial parameters v, only known to the seller, and q, only known to the buyer, are randomly selected in every period. Because of the irregularly changing parameters we can rely on rather small matching groups with four (in case of random role assignment), respectively six (in the case of constant roles) participants each. Participants were not aware of this matching group restriction in rematching.

The experiment implements a discrete version of MAC letting v only vary with grid size 0.05 from 0.05 to 0.95 and q in grid size of 0.1 from 0.1 to 0.7. Participants are informed about the possible parameter values and that all values are equally probable. Since the border cases of v and q allow misreporting in only one direction, this could trigger implicit demand effects. Even more importantly it would confound intentional misreporting with noise what would require a separate analysis of these border cases. We therefore neglect the messages $\hat{v}$ and $\hat{q}$ for all border cases in our data analysis.[7]

[6]Treatment R could appeal to global players and hedge funds whose business is to buy and sell companies and thus to trade on both sides of the market. Compared to this, Treatment C distinguishes enterprises, which mainly sell companies, and others which mainly acquire them in order to complete their competences. Examples are firms with focus on research and innovative output, and growing tech companies such as Amazon and Alphabet.

[7]Specifically, our data analysis will focus only on message data $\hat{v}$, $\hat{q}$ for $0.05 < v < 0.95$ and $0.1 < q < 0.7$ for

We did not vary $E(q)$ systematically but kept it constant at the level $E(q) = 0.4$. This avoids that role changes are accompanied by $E(q)$-changes, i.e. one confronts a priori the same MAC-game but possibly from a different role perspective and based on role-specific private information about v in seller and q in buyer role. Although trade is predicted game theoretically for $E(q) = 0.4$, both parties incur losses with positive probability, for example as buyer when $v < p = E(q)$ and as seller when $qv > p = E(q)$.

The experiment was programmed in zTree (Fischbacher, 2007) and carried out at CESARE lab (LUISS "Guido Carli"). Participants were recruited via ORSEE (Greiner, 2015) and participated in only one session. Sessions lasted, on average, 1 hour and 45 minutes and were composed of three phases, Phase 1: MAC game for 24 periods; Phase 2: risk attitude elicitation (Holt & Laury, 2002); & Phase 3: final questionnaire with (unincentivized) loss tolerance elicitation (Gächter, Johnson and Herrmann, 2010; see Appendix for its description) and other demographics.

The final payment was composed of a show-up fee of 8€to (from) which we added (subtracted) the earning (loss) of a randomly selected period of Phase 1 and the earning in Phase 2. Final average payment was 13.3 €(see Appendix for the translated instructions).

4 Results

4.1 Role and gender specific misreporting

We try to answer the following questions:

(*i*) Is misreporting predominant for both, buyers and sellers? If so, is misreporting strategic?[8]

(*ii*) Is lying affected by past experience and treatment?

(*iii*) Are there gender differences in lying and how do they depend on role and experience?

Table 1 shows the frequency of truth-telling and misreporting by treatment and specifies the frequency of strategic misreporting for v (i.e. $\hat{v} > v$) and q (i.e. $\hat{q} < q$). Participants misreport

which both, under- as well as to over-reporting is possible, i.e. the messages for the possible values $v = 0.05; 0.95$ and the possible parameters $q = 0.1; 0.7$ are omitted as they are not fully comparable with the rest of the parameters.

[8]We refer to strategic misreporting as $\hat{v} > v$, respectively $\hat{q} < q$.

frequently in both roles: around 90% of the time as sellers, and 80% as buyers. Two-independent sample t-tests, ran on frequencies at the matching-group level in order to keep observations independent, do not reveal any treatment difference in misreporting. This will be more thoroughly analyzed later in Tables 4 and 5 via regression analyses.

	Truth-telling		Misreporting: Total		Misreporting: Strategic	
	v	q	v	q	v	q
Random role	0.101	0.198	0.899	0.802	0.814	0.539
Constant role	0.092	0.197	0.908	0.803	0.851	0.514
$\boldsymbol{\delta}$			-0.009	-0.001	-0.037	0.025
p-val			0.678	0.967	0.204	0.646

Table 1: Truth-telling and misreporting frequencies by role and treatment, with t-tests assessing treatment effects

Strategic sellers' over-reporting of v is more frequent than buyers' under-reporting of q in both treatments (respectively around 80% versus 50%). Since strategic lying is self-serving, this finding is quite surprising. One reason may be that stating a higher value v, as a seller, is more intuitive than sending a lower undervaluation-coefficient q as a buyer. Another reason may be that, while both v and q can have an impact on either payoff, the value v matters directly for both players, whereas q affects directly only the seller.

	Truth-telling	Misreporting: Total	Misreporting: Strategic
Sellers	0.097	0.903	0.830
Buyers	0.198	0.802	0.528
$\boldsymbol{\delta}$		0.101^{***}	0.302^{***}
p-val		0.000	0.000

Table 2: Average truth-telling and misreporting frequencies by role

Table 2 statistically confirms the difference in misreporting frequencies by role in both total misreporting and strategic misreporting: sellers misreport more frequently than buyers, and are more likely to misreport strategically as well.

Figures 1 and 2 illustrate the dynamics of strategic misreporting (respectively $\hat{v} > v$ for sellers and $\hat{q} < q$ for buyers) in the bottom areas, of non-strategic misreporting in the middle, and of truthful reporting at the top, separately for both treatments. Comparing the left and the right panel of both figures reveals no evident differences in the dynamics across treatments: whether participants play constantly as buyer or seller or are randomly assigned to a role each period, does not have an effect on the composition dynamics of (mis)reporting types.

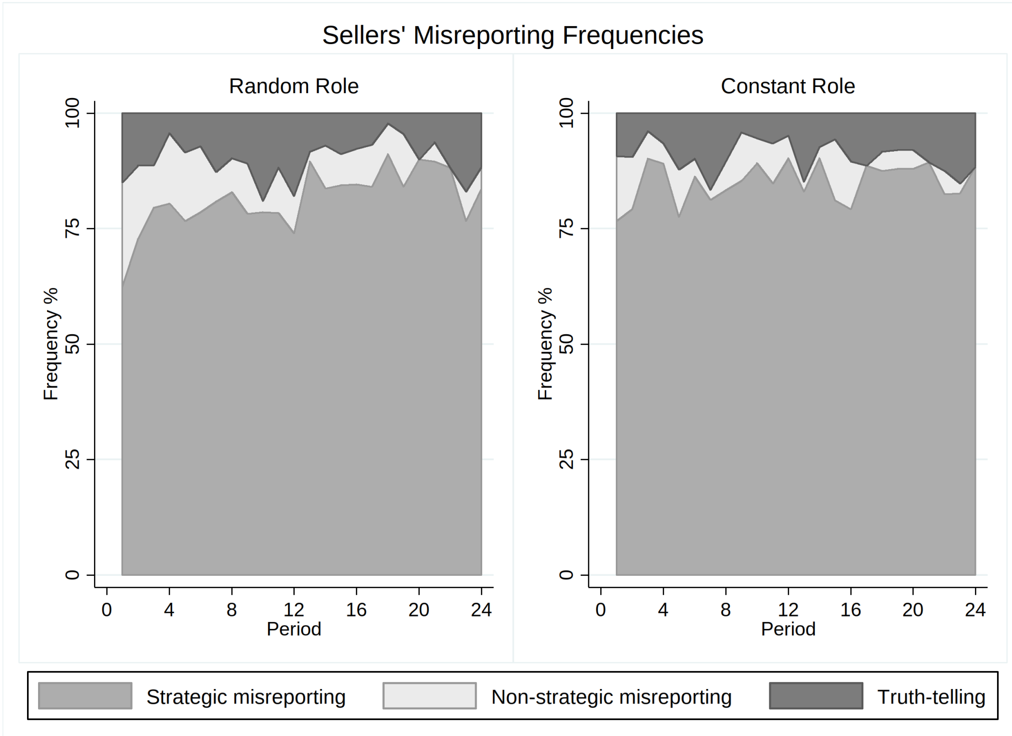

Figure 1: Frequencies of Sellers' (non) strategic misreporting and truth-telling by period

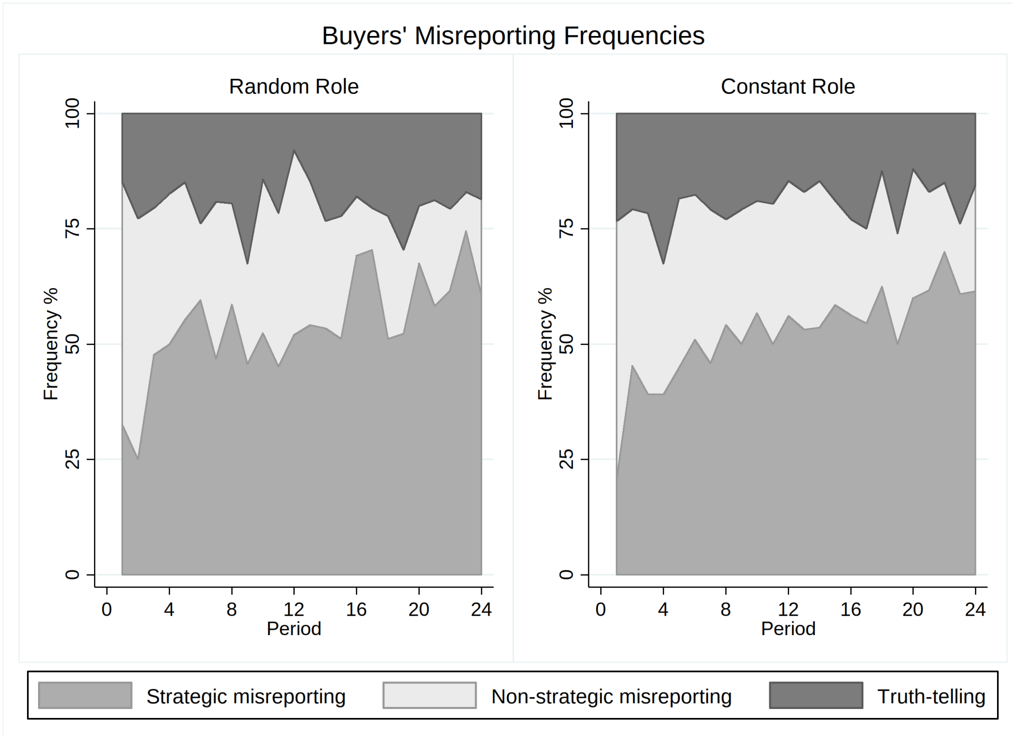

Figure 2: Frequencies of Buyers' (non) strategic misreporting and truth-telling by period

Comparing Figures 1 and 2 confirms the findings of Table 1: buyers strategically misreport less in all periods. Also, non-strategic misreporting is much more frequent for buyers than for sellers and clearly decreases across periods and virtually vanishes for sellers. Overall, the difference in strategic misreporting across roles is much larger in the first than in the last periods. This is in line with the argument that under-reporting q is less intuitive which has lead to stronger learning effects for buyers. Still, even when we focus only on the last period, buyers misreport strategically less then sellers, indicating that perhaps the intuition argument, alone, is not enough to explain the finding. In fact, truth-telling is more frequent for buyers than sellers, which remains quite constant across periods.

	Truth-telling		Miserporting: Total		Miserporting: Strategic	
	v	q	v	q	v	q
Male	0.077	0.181	0.923	0.879	0.874	0.558
Female	0.126	0.218	0.874	0.782	0.787	0.519
$\boldsymbol{\delta}$			0.049*	0.037	0.087**	0.039
p-val			0.073	0.362	0.015	0.450

Table 3: Average truth-telling and misreporting frequencies by gender and role

Table 3 shows gender differences in the frequency of truth-telling and misreporting. Analyzing gender differences relies on tests of statistical differences for non-independent observations (Moffat, 2015).[9] Our analysis, using the same test as in Table 2, reveals no gender difference in strategic misreporting in the buyer role for which strategic misreporting is not as dominant (see Table 1). Female sellers, however, over-report v significantly less often and reveal the truth more often than male sellers.

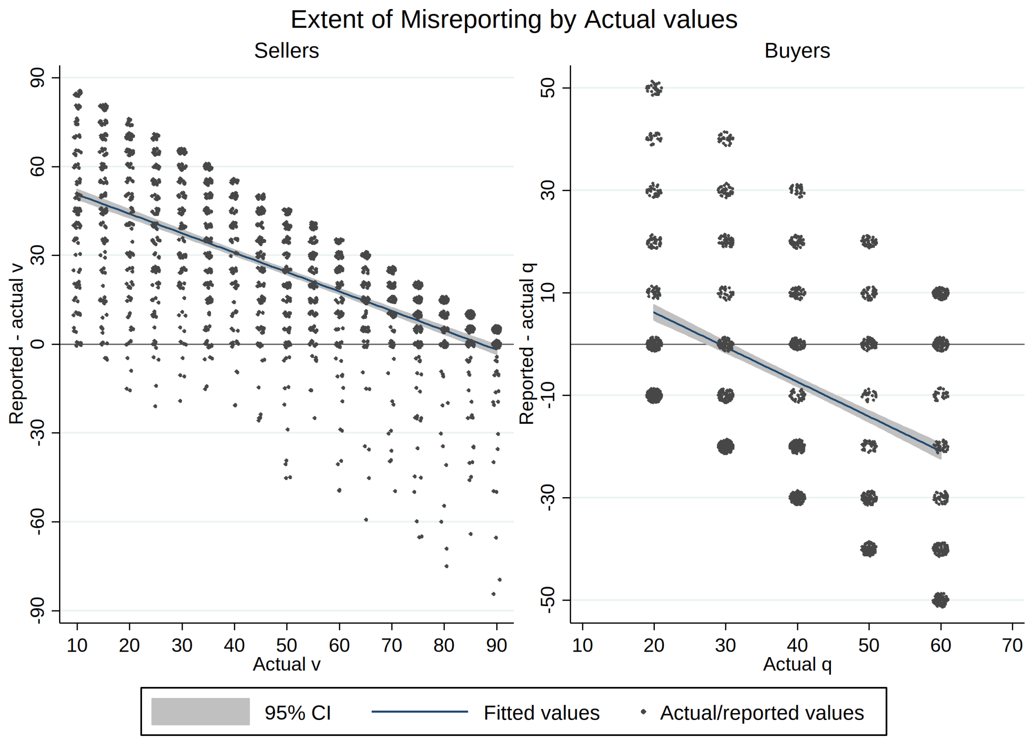

Figure 3: Extent of misreporting by actual v - and q values and by role

In the left panel of Figure 3 the extent of sellers' misreporting, i.e. the difference between reported and true v-value is indicated on the y-axis, for the value v on the abcissis. Analogously, the right panel of the same figure presents buyers' extent of misreporting on the y-axis for the under-evaluation coefficient q on the abcissis. Data points on the abcissis (no misreporting) are mostly observed for high (low) values of v (q). Strategic misreporting instead is maximal for lowest

[9]More specifically, gender effects in frequencies are tested via a regression where dependent variable is the frequency of truth-telling (or misreporting) of either males or females in the same matching group (periods pooled). Such aggregated frequencies are regressed on a single gender dummy to check whether they differ significantly from male to female participants. In order to account for dependence between the two frequencies of males and females belonging to the same group, standard errors are clustered at the matching-group level (see Moffat, 2015, pg. 84-85).

(highest) values of v (q).

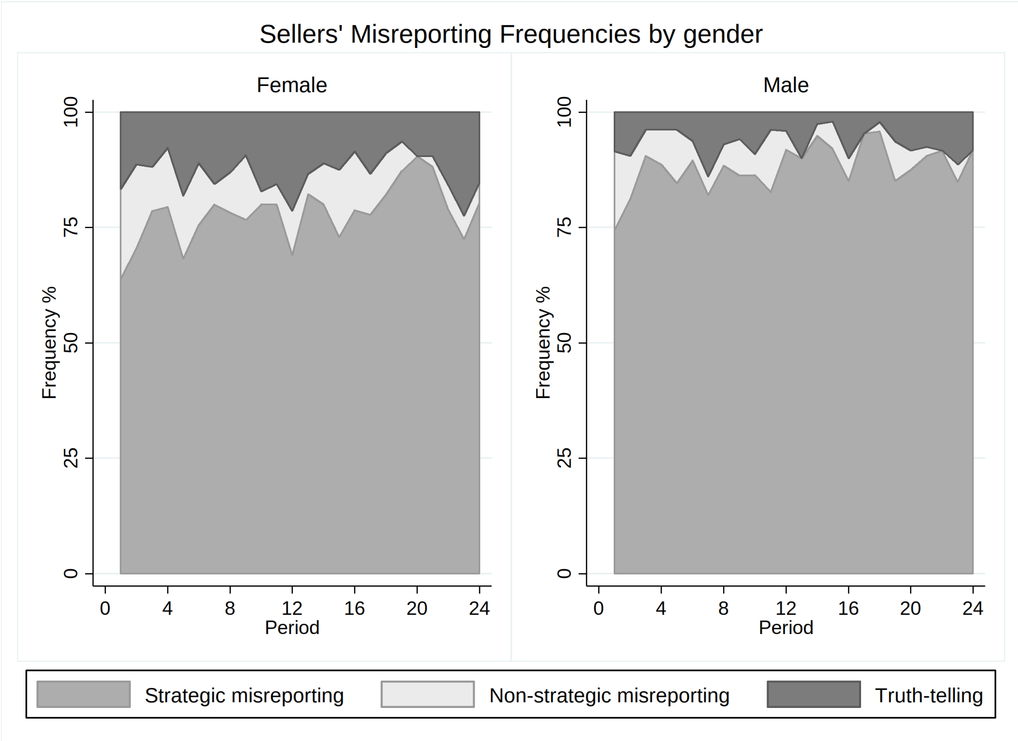

Figure 4: Frequency of strategic misreporting by period and by gender

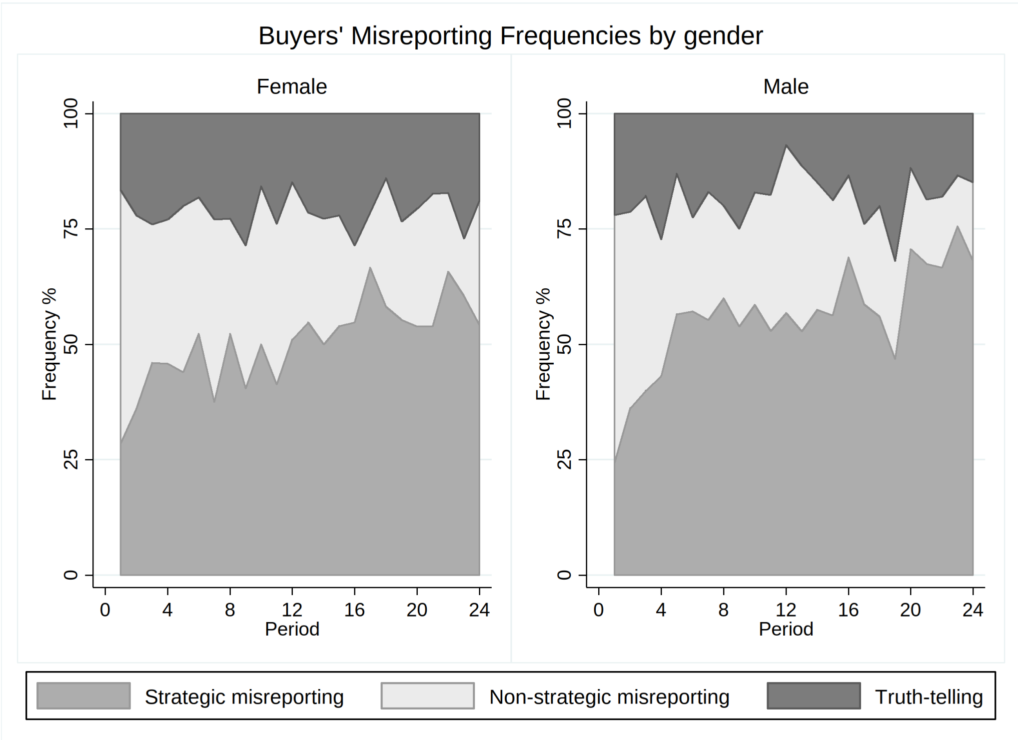

Figure 5: Frequency of strategic misreporting by period and by gender

Figures 4 and 5 illustrate the dynamics of strategic misreporting, separated by gender. According to Figure 4 female sellers start off with less strategic misreporting which increases over time. This increase is mostly at the cost of non-strategic misreporting suggesting that female participants learn how to lie strategically. The frequency of truth-telling is rather constant and higher for female sellers. Figure 5 visualizes no gender effects in the buyer role.

Sellers' strategic misreporting at period t

	Probability			Extent	
	beta	odds ratio	(se)	beta	(se)
Ref cat. no lie (t-1)					
Lied as a S (t-1)	0.33	1.39	(0.30)	4.25***	(1.49)
Lied as a B (t-1)	0.86**	2.36**	(0.41)	0.36	(1.82)
Ref. cat. did not experience a lie (t-1)					
Not accepted	−0.32	0.73	(0.27)	−0.16	(1.29)
Accepted, B lied	−0.41	0.66	(0.28)	−0.40	(1.41)
Accepted, S lied	0.01	1.01	(0.47)	2.57	(2.22)
$\pi_{t-1} < 0$ (d)	−0.25	0.78	(0.25)	1.65	(1.67)
Treatment	0.35	1.42	(0.28)	−2.09	(1.76)
Female	−0.88***	0.42***	(0.31)	−5.85***	(1.76)
Risk tolerance	0.07	1.07	(0.04)	0.29	(0.20)
Loss tolerance	0.19*	1.21*	(0.10)	0.15	(0.57)
Easy	0.05	1.05	(0.29)	0.75	(1.80)
Experienced	0.43	1.53	(0.30)	3.49**	(1.49)
Age	−0.04	0.96	(0.07)	−0.06	(0.31)
Constant	2.15	8.59	(1.95)	23.27***	(8.92)
Period dummies		✓		✓	
Observations		1,964		1,762	
Number of ID		182		180	

Robust standard errors in parentheses

$^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table 4: Sellers' probability of strategic misreporting (random-effects logit) and misreporting extent (random-effects)

Tables 4 and 5 report regression results of sellers', respectively buyers' strategic misreporting against revealing the true value (non-strategic misreporting is omitted). For both roles we first present coefficients and odds ratios for the probability of strategically misreporting, i.e. reporting $\hat{v} > v$ against $\hat{v} = v$ for sellers and $\hat{q} < q$ against $\hat{q} = q$, using a panel logit regression with standard errors clustered at the matching group level. We then focus on the extent of such strategic misreporting and report panel random effects estimates on the amount $\hat{v} - v$ (> 0), respectively $\hat{q} - q$ (< 0).[10] The set of regressors includes whether one told a lie in the previous period and in which role, whether one experienced a lie which could have been inferred from the end-of-period feedback, whether one's payoff in last period was negative, a treatment dummy and a set of demographic controls including subject's loss and risk tolerance.[11]

The regression analysis for sellers' strategic misreporting (Table 4) confirms a consistent and significant gender effect: females over-report v less often (left regression) and to a lower extent (right regression) than males.

Buyers' strategic misreporting at period t

	Probability			Extent	
	beta	odds ratio	(se)	beta	(se)
Ref cat. no lie (t-1)					
Lied as a S (t-1)	0.07	1.07	(0.24)	0.33	(1.49)
Lied as a B (t-1)	0.36**	1.43**	(0.17)	1.46*	(0.78)
Ref. cat. did not experience a lie (t-1)					
Not accepted	0.11	1.11	(0.32)	0.56	(1.27)
Accepted, B lied	0.25	1.28	(0.34)	2.91*	(1.69)
Accepted, S lied	0.55	1.74	(0.34)	−0.53	(1.47)
$\pi_{t-1} < 0$ (d)	−0.44**	0.65**	(0.21)	−0.04	(0.93)
Treatment	−0.18	0.84	(0.34)	−1.58	(1.22)
Female	−0.24	0.79	(0.28)	−1.14	(1.00)
Risk tolerance	0.03	1.03	(0.05)	0.23*	(0.13)
Loss tolerance	0.36***	1.43***	(0.11)	0.30	(0.34)
Easy	0.66**	1.93**	(0.30)	−0.01	(0.98)
Experienced	0.06	1.06	(0.30)	−0.33	(1.10)
Age	−0.15**	0.87**	(0.06)	−0.30*	(0.17)
Constant	2.23	9.30	(1.62)	30.14***	(5.24)
Period dummies		✓		✓	
Observations		1,552		1,137	
Number of ID		178		169	

Robust standard errors in parentheses

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Table 5: Buyers' probability of strategic misreporting against truth-telling (random-effects logit) and misreporting extent (random-effects)

[10] In Tables 4 and 5 the number of subjects drops when passing from the analysis of the probability of strategic misreporting to that of the extent of misreporting because in the latter we only take into account the subjects who strategically misreport (a small fraction of subjects never strategically misreport).

[11] For the computation of loss and risk tolerance score, see Appendix. Variables *easy* and *experienced* come from the Phase-3 questionnaire; they indicate, respectively, that the subject found the experiment easy and whether she participated to more than 5 experiments.

A similar analysis for strategic misreporting as buyer Table 5 does not confirm gender differences but shows that the propensity to strategically misreport in the buyer role is linked to loss aversion and cognitive load. Participants who are either less loss tolerant or experienced a loss in the previous period (dummy variable $\pi_{t-1} < 0$) are less likely to lie strategically in the current period. Also participants more able to cope with the cognitive load of (mis)reporting something which does not directly affect their own payoff (dummy variable “Easy”) more likely lie in a self serving way as buyers. In the buyer role previous strategic misreporting (variables “Lied as a B (t-1)”) affects significantly lying behavior: after lying as buyer in the previous period continuing to lie is significantly more likely.

4.2 Trade results

For common(ly known) risk- and loss-neutrality there should always be trade ($d = 1$) and prices p should be $p = p^* = E(q) = 0.4$, i.e. equal to the expected value of q. Figure 6 depicts buyers' average price proposals p for each period. It confirms the prediction $p = p^* = E(q) = 0.4$ only for the initial periods. Thereafter proposed prices steadily decline over time and converge to $p = 0.25$ later. In line with previous results, Figure 6 reveals no obvious differences across treatments C and R in initial pricing and average price dynamics.

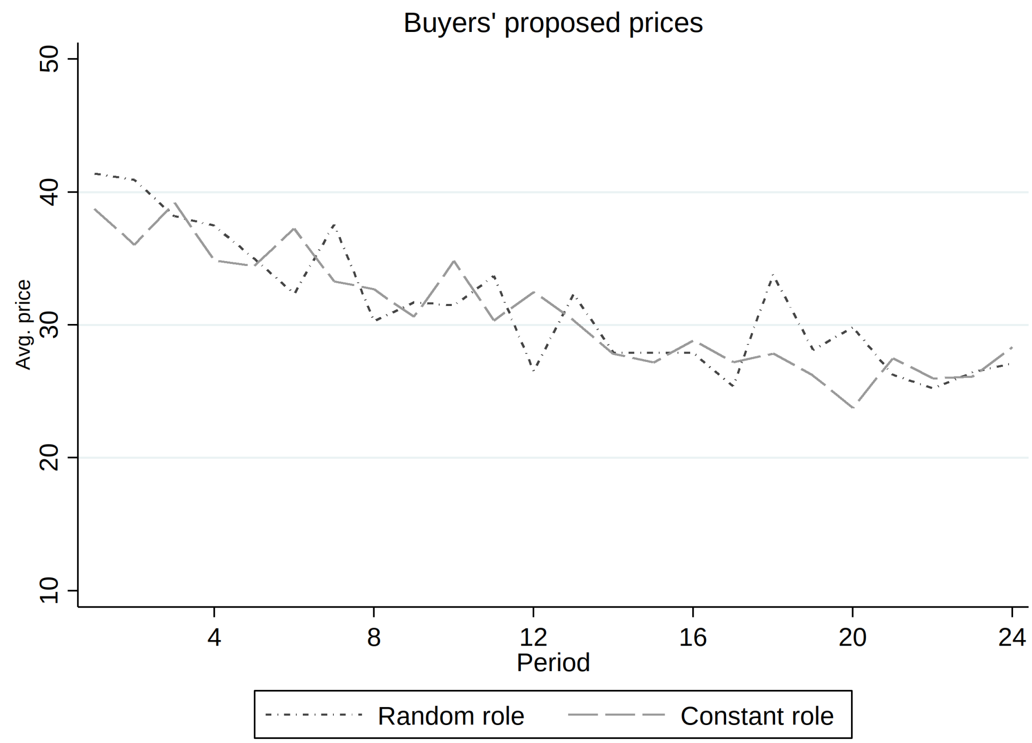

Figure 6: Average proposed price by period

The $d = 1$ prediction which states that there should always be trade is never confirmed (see Figure 7). The acceptance share of sellers is always substantial (around 50%) but nowhere near the predicted 100%, is slightly decreasing across periods and does not differ across treatments C and R.

Do Figures 6 and 7 suggest some benchmark orientation of behavior? The instructions allow participants to compute $E(q) = 0.4$ but do not state this expectation explicitly in order to weaken the demand effect to anchor price choices in the form of $p = E(q)$.

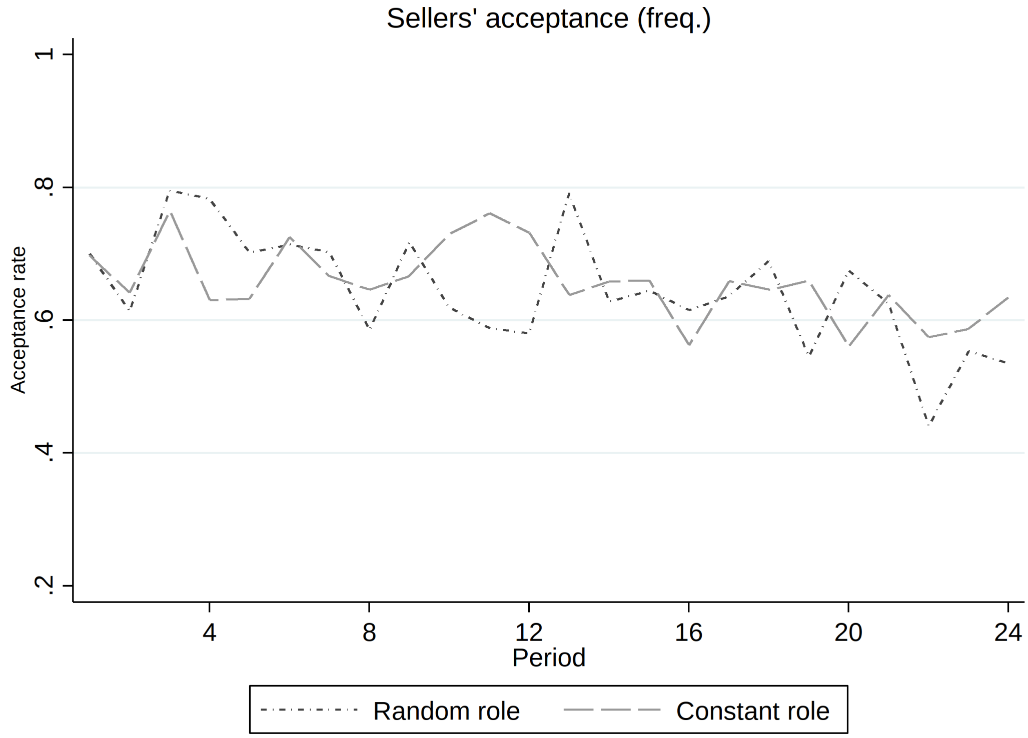

Figure 7: Acceptance rate by period

The decline of price proposals could be due to previous loss experiences stemming from believing in messages which strategically misreported. Similarly, the decrease of acceptance shares could also be due to declining price proposals and to previous loss experiences.

Figure 8 visualizes the dynamics of buyers' price proposals (left subgraph) and of seller's acceptance decisions (right subgraph) by gender. Female buyers consistently offer higher prices compared to males, while there is no clear and visible difference in acceptance rates.

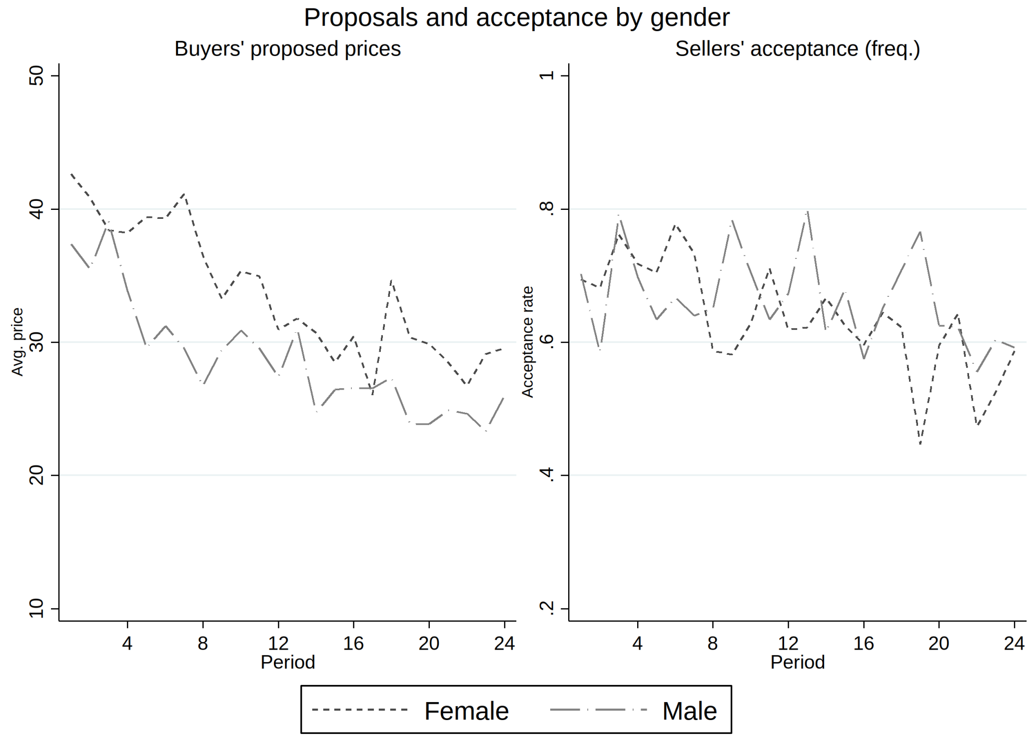

Figure 8: Acceptance rate by period

Regression results, with price proposals p as the dependent variable, are reported in Table 6. As before, we use a random-effect panel model with standard errors clustered at the matching group level and employ a similar set of regressors as in the estimates for strategic lying. Here we add, in Model 1, the private information of the buyer, i.e. the actual level of q, and the message received from the seller, $\hat{v}$. Model 2 employs the latter variable in a linear time trend interaction between the message itself and the quarter of plays, i.e. first 6 periods, second 6 periods etc., to check whether the message effects become weaker or stronger.

Results reveal no significant influence of loss but of risk tolerance: more risk tolerant participants tend to offer lower prices, thereby risking that their price may be rejected. Further findings are:

- there is no significant treatment effect,
- higher value messages $\hat{v}$ trigger significantly higher price offers p, revealing at least some influence of value messages on prices. The effect, however, becomes weaker over time,

- strategic misreporting q has a negative effect on prices, whereas higher $q-$ levels trigger higher prices (small effect), although q directly affects only the seller's payoff,

- female buyers offer substantially and significantly higher prices,

- period dummies confirm that prices consistently decline over time (see Figure 6).

Buyers' proposed price at period t				
	Model 1		Model 2	
	beta	(se)	beta	(se)
q	0.06**	(0.02)	0.06**	(0.02)
$\hat{v}$	0.30***	(0.02)		
$\hat{v}$ (*quarter 1*)			0.42***	(0.04)
$\hat{v}$ (*quarter 2*)			0.32***	(0.03)
$\hat{v}$ (*quarter 3*)			0.23***	(0.03)
$\hat{v}$ (*quarter 4*)			0.21***	(0.02)
B told a strategic lie (d)	−1.72*	(0.96)	−2.03**	(0.96)
Ref cat. no lie (t-1)				
Lied as a S (t-1)	0.04	(1.11)	0.14	(1.06)
Lied as a B (t-1)	−0.19	(0.69)	−0.15	(0.67)
Ref. cat. did not experience a lie (t-1)				
Not accepted	−0.87	(1.23)	−1.00	(1.23)
Accepted, B lied	−0.39	(1.62)	−0.51	(1.63)
Accepted, S lied	−1.55	(1.33)	−1.66	(1.32)
$\pi_{t-1} < 0$ (d)	0.28	(0.94)	0.36	(0.93)
Treatment	0.46	(1.83)	0.48	(1.82)
Female	4.58**	(1.82)	4.56**	(1.82)
Risk tolerance	−0.55**	(0.26)	−0.53**	(0.26)
Loss tolerance	−0.48	(0.59)	−0.45	(0.59)
Easy	−0.94	(1.62)	−1.00	(1.62)
Experienced	0.29	(1.80)	0.39	(1.82)
Age	−0.21	(0.29)	−0.20	(0.29)
Constant	24.88**	(9.61)	16.52	(10.20)
Period dummies	✓		✓	
Observations	2,104		2,104	
Number of ID	182		182	

Robust standard errors in parentheses
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Table 6: Random-effects logit estimates of buyers' proposed price

Table 7 analyzes acceptance $d \in \{0, 1\}$, choices via a panel logit model (both coefficients and odds ratios are reported) with standard errors clustered at the matching-group level. Analogously to Table 6, Model 1 adds the seller's private information, i.e. the value v, and the message received from the buyer, $\hat{q}$. The results show that message $\hat{q}$ hardly matters for acceptance.

Model 2 tests whether participants disregard it because, given their private information of v, p and $E(q)$, maximize their expected gains from trade.

Sellers' acceptance decision at period t

	Model 1			Model 2		
	beta	odds ratio	(se)	beta	odds ratio	(se)
v	-0.04^{***}	0.97^{***}	(0.01)			
$\hat{q}$	-0.00	1.00	(0.00)			
Ref cat. $E(\pi) \leq 0$ **(no expected gains from trade)**						
$E(\pi) > 0$				1.57^{***}	4.82^{***}	(0.15)
$\pi > 0$				4.24^{***}	69.48^{***}	(0.24)
S told a strategic lie (d)	0.35	1.42	(0.27)	-0.26	0.77	(0.22)
Ref cat. did not lie (t-1)						
Lied as a S (t-1)	0.12	1.13	(0.27)	0.12	1.13	(0.24)
Lied as a B (t-1)	0.23	1.25	(0.24)	0.41^{*}	1.50^{*}	(0.23)
Ref. cat. did not experience a lie (t-1)						
Not accepted	-0.35	0.70	(0.42)	-0.40^{*}	0.67^{*}	(0.23)
Accepted, B lied	-0.05	0.95	(0.15)	0.00	1.00	(0.20)
Accepted, S lied	-0.17	0.85	(0.33)	-0.31	0.73	(0.38)
$\pi_{t-1} < 0$ (d)	0.03	1.03	(0.21)	0.11	1.11	(0.19)
Treatment	0.04	1.04	(0.25)	0.07	1.07	(0.24)
Female	-0.10	0.90	(0.21)	0.18	1.20	(0.23)
Risk tolerance	0.05	1.05	(0.03)	0.03	1.03	(0.03)
Loss tolerance	-0.04	0.96	(0.07)	-0.01	0.99	(0.08)
Easy	-0.06	0.94	(0.25)	-0.05	0.95	(0.25)
Experienced	0.21	1.22	(0.30)	0.31	1.37	(0.26)
Age	-0.09^{*}	0.92^{*}	(0.05)	-0.08^{**}	0.92^{**}	(0.04)
Constant	4.25^{***}	69.77^{***}	(1.01)	-0.19	0.83	(1.23)
Period dummies		✓			✓	
Observations		2,104			2,104	
Number of ID		182			182	

Robust standard errors in parentheses

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Table 7: Random-effects logit estimates of sellers' acceptance decision

Results confirm the non-significant treatment effect, highlighted in Figure 7, and also show that:

- higher probability of acceptance is associated with higher values v, but not with own misreporting,
- expected gains from trade are most relevant for acceptance. Compared to the baseline category with no expected gains from trade, i.e. $p - E(q)v \leq 0$, a price which is acceptable in the sense of $p - E(q)v > 0$, substantially increases the probability of acceptance. Such increase in the probability of $d = 1$ is even more substantial when seller' losses are impossible,

i.e. when $p - 0.7v > 0$.

Strategic misreporting can be harmful to the other party's profit if believed and influencing the subsequent offer (price) and acceptance decisions. Table 6 indicates that the cheap-talk message, received from the seller, affects significantly the price proposals. A potential loss due to believing in cheap-talk messages may explain the decrease in price offers over time which in turn may be responsable for the decrease in acceptance rates (see Figure 7). Model 2 of Table 7 clearly indicates that the main driver of acceptance is "positive expected profit" and whether the price excludes any loss.

		Average profit	
		Female	Male
Role	Seller	9.54	13.43
	Buyer	4.52	7.11

Table 8: Average profit by role and by gender

Table 8 presents the average profit earned by female and male participants in seller and buyer role: sellers earn significantly more than buyers, regardless of gender, and males earn significantly more than females, regardless of the role.

Altogether, the main gender differences are: females, on average, offer higher prices as buyers; females, on average, misreport strategically less in either role where the gender effect is significantly stronger in seller role; females, on average, earn less in both roles.

5 Conclusions

Modifying the AaC-paradigm with two-sided private information and cheap-talk message exchange allowed us to explore whether mutual truth-telling enhances trade and whether there exist role and gender differences in (mis)reporting and in bargaining behavior.

Our main results reveal that mutual message exchange does not question the over-reporting value v compared to only one-sided messages (see the over-reporting results of Di Cagno et al., 2016) and that cheap-talk messages of buyers regarding the privately known q, reveal less strategic misreporting, i.e. self-serving lying. We have argued that misreporting q is cognitively more demanding since q is directly payoff relevant only for the seller what renders the incentives for self-serving misreports less obvious.

This may also explain why there are no significant gender differences in misreporting as buyers, whereas female participants strategically misreport significantly less as sellers. Given that they also offer significantly higher prices as buyers and, therefore, earn considerably less, we confirm that females surely are the fairer sex. We further find that non-strategic misreporting decreases across periods, especially for sellers, and that female participants start off with less strategic missreporting but learn to lie strategically over-time.

Surprisingly none of these conclusions is affected by the treatment, i.e. whether participants experience both roles or are constantly on the same side of the exchange. In particular, we could not confirm the expected faster learning effect for the constant role. Unlike in AaC (Samuelson and Bazerman, 1985; Di Cagno et al, 2016) where participants confront rather different tasks as sellers and buyers, in MAC the challenge of the tasks in much more balanced across roles. This, in our view, could explain why we do not confirm the predicted effect of constant versus random role assignment.

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Appendix

Loss and risk tolerance

Loss tolerance is elicited using the procedure proposed by Gächter et al. (2010). Figure 9 shows a screen-shot of the test.

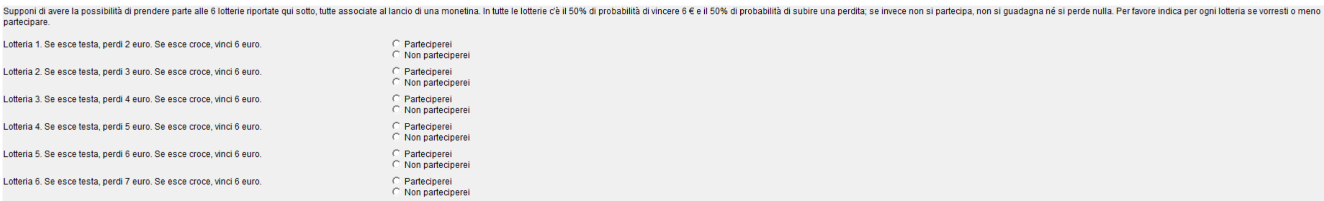

Figure 9: Loss tolerance test

The loss tolerance score is computed as follows:

- for subjects who switch from participating to not participating, the score is equal to the lottery of the first switch (from 2 to 6). Therefore, the higher the lottery number, the higher the loss that the subject is willing to bear;
- subjects who are never willing to participate to such lotteries are considered to have the lowest loss tolerance. Their loss tolerance score is set to 1;
- subjects who are always willing to participate to such lotteries are considered to have the highest loss tolerance. Their loss tolerance score is set to 7.

Following the same rationale, the risk tolerance score, based on a Holt & Laury (2002) task, is computed as follows:

- for subjects who switch from left ("safer") to right ("riskier") lottery, the score is equal to the pair of lotteries corresponding to the first switch (from 2 to 10). Therefore, the higher the lottery number, the higher the between-payoff variance that the subject is willing to bear;
- subjects who always choose the low-variance lottery are considered to have the lowest risk tolerance. Their risk tolerance score is set to 1;

- subjects who always choose the high-variance lottery are considered to have the highest risk tolerance. Their risk tolerance score is set to 11.

Experimental Instructions

[Not part of the instructions: constant role treatment]

Welcome to our experiment! During this experiment you will be asked to make several decisions and so will the other participants.

Please read the instructions carefully. Your decisions, as well as the decisions of the other participants will determine your earnings according to some rules, which will be shortly explained below. For participating in this experiment you will receive a participation fee of 5 euros. In addition to this you can earn more or loose from this amount, based on the decisions you and others may make during the experiment. However, you will never lose all your own money, as losses cannot exceed your participation fee (5 euros). The final amount you earn during the experiment will be paid individually immediately after the experiment finishes. No other participant will learn from us how much you have earned.

All monetary amounts in the experiment will be computed in ECU (Experimental Currency Units). At the end of the experiment, all earned ECUs will be converted into euro using the following exchange rate:

12 ECU= 1 euro

You will submit your decisions by clicking the appropriate buttons on the screen. All participants are reading the same instructions and taking part in this experiment for the first time.

This experiment is fully computerized. Please note that from now on any communication between participants is strictly prohibited. If you violate this rule, you will be excluded from the experiment with no payment. If you have any questions, please raise your hand. The experimenter will come to you and answer your questions privately.

After the experiment you will be asked to answer a short questionnaire; please note that the data will be treated anonymously.

Description of the Experiment

Before the first round starts, each participant is randomly assigned to one of two possible roles. Half the participants will assume the role of Buyer; the other half will assume the role of Seller. You will remain in the same role throughout the experiment, which will last for 24 rounds. In each of several successive rounds you will be randomly matched with a different participant in the other role. For example, if you are a Buyer, then you will be randomly and anonymously matched with another participant who is a Seller, and vice versa.

In each round, a firm owned by the Seller, may or may not be bought by the Buyer. The computer will randomly select the Buyer's value of the firm, v, among the following values: 5, 10, 15, 20, 25, 30, 35, 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90 and 95 (with all these values being equally likely). Although v is the Buyer's evaluation of the firm, owned by the Seller, its value will be communicated only to the Seller. The Buyer will not learn the value of the firm which is selected randomly by the computer.

The Seller's evaluation of the firm, $q\dot{v}$, is proportional to the Buyers value of the firm, selected by the computer. The disparity coefficient, q, will be randomly selected by the computer and can only take one of the following values: 10, 20, 30, 40, 50, 60 or 70 percent (with all these values being equally likely). The computer randomly selects one of the 7 possible disparity coefficients q and the random result q is revealed only to the Buyer. The Seller will not learn which disparity coefficient is selected randomly by the computer. Thus of the Seller's evaluation of the firm, $q\dot{v}$ of q, only the Buyer knows q, whereas only the Seller knows v.

As an example, suppose that the computer selects a value v of 80 and a disparity coefficient, q of 50 percent so that the Seller's evaluates the firm with 40, corresponding to 50 percent of 80 $(0.5 \cdot 80)$. In this case, the Seller will find on the screen only the value of the firm, 80, whereas the disparity coefficient of 50 percent is only revealed to the Buyer on his screen.

Before negotiating whether to sell the firm, the Seller sends a value message to the Buyer about the value of the firm, which can be either true or false. Similarly, the Buyer sends a disparity message to the Seller which also may be true or false, i.e. the messages are not necessarily reliable. Each message is restricted to the integers which are possible for v, respectively q, i.e. the value message can be 5,10,15...up to 95 and the message concerning the disparity coefficient can be 10, 20... up to 70 percent.

After having sent and received the respective messages, the Buyer offers a price, p, to the Seller which can be any integer number between 0 and 100. Having received the price proposal of the Buyer, the Seller decides whether to accept it or. If she accepts, the firm is sold at the proposed price, p, to the Buyer. If the Seller does not accept, no trade takes place.

After the Seller has decided, the payoffs of Buyer and of Seller are calculated and individually communicated at the end of each round.

Calculation of the payoff: the payoff in each period is calculated as follows.

If the Seller has accepted the offered price, the payoffs are:

- The Buyer earns the difference v-p between his value v of the firm and the accepted price p.
- The Seller earns the difference $p - q \cdot v$ between the accepted price p and his evaluation $q \times v$ of the firm.

If the Seller rejects the offer, both the Seller and the Buyer earn 0 ECU.

An example: suppose the firm value (value for the buyer) is equal to 45 and that the disparity coefficient is 70 percent, so that the Seller's evaluates the firm 31.5 ($0.7 \cdot 45$). Suppose the Buyer proposes a price equal to 40. If the Seller accepts it, the Buyer earns 45-40 = 5, and the Seller earns 40-31.5 = 8.5; if the Seller rejects, both earn 0.

At the end of the experiment, the computer will randomly select one round and the payoff you realized in that round will constitute your final payment for the experiment, plus a show-up fee of 8 euros.

Note that the exchange may also result in a loss for either the Buyer or the Seller. If you realized a loss in the round selected for payment, in any case the latter will never exceed your show-up fee.