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Neutral versus loaded instructions in a bribery experiment

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Abstract This paper contributes to the ongoing methodological debate on context-free versus in-context presentation of experimental tasks. We report an experiment using the paradigm of a bribery experiment. In one condition, the task is presented in a typical bribery context, the other one uses abstract wording. Though the underlying context is heavily loaded with negative ethical preconceptions, we do not find significant differences with our 18 independent observations per treatment. We conjecture that the experimental design transmits the essential features of a bribery situation already with neutral framing, such that the presentation does not add substantially to subjects' interpretation of the task.

Keywords Corruption · Context · Framing · Valence · Experimental Instructions · Laboratory · Trust · Reciprocity · Ethical Behaviour · Social Norms

JEL Classification C91 · D62 · D72 · D73 · K42

Introduction

Experimental economists are concerned about possible distortions of data caused by suggestive wording. Therefore, it has become a tradition to formulate instructions for economic experiments as neutrally as possible, avoiding all connotations caused by the choice of words that might affect decision behaviour.[1] Even when researchers are modelling real-life situations, they often present the task to the subjects in completely abstract terminology. Examples

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[1] See Davis and Holt (1993), Holt (1995) and Kachelmeier and Shehata (1997).

of experimental studies that interpret their results as real-life situations differing from the context of the instructions cover a broad area of research interests. Fehr et al. (1997) study experimental labour markets, but present the task to their subjects as an interaction between sellers and buyers. Fehr and Zych (1998) study intertemporal buying behaviour and draw conclusions on addiction. Güth et al. (2004) conduct a 2-period ultimatum game framed as a joint venture, and interpret it as engagement in a marriage. Keser et al. (2004) conduct a neutrally framed public good game where contributions are interpreted as joining a union. Potters and van Winden (2000) draw conclusions from a context-free signalling game with regard to behaviour of lobbyists and policymakers. Irlenbusch and Sutter (2003) conduct a neutrally framed public bad experiment supplemented with a voting procedure and interpret it as fiscal policy making of EMU-member countries in the context of the Stability and Growth Pact.

Arguably, using neutrally worded instructions has become the mainstream practice in experimental economics. However, this approach is not at all undisputed in the community. Several scholars argue that the use of neutral instructions may distort the interpretability of experimental results with respect to the real-life situations researchers are interested in. Eckel and Grossmann (1996) emphasise that the importance of social and psychological factors can only be studied by (at least to some extent) abandoning abstraction.[2] As Loomes (1999, F39) comments: "It may be rather more useful to try to study the impact of context than to pursue the impossible goal of eliminating it."

The aim of the present paper is to contribute to the research agenda suggested by Loomes in the above quotation. Though many scholars hold strong views on the "right" way of presenting experimental tasks, systematic comparisons of context-free and in-context presentations of the same task are surprisingly rare. However, the existing evidence shows that even the slightest changes in the instructions' wording can change the experimental outcomes tremendously. In the experiments reported by Ross and Ward (1996) and Liberman et al. (2004) a simple prisoners' dilemma game is labelled differently in two different treatments. In one of them it is called the "Community Game", in the other one the "Wall Street Game". Otherwise, the game and the instructions are identical. As a consequence of this small change, the rates of co-operation dropped significantly in the "Wall Street" labelling. This result, originally found with American college students, was replicated with Israeli pilot trainees. Moreover, the magnitude of this effect was dramatically underestimated in predictions made by people who knew the players well.

A comparably strong effect has been found by Burnham et al. (2000), who conduct an experiment on a two-player reciprocity game. In their sequential game, both players can improve their payoff over the subgame-perfect equilibrium by a sequence of co-operative moves. To do so, however, it is essential that the first mover trusts the second mover, and the second mover reciprocates. The game is played repeatedly in two treatments with distinct pairs on each trial. Treatments are identical except for one single expression: In the first treatment, the other player is called "partner", in the second treatment "opponent". This priming by exchanging one word only is already sufficient to induce a significant difference in trust and trustworthiness behaviour: trustworthiness is observed over twice as much when the counterpart is called "partner".[3] This reinforces trust, although both trust and trustworthiness erode over time.

[2] Similar arguments are put forward by, e.g., Gigerenzer (1996), Ortmann and Gigerenzer (1997), Loewenstein (1999), and Gächter and Riedl (2005).

[3] See also Hoffman et al. (2000). For a further study on priming effects see Ortmann et al. (2000).

These results look alarming for experimental economists. If behaviour in the laboratory is so sensitive to small changes in the presentation of the task, can we comfortably project findings from experiments into the real-life situation we are interested in, i.e. to what extent can we trust the *external validity* of laboratory results? This question seems especially crucial when the real-world context we study is heavily loaded with positive or negative preconceptions. This is the case in many experimental areas, but becomes especially obvious for the experimental modelling of crime, as e.g. in tax evasion and corruption experiments.

In this paper, we raise the question whether and to what extent the presentation of context affects behaviour in a corruption scenario. Corruption seems a very natural field to study our research question. First, as a criminal and socially harmful activity, corruption is strongly loaded with negative attitudes towards it. Thus, in light of the dramatic effects observed in previous experiments an experimental corruption scenario should be particularly susceptible to effects induced by experimental instructions. More precisely, we should expect that if the morally negative context of corruption is made salient to the subjects, this should draw their attention to the negative consequences of it and make them less prone to engage in bribery. Second, the effect of wording on individuals' propensity to engage in corrupt activities may have relevant policy implications, as getting the message right is crucial for the effectiveness of anti-corruption campaigns.

The experimental paradigm we use is the bribery game by Abbink et al. (2002, AIR hereafter), who model a bribery relationship between briber and bribee as a trust game[4] in which reciprocal behaviour is socially undesirable and subject to punishment when discovered.[5] We report two treatments of the experiment. In one treatment, we present the task as an interaction between a firm and a public official, where the firm can make private payments to get a permission for running a plant. The firm's activity causes negative consequences to the public. In the other treatment, taken from AIR, the game is presented in a completely neutral fashion.

This design allows us to test the above-mentioned hypothesis that loaded instructions reduce the level of corruption (or equivalently, that neutral instructions take away the negative connotations of it and thus increase the level of harmful reciprocation). However, we do not find support for this hypothesis. Our data exhibit only mild effects of the treatment, and we do not find significant differences. This is surprising as our manipulation appears far stronger than the ones reported in the literature mentioned earlier. We conjecture that the experimental design transmits the essential features of a bribery situation already with the neutral framing, such that the presentation does not add substantially to subjects' interpretation of the task.

Experimental evidence on framing effects

In this section we give a brief overview on the economic and psychological literature on framing effects. A framing effect is said to be present if the presentation of the task leads decision makers to change behaviour, even though the underlying information and decision

[4] In trust (or reciprocity) games a first mover can send money to a second mover, who in turn can voluntarily reward the trustor by sending money back. The games are constructed such that by doing so, both players are better off with respect to final payoffs, but in equilibrium no trust and no rewarding would be exhibited. However, subjects frequently co-operate (Fehr et al., 1993; Berg et al., 1995; Dufwenberg and Gneezy, 2000; Fahr and Irlenbusch, 2000).

[5] A considerable theoretical literature on corruption has preceded these experimental studies. References include Rose-Ackerman (1985), Klitgaard (1988), and Shleifer and Vishny (1993). Other experimental studies on corruption (for an overview see Abbink, 2005) use either only loaded or only neutral instructions.

options remain essentially the same. In this case, we may say that two statements of a problem are *logically equivalent*, but not *transparently equivalent* (Cookson, 2000; Rabin, 1998).

We report on a rather broad domain of research areas related to the goal of our study. The major categories of framing effects we are interested in are *valence* and *pure framing* effects. A valence effect is known as the fact that the same essential information is given in either a positive or negative light (Levin et al., 1998). A pure framing effect is said to be present if subjects are confronted with alternative, but objectively equivalent problem wordings (Elliott and Hayward, 1998). The dividing line between these two categories is not always sharp; one can say that in general, a pure framing effect does not involve different value judgements of the frames. Putting subjects into the context of a bribery environment may elicit associations attributable to the impact of both kinds of framing effects.

The first instances of valence effects are reported in the seminal studies by Kahneman and Tversky (1979) and Tversky and Kahneman (1981).[6] This research is concerned with the *reflection effect* of prospect theory (Kahneman and Tversky, 1979). Individuals are willing to take risks when it gives them a chance to avoid losses, but they tend to be risk-averse when they are confronted with opportunities to make gains. The famous study by Tversky and Kahneman (1981) on the Asian disease problem shows a *choice reversal effect*. The authors elicit subjects' attitudes towards a program against a fictitious disease, and present the tasks either in positive terms (number of lives saved) or in negative terms (number of lives lost). Subjects had to choose one of two options: a *risky* outcome with identical expected value and a *sure* outcome. Tversky and Kahneman find that the majority of subjects who are given the positively framed task chooses the sure outcome whereas the majority of subjects who are given the negatively framed task goes for the risky choice.

In the *economic* literature, valence effects are frequently studied in a public good setting. A gain frame in which the incentive structure is framed as a positive externality is contrasted with a loss frame that states a negative externality (c.f. Andreoni, 1995; Sonnemans et al., 1998; Willinger and Ziegelmeier, 1999; Cookson, 2000; Park, 2000).[7] All authors find strong and systematic framing effects. Co-operation in the positively framed situation is significantly higher than with a negative frame. However, effects of framing are not confined to social dilemma situations. Weber et al. (2000) find that also in an experimental market environment negative and positive framing of endowments affects outcomes. Subjects were confronted with either a potential loss or a potential gain. They were willing to pay substantially higher prices to buy assets to cover a potential loss. In addition, trading volume was affected by framing. Traub (1999) investigates the impact of the valence effect by framing a tax privilege for being married (having one or two child/ren) either as a tax rebate for married couples (with child/ren) or as a tax surcharge for singles (childless couples). He finds evidence that the tax rebates (gain frame) on average exceed the corresponding surcharges (loss frame).

A rich body of *psychological* valence research shows that cognitive, individual and situational factors reinforce or mitigate framing effects. An extensive survey on psychological valence research and a typology of valence effects is given in Levin et al. (1998)[8] showing that studies of these effects include domains as diverse as cognition, psycholinguistics,

[6] For recent studies replicating the results of the original Kahneman and Tversky studies see Kühberger et al. (1999) and Druckman (2001).

[7] Park (2000) replicated the original Andreoni (1995) experiment with more independent observations and confirmed Andreoni's findings on the framing effects.

[8] See Levin et al. (2002) for a recent study on valence effects using a within-subject design.

perception, social, health, clinical and educational psychology as well as marketing and other business fields.

Early investigations of *pure framing* effects are the experiments by Pruitt (1967) as well as Selten and Berg (1970). Pruitt (1967) compares the usual presentation of the prisoner's dilemma in bimatrix form with the decomposed form, and shows that the latter leads to substantially higher co-operation. Selten and Berg (1970) in a study of face-to-face duopoly experiments, systematically vary the presentation of the payoff-relevant variable 'financial assets', composed of initial assets and total profits. This variation, though game theoretically irrelevant, essentially influences the mode of co-operation.[9]

The impact of *instruction* framing on behaviour, as addressed in the present study, has rarely been systematically tested. Cooper et al. (1999) report an experiment on the ratchet effect game[10] with managers and students in the People's Republic of China using in-context versus generic instructions. They find that loaded instructions had a much larger and more consistent effect on managers than on students. Using a context facilitated the development of strategic play among managers but had much lower effect on students.

The wording of experimental instructions can also draw subjects' attention to social or ethical aspects of the environment. The *social* aspect of framing is addressed in the study by Burnham et al. (2000) mentioned earlier. The *ethical* aspect is tackled in two experiments on tax evasion. Baldry (1986) conducts one treatment in which a tax evasion situation is presented to subjects as such, and compares the results to a treatment in which the corresponding decision task is presented as a gambling opportunity. He finds that subjects tend to "evade taxes" much more in the gambling treatment. On the other hand, Alm et al. (1992) compare loaded and neutral instructions in an otherwise identical tax evasion task, and do not find any significant differences.

The experimental design

Consider the following situation: a firm wishes to run an industrial plant which causes negative consequences to the public (e.g. running the facility might pollute the environment). A public official must decide every period whether to give the permission to the firm. The firm, however, can make a private payment to the official in the hope (but without a possibility of enforcement) to influence the official's decision. Since bribery is illegal, binding contracts on such a mutual exchange of favours cannot be made, such that a bribery relationship must rely on trust and reciprocity.

The game we use in our experiment has been introduced by AIR. A firm, as a potential briber, first decides whether to make a private payoff to a public official. If he decides to do so, he must specify the amount to be sent, which can be an integer of the range from 0 to 9 *talers* (the taler is the fictitious experimental currency). If he transfers a positive amount, the public official decides whether to accept or reject the bribe. If she rejects, no money is transferred, but the firm must pay a relatively small transfer fee of 2 talers. The fee represents the initiation costs of the briber when he approaches the civil servant to establish a reciprocal

[9] Related studies are Albers and Harstad (1991), Schotter et al. (1994), Neale and Bazerman (1995), Johannesson and Johansson (1997), Elliott et al. (1998), Blount and Larrick (2000), and Fehr and Tyran (2001).

[10] The ratchet effect is especially important in centrally planned economies if high-productivity firms signal a low productivity by reducing their performance on which the central planner bases his targets on input usage and output levels. If firms behave in such a manner the ratchet effect can seriously reduce production and is therefore said to be especially important.

relationship. These costs can be considered as being independent from the later course, i.e. they must be paid also if the official should reject the bribe. If the public official accepts the bribe, then the amount offered is deducted from the firm's account. The amount is then multiplied by the factor three before being credited to the official's account. The multiplier reflects a difference in marginal utility: the same amount of money can be expected to mean much less to a relatively rich and large firm than to a public official with a smaller income.[11]

When a bribe has been accepted, a lottery is played out. With a probability of 0.3%, the sudden death event occurs: Both players are disqualified from the experiment. Their cumulative earnings are cleared from their accounts, and they are not allowed to play further rounds. The sudden death, which is probably the most severe penalty to be imposed in the experimental framework, represents the consequences arising from discovery of corrupt activities, namely drastic fines and job loss.

At the last stage of the game, the public official must decide whether or not to permit running the plant. Since giving the permission requires effort to justify her choice before her superiors, it is slightly less preferable to her, apart from potential bribes. However, it is much more favourable to the briber. In numbers, both players receive a payoff of 36 talers when the permission is refused, whereas in case of permission the payoffs are 56 talers for the firm, and 30 for the public official (not including private payments). In addition, the permission damages the public: each of the other participants in the session suffers a deduction of 3 talers. The consequences of a permission to a single individual are relatively small, but they add up to substantial amounts since they are spread over many people. In total, permissions are inefficient: the mutual gains obtained by the two players in a pair never exceed the efficiency loss of 48 talers caused by the damage done to the 16 other participants.[12]

As corruption is done secretly, no feedback is provided about decisions made by participants playing in other pairs. Thus, no-one possesses any information about the corruption level in the session, and consequently no subject is informed about the extent to which (s)he is damaged by others. Both treatments were played in 30-round supergames between the same players. Thus, a long-term relationship between a briber and a public official was modelled.

Figure 1 depicts the game tree of the stage game. Player "F" is the firm, player "P" the public official. "C" denotes a chance move. The "hangman" symbol illustrates the event of sudden death. The lines "–3...–3" mean that all 16 other subjects are damaged by 3 talers.

The equilibrium outcome of the stage game is straightforward to obtain. On an equilibrium path, the public official will always refuse the permission at her terminal decision nodes. Given that, the payoff the firm can get by making a private payment (34, $34 - t$, or the sudden death) is always strictly worse than the 36 talers he will receive when he transfers nothing. Thus, in an equilibrium, the firm does not pay bribes, and the official does not grant a permission.[13]

The experiment was conducted at the *Laboratorium für experimentelle Wirtschaftsforschung* at the University of Bonn. All subjects were recruited with posters on the campus advertising the experiment. Most of them were students from various disciplines, where law and economics students constituted the largest fractions. Subjects could participate in only one treatment of any bribery experiment.

[11] Further, the factor ensures that negative total earnings cannot result from the firm transferring too much.

[12] Note that only the permission causes an externality, not a bribe as such, which we interpret as a mere transfer of money that per se does not harm the public. There might be long-term consequences if pervasive corruption impedes economic development, but these are outside the scope of our study.

[13] A detailed proof is available upon request (cf. also Abbink, 2004).

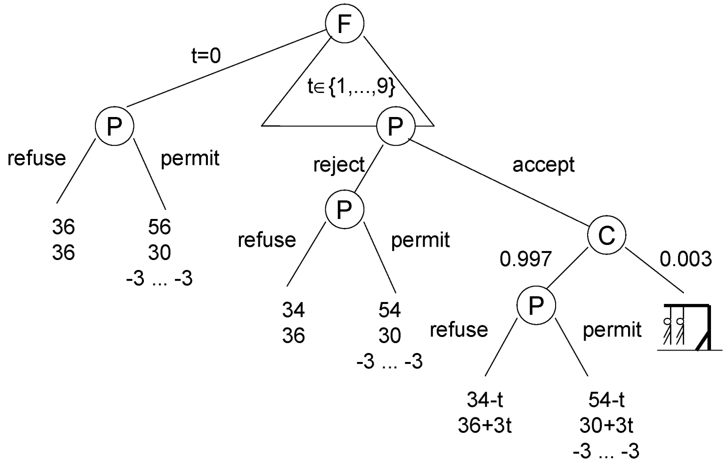

Fig. 1 The game tree of the stage game

The design of the experimental software (implemented using *RatImage*, Abbink and Sadrieh, 1995) was analogous in both treatments, except that in the loaded instructions condition the neutral wording has been replaced by the corresponding terms of the bribery story. All possible moves were visible on the same screen. After all decisions of a round had been made, the subjects were informed about their payoffs resulting from their own pair's decisions, and they were reminded that their payoffs would also be influenced by the decisions of all other pairs in the experiment.

Each session began with an introductory talk. A translation of the hand-outs is reproduced in the appendix. Payoff tables, also reproduced in the Appendix, were handed out to increase the transparency of the game. The main differences in wording between the loaded and the neutral instructions are shown in Table 1. In addition to the replacement of expressions, a paragraph was added describing the real-life situation modelled in the experiment.

The instructions were read aloud and explained in detail. After the introduction, the subjects were seated in cubicles, visually separated from one another by curtains. The terminal numbers, which determined the role of a subject as being firm or public official, were assigned to the subjects by random draw. After the subjects had been seated, the play started immediately. The role of a participant remained unchanged throughout the experiment. In the first round, pairs of participants, i.e. one firm and one public official, were matched randomly and remained unchanged throughout the experiment. The thirty rounds of the experiment were played in slightly less than an hour, such that a whole session took about $1^1/_2$ hours including instructions.[14]

To ensure that disqualified subjects would not leave the session, we gave them on-screen questionnaires, which they had to fill in while the other subjects completed the session. These questionnaires were meant to keep disqualified subjects busy rather than to collect meaningful data. A lump sum show-up fee of DM 5 incentivised disqualified players to remain seated.

[14] We did not conduct the experiment with a double-blind protocol, which seemed impractical in our computerised set-up. Thus we cannot rule out that experimenter demand effects might influence our result. We would then expect that the explicit use of loaded language increases demand effects and thus lowers corruption. Note that this possible effect only strengthens our conclusions.

Table 1 Vocabulary used in the two treatments

Loaded	Neutral
"firm"	"player 1"
"public official"	"player 2"
"private payment"	"transfer"
"grant the permission"	"choose Y"
"do not grant the permission"	"choose X"

Immediately after the session, the subjects were paid anonymously in cash, at an exchange rate of 0.03 DM per taler. The total earnings ranged from DM 5.00 (in the neutral instructions treatment, one pair of subjects was unlucky in the sudden death lotteries) to DM 46.67 with an average of DM 33.38 for $1^1/_2$ hours, which is considerably more than a student's regular per hour wage in Bonn. 1 DM is equivalent to € 0.51. At the time of the experiment the exchange rate to the US-$ was approximately 0.49 $/DM.

Two sessions with 18 subjects were conducted with each treatment. Since each session comprises nine statistically independent observations, we obtain 18 independent observations in each treatment.

Results

We measure the level of corruption mainly by two variables. The *average offered transfer* measures the firms' propensity to pay bribes, the *frequency of permissions* measures the extent to which decisions have been manipulated by bribery. Table 2 shows the average offered transfers per round made by the single firms. Table 3 shows the average frequency of permissions given by the single public officials. All numbers are ordered from the lowest to the highest value in a treatment. In both treatments the theoretical prediction of no bribes and no permissions is refuted, as a large majority of the firms bribe and public officials give permission. This is in line with most previous evidence on trust game experiments.

We do not find strong support for an instruction framing effect in the data. Figure 2 shows the average amount that is offered per round, over the 30 rounds of the experiment, in the aggregate of all sessions. On average over the whole experiment, 2.56 talers are transferred per round, compared to 2.93 talers in the neutral instructions treatment. Figure 3 shows the distribution of bribe offers. In both treatments, the distribution of offers is rather similar, with peaks at transfers of 0 and 6 talers.[15] The slightly smaller average bribe offer in the loaded instructions treatment is not significant. The Mann-Whitney-U test, applied to the average bribe offer in the 18 pairs of each treatment, does not reject the null hypothesis of equal bribe offers in both treatments ($p = 0.39$ one-sided).

Figure 4 shows the average percentage of permissions given. Though the average frequency appears to be somewhat smaller with loaded instructions (32.4% compared to 43.3% with neutral instructions), the result is far from being significant. The Mann-Whitney-U test, applied to the average percentage of permissions in the 18 pairs of each treatment, does not reject the null hypothesis of equal permissions in both treatments ($p = 0.27$ one-sided). Thus, we cannot reject the null hypothesis that the lower tendency to give permissions (choose the Y alternative) is due to (random) sampling variation.

[15] If the briber transfers 6 talers, and the official accepts and gives the permission, then both players' payoffs are equal (48 talers). Payoff equalisation appears to be the predominant fairness norm between briber and bribee.

Table 2 Average bribe offer by individual firms

Loaded		Neutral	
Pair	Avg. transfer	Pair	Avg. transfer
17	0.0	7	0.0
14	0.0	1	0.0
8	0.1	16	0.3
6	0.2	8	0.5
10	0.6	9	0.7
3	0.6	18	1.3
5	0.8	12	1.6
11	1.3	13	2.1
16	1.5	2	3.2
13	1.8	11	4.1
12	3.4	15	4.2
15	3.6	17	4.4
4	4.8	5	4.5
1	5.2	3	4.6
9	5.3	4	5.0
7	5.4	14	5.0
18	5.6	10	5.4
2	5.8	6	5.8
Avg.	2.56	Avg.	2.93
st. dev.	2.20	st. dev.	2.02

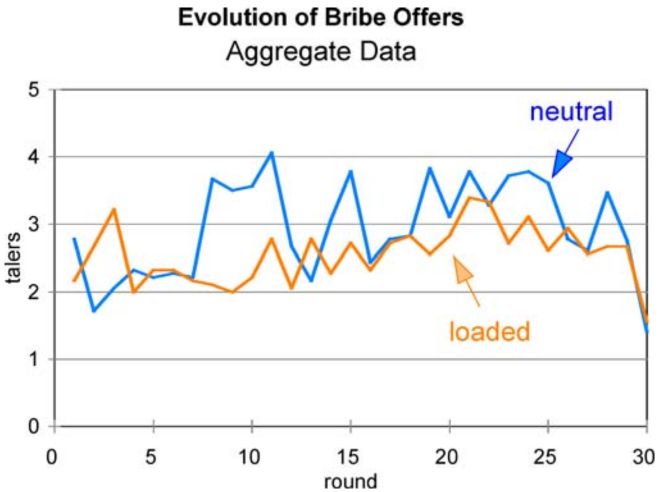

Fig. 2 Evolution of bribe offers

Table 3 Frequency of permissions by individual public officials

Loaded		Neutral	
Pair	Rel. freq. of permissions	Pair	Rel. freq. of permissions
8	0.00	16	0.00
17	0.00	1	0.00
6	0.03	8	0.03
3	0.07	7	0.03
11	0.07	12	0.07
5	0.10	9	0.07
14	0.10	13	0.23
10	0.10	18	0.33
13	0.13	2	0.47
16	0.13	15	0.50
15	0.27	3	0.70
12	0.43	10	0.70
4	0.53	5	0.70
2	0.63	17	0.70
1	0.70	11	0.73
7	0.80	4	0.77
9	0.87	14	0.83
18	0.87	6	0.93
Avg.	0.32	Avg.	0.43
st. dev.	0.31	st. dev.	0.33

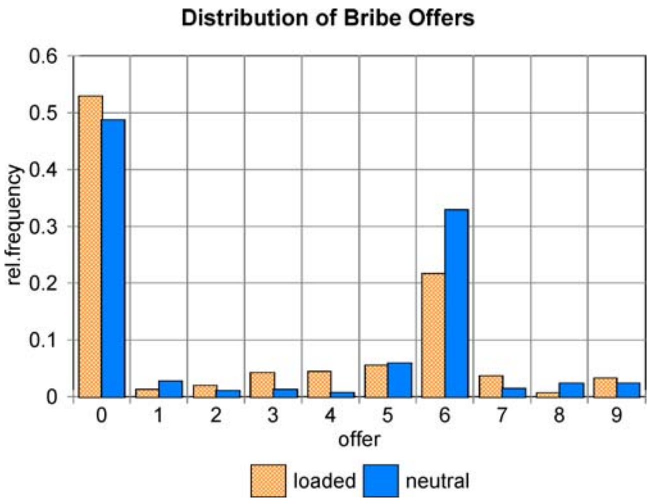

Fig. 3 Distribution of bribe offers

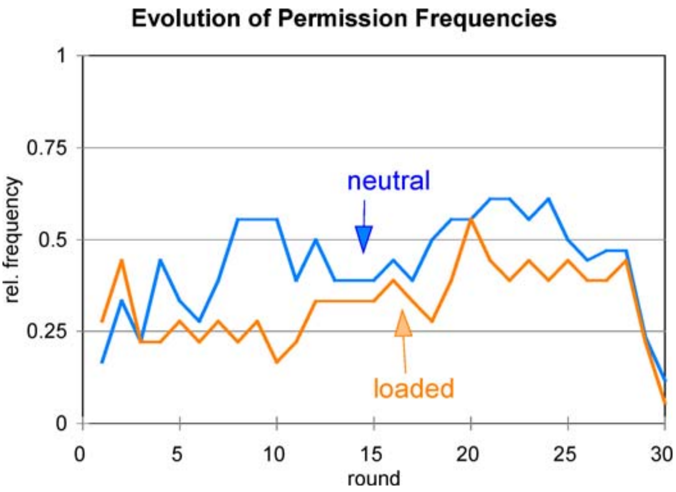

Fig. 4 Evolution of permission frequencies

Figure 2 might suggest a slightly positive trend of bribe offers over time. Indeed, linear regression coefficients over the aggregate average bribe offers are positive in both treatments (+0.240 in the loaded and +0.335 in the neutral treatment). However, a rigorous analysis of the disaggregate data does not confirm this impression. If we calculate linear regression coefficients for the single independent observations, we find a fairly balanced distribution of positive and negative coefficients. In the loaded treatment seven pairs exhibit positive and eight pairs negative regression coefficients. In the neutral treatment seven pairs have positive and nine pairs negative coefficients (the remaining observations have a coefficient of zero). The binomial test does not reject the null hypothesis of an equal probability of positive and negative trendlines at any conventional level.[16]

Table 4 shows the public officials' responses to offered bribes. Relatively few bribe offers are rejected. The percentage of rejected positive offers is slightly higher with loaded instructions (21.3% compared to 12.0% with neutral instructions), but the difference in rejection frequencies is not significant ($p = 0.342$ one-sided, according to the Mann-Whitney U-test applied to the absolute number of rejections by the individual public officials). Rejection rates do not appear to be strongly dependent on the bribe offer. The linear regression coefficients, with bribe offer as the independent and rejection rate as the dependent variable are very close to zero (+0.001 in the loaded and −0.048 in the neutral treatment). However, it should be noted that for some bribe offers only very few observations are available. Hence, a rigorous analysis using disaggregate data of the single independent observations is not possible.

For lower bribe offers it is not uncommon that public officials accept the bribe, but refuse permission. Possibly these bribes are considered too low. Opportunistic officials then prefer to accept them, but not to reciprocate by granting the permission. Again, the data are too sparse to apply reliable statistical analysis.

[16] Both treatments show an end game effect, with less co-operation towards the last period (as visible in figures 2 and 4). We chose to tell subjects the number of rounds in order to increase transparency and to obtain a clear theoretical prediction. However, the end game effect is not the cause for our failure to find significant trends. If we remove the last five rounds the conclusions remain the same.

Table 4 Responses of public officials to bribe offers (frequencies)

		Loaded framing				Neutral framing			
Offer	Bribe: Permission:	Decline Refuse	Decline Grant	Accept Refuse	Accept Grant	Decline Refuse	Decline Grant	Accept Refuse	Accept Grant
0		–	–	275	11	–	–	240	22
1		6	1	0	0	6	0	7	2
2		8	1	2	0	2	0	3	1
3		11	1	8	3	4	0	3	0
4		10	1	7	6	0	0	3	1
5		8	1	7	14	1	1	6	24
6		7	4	12	94	13	3	8	152
7		0	2	3	15	0	0	2	6
8		0	2	0	2	1	1	2	9
9		0	1	1	16	1	0	2	10

Concluding remarks

Our results do not provide strong evidence for instruction framing in the present bribery experiment. If there is a tendency towards lower levels of corruption (which might be suggested by the lower average bribe offers, the higher percentage of rejected positive offers and the lower frequency of permissions given by the public officials), it appears to be weak, at least not strong enough to show up as statistically significant in our data set of 18 independent observations per treatment. Although our design involves much stronger manipulation of the instructions than previous studies, and captures an activity heavily loaded with negative ethical connotations, we are not able to find support for instruction framing. This suggests that instruction framing, though it has been found in some contexts, is not necessarily a robust phenomenon. Rather, it seems to be very specific to the strategic environment under study.

The data suggest that the present environment is not very sensitive to framing effects through differently worded instructions. Why could we obtain no framing effect? Several answers seem plausible. First, the game was developed with the sole purpose to capture the most important features of a bribery scenario. Thus, even with neutral framing the game may appear less abstract than the more generic games studied earlier. Second, the game is very simple. The loaded framing does not seem to improve its accessibility. If subjects are guided by the structure of the game only, then the loaded framing should not influence their approach to the task. Third, all payoffs are presented in the same way. Therefore, effects arising from different presentations of numbers (as in gains versus losses frames) cannot be effective. Fourth, the treatments are not characterised by a different degree of complexity. Subjects do not have to invest cognitive effort to translate one situation into another. Therefore, subjects might "transcend the frame" (Chatterjee et al., 2000). Finally, the thirty rounds of the experiment give subjects time and opportunity to familiarise themselves with the situation. The last explanation alone, however, cannot fully explain the absence of an effect, since previous research has found framing effects in repeated games with partners matching (Liberman et al., 2004).

What do our results contribute to the debate on the "right" way to present an experimental task? On the one hand, they suggest that neutrally framed experiments are not necessarily less interpretable in terms of a real-life situation than those presented in a context. On the other hand, they indicate that using context does not necessarily distort experimental results or distract subjects from the strategic situation. Of course, our experiment can be just one step towards a comprehensive exploration of the matter. Identifying characteristics that make an experiment prone to such effects will be an important research agenda for the future.

Appendix: The written instructions (original text in German)

Loaded treatment

In this experiment you are in an interactive decision situation between a firm and a public official. The firm wishes to run a plant which causes negative consequences to the public. In each round, the public official must decide whether or not to grant the permission. In advance, the firm can offer a private payment to the public official, who can accept or reject the offer. All in all **18 persons** participate in the decision making experiment. There are two types of participants: **Firms** and **public officials**. At the beginning of the experiment, the type of each participant is randomly drawn. **The type of a participant remains unchanged throughout the experiment.**

In the first round, pairs of participants are matched randomly. One firm and one public official are matched to one another. Thus, both participants do not know with whom they play. The pairs remain unchanged throughout the experiment.

The experiment consists of **30 rounds**. At the end of the experiment you will receive a **payoff** that depends on your success.

Decision situation in a round

Stage 1: Offer of a private payment

First, the firm decides whether or not he wants to offer a private payment to the public official. If he does, then the credit of the firm is reduced by offer costs of **2 talers**, and the play is continued with stage 2. If the firm does not want to offer a private payment, then both credits remain unchanged, and the play is continued with stage 4.

Stage 2: The amount to be offered

The firm decides on the amount to be offered to the public official as a private payment. The firm can choose between **1, 2, 3, 4, 5, 6, 7, 8 or 9 talers**. The play is continued with stage 3.

Stage 3: Acceptance or rejection of the private payment

The public official decides on whether he **accepts** or **rejects** the proposed private payment.

- If the public official accepts the private payment, then the credit of the firm is reduced by the amount he proposed. The public official's credit is increased by the **tripled amount** that is paid. If a private payment is made and accepted, then this can be discovered with a certain probability. Therefore, a number out of the range from 0 to 999 is randomly drawn.

  If the randomly drawn number is **0, 1, or 2**, then the private payment is discovered. The firm and the public official are **punished with disqualification**. That means: **The play ends for these two participants, and they do not receive any payment for the play, i.e.**

also the talers that have been earned in the past are cleared from their accounts. (In the end of the experiment, both participants receive only the show up fee, see below). The two disqualified participants fill in a questionnaire, until the experiment has ended. For the other participants, the play is continued normally.

If the randomly drawn number is **3, 4**, ..., **998, or 999**, then the private payment is not discovered, and the play is continued with stage 4.

- If the public official rejects the transfer, then the credits remain unchanged (The offer costs from stage 1, however, are also paid in case of rejection). The play is continued with stage 4.

Stage 4: Decision on granting the permission

The public official chooses whether or not to **grant the permission to the firm.**

- If the public official does not grant the permission, then his credit and the credit of the firm matched with him are increased by **36 talers** each. The credits of the 16 other participants are **not changed** by this decision.
- If the public official grants the permission, then the firm's credit is increased by **56 talers,** whereas the public official's credit is increased by **30 talers**. The credit of each of the 16 other participants is **decreased** by **3 talers** by this decision.

Attention: by each of the eight other pairs, in which a permission is granted, the payoff for the firm as well as for the public official is decreased by 3 talers, i.e. at maximum eight times 3 and at minimum no talers are deducted from the firm's and the public official's credits each. The deductions by decisions of other pairs are not announced before the experiment has ended.

After stage 4, the round has ended. The round payoffs are the sum of all credit changes during the four stages.

The payoffs

You receive your payoff at the end of the experiment, where the exchange rate is DM 3.00 for 100 talers. In addition, you receive a lump sum show up fee of DM 5.00.

Neutral treatment

All in all **18 persons** participate in the decision making experiment. There are two types of participants: **Player 1** and **Player 2**. At the beginning of the experiment, the type of each participant is randomly drawn. **The type of a participant remains unchanged throughout the experiment.**

In the first round, pairs of participants are matched randomly. One player 1 and one player 2 are matched to one another. Thus, both participants do not know with whom they play. The pairs remain unchanged throughout the experiment.

The experiment consists of **30 rounds**. At the end of the experiment you will receive a **payoff** that depends on your success.

Decision situation in a round

Stage 1: Transfer or no transfer

First, player 1 decides whether or not he wants to transfer an amount to player 2. If he does, then the credit of player 1 is reduced by **2 talers**, and the play is continued with stage 2. If player 1 does not want to transfer an amount, then both credits remain unchanged, and the play is continued with stage 4.

Stage 2: The amount to be transferred

Player 1 decides on the amount to be transferred to player 2. Player 1 can choose between **1, 2, 3, 4, 5, 6, 7, 8 or 9 talers**. The play is continued with stage 3.

Stage 3: Acceptance or rejection of the transfer

Player 2 decides on whether he **accepts** or **rejects** the proposed transfer.

- If player 2 accepts the transfer, then the credit of player 1 is reduced by the amount he proposed. Player 2's credit is increased by the **tripled amount** that is transferred. In the following, a number out of the range from 0 to 999 is randomly drawn.

  If the randomly drawn number is **0, 1, or 2**, then player 2 and the player 1 matched with him are **disqualified**. That means: **The play ends for these two players, and they do not receive any payment for the play, i.e. also the talers that have been earned in the past are cleared from their accounts.** (In the end of the experiment, both players receive only the show up fee, see below). The two disqualified participants fill in a questionnaire, until the experiment has ended. For the other participants, the play is continued normally.

  If the randomly drawn number is **3, 4**, . . . , **998, or 999**, then the play is continued with stage 4.
- If player 2 rejects the transfer, then the credits remain unchanged (The transfer fee from stage 1, however, is also paid in case of rejection). The play is continued with stage 4.

Stage 4: Choice between X and Y

Player 2 chooses one of the **alternatives** *X* **or** *Y*.

- If player 2 selects alternative *X*, then his credit and the credit of the player 1 matched with him are increased by **36 talers** each. The credits of the 16 other participants is **not changed** by this decision.
- If player 2 selects alternative *Y*, then player 1's credit is increased by **56 talers,** whereas player 2's credit is increased by **30 talers**. The credit of each of the 16 other particpants is **decreased** by **3 talers** by this decision.

Attention: by each of the eight other pairs, in which *Y* is chosen, the payoff for player 1 as well as for player 2 is decreased by 3 talers, i.e. at maximum eight times 3 and at minimum no talers are deducted from player 1's and player 2's credits each. The deductions by decisions of other pairs are not announced before the experiment has ended.

After stage 4, the round has ended. The round payoffs are the sum of all credit changes during the four stages.

The payoffs

You receive your payoff at the end of the experiment, where the exchange rate is DM 3.00 for 100 talers. In addition, you receive a lump sum show up fee of DM 5.00.

The Payoff tables

– differences in wording are marked with round (neutral treatment) and squared (loaded treatment) brackets –

Round payoff if (player 2) [the public official] **accepts** (a transfer) [a private payment]

(transferred amount) [private payment]	1		2		3		4		5		6		7		8		9	
(Player 2's decision) [permission granted?]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]
Payoff (...player 1) [...firm]	33	53	32	52	31	51	30	50	29	49	28	48	27	47	26	46	25	45
(...Player 2) [...public official]	39	33	42	36	45	39	48	42	51	45	54	48	57	51	60	54	63	57
...each of the other 16 participants	0	−3	0	−3	0	−3	0	−3	0	−3	0	−3	0	−3	0	−3	0	−3

(transferred amount) [private payment]	1,...,9		0		
(Player 2's decision) [permission granted?]	(X) [no]	(Y) [yes]	(X) [no]	(Y) [yes]	
	Round payoff if (player 2) [the public official]**rejects** (a transfer) [a private payment])		Round payoff if (player 1) [the firm] does **not** (transfer an amount) [offer a private payment]		
Payoff (... player 1) [... firm]	34	54	36	56	Each of the 8 other pairs in which (Y is chosen) [a permission is given] decreases the payoff for (player 1 and player 2) [the firm and the public official] by another 3 talers each.
(... Player 2) [...public official]	36	30	36	30	
... each of the other 16 participants	0	−3	0	−3	

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