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Staff rotation as an anti-corruption policy: an experimental study

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Abstract

Within the German federal government regular staff rotation is a precautionary measure against corruption in public administration. To study the effect of this policy, an experiment was conducted where pairs of potential bribers and public officials were randomly re-matched in every round. The outcome is compared to the case where the identity of the pairs interacting remained fixed. The conclusion is that rotation of interacting pairs significantly reduces the levels of bribes as well as the frequency of inefficient decisions due to bribery.

JEL classification: C91; D62; D72; D73; K42
Keywords: Corruption; Staff rotation; Repeated games; Strangers and partners

1. Introduction

Policies that seek to reduce or preempt corruption can be directed at influencing behaviour through penalties, public relations campaigns, or organisational structures and procedures.[1] A policy in the latter category is the rotation of staff. Such a policy was adopted by the German federal government for sensitive areas such as public procurement.[2] Since corruption is based on trust and reciprocity between public officials and the

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[1] For example, through time limitations before public officials can accept employment in private sector companies with whom they have been involved in their official capacities. See Brezis and Weiss (1997).

[2] The text of the directive (Richtlinie der Bundesregierung zur Korruptionsprävention in der Bundesverwaltung, 17 June 1998) can be found (also in an English translation) on the web sites of Transparency International (1999), an international non-governmental organisation dedicated to curbing corruption.

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users of their services, we can expect that long-term relationships between potential bribers and public officials are an especially advantageous environment for bribery. In more detail, a preventive effect of rotation mechanisms on corruption can be conjectured for the following reasons.

- Public officials may be less tempted to be influenced by gifts, since they cannot expect to be rewarded for cooperation by receiving bribes again in later encounters.
- Bribers may be less trustful in the reciprocation of public officials, since they expect that cooperative behaviour by the public official is less likely.
- Users of public services (i.e., potential bribers) have had no experience with the public officials with whom they are dealing, and so will find the behaviour of the public officials more difficult to predict, which increases uncertainty attached to corrupt offers.

Although these arguments seem plausible at first glance, their validity cannot be taken for granted. Neither can the necessity of long-term cooperation as a basis for bribery be taken as fact, nor can we definitely say that the removal of long-term interaction is sufficient to reduce the level of bribery in an administration. To date, there is no clear empirical evidence confirming the bribery-reducing impact of staff rotation. Several ad hoc arguments can indeed be raised that cast doubt on the effectiveness of rotation instruments. Corruption also occurs in "one-shot" environments.[3] Experiments show that trust and reciprocation are observed even in non-repeated games under completely anonymous conditions (see next section). Evidence from experiments on fixed versus changing partner constellations in related situations is mixed (see next section). Thus, we might conjecture that the effect of rotation mechanisms in corruption scenarios is possibly weak, if not absent.

Staff rotation is also costly. Long-term relationships between public officials and their clients have the cost advantage that officials do not frequently need to become acquainted with new routines, new cases, and new clients. Rotation, on the other hand, requires adaptation costs in every new round. Outcomes may be inefficient if a new official is in charge of a case that he or she is not familiar with. Because of the costs, it seems important to consider the extent to which or if at all staff rotation reduces bribery.

This paper studies the staff rotation instrument in a laboratory experiment. The experiment is based on the two-player bribery game by Abbink et al. (2002) (hereafter AIR). The reciprocity relationship between briber and bribee is modelled as a trust game in which reciprocal behaviour is undesirable with regard to social welfare and subject to punishment when discovered. In the experiment, potential bribers and public officials are randomly matched with one another in every round, as under ideal staff rotation. I compare the results to AIR's data from the same game played by fixed pairs of a potential briber and a public official. The level of corruption is significantly reduced by staff rotation. The amount of bribes that are paid decreases by almost one half and the frequency of inefficient decisions due to bribery falls by two thirds.

[3] As an example, decisions about the venues of Olympic Games have been manipulated through bribery, though there is no interaction between the same cities and IOC members beyond the singular venue decision.

2. Related studies

There is a substantial literature on corruption, often focused on low-income countries.[4] No study, however, appears to have investigated the effectiveness of staff rotation as a policy for deterring corruption.[5] Since corruption involves a reciprocity relationship between briber and bribee, experimental research on reciprocal cooperation is a means of studying the policy.

A literature on experimental trust games has evolved, starting with Fehr et al. (1993), who analyse reciprocal behaviour in a labour market context. In trust (or reciprocity) games, a first mover can send money to a second mover (like a briber gives a gift), who in turn can voluntarily reward the person who has shown trust by sending money back (like an official makes a favourable decision). The games are constructed such that, by doing so, both players can be better off with respect to final payoffs, but in equilibrium no trust and no rewarding would be exhibited.[6] However, subjects frequently cooperate even in anonymous one-shot experimentation without pre-play communication (Berg et al., 1995; Jacobsen and Sadrieh, 1996; Dufwenberg and Gneezy, 2000; Fahr and Irlenbusch, 2000; Fershtman and Gneezy, 2001). These results show that repeated interaction is at least not a necessary condition for reciprocity. Gächter and Falk (2002), however, find that repeated play can increase reciprocal cooperation. They conduct experiments based on the gift exchange game by Fehr et al. (1993) and compare repeated interaction to a never-meet-again matching. The latter setting, in which the authors observe a lower impact of reciprocity, almost implements one-shot interaction.

Comparisons of the level of cooperation with fixed and random partners have mainly been made in experiments on public goods games. In such games, each subject of a group of n persons can decide to invest an amount x (free to choose up to a certain limit) in a public good. Everybody in the group of n individuals receives a return of cx, where $c<1$, but $nc>1$. Thus, it is a dominant strategy for rational selfish players not to invest, but the Pareto efficient solution is realised if everybody cooperates by investing the maximum amount. Andreoni (1988) was the first to compare a public goods experiment under a so-called "strangers" (random matching) to one under a "partners" (fixed groups) condition. He found that strangers contribute even more to the public goods than partners.[7] Weimann (1994), Burlando and Hey (1997), and Palfrey and Prisbey (1996), however, do not find significant differences between strangers and partners. Keser and van Winden (2000) even find higher contributions among partners on the basis of sufficiently many independent observa-

[4] See, for example, Rose-Ackerman (1978), Shleifer and Vishny (1993), Manion (1996), Hauk and Saez Marti (2002), Lambert-Mogiliansky (2002).

[5] Bac (1996) does however mention rotation as a possible means to improve monitoring in hierarchical organisations, but he does not explore this issue in depth.

[6] Throughout this paper, the term "equilibrium" is used as a short form for "equilibrium assuming rational selfish players exclusively maximising their own payoff".

[7] It should be noted that Andreoni's data comprise only one independent observation for the strangers treatment, thus the effect might be due to (unsystematic) sampling variation.

tions to allow for valid statistical inference. Croson (1996) and Fehr and Gächter (2000) obtain a similar result.

All in all, evidence is mixed, where in a majority of studies an effect towards more cooperation under repeated play conditions is observed. Of course, these studies address reciprocal cooperation in general rather than corruption in particular. A special characteristic of the reciprocity relationship between briber and corrupt official is that here reciprocal cooperation is not Pareto efficient: While briber and official can gain, negative externalities are imposed to others (the "public").[8] For that reason, bribery is subject to severe penalties in case of discovery. These special characteristics have been integrated into an experimental trust game by AIR. They first constructed a basic trust game as a control treatment. In a second treatment, negative externalities to the public were introduced through damage to all other participants in a session, making the corrupt (reciprocal) action overall inefficient. The authors, however, found that the negative externality had virtually no impact on behaviour. In a third, so-called *sudden death* treatment, giving and accepting bribes bears a risk of being discovered with a 0.3% probability, which leads to the exclusion from the experiment without payment. The introduction of the external penalty significantly reduces the frequency of corruption, despite the very low probability. The last treatment by AIR will also serve as the control treatment for the present experiment.

AIR conducted their experiment in supergames of 30 repetitions with fixed pairs of bribers and public officials. The supergame design models a long-term relationship between a firm and an official. To study the effect of staff rotation, we conduct a new experiment in which the pairs of firms and public officials are randomly re-matched in every round. Thus, we create an environment in which firms and officials cannot expect to play the same person as in the previous round again.

In the present study, we focus on a relationship between a single public official and a single potential briber. One might think of a firm applying for a licence to perform some environmentally harmful activity, or of public procurement in a case in which the supplier is a monopolist and gives bribes in order to sell more expensive products than needed. We chose the one-firm–one-official case because it appears to be the simplest one, such that it seems the most suitable starting point.[9]

3. The experimental design

The stage game of our new treatment is identical to the one introduced by AIR. The game was played 30 times in an experimental session, where the number of rounds to be played was known to the subjects in both treatments.

[8] Economists sometimes propose that bribery can increase efficiency by facilitating beneficial decisions by government officials. For an experimental study, see González et al. (2003). In this study, however, we look at situations in which bribery is socially harmful.

[9] Of course, corruption exists in many different contexts. In public procurement, for example, the government often deals with oligopolistic suppliers who pay bribes to be chosen despite of other possibly cheaper supply offers. In a theoretical paper, Lien (1988) models such a situation as an all-pay auction.

3.1. The game

A potential briber (typically a firm) first decides whether to transfer an amount to a public official. If a decision is made to do so, the briber 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 a positive amount is transferred, the public official decides whether to accept or reject the bribe. If the public official rejects, no money is transferred, but the firm must pay a relatively small transfer fee of 2 talers (which represents initiation costs needed to, for example, set up a contact). 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 large firm than to a public official with a small income.[10]

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 doable 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 choose one of two alternatives. One alternative (X) represents the "honest" option. It is, apart from eventual bribes, slightly preferable (as manipulating a decision requires effort to justify the choice before superiors). The second alternative (Y), the "manipulating" option, however, is much more favourable to the briber. In numbers, both players receive a payoff of 36 talers when X is chosen, where the payoffs on a Y choice are 56 talers for the firm, and 30 for the public official (not including transfers). In addition, alternative Y damages the public: each of the other participants in the session suffers a deduction of 3 talers. The consequences of a Y choice to a single individual are relatively small, but they add up to substantial amounts since they are spread over many people. This is typical of many decisions made in the public service, in which costs of inefficiency are born by the general population of all taxpayers. In total, Y is 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.

As corruption is carried out 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.

Fig. 1 sketches 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

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

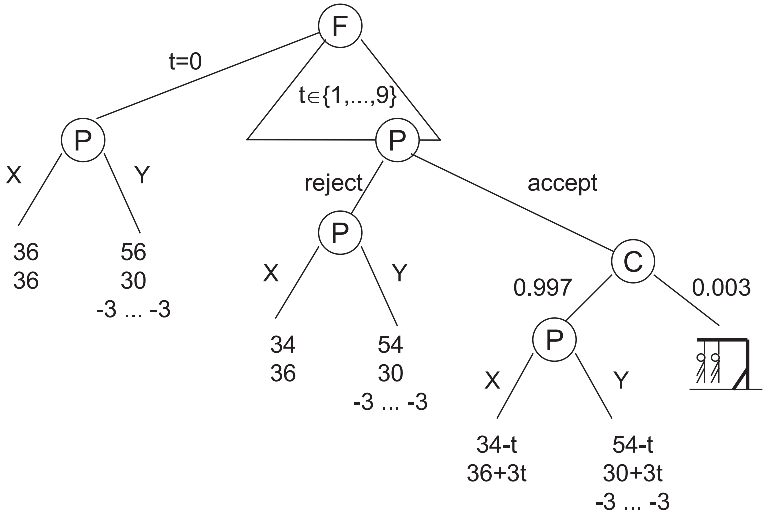

Fig. 1.

of sudden death. The lines "–3...–3" mean that all 16 other subjects are damaged by 3 talers.[11]

To model the staff rotation procedure, we re-matched firms and public officials randomly every round. A subject could therefore not know which other subject (s)he currently played. Each session was divided into three sub-populations of three firms and three public officials. Every round, each firm was matched to one of the public officials of its sub-population. In this way we gathered three independent observations in each session. A possible drawback is that the number of different partners a subject is matched to is relatively small, such that the strength of the random matching procedure to induce partner changes might be weakened.[12] We partly mitigated this problem by not informing the subjects about the sub-populations, which might have insinuated the population to be larger than it actually was.

No subject who participated in the new experiment had taken part in the treatment with fixed pairs. Hence, we chose a between-subject design. By this, we study the behaviour of the two different institutions separately, and do not consider the effect of *introducing* staff rotation in a given environment. In reality, it will not be possible to fire all public officials and hire completely new staff. However, we were worried that a within-subject design might have over-emphasised the short-term effect after announcing a change in the regime.

[11] Note that Fig. 1 is an illustration, but not a complete game tree in the formal sense. As such, it would have to incorporate the payoff consequences due to other pairs' behaviour, as well as account for the fact that the X and Y choices are not the same at different decision nodes. For readability, we dropped most of the formalism.

[12] Note that the results would only be strengthened if this effect were pronounced.

3.2. Game theoretic analysis

The intuition of the game theoretic prediction—assuming players solely maximising their own payoff—of the game can be easily obtained by applying backward induction logic to the stage game depicted in Fig. 1. The public official has no reason to choose *Y*, and foreseeing this, the firm does not transfer any positive amount. A thorough game theoretic analysis confirms this intuition, but is formally a little tedious: Since the decisions of others are not made known, the supergame does not have proper subgames, hence subgame perfection does not select among all possible Nash equilibria. However, even without applying any refinement concept, a similar (even stronger) result can be obtained by looking at equilibrium *paths*: In the stage game of the last round, it is still true that the public official will never choose *Y* with positive probability, since this would reduce his or her payoff without any possible gain. Accordingly, if the firm would ever transfer a positive amount with positive probability, this could not be an equilibrium strategy, given it will not be rewarded on an equilibrium path. This is independent from the behaviour of the other pairs of players in the game. We formulate the following proposition.

Proposition. *In an equilibrium of the finitely repeated bribery game, the firm never transfers a positive amount, and the public official never chooses Y in information sets that are reached with positive probability.*

Proof. See Appendix A. □

3.3. Experimental procedures

The experiment was conducted at the Laboratorium für experimentelle Wirtschaftsforschung at the University of Bonn. The subjects were recruited with posters on the campus advertising the experiment. Most were students from various disciplines. Law and economics students constituted the largest numbers. The subjects of the previous bribery experiments were not allowed to participate again.

To ensure comparability, the same experimental software (developed using RatImage; Abbink and Sadrieh, 1995) as in the previous experiment was used. 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. A screenshot of the main screen is reproduced in Appendix B.

Each session began with an introductory talk. The written instructions differed from AIR's only in the paragraph about the matching scheme. A translation of the handouts is reproduced in Appendix B. Payoff tables, also reproduced in Appendix B, were handed out to increase the transparency of the game. The instructions were written in a completely neutral language, where no connection was made to a bribery scenario. We decided for a neutral presentation to prevent that the results might be affected by suggestive phrasing. This seems especially important since the implicated parties in real corruption cases also tend to avoid to speak of "corruption" or "bribery".[13] 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 that subject as being firm or public official, were assigned to the subjects by random draw. After the subjects were seated, the play started immediately. The 30 rounds of the experiment were played in slightly less than an hour, such that a whole session took about 1 1/2 h including instructions. After the play, subjects were also requested to estimate overall probabilities of disqualification for nine given parameter constellations.[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 provided incentives for disqualified players to remain seated.

Immediately after the session, the subjects were paid anonymously in cash, at an exchange rate of DM 0.03 per taler.[15] The total earnings in the new experiment ranged from DM 5.00 (two pairs of subjects were unlucky in the sudden death lotteries) to DM 45.29 with an average of DM 35.61 for 1 1/2 h, which is considerably more than a student's regular per hour wage in Bonn. One DM is equivalent to 0.51 Euro.

Three sessions with 18 subjects were conducted with the new treatment. Since each session comprises three statistically independent observations, we obtain nine independent observations in total. In the control treatment with fixed pairs, two sessions had been conducted. Since in this treatment each pair is one independent observation, we have 18 independent observations in the condition with fixed pairs.

4. Results

In the following, we will denote the staff rotation treatment as the "strangers" condition, where the treatment with fixed pairs will be referred to as the "partners" condition. The raw data are available upon request.

[13] See Neckel (1995). Bribes are often paid in form of non-monetary gifts in order to not let them be perceived as such (this issue is discussed in Offer, 1997; for a practical "guide", see Davis, 1997). The role of framing effects in experiments is discussed in an early study by Pruitt (1970), who finds that the means of presentation of the prisoners' dilemma game has a significant impact on the likelihood of cooperation. For a study on the effects of wording in experimental instructions see Burnham et al. (2000). In a companion paper, Abbink and Hennig-Schmidt (2002) conduct an experiment on AIR's bribery game using loaded framing, and fail to find significant differences.

[14] The data from the questionnaires replicate AIR's findings of a tendency to under-estimate the probability of disqualification.

[15] The exchange rate may look rather low. However, a wide range of payoffs was possible in a session (from DM 5.00 to DM 61.70 as the theoretical maximum).

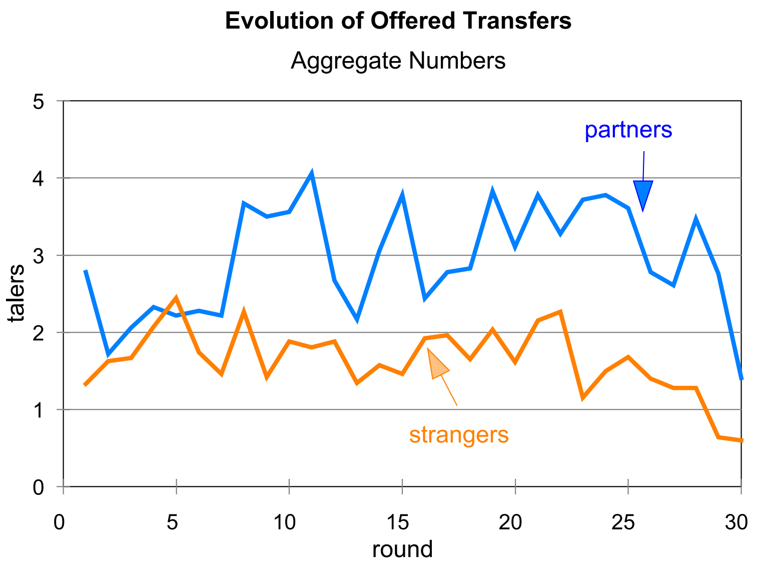

Fig. 2.

4.1. The level of corruption

We measure the level of corruption mainly with two variables. The *average offered transfer* measures the firms' propensity to pay bribes, the *frequency of Y choices* measures the extent to which decisions have been manipulated by bribery. Fig. 2 shows the average amount that is offered per round, over the 30 rounds of the experiment, in the aggregate of all sessions. Obviously, the rotation instrument has a strong negative impact on the propensity of firms to pay bribes. On average over the whole experiment, 1.65 talers are transferred per round. Compared to 2.93 talers in the partners' treatment, this is a decrease of 43.7%. Fig. 3 shows the distribution of offers. In two thirds of the rounds, no bribes are offered. We can see that the second peak at the transfer of 6 talers[16] almost disappears in the staff rotation treatment.

When testing the difference for statistical significance, the problem arises that in the partners treatment every single briber is an independent observation, where under staff rotation, three bribers interact with the same officials and cannot be treated as statistically independent from one another. Therefore, tests that require independence can only be applied to the group averages of the nine independent subject groups of the strangers' treatment. We apply Fisher's two-sample randomisation test to these group averages, compared to the transfers made by the single independent bribers in the partners' treatment. The test rejects the null hypothesis

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

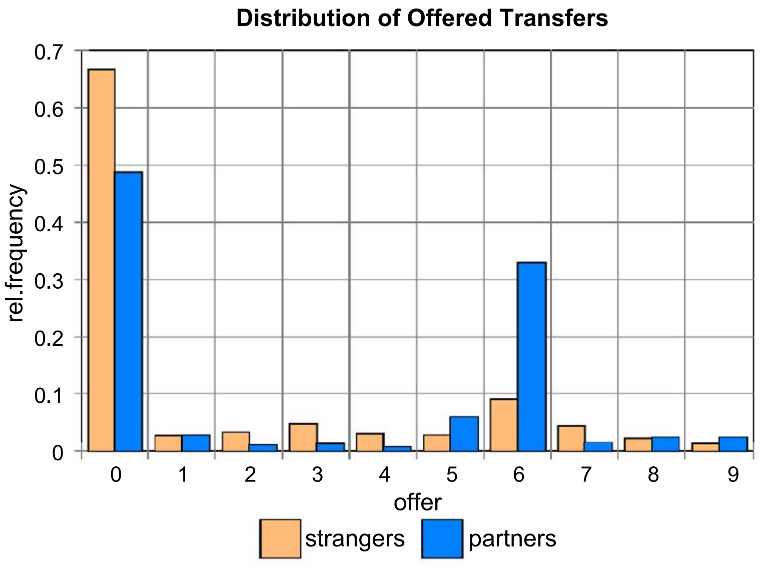

Fig. 3.

of equally high transfers in both treatments at a significance level of $\alpha = 0.05$ (one-sided).[17]

Observation 1. *In the strangers' treatment, significantly lower transfers are made than in the control treatment with fixed pairs.*

Although the level of bribes being paid is an indication of the level of corruption, the variable actually worrying policy makers is the frequency of inefficient choices induced by bribes. Fig. 4 depicts the evolution of other damaging *Y* choices over the 30 rounds of the experiment, using the aggregate data of all choices without differentiating with respect to transfer offers. The figure shows that the frequency of Y choices is much lower with staff rotation than with fixed pairs of players. In total, only in 14.3% of all rounds, alternative *Y* was chosen. Compared to the 43.3% in the partners' treatment, this means a decrease by 67%.

The two-sample randomisation test, applied to the average *Y* choice rates in the independent subject groups of the strangers treatment, compared to the *Y* choice rates of the single public officials in the partners treatment, rejects the null hypothesis of equal rates at a significance level of $\alpha = 0.01$ (one-sided). Consequently, efficiency (in terms of total payoff earned in the population) is higher under staff rotation. The average round payoff over all subjects rises from 29.59 talers with fixed pairs to 32.22 talers under staff rotation.

[17] The two-sample randomisation test is a non-parametric variant of the *t*-test, testing for differences in mean between two samples. For a discussion of the power of this test, see Moir (1998).

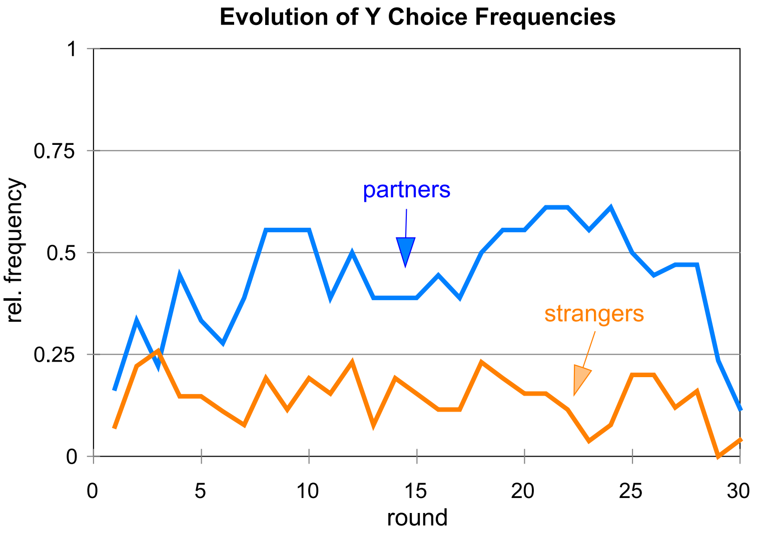

Fig. 4.

Observation 2. *The Y alternative is chosen significantly less frequently under staff rotation than with fixed pairs.*

Figs. 2 and 4 suggest that the treatment differences seem to become larger over the 30 rounds of the experiment. In fact, the Spearman rank correlation coefficients between the round number and the difference in average transfers in the two treatments (the difference in average Y choice frequency, respectively) are significantly different from zero at a significance level of at least $\alpha = 0.05$ (one-sided, $r_s = 0.35$ for transfers and $r_s = 0.49$ for Y choices).

4.2. Rejections of offered bribes

No significant differences can be observed with respect to the public officials' tendency to reject bribes. Fig. 5 shows the relative frequency of rejections that follow a certain transfer. The picture is rather similar for both treatments. The apparent peak at a bribe offer of 3 talers in the partners' treatment is probably due to random variation. Only in 7 rounds in total, an offer of 3 talers has been made.

Observation 3. *In both treatments, rejection rates are relatively low for larger transfers. Small bribes are frequently rejected. Treatment differences cannot be detected.*

4.3. Reciprocation by public officials

The sharp decrease of Y choice frequencies after introduction of staff rotation might of course be due to the lower level of transfers public officials are offered. The question arises

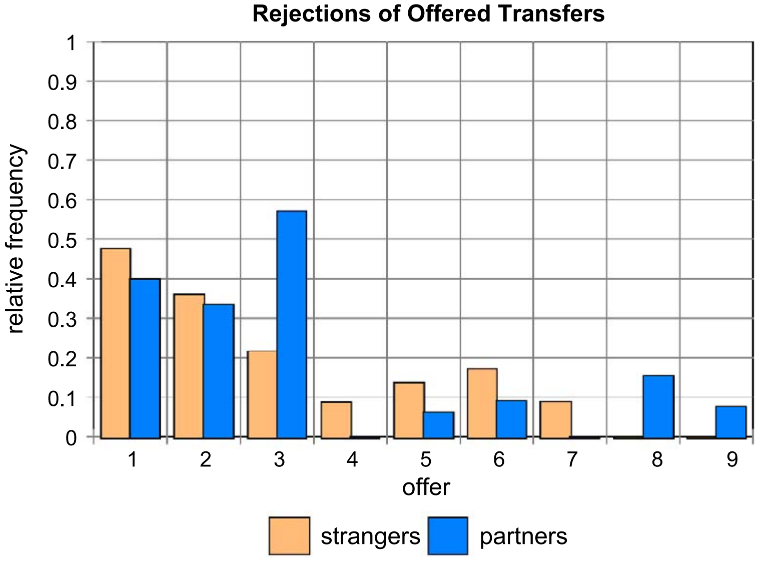

Fig. 5.

whether beyond that, officials tend to be less reciprocal in the sense that they tend to choose *Y* less often after they have received positive bribe offers. Fig. 6 shows that this seems to be the case. On almost all values of offered transfers, officials tend to choose *Y*

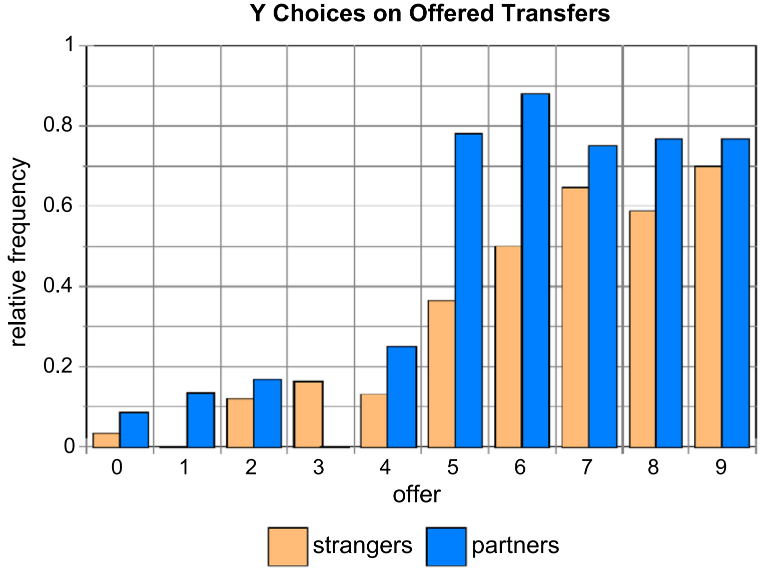

Fig. 6.

less frequently in the strangers treatment. To test whether there is in fact a systematic tendency of public officials being less reciprocal under staff rotation, we compute for every single official the frequency of Y choices after having received a positive offer. We compare these frequencies in the partners' treatment to the corresponding group averages in the strangers' treatment. The two-sample randomisation test detects that reciprocation by public officials is significantly less pronounced in the strangers' treatment (one-tail $p = 0.025$).[18]

In both treatments, we observe that—not surprisingly—public officials tend to choose Y more often the higher transfers they receive. If no transfer was offered, Y is chosen only very rarely (17 out 502 in the strangers, 22 out of 240 in the partners treatment). The frequency is higher in the partners' treatment (9.2% versus 3.4%), which is consistent with the conjecture that Y choices after a zero transfer might be meant as a solicitation to make transfers in later rounds. However, the data are too sparse to run meaningful statistical tests, such that these explorations can only be speculative.

Observation 4. *On almost all offered transfer values, Y choices are made relatively less frequently under staff rotation (strangers) than with fixed pairs (partners).*

4.4. Firms' reactions to experience

An important difference between the fixed pairs and the staff rotation set-up is the firms' inability to reward cooperative behaviour of officials by paying bribes in the next round. We measure this type of a firm's response to the official's behaviour by the difference between average transfers after experienced Y and X choices by the official. Formally, the measure of excess transfer after Y is defined as

$$R = \frac{\sum t_Y}{Y} - \frac{\sum t_X}{X},$$

where t_Y denotes the transfer after a preceding Y choice, t_X is the transfer after an X choice, $\#Y$ and $\#X$ denote the number of Y and X choices in rounds 1–29.

If we compute the same measure for the firms in both treatments, we in fact obtain much lower R measures, as a consequence of the absent possibility to reciprocate directly. For the 18 firms for whom this measure can be computed,[19] we obtain an average R-value of $+1.20$. In the partners' treatment, an average R-value of $+3.43$ was observed, which is

[18] This test is open to the objection that it does not take the amount that was offered into account. Fig. 3 shows that in the strangers treatment, slightly more low offers were made, on which officials might tend to choose X because they consider the offer as too low. Strictly speaking, only the response frequencies on the same offered amount can be compared directly. Most values, however, are too rarely offered to meaningfully apply statistical tests. If we compare the frequency of Y choices after low (1 to 5) and high offers (at least 6) separately, we observe fewer Y responses in the strangers treatment for both categories (insignificantly, one-tail $p = 0.132$, for low, weakly significantly, one-tail $p = 0.076$, for high offers), such that the lower level of offers in the strangers treatment is unlikely to explain the observed phenomenon.

[19] Nine firms never experienced a Y choice by a public official. Thus, it is not possible to compute an R measure for these subjects.

almost three times as high. The two-sample randomisation test, applied to the group averages in the strangers treatment compared to the individual measures in the partners treatment, rejects the null hypothesis of equally high *R* measures under both conditions at a significance level of $\alpha = 0.005$ (one-sided).

Nevertheless, for 16 out of 18 firms in the stranger condition, the *R*-value is positive. Applying the Wilcoxon matched pairs signed rank test to the average *R* measure in the independent subject groups, we can reject the null hypothesis that positive and negative *R* values are equally likely at a significance level of $\alpha = 0.01$ (one-sided). Thus, although a firm cannot reciprocate directly to the official's choice in the previous round, we observe that his reactions are typically pointed into the same direction. This result gives us insight into the adaptation process by the firms. After having experienced an *X* choice by the public official, they also tend to shy away from paying bribes in the next round. This pattern of reaction, however, is much less pronounced than with fixed pairs, where the difference can be attributed to direct reciprocation by firms.

Observation 5. *Under staff rotation, firms' transfer behaviour is less conditioned on previous round experience than with fixed pairs. Nevertheless, firms react on the public official's decision they experienced by transferring less after X than after Y.*

5. Summary and conclusions

In the combat against bribery, rotation of staff in the public administration is suggested as a precautionary measure. To gain insight into the effectiveness of rotation instruments, we have conducted an experiment on the bribery game by AIR with a random matching of firms and public officials rather than fixed pairing. The "strangers" design models the situation with staff rotation, where the "partners" design of AIR captures the original constellation with fixed affiliations. We find rotation to be effective in our experimental environment. On average, bribes are reduced by almost one half, and, perhaps more importantly, the average frequency of inefficient decisions caused by bribery decreases even stronger. The effect observed in the experiment is due to a lower tendency of firms of administrative services to pay bribes as well as to a lower propensity of public officials to be influenced by them in favour of the briber. As bribers cannot reciprocate on favourable decisions by paying bribes in later cases, we observe a significantly lower tendency to pay higher bribes after the firms experienced an advantageous decision.

Though the present results give a clear indication in favour of the effectiveness of staff rotation, the results are naturally not ultimately conclusive. The model used here captures important features of many corruption scenarios, but nevertheless, other features have been neglected for simplicity, again others might vary in different contexts. Future research is needed to analyse the effects of staff rotation in more and richer environments. For example, the present study does not model that potential bribers might be discouraged from bribe-giving, because they fear that their attempt might be reported by the official. It is possible that the effect of this fear might be greater under the staff rotation regime, since the official's behaviour is less predictable. Another direction for future studies would be the analysis of different information conditions concerning the level of corruption in the society. Staff rotation might be more or less effective when officials know that they are living in a corrupt society. Thus, the present experiment should be seen as a starting point in examining the effect of the policy measure under consideration.

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Appendix A. Proof of the proposition

To keep the proof general for various payoff parameter constellations, define the following variables (the values in brackets are the parameters used in the experiment). Following the sequence of moves in the game, we call the firm player 1, and the public official player 2.

π^{1X}	player 1's payoff if X is chosen	[36]
π^{1Y}	player 1's payoff if Y is chosen	[56]
π^{2X}	player 2's payoff if X is chosen	[36]
π^{2Y}	player 2's payoff if Y is chosen	[30]
f	transfer fee	[2]
$t^{\max}$	maximum transfer	[9]
θ	sudden death probability per round	[0.003]

where the inequalities $\pi^{1X}<\pi^{1Y}$, $\pi^{2X}>\pi^{2Y}$, $0\leq\theta\leq1$, $f>0$, and $t^{\max}>0$ hold. Further, denote by $t(I^1)$ player 1's transfer at an information set I^1, by $d(I^2)$ player 2's choice of X or Y at information set I^2, and by $a(I^2)$ player 2's decision at an information set I^2 at which she has to decide on accepting or rejecting. For convenience, we use a subscript to denote the round at which this information set is reached, such that I_j^1 is an information set of player 1 for round j, and by I_j^2 an information set at which player 2 decides on X or Y in round j. An information set contains the history of play of the players matched to one another during the play. In the strangers' condition, the information set does not contain the identities of previous partners.

Since the payoffs the players gain in the game are additively composed of payoffs resulting from their own moves and the damages done to them by others, we may define

$\Pi^i(I_j^k)$ as player I's cumulative payoff earned through the moves of the players of the own pair, up to the point of time when I_j^k is reached, and by $\delta(s^{-1-2})$ the (expected) damages done to each player of a pair by the other players' (mixed) strategies s^{-1-2}. For convenience, we drop the argument if it is not required.

Denote by $p(I_j^i)$ the probability with which information set I_j^i is reached. Denote by q the probability with which a player makes a certain decision at a certain information set. Let us first show that in equilibrium Y is never chosen in the last round. Consider some information set I_n^j, where n is the last round. Suppose there is an equilibrium $E=(s^1,\ldots,s^n)$ with $q(d(I_n^2)=Y)>0$ and $p(I_n^2)>0$. Now consider player 2's alternative strategy, which differs from his strategy in E only in $d(I_n^2)=X$. $p(I_n^2)$ remains unchanged. Player 2's payoff with the alternative strategy is $\Pi^2(I_n^2)+\pi^{2X}+\delta$, which is higher than player 2's payoff in E, that is $\Pi^2(I_n^2)+\pi^{2Y}q(d(I_n^2)=Y)+\pi^{2X}(1-q(d(I_n^2)=Y))+\delta$. Thus, E cannot be an equilibrium.

Now let us show that in equilibrium, player 1 will never transfer a positive amount in the last round. Suppose there is an equilibrium E with $q(t(I_n^1)=0)<1$ and $p(I_n^1)>0$. Since E is an equilibrium, player 2 will not choose Y in the following stage of the last round. Thus, the best possible payoff player 1 can get in an equilibrium characterised by $q(t(I_n^1)=0)<1$ and $p(I_n^1)>0$ is $\Pi^1(I_n^1)+\pi^{1X}q(0)+(\pi^{1X}-f)(1-q(0))+\delta$, where $q(0)$ is short for $q(t(I_n^1)=0)$. This is the case if in E, player 2's strategy involves rejecting the transfer offers made by player 1. If player 2's strategy in E would involve accepting transfer offers made by player 1, player 1's payoff can only be lower, since it is further reduced by transfers and the possibility of sudden death.

Whatever equilibrium strategy player 2 would choose, it can easily be seen that player 1's payoff by choosing the alternative strategy, which differs from his strategy in E only in that $q(t(I_n^1)=0)=1$, is always higher than his payoff in E. All other things are equal, $p(I_n^1)$ remains unchanged. Player 1's payoff in case that the information set is reached, however, is in the range from $\Pi^1(I_n^1)+\pi^{1X}+\delta$ (player 2 chooses X with probability 1 on all paths reached through player 1's alternative strategy and player 2's strategy in E) to $\Pi^1(I_n^1)+\pi^{1Y}+\delta$ (player 2 chooses Y for sure). Since all possible payoffs are higher than the maximum payoff in an equilibrium E, given in E $q(t(I_n^1)=0)<1$ holds, E cannot be an equilibrium.

Note that the results for the last round hold independent of partner rotation.

The proposition can now be proved by mathematical induction. Denote by $S=\{j,\ldots,n\}$ a set of consecutive rounds for which in equilibrium $d(I_j^2)=\ldots=d(I_n^2)=X\ \forall I_i^2$ with $p(I_i^2)>0$, $i=j,\ldots,n$, and $t(I_j^1)=\ldots=t(I_n^1)=0\ \forall I_i^1$ with $p(I_i^1)>0$, $i=j,\ \ldots,\ n$. We have shown that $n\in S$. Now consider round $j-1$. Suppose there is an equilibrium E in which for some information set $p(I_{j-1}^2)>0$ and $q(d(I_{j-1}^1)=Y)>0$. Since E is an equilibrium, player 2's payoff in case that I_{j-1}^2 is reached is $\Pi^2(I_{j-1}^2)+\pi^{2Y}q(d(I_{j-1}^2)=Y)+\pi^{2X}(1-q(d(I_{j-1}^2)=Y))+\pi^{2X}|S|+\delta$. Note that since E is an equilibrium, there will be no positive transfers in later rounds. Further, since E is an equilibrium, player 2's strategy must be a best reply to all other players' strategies. It is now sufficient to show that player 2 has at least one alternative strategy, which is a better reply to all other players' strategies in E. Consider, for example, player 2's alternative strategy that differs from his or her strategy in E in that $d(I_i^2)=X$ and $a(I_i^2)=$ reject $\forall i\in S$. All other things equal, $p(I_j^2)$ remains unchanged, but player 2's payoff given I_j^2 is reached is $\Pi^2(I^2_{j-1})+\pi^{2X}(|S|+1)+\delta$, which is higher than in E. Thus, E cannot be an equilibrium.

Note that this holds independent of partner rotation, since it is true that *no* player 1 will choose a positive transfer in the last round.

Now suppose there is an equilibrium E, in which $q(t(I^1_j)=0)<1$ and $p(I^1_j)>0$ for some information set I^1_j. Since E is an equilibrium by assumption, $d(I^2_k)=X\ \forall k\geq j-1$ and $p(I^2_k)>0$, and $t(I^1_k)=0\ \forall k\geq j$ and $p(I^1_k)>0$. Thus, player 1's expected payoff, given I^1_j is reached, and given $q(t(I_{j-1}^{1})=0)$, is in the range from $\Pi^1(I^1_j)+\pi^{1X}(|S|+1)q(0)+(1-\theta)(1-q(0))(\pi^{1X}-f-t^{\max}+\pi^{1X}(|S|+1))+\delta$, if player 1 transfers the maximum with probability 1, and player 2 accepts for sure, to $\Pi^1(I^1_j)+\pi^{1X}(|S|+1)q(0)+(1-q(0))(\pi^{1X}-f+\pi^{1X}|S|)+\delta$, if player 2 rejects all positive amounts transferred. However, since E is an equilibrium, player 1's strategy must be a best reply to all other players' strategies. It is now sufficient to show that player 1 has at least one alternative strategy which is a better reply to all other players' strategies in E. Consider, for example, player 1's alternative strategy that differs from the equilibrium strategy in that $q(t(I^1_k)=0)=1$ $\forall I^1_k$ with $k\geq j$. Player 1's payoff, given I^1_j is reached, is then in the range from $\Pi^1(I^1_j)+\pi^{1X}(|S|+1)+\delta$, if player 2 chooses X in all later rounds with probability 1, to $\Pi^1(I^1_j)+\pi^{1Y}(|S|+1)+\delta$, if player 2 always chooses Y for certain. No matter what player 2's strategy is off the equilibrium path of E, the alternative strategy is a better response to all other players' strategies in E than player 1's strategy in E. Thus, E cannot be an equilibrium. This implies that $j-1\in S$. □

Note that this holds independent of partner rotation, since it is true that *no* player 2 will choose Y in any round $j\in S$.

Appendix B. Instructions, payoff tables, and the main screen

B.1. The instructions for the experiment (original text in German)

All in all, 18 persons participated 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 every round, pairs of players are matched randomly. One player 1 and one player 2 are matched to one another. Thus, both players do not know with whom they play in a particular round. The experiment consists of 30 rounds. At the end of the experiment you will receive a payoff that depends on your success.

B.2. Decision situation in a round

B.2.1. 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.

B.2.2. 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 and 9 talers. The play is continued with stage 3.

B.2.3. 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.

B.2.4. 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 are 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 participants 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 changes of credits during the four stages of the round.

B.3. 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.

Round payoff if player 2 **accepts** a transfer

Transferred amount	1		2		3		4		5		6		7		8		9	
Player 2's decision	X	Y	X	Y	X	Y	X	Y	X	Y	X	Y	X	Y	X	Y	X	Y
Payoff ... Player 1	33	53	32	52	31	51	30	50	29	49	28	48	27	47	26	46	25	45
... Player 2	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

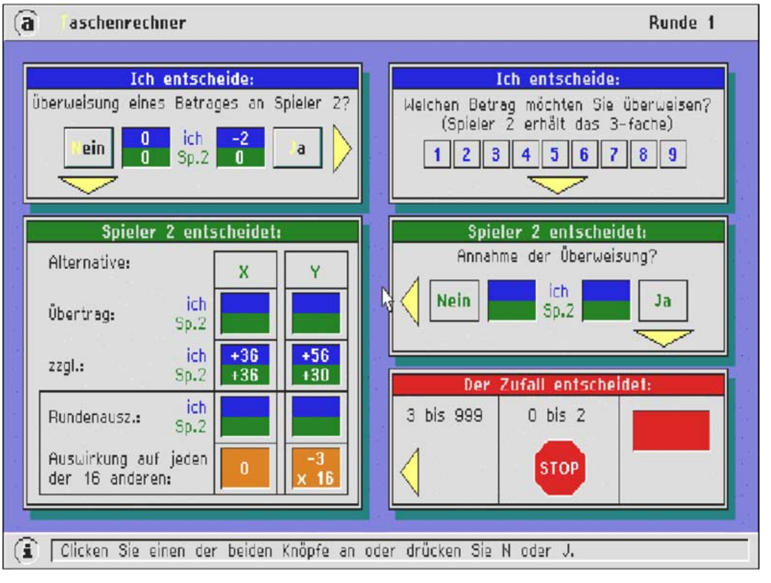
Round payoff if player 2 **rejects** a transfer

Transferred amount	1,...,9	
Player 2's decision	X	Y
Payoff... ... Player 1	34	54
... Player 2	36	30
... each of the 16 other participants.	0	–3

Round payoff if player 2 does **not** transfer an amount

Transferred amount	0	
Player 2's decision	X	Y
Payoff... ... Player 1	36	56
... Player 2	36	30
... each of the 16 other participants.	0	–3

Each of the 16 other pairs in which Y is chosen decreases the payoff for player 1 and player 2 by another 3 talers each.



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