Staff Rotation:
A Powerful Weapon Against Corruption?

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Abstract

The German federal government intends to introduce regular staff rotation as a precautionary measure against corruption in public administrations. To test the effectiveness of this instrument, we conduct an experiment using the bribery game by ABBINK, IRLENBUSCH, and RENNER (1999), in which pairs of potential bribers and public officials are randomly re-matched in every round. The data are compared to the analogous treatment with fixed pairs. The results provide strong evidence for the effectiveness of staff rotation in the experimental environment. The level of bribes as well as the frequency of inefficient decisions caused by bribery are reduced significantly.

Keywords

Corruption, staff rotation, repeated games, strangers and partners

JEL Classification Codes

C91, D62, D72, D73, K42

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1. Introduction

As corruption is pervasive, combating it is a tedious job. Initiatives are manifold, pointing either at influencing behaviour directly (such as the public relations campaigns or codes of conduct), or at reforming organisational structures and procedures to create a less fertile ground for corruption. An instrument from the latter category is the rotation of staff. Recently, the German federal government has released a directive to implement rotation of personnel in sensitive areas on a regular basis[1]. Since corruption is built on trust and reciprocity between public officials and the users of their services, we can expect that long-term relationships between potential bribers and public officials are an especially advantageous environment for bribery to emerge. 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 co-operation by receiving bribes again at later encounters.
- Bribers may be less trustful in the reciprocation of public officials, since they expect that co-operative behaviour by the public official is less likely.
- Since the users of public services (i.e. potential bribers) do not know the public officials he deals with from previous cases, they will find their behaviour 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 co-operation 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 proving the bribery-reducing impact of staff rotation. Several ad hoc arguments can be raised that cast doubt on the effectiveness of rotation instruments.

- Corruption grows also in "one-shot" environments. As an example, decisions about the venues of Olympic Games have been manipulated through bribery, although there is no interaction between the same cities and IOC members beyond the singular venue decision.
- 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. Thus, we might conjecture that the effect of rotation mechanisms in corruption scenarios is possibly weak, if not absent.

Staff rotation is costly. Public officials have to settle into a new job in every round of rotation. Resources required for training employees increase. Since officials have to cover a broader scope of capabilities, they must be better educated than if they have fixed responsibilities,

[1] 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.

which induces a tendency towards higher salaries and thus higher personnel costs. Because of the high costs, staff rotation should be introduced only if its effect of reducing bribery is substantial, which is an empirical question.

Empirical evidence from the field, however, is difficult to obtain because of mainly two circumstances. First, the level of corruption before and after the introduction of rotation mechanisms is, for obvious reasons, extremely difficult to measure. Second, initiatives to fight bribery are mostly taken after corruption scandals have been discovered. Even if there was a reliable measure of corruption, it would be unclear which part of an eventual effect had to be attributed to greater caution and increased sensitivity of individuals. Therefore, we suggest a laboratory experiment, which allows to study the two different regimes under consideration (staff rotation and stationary jobs) in a controlled and observable environment.

To test the effectiveness of the staff rotation regime, we conduct an experiment using the two-player bribery game by ABBINK, IRLENBUSCH, and RENNER (1999, hereafter AIR). In the new experiment, potential bribers and public officials are randomly assigned to one another in every round, as under ideal staff rotation. We compare the results to AIR's data from the same game played by fixed pairs of a potential briber and a public official. We find strong evidence that the level of corruption is dramatically reduced by the introduction of staff rotation. The amount of bribes that are paid is decreased by almost one half, the frequency of inefficient decisions due to bribery falls even by two thirds.

2. Links to Related Studies

The influence of staff rotation on corruption has not been studied in the literature before[2]. To our knowledge, there are neither theoretical nor empirical studies on that issue[3]. In the existing experimental studies on related topics, the nature of reciprocal co-operation that is considered is essentially different from a corruption scenario. Nevertheless, they allow us to hypothesise that frequent occurrence of corruption might be expected under staff rotation, too.

Hints that a long-term relationship might not be required to induce reciprocal behaviour are found in the literature on one-shot trust and reciprocity games (BERG, DICKHAUT, and MCCABE 1995, DUFWENBERG and GNEEZY 1996, JACOBSEN and SADRIEH 1996, FERSHTMAN and GNEEZY 1998, FAHR and IRLENBUSCH, forthcoming). In reciprocity games the first mover can show trust by sending money to the second mover, who in turn can reward the trust 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, even though all these studies involve anonymous one-shot experimentation without pre-play communication, trust and reciprocation are frequently observed. These results show

[2] BAC (1996) mentions rotation as a possible means to improve monitoring mechanisms.

[3] In this paper, we do not report the general literature on corruption, and refer the reader to the standard references by ROSE-ACKERMAN (1985), KLITGAARD (1988), or SHLEIFER and VISHNY (1993).

that repeated interaction is at least not a necessary condition for reciprocity, although we cannot infer much more from these studies with respect to the present question. The analogy of these games to a corruption scenario does not go beyond that corruption is not workable without some notion of reciprocity. Further, direct comparisons between different matching schemes are not available.

Such comparisons of the level of co-operation with fixed and random partners have mainly been made in experiments on public goods games.[4] 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 players not to invest, but the pareto efficient solution is realised if everybody co-operates by investing the maximum amount. ANDREONI (1988) was the first to compare a public goods experiment under the so-called "strangers" (random matching) to one under the "partners" (fixed groups) condition. He finds that strangers contribute even more to the public goods than partners[5]. WEIMANN (1994) and BURLANDO and HEY (1997) could not replicate ANDREONI's results. In the former study, no significant differences can be found between stranger and partner sessions. In the latter experiment, differences in contribution levels could only be detected in one of two subject pools, in which, contrary to ANDREONI's findings, partners contribute more than strangers. KESER and VAN WINDEN (forthcoming) also find that contributions are higher under a partners matching scheme, on the basis of sufficiently many observations to allow for valid statistical inference. CROSON (1996) and FEHR and GÄCHTER (1998) obtain a similar result. PALFREY and PRISBEY (1995) find that the two treatments differ mostly in greater noise in the strangers data.

All in all, evidence is mixed, and a reinforcing effect of long-term relationships on the level of co-operation is possibly weak. In general, the interpretation of these results with respect to a bribery situation is naturally difficult, since contributions to public goods can be compared to bribes only in the sense that both attempt to induce reciprocal co-operation. The first experimental game explicitly modelling a bribery situation has been introduced by AIR (1999). The authors construct a two-player two-stage game, in which they separate three characteristics of corruption from one another. The control treatment is a basic reciprocity game, in which the important influence of reciprocity is shown. In a second treatment, the reciprocal action damages all other participants in a session, making the corrupt (reciprocal) action overall inefficient. The authors, however, find that the negative externality has 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 fre-

[4] GÄCHTER and FALK (1999) conduct experiments based on the gift exchange game by FEHR, KIRCHSTEIGER, and RIEDL (1993) and compare repeated interaction to a never-meet-again matching. The latter setting, in which the authors observe a lower impact of reciprocity, implements almost one-shot interaction. In practice, however, a never-meet-again scheme is typically not a doable alternative.

[5] 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.

quency 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 bribers typically do not play the same person of the previous round again, and in which players cannot identify their partner from previous play.

3. The Experimental Design

The stage game of our new treatment is identical to the one introduced by AIR. A potential briber (a user of public services, typically a firm, for simplicity we will speak of "users" or "firms" in the following[6]) first decides whether to transfer an amount 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 (which represents initiation costs). 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[7].

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 to her (as manipulating a decision requires effort to justify her choice before her 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. Since the total session involves nine pairs of players, 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.

[6] We use male pronouns for the potential briber, and female pronouns for the public official.

[7] The factor is introduced to avoid the possibility of negative total earnings by the firm transferring too much.

As corruption is done secretly, no information is provided about decisions made by participants playing in different pairs. Thus, no-one possesses any information about the corruption level in the session, and consequently no subject is informed about the damages done to him by other pairs.

The game models a real life corruption scenario in a stylised manner: the firm can offer a bribe to a public official in the hope that she will reciprocate by manipulating the decision to the benefit of the firm. By manipulating, however, she violates the principle to act in the public interest. Exchanging gifts is liable to severe punishment, but is discovered with a very low probability only.

Figure 1 depicts the game tree of the stage game. Player “U” is the user, 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.

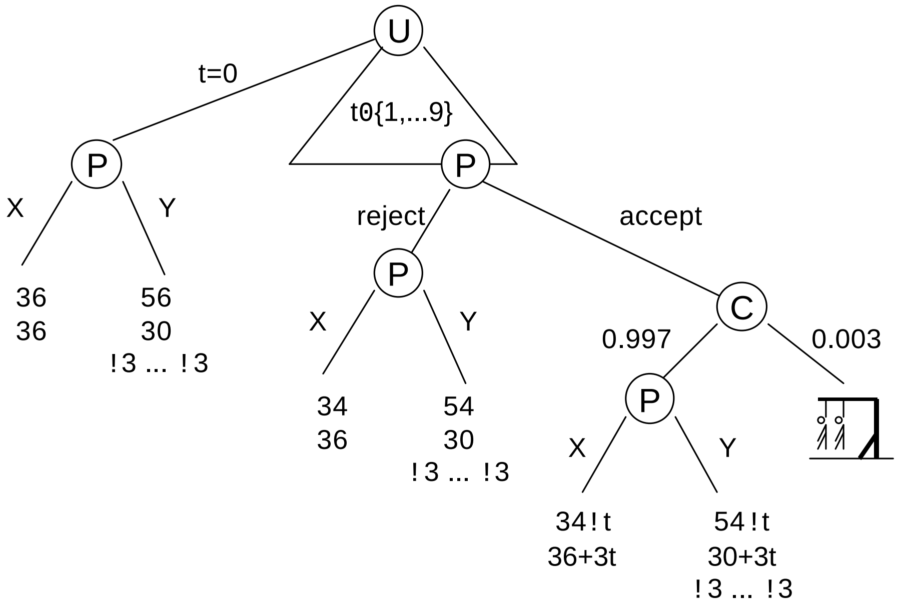

Figure 1

The equilibrium outcome of the stage game can easily be obtained. On an equilibrium path, the public official will always choose X at her terminal decision nodes. Whatever payoff the user can get by transferring a positive amount (34, 34 – t, or the sudden death) is strictly worse than the 36 talers he will receive when he transfers nothing. Thus, in an equilibrium, the user does not pay bribes, and the official chooses X.

To model the staff rotation procedure, we now re-match firms and public officials randomly every round. A subject could therefore not know which other subject (s)he currently played. The random matching procedure is an idealised model of the real staff rotation scenario. Of course, in practice it would be difficult to implement a procedure in which a firm is matched to a different public official every time. Through the idealisation the random matching procedure allows to study the qualitative effects of the staff rotation instrument, but does not draw a quantitatively precise picture of the staff rotation practice. In real-life administrations, it is also in general not possible to create a completely anonymous environment, in which firms and public officials cannot identify the person they are assigned to. However, since both experimental treatments were conducted under anonymous conditions, we controlled for possible effects from that source with respect to the comparability of the treatments[8].

To increase the number of statistically independent observations, 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 her sub-population. In this way we gathered three independent observations in each session. The subjects were not informed about the sub-populations, they only knew that the pairs were randomly re-matched in every round. By not informing them about the population size they were matched with, we intended to insinuate the population to be larger than it actually was.

The experiment was conducted in March 1999 at the *Laboratorium für experimentelle Wirtschaftsforschung* at the University of Bonn. The 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. 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 1.

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 hand-outs is reproduced in appendix I. Payoff tables, also reproduced in appendix I, 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. 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 user or public official, were assigned to the subjects by random draw. After the subjects were seated, the play started immediately. 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 introduction. After the play, subjects were also requested to estimate overall probabilities of disqualification for nine given parameter constellations[9].

[8] For the impact of communication and social interaction see FREY and BOHNET (1995).

[9] The data from the questionnaires replicate AIR's findings of a tendency to under-estimate the probability of disqualification. Since estimations are not the topic of the present study, the data are not reported here.

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.

Immediately after the session, the subjects were paid anonymously in cash, at an exchange rate of 0.03 DM per taler. The total earnings in the session 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 $1^1/_2$ hours, 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 the partner treatment each pair is one independent observation, we have 18 independent observations in the treatment 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.

4.1. The level of corruption

We measure the level of corruption mainly with two variables. The *average offered transfer* measures the users' propensity to pay bribes, the *frequency of Y choices* measures the extent to which decisions have been manipulated by bribery[10]. Figure 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 instrument of staff rotation has a strong negative impact on the propensity of users 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 by 43.7%. Figure 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[11] almost disappears in the staff rotation treatment.

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

[10] It is not self-evident that all Y choices are induced by bribes and not made for other reasons. However, it will be seen that Y is rarely chosen after no transfers have been made.

[11] 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".

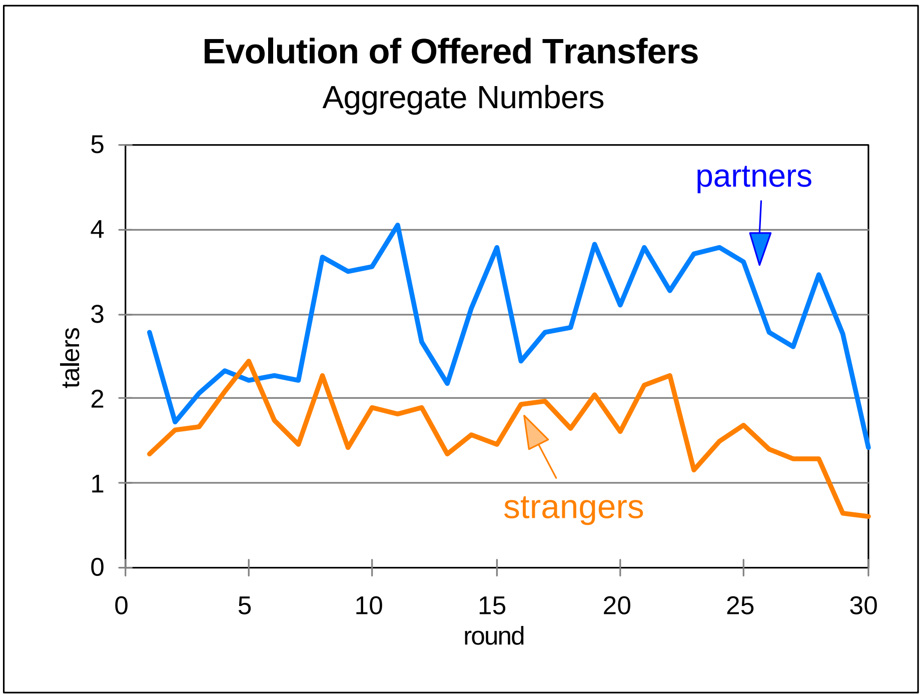

Figure 2

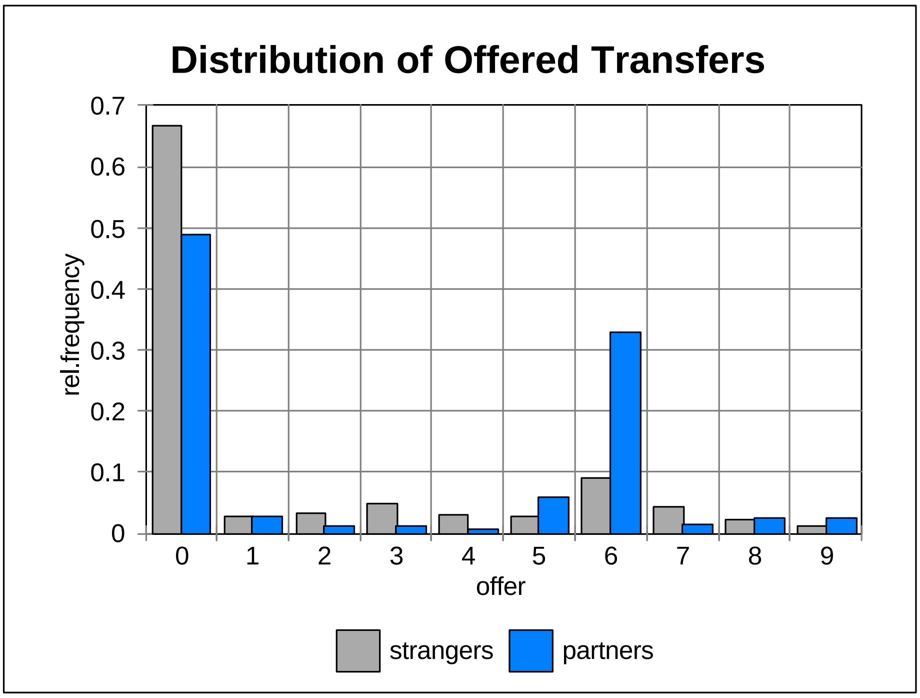

Figure 3

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 which require independence can only be applied to the group averages of the 9 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 of equally high transfers in both treatments at a significance level of " = 0.05 (one-sided)[12].

Figure 4 depicts the evolution of other-damaging Y choices over the thirty rounds of the experiment. 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%.

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

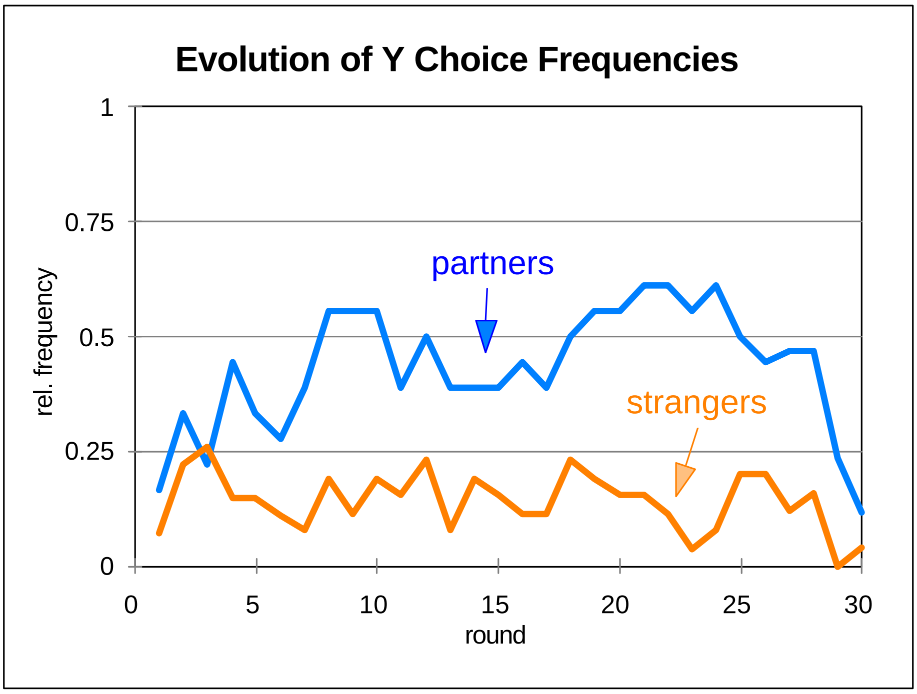

Figure 4

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 " = 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.

The figures 2 and 4 suggest that over time, average transfers rise in the partners and fall in the strangers treatment, whereas the opposite seems to hold for the Y choice frequencies. The disaggregated data, however, show substantial variation in the time trend across the single observations, such that homogeneous trends are not evident.

[12] 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).

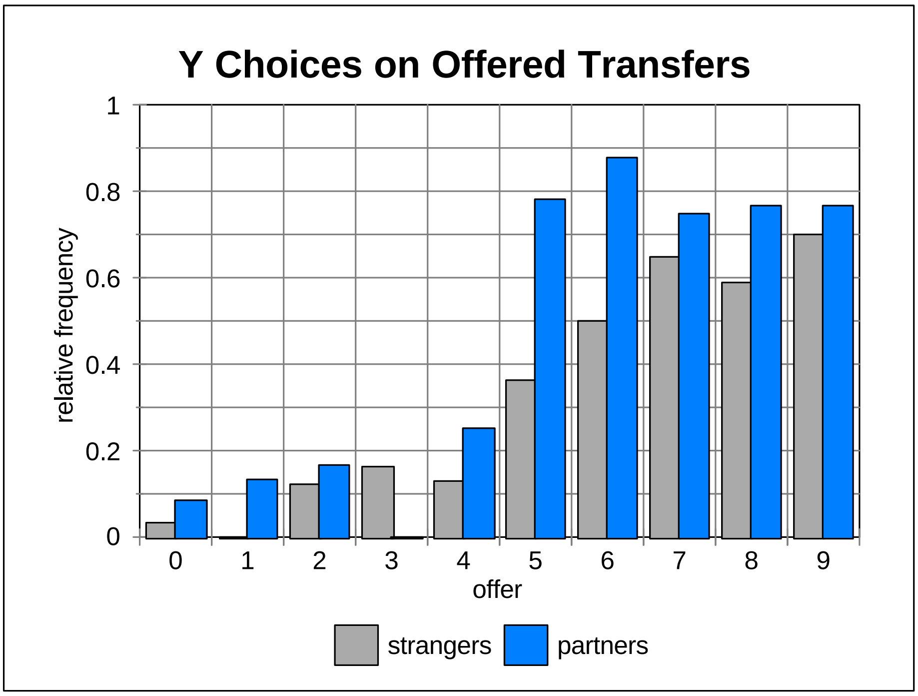

Figure 5

4.2. 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 whether beyond that, officials tend to be less reciprocal in the sense that they tend to choose Y less often also after they received bribe offers. Figure 5 shows that this seems to be the case. On almost all values of offered transfers, officials tend to choose Y less frequently in the strangers treatment. To test whether there in fact is 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 $p = 0.025$).[13]

Observation 3. *On almost all offered transfer values, Y choices are made relatively less frequently under staff rotation (strangers) than with fixed pairs (partners).*

[13] This test is open to the objection that it does not take the amount that was offered into account. Figure 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.

4.3. Users' Reactions to Experience

An important difference between the fixed pairs and the staff rotation setup is the lacking possibility for users to reward co-operative behaviour of officials by paying bribes in the next round. We measure this type of user 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 a X choice, $\#Y$ and $\#X$ denote the number of X and Y choices in rounds 1-29.

If we compute the same measure for the users in both treatments, we in fact obtain much lower *R* measures, as a consequence of the absent possibility to reciprocate directly. For the 18 users for whom this measure can be computed[14], we obtain an average *R* value of +1.20. In the partners treatment, an average *R* value of +3.43 was observed, which is almost three times as high. The two-sample randomisation test rejects the null hypothesis of equally high *R* measures in both treatments at a significance level of " = 0.005 (one-sided).

Nevertheless, for 16 out of 18 users, 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 " = 0.01 (one-sided). Thus, although a user cannot reciprocate directly to the official's choice in the previous round, we observe that his reactions are typically pointed in the same direction. This result gives us insight into the adaptation process by the users. After having experienced a 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 users.

Observation 4. *Under staff rotation, users' transfer behaviour is less conditioned on previous round experience than with fixed pairs. Nevertheless, users react on the public official's decision they experienced by transferring less after X than after Y.*

4.4. Rejections of Offered Bribes

No significant differences can be observed with respect to the public officials' tendency to reject bribes. Figure 6 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.

[14] Nine users never experienced a Y choice by a public official, thus it is not possible to compute a *R* measure for these subjects.

Observation 5. *In both treatments, rejection rates are relatively low for larger transfers. Small bribes are frequently rejected. Treatment differences cannot be detected.*

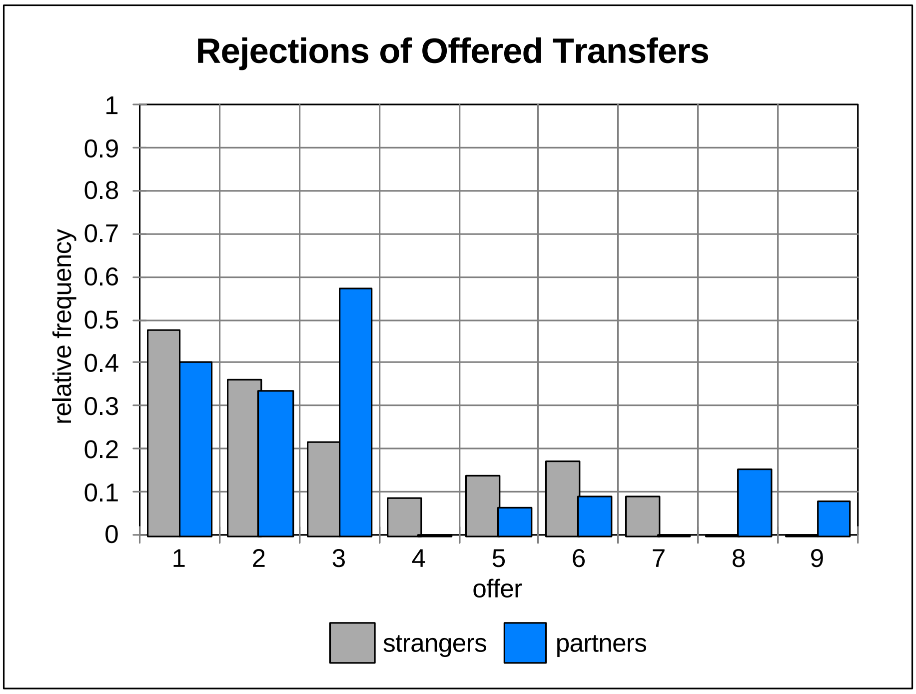

Figure 6

5. Summary and Conclusions

In the combat against bribery, rotation of staff in the public administration is suggested as a precautionary measure. To test the power of this policy instrument, we have conducted the bribery experiment by AIR with a random matching of partners 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 strong evidence for the effectiveness of the rotation instrument 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 of staff rotation on reducing corruption is due to a lower tendency of users 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 users experienced an advantageous decision.

Our data provide clear evidence that staff rotation is a suitable instrument to reduce bribery in situations which are similar to the one modelled in our experiment. Of course, as the experiment was designed to investigate the pure effect of staff rotation, our data are silent about the costs of rotation procedures. Efficiency losses caused by lower acquaintance of public officials with their current affairs, additional training costs and possibly higher salaries necessary for better educated officials are not modelled in our experimental design. Such costs must be traded off against the gains obtained by a lower level of corruption.

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Appendix 1. Instructions, Payoff Tables, and the Main Screen

The Instructions for the Experiment

(original text in German)

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 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.

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.

<u>Attention:</u> 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.

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

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.

The Main Screen Display