Experimental Studies on cross-cultural Behaviour between Germans and Chinese

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Introduction

"For too long, people have taken their own ways of life for granted, ignoring the vast, international cultural community that surrounds them. Humankind must now embark on the difficult journey beyond culture, to the discovery of a lost self and a sense of perspective."

Edward T. Hall, "Beyond Culture"

The world has never been so integrated as today. Globalization, as an irreversible trend, takes place in all economic, political and social areas. Modern technique (IT, media, logistics etc.) provides support to shorten the spatial and time distances among human and stimulates the globalization. However, the globalization, symbolized by enormously reduced physical boundaries among people, does not equal to the unification of cultures, the mental recognitions of people. On the contrary, the process of globalization has always been accompanied by crashes of cultures (Hopper, 2007). This is a crucial issue for many aspects. For example, understanding the cultural background of business partners can avoid unnecessary confusions caused by the misunderstanding or misinterpretation of another way of thinking. Recent researches have already been aware of the importance of cross-cultural study. Hofstede (1984, 2001), as a pioneer researcher in this area, did a questionnaire investigation on culture dimensions using IBM employees in 125 countries. He classified the characters of a culture into five dimensions: power distance, individualism, masculinity, uncertainty avoidance, and long-term orientation.[1] The investigated countries present great deviations in these dimensions.

It is a new research method in cross-cultural studies to run laboratory experiments using subject pools with different cultural backgrounds. Since data are collected in a controlled environment, laboratory experiments fit especially for cross-cultural study purposes. By

[1] Hofsted found the first four dimensions in the first version of his study. The fifth dimension of long term orientation was suggested by Chinese Culture Connection.

strictly controlling possible noises, experimenters can maximally ensure the observed behavioural differences are due to different cultural impacts.

Recent experimental evidences have shown great behavioural deviations across cultures (see for example Hermann et al., 2008a). But to our knowledge, few experiments have been done to systematically study cross-cultural behaviours in two certain countries. In this thesis, we report three cross-cultural laboratory experiments to deliver an insight into the behavioral similarities and differences between Germans and Chinese under different economic and political contexts. One may ask the question why we have choosen Germany and China for the cross-cultural research. There are two reasons. First, in many aspects of defining culture, German and Chinese cultures have few in common. According to Hofstede's (2001) 5-Dimension model, China and Germany are significantly different in four dimensions of "power distance" (China scores 80 and Germany 35), "individualism" (China scores 20 and Germany 67), "uncertainty avoidance" (China scores 30 and Germany 65) and "long-term orientation" (China scores 118 and Germany 31). Only in the dimension "masculinity", China and Germany share the same score (both countries score 66). We can conclude that in comparison to Germany, China is a country with larger human inequality, more collective thinking, less uncertainty avoidance and attach greater importance to the future. Secondly, cross-cultural study is a newly developed academic subject which found its' infancy in North America and Europe. The cross-cultural theories are mainly made upon the western mind. In other words, these studies themselves are culturally bounded and not culture-free because they are constructed on the basic of Western perception of cultural determinants. Eastern ways of thinking could therefore have been neglected (see the example of Hofstede's five cultural dimensions in footnote 1). As Asian countries, especially China, are playing a more and more important role in the globalization process, it increases the necessity to understand how people with eastern mind behave (Tse et al., 1998; Denison et al., 2004).

As mentioned before, a very crucial issue in running cross-cultural experiments is to control possible noises. Roth et al. (1991) already pointed out in one of their earliest cross-cultural experiments, if the research goal of the experiment is to investigate possible cultural differences of behaviour, experimenters must solve the problems of controlling those effects which would induce noises by design. In all three cross-cultural experiments presented in this thesis, we have controlled the following six effects.

- *Controlling subject pool effect*

The German subjects are students from University of Erfurt (chapter I and II). The Chinese subjects are students either from Southwest Jiaotong University (chapter I and II) or from Nankai Universtiy (chapter III). The German subjects were recruited by online recruitment system Orsee (Greiner, 2004). The Chinese students were recruited by campus advertisements promising a monetary reward for participation in a decision-making task. Both German and Chinese subjects are almost all undergraduate students majoring in various disciplines. We also controlled gender and age of our subjects. In all three experiments, the ratio of female to male subjects is between 40:60 and 50:50 in both German and Chinese sessions. Due to different school system, the German subjects are about 1-2 years older than the Chinese subjects.[2]

- *Controlling language effect.*

The experimental introduction for each individual experiment is originally written either in German (chapter I and III) or in Chinese (chapter II).[3] Independent of which language being used as the original one, we use double translations for the instruction in the other country. The first translation is done by a first interpreter to translate the original instruction (German or Chinese) into the other language (Chinese or German in accordance); the second translation, the so-called back-translation (Brislin, 1970), is done by a second independent interpreter to translate the translated text (Chinese or German) back into the original language (German or Chinese in accordance). The final instruction in the second language will be determined after the back-translated text is compared to the original instruction.

- *Controlling experimenter effect.*

In order not to let subjects wonder about our cross-cultural research questions (if they faced a

[2] In china, basic education takes 12 years and military service is not compulsory. In Germany, basic education takes 13 years and military service is compulsory. So most Chinese students enter college at the age of 18 while for German students it is 20.

[3] Whether the original instruction is written in German or in Chinese depends on which session is run as first. If the first session is a German one, then the original instruction is written in German. If the first session is a Chinese one, then the original instruction is written in Chinese.

foreign experimenter), we kept the nationality of the experimenter constant to the subject pool. I myself was the experimenter for the Chinese sessions of the market experiment (chapter I), the dictator experiment without number promising (first part of chapter II), and the bribery experiment (chapter III). The Chinese sessions of dictator experiment with number promising (second part of chapter II) was conducted by a comrade of Southwest Jiaotong University. One of my co-author (Weiss) ran the German sessions of the market experiment (chapter I), a comrade of University of Erfurt ran the German sessions of the dictator experiment (chapter II). All experimental procedures are standardized in form of written documents which are followed by all experimenters in running the experiments.

- *Controlling currency effect.*

Subjects' earnings in the experiments are firstly neutrally framed as *points* in both the German and the Chinese experimental instructions so that subjects from the two countries face the identical calculation problems when they make their decisions. After the experiments, all points subjects earned in the experiments are converted into the local currencies and paid off. Both for the German and Chinese students, the monetary rewards are calculated to equal the local hourly wage in a typical students' job.

- *Controlling laboratory environment effect.*

In this thesis, we used three laboratories for our cross-cultural studies: Laboratory for Experimental Economics (eLab) of University of Erfurt, Germany; Herbert A. Simon & Reinhard Selten Behavioral Decision Research Lab of Southwest Jiaotong University in Chengdu, China; and Laboratory for Experimental Economics of Nankai University in Tianjin, China. All the three laboratories share the same design and thus create almost the same experimental surroundings in our experiments. In all laboratories, all computer terminal cubicles where subjects make their decisions are randomly numbered and separated from each other. Both German and Chinese subjects sit isolated in their own cubicles to make their decisions undisturbed by the experimenters or by other subjects.

- *Controlling computer program effect.*

All experiments presented in this thesis are computerized experiments. We used *zTree*

(Fischbacher, 2007) to program the experiments. For each experiment, both German and Chinese sessions use the same computer programs. Thus subjects from the two countries experienced the same program structure of the experiments.

Chapter I reports a repeated incomplete contracts market experiment with asymmetric power distribution which extends the design of Brown, Falk and Fehr (2004). This experiment aims to study whether and to what extent players, buyers and sellers, use private relational contracting to overcome their disadvantages of being in a weak power position. We distinguish between two powers: relative strategic power (abbreviated as strategic power) and competitive power. Strategic power is owned by the party who can freely make his choices after the other party has already fixed her choices. In the treatments sellers have strategic power, buyers first make a binding price offer for a good, sellers will decide on the quality of the good if they accept an offer. The higher is the quality, the higher is the efficiency. In the treatments buyers have strategic power, the price offer is not binding, the actual price of the good will be determined by buyers after contracted sellers already set the quality of the good. Competitive power is owned by the players who have the short market side. There are either five buyers and seven sellers in the treatments buyers have competitive power, or vice versa in the treatments sellers have competitive power. Buyers have the opportunity to post their price offers publicly to all sellers or send their private price offers to a particular seller. Since Chinese are famous for their dependence on *Guanxi* (interpersonal relationship), we assume to have a higher degree of relational contracting, defined as repeated private trades between two fixed partners, in the Chinese sessions than in the German sessions. Indeed, we find that Chinese subjects rely more on relational contracting than German subjects, especially in the treatments in which sellers have strategic power. The experimental results show that both strategic and competitive power have a robust effect on rent-sharing across different cultures and degrees of relational contracting. Strategic power has a larger impact on rent-sharing than competitive power. In Germany, strategic power brings higher efficiency levels when the power is owned by buyers. This observation, however, vanishes in the Chinese sessions. Take a deeper insight into the data, we find between the two countries, the quality choices are not different in the treatments in which buyers have strategic power. But in the treatments where sellers have strategic power, Chinese subjects choose significantly higher qualities than German subjects. We attribute the increased quality choices to a higher degree of relational contracting in this Chinese treatment. Behaviour both within and outside relations is astonishingly similar between China and Germany. We therefore argue that the main driving force for differences in the effects of strategic and competitive power is the higher level of relational contracting in the Chinese sessions when sellers have strategic power. Rent-sharing is more pronounced and efficiency higher within than outside relations. Efficiency-levels within relations are almost undistinguishable across treatments and cultures. Nevertheless, power still matters for rent-sharing also within relations. Relations therefore seem to be based on a notion of fairness that includes both elements of equality and equity. Relations buffer perfectly against the forces of competitive and strategic power in terms of efficiency but only imperfectly in terms of rent-sharing.

Chapter II reports a voting dictator experiment which aims to stress-test the role of voting to limit power in China and Germany. Voting is widely accepted as an effective mechanism to limit totalitarity and to protect public interests. Experimental researches so far also deliver evidence to support this point of view (see for example Weiss, 2009 and Walkowitz and Weiss, 2009). These studies, however, do not separate the voting processes from other possibly considerations like commitment to promise or reputation building. Indeed, few studies are done to research the voting process per se. We intend to experimentally stress-test the power of a *pure* voting process, and to study whether this power is stable in different cultural and democratic backgrounds of China and Germany. In our experiment, five players, two candidates and three citizens, interact in a group. One of the candidates will be either voted by the citizens (voting treatment) or randomly selected by the program (random treatment) to be the dictator of the group. The dictator will decide how to distribute 100 points among himself and the citizens in his group. Before the dictator is selected, the two candidates have to make a statement each. In order to eliminate any effect of the candidates' statements on their later decisions as dictators (such as the effect of commitment to promise), we let the candidates choose adjectives to describe their personalities, which are not directly related to the decision-making, as their statements. Dynamic considerations such as reputation building are also ruled out in the experiment: the game is played only once. Since the dictator has full power to decide on the welfare of his group, the design allows us to stress-test whether a pure voting process can limit the power and affect the dictators' behavior in favor of the citizens' interests. The results are quite surprising: we cannot find evidence to support the null hypothesis that voting has an effect on the dictators' transfer decisions, neither in China nor in Germany. The results send a cautionary signal that voting does not survive the stress test to limit power, neither in a subject pool from centralized country, nor in a subject pool from democratic country. A similar experimental voting study by Corazzini, Kube and Marechal (2007) differs from our design only in the statements of candidates. In their experiment, the candidates make a number promise as their statements to attract citizens' votes. This experiment was run in Bonn, Germany. Corazzini et al. (2007) find that voting does have an effect on dictators' decisions. In order to test the role of number promise in a different cultural background, we ran two new treatments using number promises as candidates' statements in China with the same subject pool as in the stress-testing experiment. In this experiment, voting shows an effect even in the Chinese sessions: the voted dictators transfer significantly more to their citizens than the randomly selected dictators do. The results of the experiments suggest that the power of voting to limit the self-oriented exertion of power is highly dependent on procedural details.

Chapter III reports a Chinese bribery experiment which is actually a follow-up study to the original German experiment conducted by Abbink and Hennig-Schmidt (2007). The experiment aims to study Chinese subjects' sensitivity to bribery by letting them make bribery decisions either with salient (the loaded treatment) or with implicit information (the neutral treatment) about the negative consequences of a bribery scenario. In the loaded treatment, the decision-making task is presented as an interaction between a firm and a public official. The firm can make private payments to get the public official's permission for running a plant. The permission causes negative consequences to the public. Both the firm and the public official are better off when the private transfer is accepted and the permission is granted. In the neutral treatment, no real-life context is used. Firm/officer is replaced by player A/B, private payment is replaced by transfer, and decision on permission is replaced by choose X or Y. In their German experiment, Abbink and Hennig-Schmidt (2007) could not find evidence supporting the null hypothesis that subjects' behaviors are affected by the two differently framed instructions. We challenge this result by using a very different subject pool of Chinese students. We find that although the Chinese firms do not behave differently between these two treatments, the Chinese public officers, however, both accept significantly less bribe offers and grant significantly less permission in the loaded treatment than in the neutral treatment. Together with the observations of Abbink and Hennig-Schmidt (2007), our findings suggest that subjects with different cultural backgrounds may be differently sensitive to bribery. We conjecture the different treatment effects in the German and Chinese sessions are due to the different current corruption situation and anti-corruption publicities in these two countries. The Chinese subjects may be more sensitive to the loaded context used in our experiment due to the present strong anti-corruption campaign in China. The findings in our experiment may also have some important meanings for policy-makers in China. As firms are always motivated to offering bribes for higher corporate gain, public officials' intention to corruption may be reduced sharply by receiving correct messages.

Chapter I
When Power Meets Relations – Competitive and Strategic Power in Incomplete Contracts Market[1]

1 Introduction

Standard microeconomic theory and experiments (see Holt, 1995 for an overview) are surprisingly well-aligned in predicting behaviour in complete contract environments. Little is known, by contrast, about the incentives to cooperate in contracts that are incomplete in the sense that an obligation of at least one party cannot be enforced[2]. Once we depart from the idealised world of zero transaction cost, real-world contracts have to be acknowledged as being necessarily incomplete to at least some degree. Incentives to either defect or to cooperate may be differently strong for the contracting parties. They may either stem from aspects within or outside the contract relationship. In this paper we will study on both internal and external sources of these incentives: *competitive power* and *relative strategic power*.

Competitive power results from the competitive conditions on the market in which the contract is embedded. The degree of competition may vary with supply and demand if the market is imperfect. The standard case of market imperfections is short-run capacity constraints that make it unprofitable or even technically impossible to quickly build or reduce capacity[3]. A contracting partner has more *competitive power* if she faces less competition than the potential transaction partner. The threat to terminate a relation is, therefore, more credible for the side with competitive power. In case contracts are only partially incomplete, competitive power may already influence the contracting terms to the disadvantage of the side which faces more competition.

How to split the profit is one of the key points for all parties intending to reach a deal. In a world of incomplete contracts, the division of profits may not be contracted in advance. It is

[1] based on: "When Power Meets Relations – Competitive and Strategic Power in Incomplete Contracts Market" by Hong Geng, Bettina Rockenbach and Robert Arne Weiss (2009), Mimeo, University of Bonn.

[2] The contract may not specify an obligation at all, as contract-writing costs increase with the degree of completeness, or it may not be enforceable by a third party due to the absence of a third party with enforcement power or because compliance with the contract is unobservable for a third party.

[3] Constraints to build capacity may be due to the costly and time-consuming transaction cost of taking the necessary steps as well as the uncertainty whether firm-specific investments into capacity will turn out to be profitable. Once firm-specific investments have been made and cost therefore sunk, a firm may decide to sell below average cost as long as the price is above marginal cost which leads to downward capacity stickiness.

rational to assume that each contracting party would take advantage of his own position to seek for a higher rent. For example, imagine such a case in which one party can freely make his choices while the other party is already committed to her decision.[4] Compared to the latter side, the former one has a relative advantage in deciding how to share the rent between the two parties. We coin this advantage *relative strategic power* (for the sake of convenience "relative strategic power" is abbreviated as *strategic power* through the remaining of this paper)[5]. To put it more general, strategic power stems from the resulting internal structure of the contract and lies with the side that still has discretion over whether to abide by its promises once the other side is no longer endowed with a reneging option itself.

Economic theory gives us little guidance to explore strategic and competitive power if contracts are incomplete and contract compliance is costly. Standard game-theory based on the assumption of common knowledge of rationality only allows for cooperation in equilibrium if the parties have an infinite horizon[6]. Unfortunately, as stipulated by the folk theorem for repeated games, there is typically a multitude of equilibria of which no cooperation is always one. Clear predictions can, therefore, not be derived. If there is a known end to any interaction between the parties, standard game theory fails to predict any cooperation above enforceable levels and hence leaves very little room for effects of competitive and strategic power. Voluntary cooperation, however, is a robust phenomenon in sequential cooperation games in finite play (compare Healy, 2007). Furthermore, viewing existing experimental evidence (Brown, Falk and Fehr, henceforth BFF, 2004 and 2008); Wu and Roe, henceforth WR, 2007[7]) in the light of our notions of competitive power and strategic power shows both to matter. Recent economic theories of other-regarding preferences (e.g. Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Falk and Fischbacher, 2006) built on experimental evidence do predict cooperation even in finite play and go already some way in explaining consequences of strategic power in static play (compare Fehr et al., 2007).

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[4] Another case has the same consequences: One party must announce his choices while the other party can hold her choices secret before end of the game.

[5] We are aware that the definition of "relative strategic power" is not 100% explicit. Defined by Dahl (1957), interpreted by Harsanyi (1962a, b) and summarized by Selten (2001), power is "the capability to induce others to do something which they would not do otherwise". In this sense, the side with strategic power in our study cannot induce the other party to make choices as they wish. Their advantage comes rather from the structure of the game that they do not have to specify all parameters while the other side has to. Theoretically, the strategic last mover advantage mentioned in the text is equal to the first mover advantage mentioned in the footnote 4. (We own this point to Prof. Dr. Dr. Reinhard Selten.)

[6] Technically, cooperative equilibria are also possible in finite games with unknown end in which the players attach a positive probability to the continuation of the finite game in every round.

[7] WR (2007) for strategic power and the comparison of BFF (2004, 2008) for competitive power.

However, they are so far not generic enough to fully understand the complex interaction between strategic and competitive power in repeated play.

Hence, empirical data are needed. An ideal data set for this research goal would entail exogenous changes in competitive and strategic power keeping everything else constant. This data set is almost impossible to get in the field as collected data would suffer from endogeneity problems. The allocation of strategic power is likely to also depend on the relative competitive power of the contracting parties. The side with competitive power will try to design the contract in a way that gives itself strategic power as well. Competitive power itself may depend on the institutional determinants of strategic power. A market that suffers for example from enforcement problems for quality is less attractive for buyers to enter than one in which well-functioning enforcement institutions are in place. We circumvent the endogeneity problem by exogenously shifting competitive and strategic power between buyers and sellers using controlled experiments. Previous experiments, albeit not specifically designed to do so, already provide some insight on either competitive or strategic power.

BFF (2008) experimentally analysed the role of unemployment for the emergence of relational contracting and efficiency. They found that wages and in reciprocal manner rents react – in our terminology – to changes in competitive power while efficiency remains unaffected. They also showed that long-term relations emerge to a larger extent if competitive power sides with the principals. In all their treatments, agents had strategic power; principals and agents are analogous to respectively buyers and sellers in our terminology.

According to Fehr et al. (2007), efficiency in a one-shot principal-agent game is higher if the side with lower cost to trust has to trust by moving first. Efficiency is higher in a bonus contract which – in our terminology – gives strategic power to the buyers. Buyers also achieved higher profits if they had strategic power. WR (2007) support this result in a repeated play framework. They found efficiency to be higher and relational contracting to be reduced if buyers had strategic power. They also found the side with strategic power to earn a larger share of rents. In their experiment, competitive power always stayed with the buyers.

To the best of our knowledge, this is the only study based on the notions of competitive and strategic power and which is able to study their interaction by systematically varying both types of power. This is our first research aim. Our second research goal takes an interesting comparison of two very similar treatments in two different subject pools as a starting point. WR´s (2007) results show less relational contracting on the one hand and lower efficiency and higher inequality on the other hand than BFF´s (2004) almost identical experiment (compare section 5.2). We want to investigate the interaction of relational contracting with strategic and competitive power. In order to achieve this, we run the experiment with two different subject pools, in Erfurt in Germany and in Chengdu in PR China, which we expect to have different tendencies to engage in relational contracting. This also allows us, as a third research goal, to give a first cross-cultural account on the nature of relational contracting.

We find competitive and strategic power both matter. Competitive power influences rent-sharing even if contract enforcement is entirely absent. Strategic power has a larger impact on rent-sharing than competitive power. Competitive power does not affect efficiency. Strategic power only influences efficiency in case buyers react weakly to the strategic value of relational contracting, as they do in Erfurt, Germany. In this case efficiency is raised when buyers have strategic power. If, however, subjects employ relational contracting more as a contract enforcement device, as they do in our control sessions in Chengdu, China, neither competitive nor strategic power affects efficiency. Relational contracting is used more when buyers, who initiate offers, do not have strategic power. Despite different levels of relational contracting, the characteristics of relations are astonishingly similar across cultures as well as market conditions and contract structures. Relations are always based on a high degree of gift-exchange. We conclude that cross-cultural differences in the behavioural response to the strategic value of relational contracting drive the disparities in the effects of strategic power between our sessions in Germany and China.

The rest of our paper is organised as follows: we introduce the experimental design and procedure in the following section. Market equilibria and behavioural predictions in the absence of common knowledge of money-maximising rationality are discussed in section 3. In section 4, we present the results on competitive and strategic power from our German sessions. We highlight the role of relational contracting and lead over to our second research question, on the robustness of our effects, in section 5. In section 6, cross-cultural hypotheses are discussed; comparative results of our Chinese sessions are presented in section 7. Section 8 provides a first account of the cross-cultural nature of relational contracting. The contributions of this paper are summarised in section 9, while the final section 10 provides a discussion and an outlook for future research.

2 Experimental design and procedure

2.1 Treatments

Our experiment was implemented on the proven experimental platform of BFF (2004), used also by WR (2007) and BFF (2008). A market consists of 12 players. They are either 5 buyers and 7 sellers (in the treatments that buyers have competitive power), or 7 buyers and 5 sellers (in the treatments that sellers have competitive power). Every player gets a unique ID number through the whole experiment. Each buyer may buy one unit of a homogenous good while each seller may sell one unit. An experimental session is made up of 15 repeated playing rounds. Depending on different strategic power distribution, a playing round is composed of either two (in the treatments that sellers have strategic power) or three (in the treatments that buyers have strategic power) sequential phases.

The first phase is so-called trading phase which lasts 180 seconds. In the trading phase, buyers first announce their offers. Each offer contains a price offer for the good and a desired quality of this good. Buyers can choose to post their offers publicly to be seen by all players or/and send private offers to specific sellers. Each buyer may send as many offers as he wants. Each seller sees all public offers and the private offers she received from the buyers. A deal is contracted when a buyer's offer is accepted by a seller. Each seller may accept exactly one buyer's offer. Once an offer has been accepted, all outstanding offers of the respective buyer are deleted[8]. Buyers get the information which sellers have already reached a deal and which sellers are still available. Notice that a deal, either public or private, can only be initiated by buyers through sending offers. Sellers do not have the opportunity to induce any deal. The trading phase is ended either when all players of the short market side have reached a deal or the trading time (180 seconds) has elapsed.

In the second phase, sellers who reached a deal in the trading phase must deliver a quality of the good to their contractual buyers. The actual chosen quality of the seller may differ from the desired quality of the contractual buyer. In the treatments that sellers have strategic power, a playing round is ended after the second phase is completed. In these treatments, buyers' price offers are binding. That is to say, buyers must pay the contracted price they offered in the trading phase as soon as their offers are accepted by a seller.

In the third phase which only takes place in the treatments where buyers have strategic power, the price offers that buyers made in the trading phase are not binding. After sellers have

[8] Hence, if more than one seller wants to accept a buyer´s offer, the one who decides first gets the offer.

delivered the quality of the good, buyers can decide the actual price to be paid to the seller. The actual price may differ from the contracted price offer. A playing round of treatments in which buyers are the last movers ends after the third phase.

We denote buyers' offered price and desired quality of a contracted deal as p' and q' respectively. The actual chosen quality by sellers and price by buyers are called p and q respectively. In the treatments seller have strategic power, p'=p.

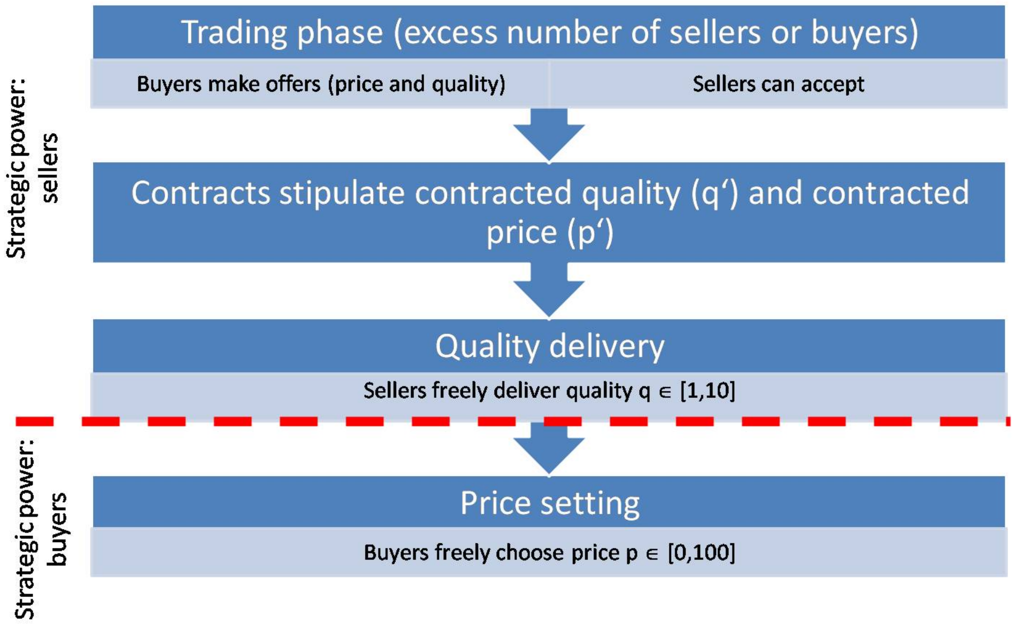

Figure 1: Sequence of players´ moves

Figure 1 illustrates the sequence of players' moves in our experiment. The combination of the presence and absence of competitive and strategic power constitutes our 2x2 design, as displayed in Table 1.

Table 1: Experimental treatments

		competitive power	
		Sellers	Buyers
strategic power	Sellers	spS-cpS	spS-cpB
	Buyers	spB-cpS	spB-cpB

We understand the treatments spS-cpB and spB-cpS as mixed-power treatments, as competitive and strategic power are allocated to different sides, whereas we think of spS-cpS and spB-cpB as concentrated-power treatments, as one side has both types of power.

Apart from slight modifications, the treatment spS-cpB corresponds to the treatments ICF by BFF (2004) and IC1 by WR (2007), while the treatment spS-cpS is equivalent to the IC-treatment in BFF (2008). In a slightly different form spB-cpB was also employed by WR (2007) as their IC2-treatment. The novel treatment spB-cpS is central to the research question as it allows us to study the relative effects of competitive and strategic power.

2.2 Payoffs

Payoff-functions for buyers (π_B) and sellers (π_S):

$$\pi_B = 10 * q - p$$

$$\pi_S = p - c(q)$$

Cost of providing quality rises in quality according to the following convex schedule

q	1	2	3	4	5	6	7	8	9	10
$c(q)$	0	1	2	4	6	8	10	12	15	18

The outside option both sellers and buyers earn if they do not conclude a trade is 4:

$$o_B = o_S = 4$$

Subtracting the outside option from the payoffs yields buyer- and seller-rents:

$$r_B = 10 * q - p - 4$$

$$r_S = p - c(q) - 4$$

Buyer-rents are an increasing function of q with a constant marginal rent of 10:

$$\frac{\partial r_B}{\partial q} = 10$$

Seller-rents are a decreasing function of q with a maximum marginal cost of 3:

$$\frac{\partial r_S}{\partial q} \geq -3$$

Since the marginal rent for buyers is always larger than the marginal cost for sellers, joint rents increase in quality. The social optimum is, therefore, reached if sellers provide the maximum quality of 10. The maximum achievable joint rent per trade is

$$r_{max} = r_B(q = 10) + r_S(q = 10) = 100 - 18 - 8 = 74$$

In this case, trading efficiency is 100%; it is defined as achieved gains from trade relative to maximum possible gains from trade:

$$TE = \frac{r_B(q) + r_S(q)}{r_B(q = 10) + r_S(q = 10)}$$

2.3 Defining "relational contracting"

As each player can only make one transaction per round, at least two subjects of the side without competitive power will therefore not conclude a trade in any round. For any type of action buyers and sellers can be identified by their unique identification numbers. Furthermore, subjects are asked to document the history of their trades, including the identification number and the profit of their respective trading partner, on a separate documentation sheet. Hence the design allows for relational contracting, i.e. for repeated trading between the same seller and buyer. Technically repeated trading is possible both by public and private trades. However, the only reliable way for a buyer to form a relation is to make a private offer to the seller of the previous round. By making public offers, buyers are effectively entering a spot-market as they have no control over who accept their offers. If a seller accepts a private renewal offer we think of the pair as engaging in relational contracting. According to our notion of relational contracting, relationships consist of any type of first round trade plus at least one *private renewal trade*. We distinguish *private renewal trades* from *public trades*, i.e. publicly initiated trades, and *private new trades*, i.e. privately initiated trades within a transaction pair that is different from the previous round. Private new trades can be understood as an attempt to establish a relation.

2.4 Procedure

The Erfurt-sessions of the experiment were run in April 2007 in the *Erfurter Laboratorium für experimentelle Wirtschaftsforschung* (elab). The Chengdu-sessions of the experiments were run in April 2008 at the *Herbert A. Simon & Reinhard Selten behavioral decision research lab* of the Southwest Jiaotong University in Chengdu, China. In each location, we collected six independent observations for each treatment. Therefore, in both Erfurt and Chengdu 288 subjects[9] participated. Sessions lasted between 90 to 120 minutes, and subjects earned about 12.5 Euros in Erfurt and about 61 RMB in Chengdu (earnings are equivalent to about 20 USD and 8.5 USD at the time of the experiment in Germany and China respectively). The hourly average payments were set according to local standards and paid in cash after the experiments had been finished. Test questions made sure that subjects were aware of the structure of the game, including the number of players on each side, as well as how profits are

[9] Apart from one subject in Erfurt who was a pupil, all subjects were university students.

calculated. The experiment only started when all subjects of a session correctly answered all the test questions. The experiment was programmed in z-tree (Fischbacher, 2007).

3 Market equilibria and behavioural hypotheses

3.1 Analyses of market equilibria[10]

In order to derive the market equilibria for the four treatments, we first assume that it is common knowledge that all players are rational and selfish. Thus no cooperation above minimum levels of price and quality will take place in the last round of any treatment. We also assume that the strategy which brings higher payoff than the alternative outside option is strictly preferred than the strategy which yields an equal payoff to the outside option.

In the last round of the treatments that sellers have strategic power (spS-treatments), sellers will only accept offers at a price of at least 5 and will set minimum quality of 1 for any possible price. Buyers, anticipating this, will offer a price of 5 in order to be marginally better off than their outside option of 4. Since both parties are better off by concluding a trade than by taking their outside option, all trades are predicted to take place and market efficiency (defined as realised number of trades over maximum number of trades)[11] is therefore 100%. By backward induction, the same equilibrium will be observed in all previous playing rounds.[12]

In the last round of the treatments that buyers have strategic power (spB-treatments), buyers will pay the minimum price of zero as the contracted price is non-binding. Anticipating this, sellers will consequently not accept any offer since they would be strictly worse off by concluding a trade than by gaining their outside option of 4. Sellers will consequently not accept any offer. By backward induction, no trade will take place in any previous playing round. Market efficiency is therefore zero.[13]

[10] In the trading phase speed of making offers and accepting offers may play a role (compare footnote 8). Since speed as a personal characteristic is outside any game-theoretic model, we cannot solve the entire game. We therefore abstract from giving an explicit account of the trading phase and treat the game as if speed played no role. We are then not able to predict which seller or buyer will conclude a trade. Nevertheless, we are able to describe the trades that will occur in equilibrium if players are commonly known to be rational and money-maximising.

[11] We distinguish market efficiency and trading efficiency. Market efficiency refers to the number of undertaken trades as a percentage of all possible trades. Trading efficiency refers to the percentage of achieved gains from trade for the trades undertaken.

[12] See also BFF (2004) and BFF (2008) for deriving the equilibrium in spS treatments.

[13] See also WR (2007) for deriving the equilibrium in spB treatments.

3.1.1 Predictions for strategic power

Shifting strategic power to the buyers is predicted to decrease efficiency but not to affect relative rent-sharing. At minimum level of quality trading entails a social gain per trade of 2, which is lost in the spB-treatments. Regardless of strategic power, both parties are therefore predicted to earn the same share of rents.

3.1.2 Predictions for competitive power

Based on the assumptions made, a price of 5 and a quality of 1 is predicted for both cases of competitive power when sellers have strategic power, while no trades are predicted to take place for both cases of competitive power when buyers have strategic power. Competitive power is therefore not predicted to have any effect on the behaviour of the players. Consequently, also efficiency and rent-sharing is predicted not to be affected by competitive power.

Table 2 summarises the market equilibria based on common knowledge of rationality:

Table 2: Summary of market equilibria

	spS-cpB	spS-cpS	spB-cpB	spB-cpS
Market efficiency	100%	100%	0%	0%
Price	5	5	-	-
Quality	1	1	-	-
Seller rent	1	1	0	0
Buyer rent	1	1	0	0
Share of buyer profits	50%	50%	50%	50%

3.2 Behavioural hypotheses

The hypotheses change substantially if we relax the assumption of common knowledge of money-maximising rationality. As long as subjects expect to earn rents from cooperation at the end of the experiment, due to imperfect knowledge of rationality (for a theoretical analysis see Kreps et al., 1982; for experimental results see Andreoni and Miller, 1993), or do not induct the entire game backwards (Rapoport, 1997; Weber et al., 2004; Charness and Levin, 2005), there is scope for cooperation throughout the experiment. Rents from cooperation even at the end of the game may arise out of other-regarding preferences (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). In order to derive hypotheses in the absence of common knowledge of rationality, we assume the co-existence of two types of players that are hidden information: reciprocal players and money-maximising players. Reciprocal players always reciprocate gifts for intrinsic reasons, i.e. without any expected material gain. We understand gifts to be actions that may lead to positive rents for the last-moving transaction partner. Reciprocal players are assumed to reciprocate in a way that gives the other party a positive and constant share of rents; that is to say, the share of rents is assumed to be independent of the sum of both parties´ rents[14]. Reciprocity then implies a rising relation between quality and price (when sellers have strategic power) or price and quality (when buyers have strategic power). Money-maximising players, by contrast, always choose the action that maximises their own payoff; money-maximising players consequently only reciprocate if they expect to gain from it. We assume money-maximising players to reciprocate for strategic reasons, but never to a stronger degree than reciprocal players[15].

In the presence of reciprocal players considerable cooperative gains are available to be distributed between the transaction partners, and incentives to conclude trades are strong. We consequently expect market efficiency not to be significantly lower than 100% in all treatments.

Hypothesis 1: *Market efficiency is 100% in all treatments.*

In order to think about the effects from strategic and competitive power in the presence of both reciprocal and money-maximising players, it is helpful to separate the static and dynamic incentives. Reciprocal players reciprocate gifts in both static and dynamic games. In a one-shot game or at the end of a repeated game, the money-maximising action is unambiguously not to reciprocate. In a repeated game, also money-maximising players may reciprocate if, by reciprocating in one round, they can sufficiently increase the chance to gain rents in the rounds that follow. A player with strategic power may earn rents in future rounds by concluding repeated trades in a high-trust relation. Within relations, we therefore expect money-maximising players to behave as if they were reciprocal players. Outside relations, dynamic incentives are weak so that money-maximising players will reciprocate significantly less than reciprocal players. Building on the findings from BFF (2004, 2008) and WR (2007) we expect trades to occur both within and outside relations.

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[14] To be precise, we add a further assumption that follows the models of for instance Fehr and Schmidt (1999) and Bolton and Ockenfels (2000): the reference point of a reciprocal player is solely based on the payoff-comparisons within their own transaction with their current partner. A reciprocal second mover is therefore not willing to pay a premium for the risk of the first mover being the sucker.

[15] Money-maximising players may try to mimic the behaviour of reciprocal players in order not to reveal their type (for further discussion see BFF, 2004). However, it never pays to reciprocate stronger than a reciprocal player as a money-maximising player could then increase her payoff without revealing her type.

3.2.1 Hypotheses for strategic power

In the spB-treatments, buyers are the last movers. For each trade, buyers can always adjust rent-sharing such that they earn at least as much as their trading partner, while sellers are dependent on reciprocating buyers to earn a rent. Given our assumptions, not all buyers will reciprocate. The corresponding analysis holds for the spS-treatments. Buyers are dependent on sellers to reciprocate in order to earn a rent, while sellers can always adjust rent-sharing to their own favour. Again, not all sellers are expected to do so. Consequently, we expect the side with strategic power to earn, ceteris paribus, a larger share of rents.

Hypothesis 2.1: *Strategic power increases ceteris paribus the share of rents:*

$$\left(\frac{r_B}{r_S+r_B}\right)_{spB-cpB} > \left(\frac{r_B}{r_S+r_B}\right)_{spS-cpB} \Leftrightarrow \left(\frac{r_S}{r_S+r_B}\right)_{spB-cpB} < \left(\frac{r_S}{r_S+r_B}\right)_{spS-cpB}$$

$$\left(\frac{r_B}{r_S+r_B}\right)_{spB-cpS} > \left(\frac{r_B}{r_S+r_B}\right)_{spS-cpS} \Leftrightarrow \left(\frac{r_S}{r_S+r_B}\right)_{spB-cpS} < \left(\frac{r_S}{r_S+r_B}\right)_{spS-cpS}$$

In order to think about the efficiency-consequences of strategic power, it is instructive to first look at the static incentives of the game, i.e. treating the game as if it was played only once, and then analyse how dynamic incentives may change the analysis. When sellers have strategic power, money-maximising players will only provide minimum-quality so that trades that involve money-maximising players will be characterised by low trading efficiency. By contrast, when buyers have strategic power, both reciprocal and money-maximising sellers may provide above minimum quality if on average buyers as a group reciprocate sufficiently to render providing above-minimum quality profitable. In this case, non-reciprocal behaviour by money-maximising buyers may have no negative efficiency consequences at all. For any positive degree of rent-sharing, i.e. both sides of the transaction receiving a positive share of the generated rent, the cost to trust are higher for buyers than for sellers. Sellers only bear the cost to provide quality while buyers have to additionally pay upfront the share of rent going to the seller in order to induce a reciprocal seller to provide this quality level. We may therefore expect sellers to be more willing to provide a given quality level than buyers are willing to pay the required price to induce on average the same quality level[16]. In a one-shot game, we

[16] An example helps clarify this point: a buyer has to pay 59 upfront in order to induce a quality level of 10 by a reciprocal player; in this case, both players earn the same rent of 37. If, however, the buyer meets a money-maximising seller who is only willing to provide a quality of 1, the buyer makes a rent of -53. For sellers the equivalent risk is only 22.

therefore expect trading efficiency to be higher when buyers have strategic power. This is also the result found by Fehr et al. 2007 in a similar one-shot experiment[17].

Our experiment, of course, is not played once but repeatedly. In a repeated game, also money-maximising players may reciprocate if the dynamic incentives are sufficiently strong. In case sellers have strategic power, efficiency would then be directly raised compared to one-shot play. If money-maximising buyers reciprocate in the spB-treatments, efficiency can also be indirectly affected through the quality choices of sellers. If more buyers reciprocate in repeated compared to one-shot play, the profitability to choose above-minimum quality increases for sellers; sellers may react by increasing their quality choices.

As we argued in the introduction of section 3, strong dynamic incentives can only be expected within relationships, i.e. through relational contracting. Outside relationships, dynamic incentives are weak. Dynamic incentives are an endogenous outcome of the behaviour of players. They depend on the degree subjects make use of relational contracting as a contract enforcement mechanism, which we cannot predict with the current theories at hand. The relative strength of the incentives for sellers to provide above-minimum quality is therefore a question open to empirical investigation. We therefore stick to existing experimental evidence from WR (2007) that show efficiency to be higher when buyers have strategic power. The results of WR (2007) consequently imply that differences in static incentives for sellers to provide above-minimum quality drive efficiency differences. We nevertheless concede that this result may be sensitive to the degree of relational contracting.

Hypothesis 2.2: *Quality levels and consequently trading efficiency are higher if buyers have strategic power:*

$$q_{spB} > q_{spS}$$

3.2.2 Hypotheses for competitive power

In cpS-treatments, buyers face strong competition to find a trading partner. Compared to cpB-treatments, this pressure should increase contracted prices.

Hypothesis 3.1: *Contracted prices are higher in cpS than in cpB.*

[17] Fehr et al. (2007) formally explain this result by using the Fehr and Schmidt (1999) model of inequity aversion. The main difference in their line of argumentation is that they assume reciprocal players, which they call fair players, not to be willing to display maximum trust because of the fear of suffering from disadvantageous inequality. The qualitative result, however, is robust to this assumption.

$$p'_{cpS} > p'_{cpB}$$

In case sellers have strategic power (spS-treatments), higher contracted prices directly translate into higher actual prices and hence, for a given quality level, higher seller-rents and lower buyer-rents. As the long market side in spS-cpB, sellers have a higher incentive to reciprocate in spS-cpB than in spS-cpS in order to increase their chance of receiving a renewed offer from their current buyer. We therefore expect lower quality choices in spS-cpS than in spS-cpB for any given price level. Both effects from giving competitive power to sellers, higher prices and – ceteris paribus – lower quality, let us predict a higher share of rents for sellers in spS-cpS than in spS-cpB.

In case buyers have strategic power (spB-treatments), contracted prices are non-binding. Nevertheless actual prices may ceteris paribus be higher when sellers have competitive power (in spB-cpS). Buyers have a higher incentive to be re-matched with their current seller if they do not have competitive power. Buyers may be able to increase their chance to enter a relation with their current seller by paying high prices and thereby building the reputation as a reciprocal buyer. For sellers, the incentive to enter a rent-generating relation is higher if buyers have competitive power (in spB-cpB). Sellers may attempt to enter a relation by providing high quality and thereby building a reputation as a trusting seller. To summarise, we expect two effects by shifting competitive power from sellers to buyers when buyers have strategic power: firstly, higher quality given expected prices and secondly, lower prices given quality. Both effects lead us to predict a higher share of rents for buyers in spB-cpB than in spB-cpS and vice-versa for sellers.

Hypothesis 3.2: *Competitive power increases ceteris paribus the share of rent:*

$$\left(\frac{r_B}{r_S+r_B}\right)_{spB-cpB} > \left(\frac{r_B}{r_S+r_B}\right)_{spB-cpS} \Leftrightarrow \left(\frac{r_S}{r_S+r_B}\right)_{spB-cpB} < \left(\frac{r_S}{r_S+r_B}\right)_{spB-cpS}$$

$$\left(\frac{r_B}{r_S+r_B}\right)_{spS-cpB} > \left(\frac{r_B}{r_S+r_B}\right)_{spS-cpS} \Leftrightarrow \left(\frac{r_S}{r_S+r_B}\right)_{spS-cpB} < \left(\frac{r_S}{r_S+r_B}\right)_{spS-cpS}$$

In order to think about the possible efficiency implications of competitive power, we need to look at the behaviour of sellers as the quality level determines the gains from trade, while the price paid by buyers is only redistributive. Based on the above analysis, no clear prediction on efficiency consequences of competitive power emerge as we predict two counter-running effects for both cases of strategic power: in spS-treatments, shifting competitive power to sellers should lead to higher prices (which may raise quality as a reciprocal response) but, at the same time, to lower quality given prices. In spB-treatments, competitive power lying with sellers instead of buyers should lead to higher quality for a given level of reciprocity by the buyers but at the same time to less reciprocal behaviour by the buyers.

3.2.3 Hypotheses for the interaction between strategic and competitive power

One novel aspect of this paper is analysing the interaction between competitive power and strategic power. The first case is comparing concentrated-power and mixed-power treatments[18]. Based on our prediction that both strategic and competitive power matter we predict buyers to earn more than sellers when they have both strategic and competitive power (in spB-cpB) while sellers are predicted to earn more than buyers when they have strategic and competitive power (in spS-cpS).

Hypothesis 4.1: *The favoured side will earn higher rents than the unfavoured side in concentrated-power treatments:*

$$r_{B_{spB-cpB}} > r_{S_{spB-cpB}}$$

$$r_{B_{spS-cpS}} < r_{S_{spS-cpS}}$$

The second case is comparing rent-sharing in the two mixed-power treatments spB-cpS and spS-cpB. Without any more elaborate theory, we cannot predict which side earns a larger share of rents. We do, however, expect rent-sharing to be more pronounced when competitive and strategic power are each allocated to different sides. In order to control for possible differences in quality levels, we predict relative inequality, i.e. equality for given gains from trade, being lower in the mixed-power treatments spS-cpB and spB-cpS than in the concentrated power treatments spS-cpS and spB-cpB.

Hypothesis 4.2: *Relative inequality is lower in mixed-power treatments than in concentrated-power treatments:*

$$\left(\frac{|r_S - r_B|}{r_S + r_B}\right)_{spB-cpS,spS-cpB} < \left(\frac{|r_S - r_B|}{r_S + r_B}\right)_{spB-cpB,spS-cpS}$$

3.2.4 Hypothesis for the interaction of strategic power and competitive power with relational contracting

In light of existing experimental evidence (BFF, 2004), relational contracting is based on a high degree of rent-sharing and trust. We therefore hypothesise that the effects of competitive

[18] Recall that in concentrated-power treatments one side has both strategic power and competitive power (spB-cpB and spS-cpS), while in mixed-power treatments each sides has either strategic or competitive power (spB-cpS and spS-cpB).

and strategic power on rent-sharing will be lower within relations than outside relations resulting in a lower degree of relative inequality.

Hypothesis 5: *Relative inequality is lower within relations than outside relations:*

$$\left(\frac{|r_S - r_B|}{r_S + r_B}\right)_{within\ relations} < \left(\frac{|r_S - r_B|}{r_S + r_B}\right)_{outside\ relations}$$

4 Results of the German sessions

Already the rough sketch of the data in table 3 shows that the predictions based on money-maximising players have to be rejected. There is a substantial degree of cooperation in all treatments. Both average prices and average quality choices lie significantly above minimum levels. Furthermore, the prediction that no trade would occur in the spB-treatments has to be strongly rejected. In fact, 99.56% of all possible trades are undertaken in spB-cpB, and all possible trades are undertaken in spB-cpS. Consequently, hypotheses H1 is supported by the data. There is also no statistical difference in market efficiency between the four treatments.

Table 3: Descriptive summary statistics of the German treatments

	spS-cpB	spS-cpS	spB-cpB	spB-cpS
Market efficiency	97.56%	99.11%	99.56%	100%
Mean price	34.94	47.29	29.47	33.79
Mean quality	5.20	5.83	6.83	6.86
Mean seller rent	23.57	34.51	15.01	19.39
Mean buyer rent	13.11	6.98	34.86	30.85
Share of buyer rents	35.72%	16.82%	69.90%	61.41%
Share of trades within relations	30.24%	33.24%	19.79%	8.89%

4.1 The role of strategic power

Let us remind the reader that strategic power means freely making the choices while the other party is already committed to his decision. For buyers, strategic power means that they can deviate from the contracted price after the seller is already committed to a quality level. If sellers have strategic power, they are free to choose the levels of quality when the buyers are already bound to pay the contracted price. Does strategic power pay off?

Figure 2 illustrates the aggregated effects of strategic-power both for rent-sharing and efficiency. The total height of the columns depicts efficiency, and the separate heights of the seller and buyer columns show the share of rents that accrues to sellers and buyers respectively.

Figure 2: Effects of strategic power for rent-sharing and trading efficiency

Strategic power strongly influences rent-sharing to the advantage of the strategically favoured side. Buyers gain a significantly larger share of rents in the spB-treatments (on average 65.63%) than in the spS-treatment (on average 25.71%)[19,20]. Buyers also gain a significantly larger share of rents than sellers (34.36%) in the spB-treatments, while the opposite is true in the spS-treatments[21]. The impact of strategic power therefore goes beyond the ceteris paribus changes in rent-sharing that were hypothesised in H2.1. The side with strategic power always gains more than the side without strategic power. How does strategic power pay off?

There is a substantial degree of reciprocity in both types of strategic power-treatments as prices and quality-levels are strongly correlated: Spearman's rank correlation coefficients between group-average prices and group-average quality-levels reach 0.846 in spS-treatments and 0.888 in spB-treatments, which are both significant at 0.01 level. The same holds for within-group correlation between prices and quality: Spearman's rank correlation coefficients vary from 0.251 to 0.839 in spS-treatments and from 0.377 to 0.756 in spB-treatments; binomial tests reject the null hypothesis of an equal probability of positive and negative correlation coefficients in both types of treatments[22].

[19] All relative rents or relative inequality data in this paper are based on the ratio of group averages (instead of on the group averages of ratios for each trade). Otherwise group level ratios may be biased by individual ratios that can lie considerably below 0 or above 1 because subjects could and did make losses. Nevertheless, the results reported only change marginally if we use group averages of individual ratios.

[20] Mann-Whitney U test, two-sided: $p<0.001$.

[21] Wilcoxon signed ranks test, two-sided: $p<0.001$ in both cases.

[22] Binomial test, two-sided: $p<0.001$.

While investments in above minimum prices or in above minimum quality are profitable on average, the side with strategic power, nevertheless, leaves more for itself. Reciprocity is on average not payoff-equalising, but self-serving, which supports our framework on which the hypotheses in section 3 are built. In the spB-treatments, sellers must provide a higher quality in order to receive a certain price level than sellers are willing to provide in the spS-treatments given this price level. An analogous relation holds for buyers: the price buyers pay in spB-treatments for a certain quality level is lower than the price buyers need to pay in spS-treatments in order to induce sellers to provide, on average, this quality level. Hence, the strategically powerful side profits from being able to leave more for itself; at the same time, it sets, as a group, incentives for the strategically unfavoured side to trust. The result is rent-shifting in favour of the strategically strong side.

Result SP1: *Higher rents for the side with strategic power irrespective of competitive power due to self-serving reciprocity.*

As hypothesised in H2.2, strategic power influences efficiency (as market efficiency is statistically indistinguishable from 100%, trading efficiency is equivalent to total efficiency[23]). If buyers have strategic power, quality choices and therefore efficiency are significantly higher than if sellers have strategic power[24]: quality reaches an average of 5.52 in spS-treatments but an average of 6.85 in spB-treatments; thereby, nearly 70% of maximum gains from trade are realised when buyers have strategic power, while trading efficiency hovers just above 50% when sellers have strategic power.

In order to understand why efficiency is higher when buyers have strategic power, it again helps to first look at the static incentives sellers face. When buyers have strategic power, providing quality is on average highly profitable for sellers. The average degree of reciprocity turns maximum quality the payoff-maximising choice when buyers have strategic power (see Figure 3); therefore, for both money-maximising and reciprocal sellers it pays off to provide above-minimum quality. In the spS-treatments, by contrast, sellers have no static incentive to provide above minimum quality; therefore, only reciprocal sellers would provide above minimum quality in static play. The results seem to reflect the differences in static incentives: there are very few minimum quality choices (5.5% of all quality choices) in the spB-treatments, while the mode is the maximum quality of 10 (23.5% of all quality choices); the

[23] Total efficiency is maximised if all possible gains from trade are realised, i.e. the side with competitive power always finds a transaction partner (market efficiency = 100%) and quality is always set at the maximum of 10 (trading efficiency = 100%). As market efficiency is statistically indifferent from its maximum level of 100% in all treatments total efficiency is statistically the same as trading efficiency.

[24] Mann-Whitney U test, two-sided: p=0.015.

mode in the spS-treatments, by contrast, is minimum quality of 1 (24% of quality choices compared to 21.1% of choices for the maximum quality of 10). Analogous to sellers in the spS-treatments (see Figure 12 in the appendix), also many buyers in the spB-treatments behave non-reciprocally so that sellers earn negative rents (see Figure 13 in the appendix)[25]. In contrast to a seller's behaviour when she has strategic power, non-reciprocal behaviour by a buyer with strategic power has no direct cost for efficiency as buyers´ decisions are only redistributive.

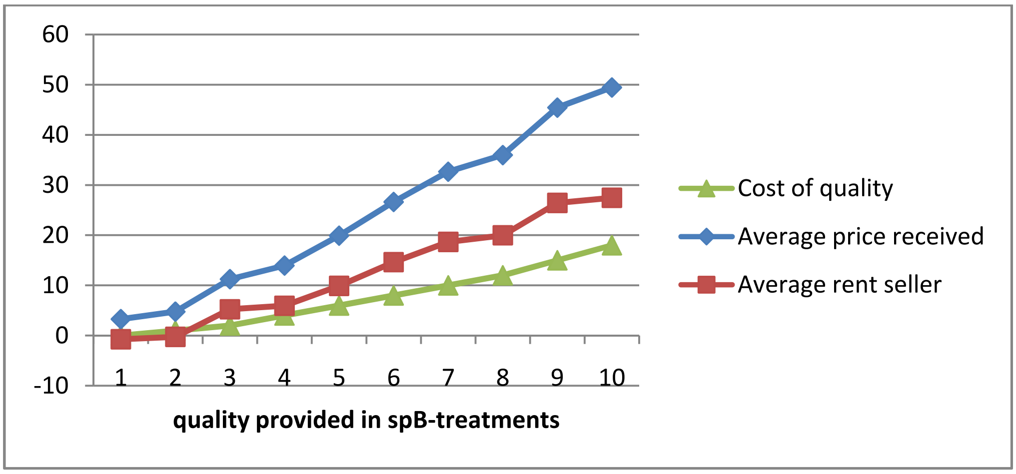

Figure 3: Reciprocity of buyers and profitability for sellers to provide quality in spB-treatments

For any degree of rent-sharing, costs to trust are higher for buyers than for sellers (see derivation of hypothesis H2.2). Looking at the data, we see that sellers reciprocate less than buyers in the sense that they favour themselves more than buyers do for given gains from trade. Relative inequality between sellers and buyers is significantly higher when sellers have strategic power[26]. The (on average) less reciprocal behaviour by the sellers further drives up costs to trust for buyers and down the rate of return on trusting. Figure 4 shows that, over a large range of prices paid (from about 10 to 40 and above 70), the profitability of trusting is rather low for buyers and much lower than the profitability of trusting for sellers, which can be seen in Figure 3. Absolute rents for the strategically unfavoured side are, on average, also lower when sellers have strategic power: sellers in spB-treatments (17.20 on average) earn significantly more than buyers in spS-treatments (10.05 on average) [27]. Given the lower profit expectations and the higher possible losses for trusting buyers in the spB-treatments compared to sellers in the spS-treatments, part of the quality differences can be attributed to sellers, as first movers, being more willing to provide a high level of quality than buyers are, in turn,

[25] Surprisingly, there are even a considerable number of "super-fair" decisions that lead to lower and even negative own rents.
[26] Mann-Whitney U test, two-sided: p=0.020.
[27] Mann-Whitney U test, two-sided: p=0.033.

willing to pay up-front to induce, on average, the same quality level by the sellers. This can also be shown in the data: even if all sellers played as reciprocally as to always equalise payoffs (and thereby reciprocated more than buyers in the spB-treatments), efficiency in the spS-treatments would only just about reach the level as efficiency in the spB-treatments[28]. For any self-serving degree of reciprocity, which we have also observed in the spB-treatments, efficiency in the spS-treatments falls further below the level of the spB-treatments.

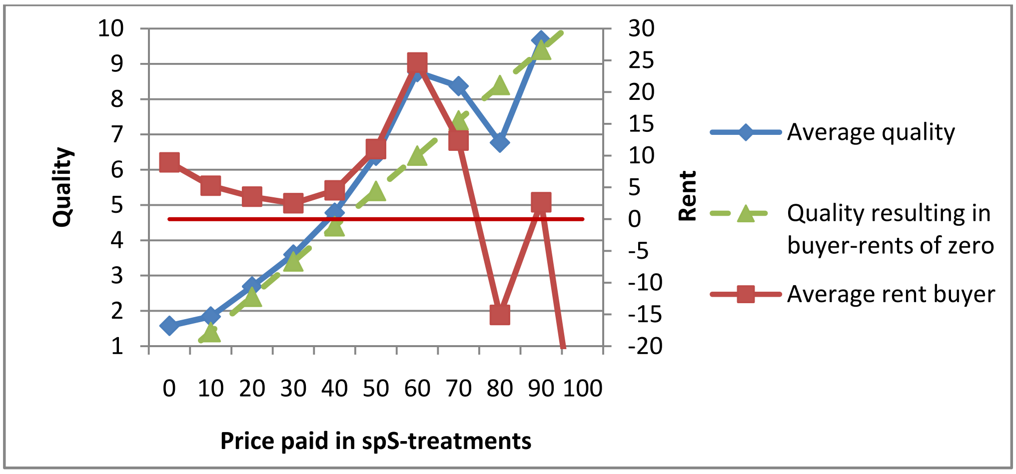

Figure 4: Quality-choices of sellers and implied profitability for buyers dependent on prices paid in spS-treatments

As we argued in section 3, strong dynamic incentives may enter through relational contracting, i.e. repeated trading between the same transaction partners. Those trades within relations, however, only make up 31.74% of all trades in the spS-treatments and even less, 14.34%, in the spB-treatments. The dynamic incentives for most sellers in the spS-treatments and most buyers in the spB-treatments are therefore not strong. The efficiency effects of strategic power in our data are hence best explained by the differences in static incentives.

Result SP2: *Efficiency is higher in the spB-treatments due to strong static incentives therein for sellers to provide high quality.*

4.2 The role of competitive power

Let us also recall that being on the short side of the market gives competitive power because each market participant may each round only conclude a single trade, which, and as we have already seen, generates significant rents on average. Is having competitive power advantageous? The basic competitive power hypothesis H3.2 is supported by the data. Competitive power significantly shifts rent-sharing towards the favoured side. This means

[28] We calculate a hypothetical average quality of 6.7 in the spS-treatments if all sellers always chose the payoff-equalising quality level. This would imply a stronger degree of reciprocity than buyers displayed in the spB-treatments. Even then average quality levels would just about reach the level of the spB-treatments (6.85).

that holding strategic power constant, a side earns more if it has competitive power. Buyers earn significantly more[29] in spS-cpB (13.10 on average) than in spS-cpS (6.98 on average) and they earn more in spB-cpB (34.86 on average) than in spB-cpS (30.85 on average), whereas the opposite is true for sellers (see Figure 6). There is also a tendency towards rent-sharing being shifted towards the favoured side irrespective of strategic power, as illustrated in Figure 5. Competitive power overall, however, fails to reach a significant level[30] since, as we will analyse in more depth later, the rent-sharing consequences of strategic power dominate.

Result CP1: *Holding strategic power constant, competitive power pays off for the favoured side.*

Figure 5 also reveals that efficiency is unaffected by competitive power. Regardless of which side has competitive power, the transacting parties reach about 60% of maximum gains of trade. How does competitive power pay off?

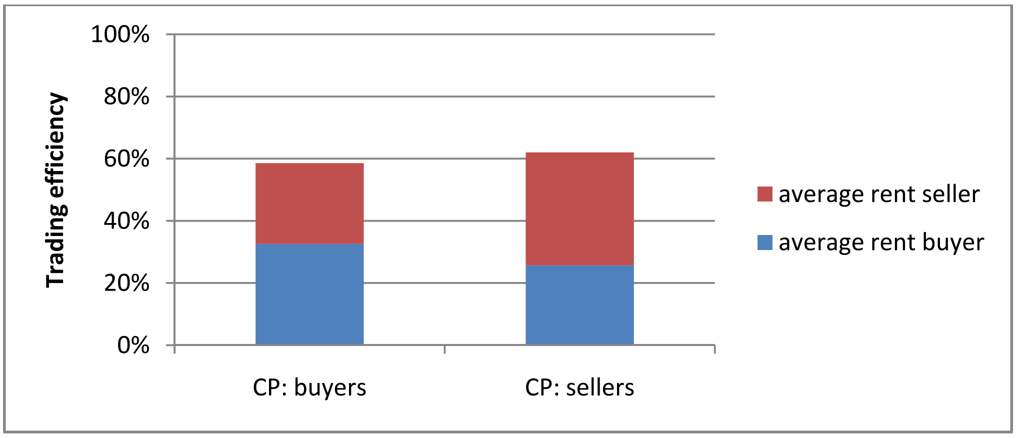

Figure 5: Effects of competitive power for rent-sharing and efficiency

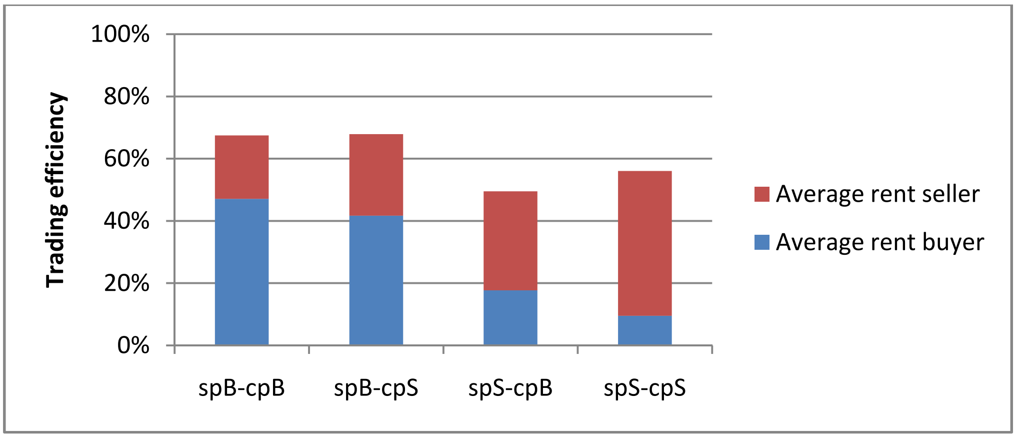

Figure 6: Efficiency and rent-sharing comparisons of all treatments

29 Mann-Whitney U test, two-sided: p=0.041 (spS-cpB vs. spS-cpS); p=0.026 (spB-cpB vs. spB-cpS).

30 Mann-Whitney U test, two-sided: p=0.128 (cpB vs. cpS).

When sellers have competitive power, strong competition among buyers to find a transaction partner leads to significantly[31] higher contracted prices (41.11 on average) compared to when buyers have competitive power (31.63 on average). Hypothesis H3.1 therefore receives support from the data. The different degree of competition among buyers to find a seller is visible in the number of offers buyers make. Each buyer makes, on average, 1.82 offers in spB-cpS and 2.27 in spS-cpS, but only 1.16 offers in spB-cpB and 1.34 in spS-cpB. As in any round two buyers in every group of the cpS-treatment cannot conclude a trade, the number of offers in excess of possible trades is, on average, 116.33 in spB-cpS and 163.17 in spS-cpS, but only 11.83 in spB-cpB and 25.5 in spS-cpB.

Result CP2: *Contracted prices are higher in cpS-treatment than in cpB-treatments.*

In case sellers have strategic power, higher contracted prices directly shift rents from buyers to sellers if quality levels are unchanged; in fact, quality levels are statistically indistinguishable between the two treatments[32], despite higher prices in spS-cpS compared to spS-cpB.

Result CP3: *When contracted prices are binding (spS-treatments), sellers profit from competitive power as higher contracted prices directly translate into higher seller-rents.*

In case buyers have strategic power, contracted prices are not technically linked to actual prices as buyers have full discretionary power to re-set prices; indeed, we do not find significantly higher prices in spB-cpS compared to spB-cpB even though average values are somewhat different (33.79 versus 29.53 respectively)[33]. Nevertheless, competitive power also influences rent-sharing when contracted prices are non-binding. The reason is that buyers are more reciprocal and pay a higher price for a given quality level. We use the ratio of actual prices over quality $R_B = p/q$ to measure buyers' reciprocity, which is weakly significantly higher in spB-cpS than in spB-cpB[34].

Result CP4: *When contracted prices are non-binding (spB-treatments), sellers profit from competitive power by receiving higher prices for a given quality level.*

[31] Mann-Whitney U test, two-sided: cpB vs. cpS: p=0.017.
[32] Mann-Whitney U test, two-sided: p=0.310.
[33] Mann-Whitney U test, two-sided: p=0.310.
[34] Mann-Whitney U test, two-sided: p=0.065 based on group level comparisons of $1/n \sum_{i=1}^{n} (p_i/q_i)$, n being the number of trades in a group.

4.3 The interaction between competitive and strategic power

Result CP3 already hints at an interacting effect between competitive and strategic power, which we now look at in a more general way. We find that for both cases of strategic power, competitive power leads to more reciprocal play by the side with strategic power. In case sellers have strategic power, they provide higher quality for a given price level if they do not have competitive power. Sellers' reciprocity, the ratio of quality over price $R_S = q/p$, is significantly lower in spS-cpS than in spS-cpB[35]. This mirrors result CP3, which was derived for the spB-treatments. The common underlying mechanism is that dynamic incentives change with competitive power. Members of the side without competitive power face strong competition to find a transaction partner. Compared to having competitive power themselves, they have, therefore, a higher incentive to please their current transaction partners in order to increase their chance of being re-matched in the next round[36]. By looking at individual data, we can analyse more deeply how the behaviour of the side with strategic power is affected by competitive power. If the side with strategic power does not have competitive power, more subjects play in a manner that gives their transaction partners at least an equal profit. Let us start with spB-treatments: if buyers have both strategic and competitive power, only 6.7% of buyers reciprocate on average as much as to give their transaction partners rents that are at least high as the buyers' own rents. If, by contrast, sellers have competitive power, 26.2% of buyers reciprocate on average as much as for their transaction partners to receive an at least equal payoff. There are similar effects in case sellers have strategic power on the other end of the behavioural spectrum: 43.3% of sellers play in a way that leads on average to negative rents for their transacting buyers when they have both competitive and strategic power; this percentage drops to 28.6% if sellers only have strategic but not competitive power. Hence, if buyers have strategic power, giving competitive power to sellers induces more buyers to play in a payoff-equalising way, whereas if sellers have strategic power, shifting competitive power to the buyers leads to less sellers acting in a non-reciprocal way. Interestingly, the reciprocity diminishing effects of competitive power happen at different ends of the behavioural spectrum depending on who has strategic power[37].

[35] Mann-Whitney U test, two-sided: p=0.015 based on group level comparisons of $1/n \sum_{i=1}^{n} (q_i/p_i)$, n being the number of trades in a group.

[36] Also recall from section 3 that a player of the strategically favoured side would only pay above-minimum prices or deliver above-minimum quality if he expects this to pay off dynamically.

[37] In case sellers have strategic power and also competitive power, 6.7% of sellers play in a non self-favoured way, whereas this percentage rises to 11.9% if buyers have competitive power. If buyers have strategic power, less than 10% play in a way that leads to negative rents for sellers, regardless of competitive power.

Result SP-CP1: *Competitive power leads to less reciprocity by the side with strategic power.*

The above analysis provides us with the means to explain why efficiency is unaffected by competitive power. In both cases of strategic power, two counter-running effects cancel each other out. For the spB-treatments, endowing buyers also with competitive power leads on the one hand to higher dynamic incentives for sellers to provide above minimum quality in order for them to increase their chance of receiving an offer in the next round. On the other hand, sellers face a lower profitability of their investments in above-minimum quality as buyers, in turn, have a lower dynamic incentive to please sellers if they have competitive power (see result CP4) [38]. In the spS-treatments, sellers act less reciprocally if they are also endowed with competitive power (compare result SP-CP1). At the same time, sellers receive higher prices (see result CP3) to which reciprocal players react with increased quality levels. The resulting net-effect of competitive power on quality is zero in both cases of strategic power.

Result SP-CP2: *Efficiency is unaffected by competitive power as two counter-running effects cancel each other out in both cases of strategic power.*

The analysis of strategic power already confirmed the dominating effect of strategic power for rent-sharing (see result SP1) as the strategically favoured side always, i.e. regardless of competitive power, gains a larger share of rents (compare Figure 6). Hence, in the mixed-power treatments the side with strategic power (sellers in spS-cpB and buyers in spB-cpS) can expect to gain a significantly larger share of rents[39]. Consequently, changes in rent-sharing and profits are larger from spS-treatments to spB-treatments than from cpB-treatments to cpS-treatments. The share of buyer-rents drops rather modestly from 55.42% to 41.25% (or from 23.98 to 19.29 in absolute rents) by shifting competitive power from buyers to sellers. By contrast, the share of buyer-rents decreases considerably, from 65.63% to 25.71% (or from 32.85 to 10.05 in absolute rents), if strategic power shifts from buyers to sellers.

Result SP-CP3: *Strategic power dominates rent-sharing.*

[38] Sellers expect a weakly significantly (Mann-Whitney U test, two-sided: p=0.093) lower degree of reciprocity when buyers have competitive power. This may be interpreted one the one hand as sellers being content with a lower price for a given quality level when they do not have competitive power. On the other hand, it may show that the lower dynamic incentives of buyers to please sellers also entered sellers´ expectations. p-value calculation is based on averages over ratios of expected price over quality.

[39] Wilcoxon signed ranks test, two-sided: p=0.031 in both cases.

Furthermore, as hypothesised in H4.2, relative inequality between buyers and sellers is lower in the two mixed-power treatments spS-cpB and spB-cpS than in the two concentrated-power treatments spS-cpS and spB-cpB, in which one side is favoured twice[40].

Result SP-CP4: *Relative inequality is lower in mixed-power than in concentrated-power treatments.*

The differences in rent-sharing also translate into absolute differences in rents. The following descending order for rents of buyers emerges: spB-cpB > spB-cpS > spS-treatments. While the order between spB-cpB, spB-cpS and either of the spS-treatments is (weakly) significant[41], rents for buyers are only somewhat higher in spS-cpB than in spS-cpS[42]. For sellers, we find the following corresponding order of descending rents: spS-cpS > spS-cpB > spB-treatments[43]. We again observe strategic power to dominate rent-sharing so that both sides are better off in absolute terms if they have strategic power. Hypothesis H4.1, which predicts the favoured side in concentrated-power treatments to be better off, consequently also receives supported by the data. Interestingly, competitive power only affects absolute rents in the interaction with strategic power. Both buyers and sellers earn more when competitive power is added to strategic power; by contrast, when they do not have strategic power, both sides do not earn significantly more if they are given competitive power.

Result SP-CP5: *The side with strategic power is always better off, while competitive power only changes absolute rents when added to strategic power.*

5 The role of relational contracting

5.1 Treatment differences mainly driven by trades outside relations

Looking at rent-sharing and efficiency in different types of trades (compare Figure 7) we see treatment differences to be mainly driven by trades outside relations[44]. There are no treatment-differences in quality levels within relations[45]. Prices within relations are only

[40] Mann-Whitney U test, two-sided: p=0.002 for both concentrated-power treatments vs. both mixed-power treatments.
[41] Mann-Whitney U test, two-sided: p=0.065 (spB-cpB vs. spB-cpS); p=0.002 (spB-cpB vs. either spS-cpB or spS-cpS).
[42] Mann-Whitney U test, two-sided: p=0.132.
[43] Mann-Whitney U test, two-sided: p=0.002 (spS-cpS vs. either spS-cpB, spB-cpS or spB-cpB); p=0.002 (spS-cpS vs. spS-cpB), p=0.053 (spS-cpB vs. cpB-treatments).
[44] Recall from the introduction of our design in section 2 that we consider relational contracting to take place as repeated trading through private offers.
[45] Kruskal Wallis Test, p=0.136 for quality; however, a Mann-Whitney U test reveals a weak two-sided significance (p=0.093) for a higher quality level within relations in spS-cpS than in spS-cpB.

different between spS-cpB and spS-cpS; in this case, prices are higher if sellers have competitive power[46].

Result RC1: *Within relations, quality levels do not differ between treatments, and prices only differ between spS-cpB and spS-cpS.*

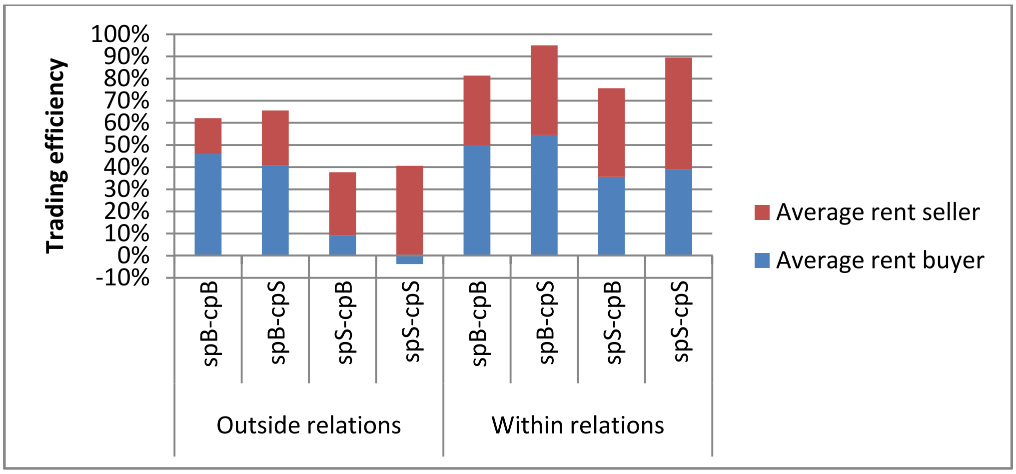

Figure 7: Efficiency and rent-sharing within and outside relations

There are also no treatment differences in relative inequality within relations[47] as rent-sharing is very pronounced within relations. The shares of buyer rents are relatively close to 50% in all treatments (43.43% in spS-cpS, 47.05% in spS-cpB, 57.38% in spB-cpS and 61.24% in spB-cpB). Outside relations, by contrast, relative rents depart considerably more from 50%, as can be seen from Figure 7; consequently, relative inequality is significantly higher outside relations than within relations[48].

Result RC2: *Rent-sharing is more pronounced within than outside relations.*

Figure 7 also impressively shows that within relations transaction partners manage to seize a large part of the available gains from trade. Trading efficiency is above 78% in all treatments, whereas the corresponding trading efficiencies for trades outside relations lie between 37.1% and 65%[49]. Trading efficiency is consequently highly significantly higher within than outside relations, and so are price levels[50].

[46] Mann-Whitney U two-sided: p=0.009.
[47] Kruskal Wallis Test for relative inequality across all four treatments, two-sided: p=0.504.
[48] Wilcoxon signed ranks test for all 21 groups in which private relations emerged, two-sided: p<0.001.
[49] Trading efficiency for trades within and outside relations respectively for each treatment: 89% vs. 62.12% (spB-cpB); 94.5% vs. 65.3% (spB-cpS); 78.2% vs. 37% (spS-cpB); 88.9% vs. 39.6% (spS-cpS) .
[50] Wilcoxon signed ranks test for all 21 groups that entailed relational contracting, two-sided: p<0.001 for both efficiency and prices. Separate tests for every treatment reveals at least weakly significant (two-sided) results on both efficiency and price-levels for all treatments but spB-cpS, in which only 3 groups engaged in relational

Result RC3: *Efficiency and price-levels are higher within relations than outside relations.*

Trades within relations are less dependent on market conditions and contract structure than trades outside relations. The fairness norms inherent in relations appear not to take the trading partners' respective outside options on the spot-market fully into account, which are, as our results show, strongly influenced by the allocation of strategic and competitive power; this also implies that the weak side outside relations gains more from being inside a relation. Despite the large cooperative gains from relational contracting, the strong side outside relations gains little (sellers in spS-treatments) or, interestingly, even nothing (buyers in the spB-treatments[51]). In the concentrated-power treatments, the favoured side gains in terms of absolute rents on average as little as 3.85 (sellers in spS-cpS) and 2.19 (buyers in spB-cpB) from relational contracting.

Result RC4: *The side without strategic power gains more from relational contracting.*

5.2 Are the treatment effects dependent on the subjects´ reliance on relational contracting?

As we already argued in section 4.1, the degree of relational contracting is fairly low. Consequently, sellers face rather weak dynamic incentives so that differences in static incentives drive the higher efficiency when buyers, and not sellers, have strategic power. Looking beyond the results of our own experiment supports this view. Comparing the results of our spS-cpB treatment to previous experiments using an almost identical design, our results apparently contain less relational contracting and at the same time lower efficiency than BFF´s (2004) ICF-treatment but similar levels of relational contracting and efficiency as WR's (2007) IC1-treatment. 45% of all trades in BFF´s (2004) ICF-treatment take place in relationships that lasted more than 5 rounds[52], and roughly 2/3 of trades occur in relationships of any length. In our spS-cpB treatment, less than 12% of trades occur in relationships of more than 5 rounds, and 42.1% of trades take place within any kind of repeated interactions[53]. WR (2007) do not report on length of relations but on the share of private trades. The share of private trades may be considered a rough – albeit very imprecise – proxy for the degree of relational contracting as relations are mostly initiated through private offers. They report on

contracting; nevertheless, the evidence in spB-cpB goes into the same direction as prices and efficiency was also higher within relations wherever relations were formed.

[51] Rents of buyers are not significantly higher within than outside relations.

[52] Reported in BFF (2008).

[53] Figure also includes repeated trading by public offers. We think this corresponds to BFF's (2004) definition of relationships which seems to be wider than ours as there is no explicit reference therein to how relations are initiated. The share of trades based on our narrow definition of relationships, which is restricted to explicitly initiated relations (i.e. through private renewal trades) is 30.3%.

51.4% of private trades, which seems considerable less than the roughly 70%[54] of private trades in BFF-ICF but about the same as the 48.3% of private trades in our spS-cpB treatment. The differences in relational contracting coincide with differences in trading efficiency: BFF's (2004) ICF-treatment boasts an average quality level of 6.9, while WR-IC1 only reaches an average quality of 5.4, which seems roughly the same as the average quality level of 5.2 in our spS-cpB treatment.

The degree of relational contracting in our data may have repercussions on the treatment effects studied. The cross-experimental observation, that BFF´s (2004) data entail both more relational contracting and higher efficiency compared to us and WR (2007), fits our own inter-experimental observation that efficiency is higher within relations. We have also shown that there are no treatment differences in prices and relative inequality within relations. An obvious follow-up research question therefore is how robust the effects from strategic and competitive power are to the degree of relational contracting.

Possibly, the discrepancies between the results of BFF (2004), WR (2007) and us are due to a different underlying tendency in the subject pool towards relying on relational contracting as a contract enforcement mechanism[55]. Subjects may have a different understanding of relational contracting as a contract enforcement device, possibly rooted in different abilities to behave strategically in the experiment or in distinct experiences outside the laboratory. These experiences may vary markedly between different subject pools because of unlikeness in formal institutions (such as the judicial system) and informal institutions (such as culture).

If the differences in the level of relational contracting between BFF (2004) on the one hand and us as well as WR (2007) on the other hand are indeed due to a subject pool effect, we may be able to answer our follow-up research question by replicating the experiment in a subject pool that we expect to be more conducive to relational contracting than our subjects in Erfurt, Germany. For reasons discussed in the following section, we hypothesise to find this subject pool in the People´s Republic of China. The results of the sessions in Chengdu, China, are presented and discussed in section 7. A cross-cultural account of the nature of relational contracting is given in section 8. Section 9 provides a summary, and section 10 concludes the paper with a discussion as well as an outlook for future research.

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[54] Own calculations based on figure 1 in BFF (2004).

[55] There are more differences between us, WR (2007) and BFF (2004) such as larger groups in BFF (2004) with a slightly different asymmetry between buyers and sellers, differences in stakes, instructions and so on. While we are not able to rule these out ex-ante, none seems a good candidate to explain the differences in relational contracting.

6 Cross-cultural hypotheses

Why do we choose China as control country for our experiment? In contrast to Western countries, relational contracting is said to play a paramount role in business interactions in China (Solinger, 1989; Xin and Pearce, 1996; Luo and Chen, 1997; Li, 2002), and McMillan and Naughton (1996) see contracts in China to be less legal than relational; therefore, the first step to enter a successful business in China is to create a reliable and effective net of relations (Yeung and Tung, 1996). The extensive use of informal contract enforcement in China such as self-enforcement through a long-term cooperative solution (see Clark et al., 2006) may be due to malfunctioning formal contract enforcement institutions (Dixit, 2003; Li, 2003) or to a cultural emphasis on relations rooted in Confucianism. In China networks of inter-personal relationship are said to be a widespread socio-cultural phenomenon (King, 1991). The Chinese word *guanxi* is becoming a standard term for understanding inter-personal networks (see for example Arias, 1998; Farh et al., 1998); it is defined by Chen and Chen (2004) as "an implicit psychological contract between people to follow social norms such as maintaining long-term relationship". Based on this view, *guanxi* is not restricted to business interactions only.

According to the 5-dimension culture model of Hofstede (2001), Chinese are comparatively very long-term oriented. Out of all 23 investigated countries, China is ranked the country that pays, according to the 5th dimension "long/short term orientation", most attention to long-term relationship whereas Germany is placed at rank 14. The 5th dimension is related to the choice of focus for people's efforts: the future or the present."[56] This behavioural trait should therefore facilitate relational contracting. There also exists some experimental evidence that points to Chinese reacting stronger to dynamic considerations. In a German-Chinese trust experiment Geng (2008), Chinese subjects show a higher trust level in repeated interactions than in one-shot interactions, while German subjects do not behave differently between one-shot and repeated interactions. The authors conclude that the effect in China is due to a higher appraisal of long-term relations by Chinese subjects compared to their German counterparts.

[56] In the first version of Hofstede's work (1984), the "long/short term orientation" is not a dimension of the model due to the "Western minds of the designers" (Hofstede 2001, p.351) of the study. The Chinese Value Survey (CVS) found this new dimension which is added to the second version of Hofstede's work (2001).

Translating these findings into our market-experiment, we hypothesise that we will see more relational contracting in Chinese sessions than in German sessions[57].

Hypothesis 6: *Subjects will rely more on relational contracting in Chinese sessions than in German sessions.*

As we have already seen from the data of the German session, trades within relations are characterised by a higher trading efficiency than trades outside relations. If Chinese subjects do indeed rely more on relational contracting, as stipulated in H6, we also expect trading efficiency to reach a higher level in the Chinese than in German sessions.

Hypothesis 7: *Trading efficiency is higher in the Chinese sessions than in the German sessions.*

The higher level of trades within relations, as hypothesised in H6, means simultaneously a lower level of trades outside relations. In our German experiment, treatment effects on rent-sharing and trading efficiency are driven by trades outside relations. Consequently, we predict these treatment effects to be smaller in Chinese sessions than in German sessions.

Hypothesis 8: *Treatment effects on trading efficiency and rent-sharing are smaller in the Chinese sessions than in the German sessions.*

7 Results of the Chinese sessions

7.1 Relational contracting

Recall from section 2.3 that we define relational contracting as occurring through privately initiated renewed trades between the same pair of transaction partners plus their initial rounds. We consequently use the share of trades within relations to all trades as a measure for the degree of relational contracting in the Chinese and German sessions. The higher the degree of relational contracting is, the more subjects rely on relationships in order to enforce incomplete contracts.

Figure 8 shows the share of trades within relations in the Chinese and German data. Comparison of group averages reveals that the obvious difference is indeed significant[58]. Our hypothesis H6, that Chinese subjects rely more on relational contracting than the German subjects, therefore receives support from the data. In addition to this, the average duration of a

[57] For reasons of readability, we speak of German and Chinese data and sessions as well as Germany and China as the respective locations of the experiment. This does, however, not imply that our findings are representative for either Germany or China, nor was a cross-cultural comparison of Chinese and Germans our research goal.
[58] Mann-Whitney U test, two-sided: p=0.013.

relation is 4.75 rounds in China, which is significantly longer than the corresponding average duration of 3.74 rounds in Germany[59]. Hence, if a relationship is established, it survives longer in the Chinese sessions than in the German sessions.

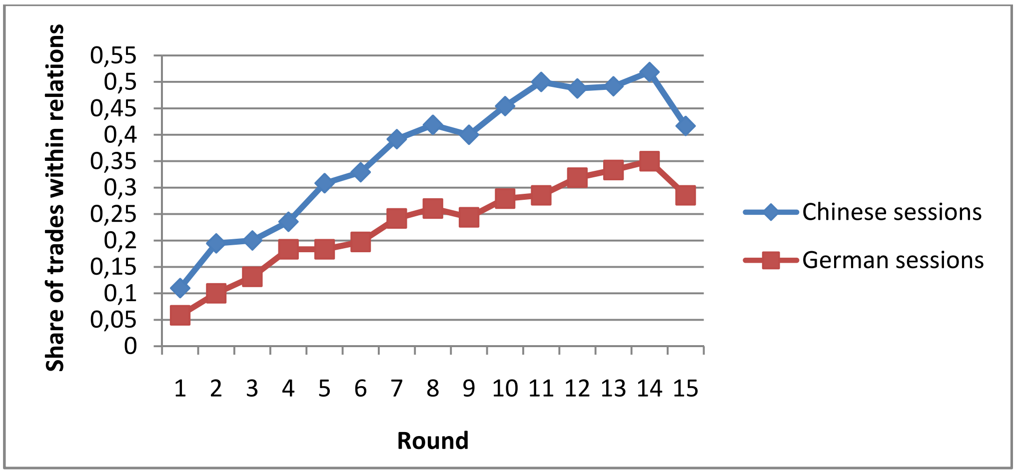


Figure 8: Share of trades within relations in the Chinese and German sessions

7.2 Cooperation and efficiency

As in Germany, the predictions based on money-maximising common knowledge of rationality need to be rejected also for the Chinese data. The descriptive summary data of the Chinese sessions presented in Table 3 shows a large degree of cooperation: both average prices and average quality are significantly above minimum levels. Again, we find in all treatments market efficiency not to be lower than 100%.

We predicted trading efficiency to be higher in China than in Germany if there is more relational contracting in China. On average, Chinese groups reached a quality level of 7.07 which is significantly higher than the corresponding quality of the German sessions (6.18)[60]. Our hypothesis H7 is, therefore, also supported by the data.

Table 4: Descriptive summary statistics of the Chinese treatments

	spS-cpB	spS-cpS	spB-cpB	spB-cpS
Market efficiency	99.33%	99.56%	98.22%	98.67%
Mean price	42.79	51.23	30.75	35.78
Mean quality	6.96	7.11	7.11	7.10
Mean seller rent	27.82	35.96	15.41	20.58
Mean buyer rent	22.83	15.85	36.35	31.16
Share of buyer rents	45.07%	30.59%	70.23%	60.22%
Share of trades within relations	51.06%	43.59%	27.67%	23.14%

[59] Mann-Whitney U test, two-sided: p=0.017.
[60] Mann-Whitney U test, two-sided: p=0.009.

We have seen that relational contracting is at a higher level in China than in Germany; this allows us to answer our second main research question whether the rent-sharing and efficiency consequences of strategic and competitive power are robust to a higher level of relational contracting.

7.3 When relations meet strategic power

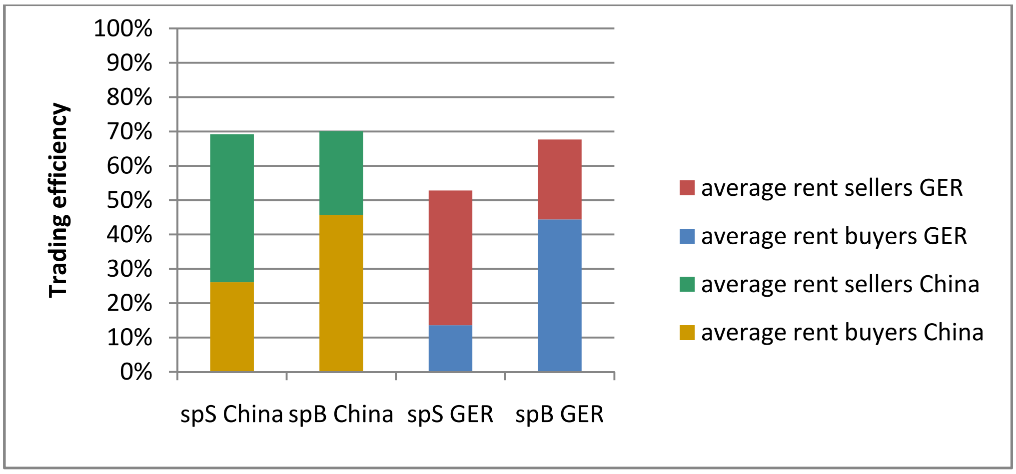


Figure 9: Effects of strategic power on rent-sharing and efficiency in China compared to Germany

The first two columns of Figure 9 present the efficiency and rent-sharing consequences of strategic power in the Chinese sessions, whereas the last two columns re-show the respective results from the German sessions (Figure 2). It becomes obvious that the efficiency effect of changing strategic power no longer exists in the Chinese sessions. The average quality is 7.04 if sellers have strategic power and 7.10 if buyers are strategically favoured, which translates into a trading efficiency of about 70% in both cases[61]. Significance tests do not detect a different quality level when strategic power is switched from one side to the other[62]. Thus, our hypothesis H2.2, that switching strategic power from seller to buyer leads to a higher efficiency level, has to be rejected for the Chinese data. In the spS-treatments, quality levels are significantly higher in China than in Germany (5.52 on average)[63], while quality-levels in the spB-treatments are almost identical in both countries (on average, 6.85 in Germany)[64].

In order to understand why quality-levels are higher in the Chinese spS-treatments than in the German spB-treatments and efficiency thereby unaffected by strategic power in the Chinese sessions (in contract to Germany), let us look at the incentives of sellers to provide above-

[61] Recall that trading efficiency is defined as achieved gains from trade relative to maximum gains from trade. Trading efficiency is about 2.7% (1/37) at minimum quality (q=1) and 100% at maximum quality (q=10).
[62] Mann-Whitney U test, two-sided: p=0.831.
[63] Mann-Whitney U test, two-sided: p=0.001.
[64] Mann-Whitney U test, two-sided: p=0.399.

minimum quality. As we discussed in section 3.2.2, sellers' incentives to provide quality change with strategic power. In a market where buyers have strategic power, sellers are motivated to provide a high quality if they expect a reciprocal behaviour from buyers. By contrast, in markets where sellers have strategic power, quality choices reflect sellers' reciprocity to buyers' price offers. If buyers deliver a high price offer, it can trigger high quality choices by sellers. Reciprocal sellers would reciprocate both within and outside relations, whereas money-maximising sellers would only reciprocate if they can thereby enter or stay within a rent-generating relation.

The reciprocal behaviour by sellers is also shown by the data: sellers' quality choices are strongly correlated with buyers' prices. Within-group Spearman's rank correlation coefficients vary from 0.492 to 0.888 in spS-treatments; the binomial test rejects the null hypothesis of an equal probability of positive and negative correlation coefficients[65]. This finding allows us to find a first explanation why efficiency is higher in the Chinese spS-treatments than in the German spS-treatments[66]. When sellers have strategic power, Chinese buyers choose significantly higher prices than German buyers[67]. As Chinese sellers display, on average, a level of reciprocity at least as high as German sellers[68], higher prices in Chinese spS-treatments imply a higher quality level. Why do Chinese buyers pay higher prices? We connect this question with our original idea about relational contracting. As expected, we find that the degree of relational contracting is significantly higher in China than in Germany when sellers have strategic power (47.37% in China versus 31.73% in Germany)[69]. Re-examining price-differences between German and Chinese data reveals that Chinese buyers in spS-treatments do not pay significantly higher prices within relations (53.22 in China versus 51.39 in Germany) but tend to pay more outside relations (41.05 in China versus 36.1 in Germany)[70]; as prices are higher within relations than outside relations[71], higher average prices in Chinese spS-treatments compared to German spS-treatments are therefore partly driven by a higher level of relational contracting but also to somewhat higher prices outside relations. Outside relations, the somewhat higher prices bring back a significantly higher quality level in China

[65] Binomial test, two-sided: $p<0.001$.
[66] Mann-Whitney U test, two-sided: $p=0.002$.
[67] Mann-Whitney U test, two-sided: $p=0.060$.
[68] Both the correlations between prices and quality (Mann-Whitney U test, two-sided: $p=0.525$) and the level of reciprocity, as measured by q/p, (Mann-Whitney U test, two-sided: $p=0.319$) are indistinguishable between the Chinese and the German spS-treatments, with average level of reciprocity (q/p) being slightly higher in the Chinese than in the German spS-treatments (0.155 in China versus 0.151 in Germany).
[69] Mann-Whitney U test, two-sided: $p=0.007$.
[70] Mann-Whitney U test, two-sided: $p=0.713$ (within relations); $p=0.178$ (outside relations).
[71] Wilcoxon signed ranks test, two-sided: $p<0.001$.

than in Germany (5.59 in China versus 4.20 in Germany)[72]. By contrast, quality-levels within relations are indistinguishable between the Chinese and the German spS-treatments (8.65 in China versus 8.29 in Germany) and, in China as in Germany, higher than outside relations[73]. Therefore, quality is higher in the Chinese spS-treatments partly as a direct consequence of a higher level of relational contracting; it is furthermore a result of a higher quality level outside relations.

While the data do not allow finding a comprehensive explanation for somewhat higher prices and higher quality outside relations in Chinese spS-treatments compared to German spS-treatments, relational contracting may well play a role. Based on our cross-cultural hypotheses, we expect our Chinese subjects to rely more on relational contracting than our German subjects. This may not only show in the *degree* of relational contracting but also in attempts to create relationships. Higher prices by buyers and higher quality choices by sellers may be seen as stronger attempts to form a relation. Alternatively, our Chinese subjects may be more cooperative, for reasons unrelated to the dynamic incentives from relational contracting, which should also be conducive to the establishment of relations (we will come to this question in more depth in section 8.2).

Interestingly, switching strategic power from sellers to buyers seizes to affect efficiency in China; nor are, in fact, quality levels in the Chinese spB-treatments higher than in the German spB-treatments. At first sight, this seems puzzling; the level of relational contracting is still higher in the Chinese sessions than in German sessions when buyers have strategic power (25.40% in China and 14.34% in Germany) – albeit less so than in the spS-treatments and only at a weakly significant level[74]. Hence, on a group-level, the stronger dynamic incentives arising for sellers to perform from a higher level of relational contracting may not simply be added to stronger static incentives when buyers have strategic power. This is also visible in that we find no positive correlation between the share of relations and the quality level in the Chinese spB-treatments. Looking at individual trades, we nevertheless find, as in Germany, trades within relations to be characterised by higher efficiency (9.03 inside relations versus 6.34 outside relations[75]). On a mechanical level, we can therefore say that the level of relational contracting is not sufficiently higher in the Chinese spB-treatments compared to the German spB-treatments to increase efficiency above the level of the corresponding German

[72] Mann-Whitney U test, two-sided: $p=0.024$.
[73] Wilcoxon signed ranks test, two-sided: $p<0.001$.
[74] Mann-Whitney U test, two-sided: $p=0.091$.
[75] Wilcoxon signed ranks test, two-sided: $p<0.001$.

treatments. Behaviourally, however, we still need an explanation why the dynamic and static incentives do not seem to add up; in fact, they seem to have a substitutive relation: on a group-level, the share of trades within relations is significantly and strongly negatively correlated with quality-levels outside relations[76]. Hence, it seems the higher the quality that sellers provided outside relations, the less did buyers rely on relations in order to enforce contracts.

To conclude, the effect of strategic power on efficiency found in our German data as well as by WR (2007) is not robust. If subjects tend to rely more on relational contracting, as in China compared to Germany, efficiency is raised when sellers have strategic power but not when buyers have strategic power. The reason seems to be Chinese buyers relying less on relational contracting if their profit prospects outside relations are good. The potential efficiency benefits from more relational contracting (in China compared to Germany) is thereby lost when static incentives for sellers to perform are already strong.

Result C-SP1: *Efficiency is not affected by strategic power in China. Compared to the German treatments, efficiency is higher in Chinese spS-treatments but not higher in German spB-treatments.*

From Figure 9, we can see that also in China the strategically favoured side gets a larger share of rents, both compared to their trading-partners and to an environment in which they are the strategically unfavoured side. In the spB-treatments, buyers earn a significantly higher share of rents than sellers[77] and than buyers in the spS-treatments[78]. The same holds when sellers have strategic power. Sellers gain a significantly higher share of rents than buyers[79] and compared to their counterparts in the spB-treatments[80]. The finding in Germany that strategic power pays off for strategically favoured side is therefore robust to the degree of relational contracting.

Result C-SP2: *Strategic power pays off for the strategically favoured side also under more relational contracting.*

[76] Spearman's rho: -0.706, p=0.01 (two-sided).
[77] Wilcoxon signed ranks test, two-sided: p<0.001.
[78] Mann-Whitney U test, two-sided: p<0.001.
[79] Wilcoxon signed ranks test, two-sided: p<0.002.
[80] Mann-Whitney U test, two-sided: p<0.001.

7.4 When relations meet competitive power

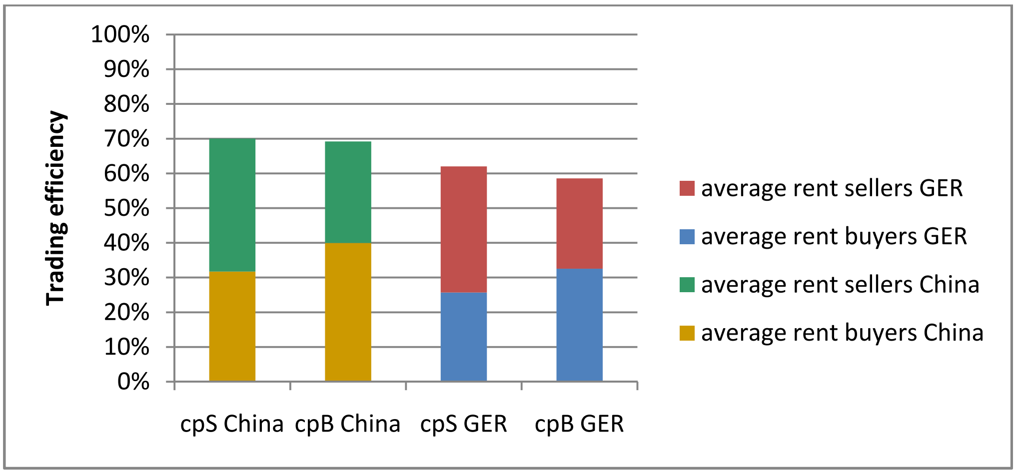


Figure 10: Effects of competitive power for trading efficiency and rent-sharing in China compared to Germany

The first two columns of Figure 10 depict the consequences of competitive power for trading efficiency and rent-sharing in the Chinese sessions, while the last two columns re-present the respective results from the German sessions (compare Figure 5). As in Germany, quality levels and thus trading efficiency is not affected by competitive power. On average, quality is 7.04 when buyers have competitive power and 7.11 when sellers have competitive power; in both cases, about 70% of maximum gains from trade are reached (see Figure 10).

Nevertheless, if we compare the Chinese data with the German data, we find that trading efficiency is higher in China when buyers have competitive power[81]. In the spirit of hypothesis H7, we test whether the higher efficiency level coincides with a higher level of relational contracting. We, indeed, find this to be the case. The share of trades within relations is significantly higher in the Chinese cpB-treatments than in the German cpB-treatments[82]. In the cpS-treatments, however, the level of relational contracting is not different between the two countries.

Result C-CP1: *Trading efficiency is not affected by competitive power in China, but is higher in the Chinese cpB-treatments than in the German cpB-treatments.*

As in Germany, we observe that contracted prices are higher when buyers face competition to find a seller, i.e. when sellers have competitive power[83]. Our hypothesis H3.1 is hence confirmed also in China. Actual trading prices also seem to be higher in the cpS-treatments

[81] Mann-Whitney U test, two-sided: p=0.043.
[82] Mann-Whitney U test, two-sided: p=0.034.
[83] Mann-Whitney U test, two-sided: p=0.014.

than in the cpB-treatments, but fail to reach a significance level[84]. When sellers have strategic power, higher contracted prices directly result in higher actual prices[85] as buyers are bound to pay what they offered. When buyers have strategic power, to the contrary, contracted prices are not binding and buyers can arbitrarily adjust their actual prices after sellers made their quality choices. As the data shows, buyers use this advantage so that actual trading prices are not different between spB-cpS and spB-cpB treatments[86].

Result C-CP2: *Contracted prices are higher in cpS-treatments than in cpB-treatments, but actual prices are only significantly affected when contracted prices are binding.*

From Figure 10, it seems that the competitively favoured side earns relatively more, both in comparison to their trading party and their counterparts that are unfavoured by competitive power. Among these two differences, however, only sellers' profit is significantly higher in cpS treatments than in cpB treatments[87]. Other differences do not reach a significant level overall[88]. If we hold strategic power constant, the advantage of competitive power nevertheless becomes obvious. In treatments where sellers have strategic power, sellers' shares of rents are significantly higher when they have competitive power[89]. The same is true for treatments in which buyers have strategic power, despite prices being non-binding. Buyers earn a weakly significantly higher share of rents in spB-cpB than in spB-cpS[90]. The reason is that, as in Germany, buyers reciprocate weakly significantly more if they do not have competitive power[91].

Result C-CP3: *Holding strategic power constant, competitive power pays off for the favoured side.*

7.5 Relational contracting and the interaction between strategic and competitive power

As stipulated in hypothesis H8, we expect smaller treatment effects given a higher degree of relational contracting. The data provides some support for H8. First, trading efficiency is almost identical across all Chinese treatments, the standard deviation over the treatment averages being as low as 0.07. Neither strategic power nor competitive power consequently

[84] Mann-Whitney U test, two-sided: p=0.114.
[85] Mann-Whitney U test, two-sided: p=0.041.
[86] Mann-Whitney U test, two-sided: p=0.310 .
[87] Mann-Whitney U test, two-sided: p=0.089.
[88] Wilcoxon signed ranks test for sellers' profit vs. buyers' profit, two-sided: p=0.424 (cpS treatments); p=0.151 (cpB treatments). Mann-Whitney U test for buyers' profit, two-sided: p=0.266 (cpS vs. cpB treatments).
[89] Mann-Whitney U test, two-sided: p=0.026.
[90] Mann-Whitney U test, two-sided: p=0.065.
[91] Mann-Whitney U test, two-sided: p=0.093.

has an effect on total gains from trade (see data in table 4), whereas in Germany trading efficiency varies with strategic power. Second, relative inequality is lower in China than in Germany when sellers have strategic power[92] but not different when buyers have strategic power. This is in line with our finding that there is more relational contracting in China than in Germany only when sellers are strategically favoured. Therefore, as we hypothesized in H8, if subjects rely more on relational contracting, as in the Chinese spS-treatments, rent-sharing is more pronounced and, hence, the treatment-effects on rent-sharing are smaller than in Germany. This is also visible in a comparatively smaller change in rents in China when strategic power is shifted between the sides: from spB-treatments to spS-treatments, buyers' share of rents drops from 65.22% to 37.75% (or 14.42 in absolute rents), whilst it drops from 65.63% to 25.71% (or 22.80 in absolute rents) in Germany. The rent-sharing effects of changing competitive power, by contrast, are about the same in both countries: when buyers lose competitive power, their share of rents decreases from 57.79% to 45.40% (or 6.08 in absolute rents) in China and from 55.42% to 41.25% (or 5.06 in absolute rents) in Germany.

As in Germany, we find both powers to affect rent-sharing also when they interact: relative inequality is significantly lower in mixed-power treatments than in concentrated-power treatments[93]. Contrary to Germany, however, we do not find the same degree of dominance of strategic power in our Chinese data. In the mixed-power treatments, the side endowed with strategic power does not necessarily earn significantly more than the side with competitive power. Whereas buyers earn more than sellers[94] also in the Chinese spB-cpS treatment, sellers do not earn significantly more than buyers[95] in the Chinese spS-cpB treatment as relative rents of both sides are very close to 50%. The different impact of strategic power may be due to differences in the level of relational contracting: in the Chinese spS-cpB treatment, relational contracting is used to a high degree (51.2%), whereas in both German mixed treatments (30.2% in spS-cpB and in 8.9% spB-cpS) and in the Chinese spB-cpS treatment (23.1%), relations make up a relatively small part of the trades.

Result: C-RC1: *Strategic power has a larger impact on rent-sharing also when relational contracting is used more. However, the dominance of strategic power found in the German sessions disappears when sellers have strategic power, and a high share of trades take place within relations.*

[92] Mann-Whitney U test, two-sided: p=0.093 for both spS-cpB and spS-cpS treatments.
[93] Mann-Whitney U test, two-sided: p=0.003.
[94] Wilcoxon signed ranks test, two-sided: p=0.031.
[95] Wilcoxon signed ranks test, two-sided: p=0.156.

In all four treatments, Chinese subjects tend to rely more on relational contracting than German subjects (compare data of tables 3 and 4). The tendency seems to be much stronger in the two mixed-power treatments than in the two concentrated-power treatments: the level of relational contracting is significantly higher in the two Chinese mixed-power treatments than in the respective German mixed-power treatments[96], whilst the cross-cultural differences do not reach statistical significance in the two concentrated-power treatments[97]. Apparently, compared to Germany, power being shared stimulates the formation of relational contracting in China (for a further discussion see section 8.2).

Result C-RC2: *Chinese subjects rely more on relational contracting than German subjects when strategic power is given to one market side and competitive power is given to the other market side.*

8 The nature of relational contracting

8.1 The cross-cultural characteristics of relational contracting

As we collected data in two subject pools with two distinct cultures, we can now give a first account of the cross-cultural nature of relational contracting. As in Germany (compare section 5.1), we find the rent-sharing effects of strategic and competitive power to be driven more by trades outside than within relations; relative inequality is again significantly lower within than outside relations[98]. We also confirm for the Chinese sessions that relations are characterised by a higher degree of gift exchange than trades outside relations; average prices and quality-levels are both higher within than outside relations[99]. Taken together, in both countries relations are characterised by high efficiency and low inequality.

Since relative inequality is lower within relations, the gains from relational contracting are unevenly distributed in favour of the weak side outside relations. Rent-sharing inside relations is compared to rent-sharing outside relations therefore shifted towards equality. However, treatment effects on rent-sharing are still visible within relations, more so in the Chinese sessions than in the German sessions. In the Chinese sessions, rent-sharing is always affected in the predicted direction[100], whilst in the German sessions only the effects of strategic power

[96] Mann-Whitney U test, two-sided: p=0.009 (spS-cpB); p=0.041 (spB-cpS).
[97] Mann-Whitney U test, two-sided: p=0.195 (spS-cpS), p=0.461 (spB-cpB).
[98] Wilcoxon signed ranks test, two-sided: p<0.001 for all 24 Chinese groups; this difference is even more visible in the Chinese sessions as we find significantly lower relative inequality within relations even for each separate treatment with only six observations: p=0.031 (spS-cpS, spB-cpB and spB-cpS); p=0.094 (spS-cpB).
[99] Wilcoxon signed ranks test, two-sided: p=0.031 in each treatment.
[100] Mann-Whitney U tests for buyer share of rents in the Chinese sessions, two-sided: p=0.015 (spB-cpB > spS-cpB); p=0.002 (spB-cpS > spS-cpS); p=0.026 (spS-cpB > spS-cpS); p=0.009 (spB-cpB > spB-cpS).

are still statistically significant within relations[101]. In addition, in the concentrated-power treatments spS-cpS and spB-cpB, both in China and Germany, the favoured side earns more than the unfavoured side even within relations[102]. Consequently, equity matters too. In both countries, relations are therefore mostly built on a notion of fairness that entails elements of both equity and equality. This means that relationships are, in both countries, an imperfect buffer to the rent-sharing and efficiency consequences of competitive and strategic power.

Result R1: *Relations are an imperfect buffer to the forces stemming from the market environment and the contract structure.*

Given the similar effects of relations in both countries, how are relations in turn affected by the cultural background of the subjects? In order to answer these questions, we compare average effort, average price, and relative inequality between buyers and sellers within relations in every treatment between China and Germany. Comparing Figure 11 and Figure 7, we see that efficiency and rent-sharing within relations are very similar in the two countries. In fact, only two instances of (weakly) significant differences can be found when comparing price- and quality-levels as well as relative inequality between China and Germany. In spS-cpB quality levels are weakly significantly higher in China whereas relative inequality is significantly higher in the German spB-cpS treatment than in the corresponding Chinese treatment. In the other 10 cases, differences in the characteristics of relational contracting between the countries are statistically indistinguishable. This result implies that relations not only seem to be a buffer against forces stemming from strategic and competitive power; they are also largely unaffected by the cultural background of the subjects.

Interestingly, also trades outside relations are similar across the subject-pools. In the spB-treatments, no differences emerge in trades outside relations between China and Germany, neither in prices, quality nor relative inequality. On a treatment-basis, cross-cultural differences only reach a weakly significant level in the spS-cpS, in which relative inequality is weakly significantly higher in the German session[103]. However, in the merged Chinese spS-treatments, also prices and quality-levels outside relations tend to be higher than in the merged German spS-treatments; as already shown in section 7.3, the differences in quality

[101] A cautionary note: in the German spB-cpS treatment, in only 3 groups private relations were formed. Hence, test statistics for this treatment suffer from limited data; Mann-Whitney U test for buyer share of rents in the German sessions, two-sided: p=0.015 (spB-cpB > spS-cpB); p=0.024 (spB-cpS > spS-cpS); p=0.485 (spS-cpB vs. spS-cpS); p=1.000 (spB-cpB vs. spB-cpS).

[102] Wilcoxon signed ranks test, two sided: p=0.031 (for both German and Chinese spB-cpB and spS-cpS treatments); in the other 4 treatments, no significant differences between seller and buyer rents can be detected within relations.

[103] Mann-Whitney U test, two-sided: p=0.093.

become significant once we compare both spS-treatments between China and Germany. The same applies to relative inequality, which is also significantly higher in the Chinese spS-treatments than in the German spS-treatments.

Result R2: *Relations are very similar across cultures, as are trades outside relations in the spB-treatments; however, some cross-cultural differences exist between trades outside relations in the spS-treatments.*

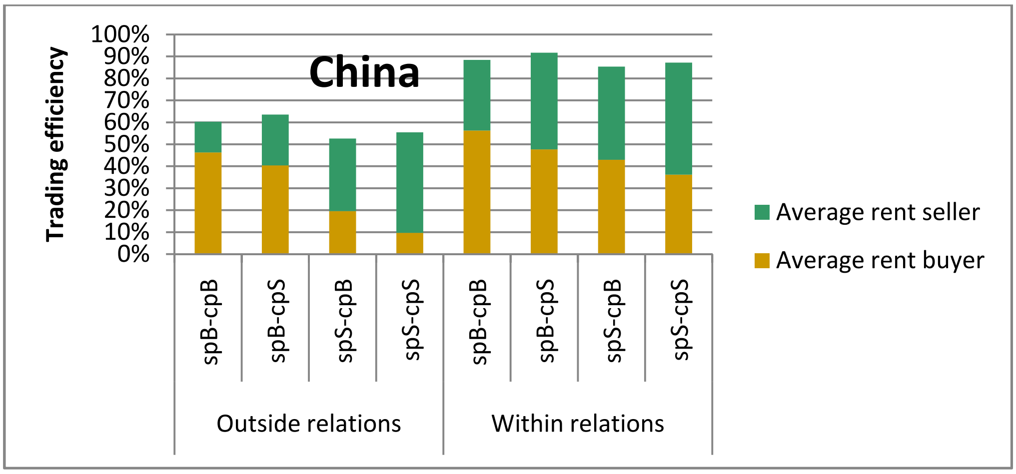


Figure 11: Trading efficiency and rent-sharing within and outside relations in Chinese sessions

8.2 The determinants of relational contracting

8.2.1 Relational contracting – causality or correlation?

As relations arise endogenously in the experiment, a few questions are warranted: first, how can we be assured that differences in relational contracting between the German and the Chinese sessions do indeed provide some explanation for differences in the effects of strategic and competitive power? Secondly, how do we get insight into what drives differences in relational contracting between China and Germany? In this section, we will attempt to find a common answer to both questions that makes us confident that relational contracting in our experiment is not a behavioural black box and indeed provides some insight into cross-cultural differences in the effects of strategic and competitive power.

Imagine our Chinese subjects were more cooperative than our German subjects for reasons that are unrelated to the dynamic incentives within relations, for example rooted in cultural differences. More cooperative behaviour would, within our experiment, result in higher quality and price choices as well as more pronounced rent-sharing. As we have argued, relations are, in turn, based on a high degree of gift exchange and pronounced rent-sharing. Therefore, more relations in China could be a consequence of more cooperative play (or of a greater number of cooperative players who self-select into relations) and not a reason for it.

However, for such an explanation to be plausible, we would need an explanation why our Chinese subjects should be more cooperative and, more importantly, we would expect behavioural differences between Chinese and Germans not to systematically vary with treatment-conditions. If the degree of relational contracting and the degree of gift-exchange were both merely a function of the number of cooperative players in the subject pool, we would also not expect systematic differences in behaviour between the different phases of a relation. Cooperative players should display the same level of cooperation regardless of whether they are in the initial round of a relation or in later phases. In the following paragraphs, we will test the plausibility of a higher level of relational contracting in the Chinese sessions being merely a consequence of more cooperative behaviour by the Chinese subjects.

8.2.2 When power meets relations

In fact, as we have already shown, the level of relational contracting reacts to treatment conditions in both countries. We find in both countries relational contracting to be used to a higher degree when sellers have strategic power compared to when buyers have strategic power (see data in Table 3 and Table 4)[104]. This gives a robust support to a similar finding of WR (2007), who also find less relational contracting in their corresponding spB-cpB treatment compared to their corresponding spS-cpB treatment. In contrast to WR (2007), we measure this effect by defining relations as happening through private renewal trades rather than looking at the possibly noisy proxy private trades; we also find this effect to be robust to the allocation of competitive power and to distinctly varying the cultural background of the subjects. Competitive power only has a very limited and cross-culturally non-robust effect on the level of relational contracting: albeit starting at a very low level, the level of relational contracting is weakly significantly higher in the German spB-cpB than in the German spB-cpS treatment[105] (see Table 3); in the Chinese sessions, competitive power does not affect the level of relational contracting (see Table 4).

[104] Mann-Whitney U test, two-sided: p=0.001 (spS vs. SpB) for both countries. For comparison between individual treatments: p=0.041 (spS-cpB vs. spB-cpB in Chinese sessions); p=0.026 (spS-cpS vs. spB-cpS in Chinese sessions); in Germany, we find a significant effect of changing strategic power when sellers have competitive power (p=0.011, spS-cpS vs. spB-cpS) but, despite a pronounced treatment effect, only marginally significantly more relations if strategic power is switched from buyers to sellers when buyers have competition power (p=0.119, spS-cpB vs. spB-cpB).

[105] Mann-Whitney U test, two-sided: p=0.063.

Result R3: *In both countries, the level of relational contracting is higher when sellers have strategic power; by contrast, competitive power only has a limited effect on the level of relational contracting that is only found in the German sessions.*

When buyers have strategic power, they hold the key both to initiate relations, by making *private renewal offers*, and to maintain a relation, by behaving reciprocally. This situation is apparently less conducive for relational contracting to emerge[106] compared to sellers having to maintain a relation. It seems that buyers' incentives to form relations drive the level of relational contracting in both countries.

When sellers have strategic power, buyers are dependent on reciprocating sellers in order to earn a positive rent. By relying on relational contracting, buyers may be able to improve their position in two ways: first, they can potentially select more reciprocal sellers by re-offering them a contract. Second, they can set incentives to money-maximising sellers to provide above-minimum quality by re-offering performing sellers a rent-generating contract. By contrast, when buyers have strategic power, they do not have to fear opportunistic behaviour of sellers as they can always rent-sharing in a way as to earn a positive rent; in fact, sellers have to fear opportunistic behaviour by buyers. As we have shown, buyers as a group set strong incentives for sellers to deliver high quality even outside relations – what sellers duly do. The opposite holds for sellers: sellers would minimise their risks and increase their profit prospects by entering relations when buyers have strategic power; relations are of little use when they themselves have strategic power, as buyers also pay high prices even outside relations. Therefore, the strategic value of relational contracting varies with strategic power. This is also shown by the data: when sellers have strategic power, buyers can considerably improve their profit-prospects by engaging in relational contracting; the average surplus in buyer-rents from trades within relations to trades outside relations is 19.44 (spS-cpB) or 31.80 (spS-cpS) in Germany[107] and 17.55 (spS-cpB) or 19.61 (spS-cpS) in China[108]. By contrast, when they themselves have strategic power, buyers can only expect to make an additional rent

[106] WR (2007) explain less relational contracting in their corresponding spB-cpB treatment than in their corresponding spS-cpB treatment by arguing that buyers care less about the identity of the sellers when they have the last move. WR (2007) generalise this finding by linking the prevalence of relational contracting to the degree of discretionary latitude of the contract structure. We argue that this claim is premature as they did not vary the side that initiates relations. Based on our results, we rather think that the incentives to form relations, which vary with strategic power, from the perspective of the initiators of relations, which happen to be the buyers, are crucial for the emergence of relations.

[107] Wilcoxon signed ranks test, two-sided: p=0.063 (spS-cpB); p=0.031 (spS-cpS).

[108] Wilcoxon signed ranks test, two-sided: p=0.031 (spS-cpB); p=0.031 (spS-cpS).

of 2.74 (spB-cpB) or 9.70 (spB-cpS) in the German sessions[109] and 7.39 (spB-cpB) or 5.39 (spB-cpS) in the Chinese sessions[110] by entering relations (see also Figure 7 and Figure 11). Sellers, in turn, profit somewhat more from relations when buyers have strategic power compared to they themselves having strategic power; this difference, however, is much less pronounced than for buyers (see also Figure 7 and Figure 11). Sellers gain, on average, an additional rent of 11.49 (spB-spB) or 10.11 (spB-spS) in Germany[111] and 13.46 (spB-cpB) or 15.49 (spB-cpS) in China[112] from trades within relations when buyers have strategic power, while they gain, on average, 8.65 (spS-cpB) or 4.93 (spS-cpS) in Germany[113] and 6.81 (spS-cpB) or 3.87 (spS-cpS) in China[114].

The strategic value of relations also varies with competitive power: as the data has just shown, the profitability of concluding trades within relations only changes marginally with competitive power; yet, individual members of the side without competitive power can considerably increase their earnings by engaging in relational contracting. Thereby, they can conclude more, profitable trades than what they can expect by competing on the spot market to find a transaction partner (recall that in any round two members of the side without competitive power have to content themselves with the outside option). Hence, *being* in relationships is also more attractive individually when the other side is endowed with competitive power. However, this may not necessarily hold for trying to *form* relations. Buyers, for instance, may be better off by concluding trades with changing partners rather than by trying, possibly in vain, to re-trade with the seller of the previous round; in line with this, buyers make fewer *private renewal offers* when sellers have competitive power compared to when they have competitive power themselves[115]. Consequently, neither buyer nor seller incentives that stem from the allocation of competitive power seem to explain changes in the level of relational contracting. To conclude, the buyer's incentives to form relations, rather than to be in relations, seems to drive the level of relational contracting. Buyer-incentives may play a stronger role than incentives of sellers as buyers are the initiators of relations. While seller-incentives may still matter in explaining the absolute level of

[109] Two-side Wilcoxon signed ranks tests do not detect a significance for either spB-cpB (p=0.438) or spB-cpS (p=0.250). For spB-cpS, it may due to too low observations since only three groups applied relational contracting after all.
[110] Wilcoxon signed ranks test, two-sided: p=0.063 (spB-cpB); p=0.031 (spB-cpS).
[111] Wilcoxon signed ranks test, two-sided: p=0.094 (spB-cpB); p=0.250 (spB-cpS). The insignificance in spB-cpS may be due to too low observations.
[112] Wilcoxon signed ranks test, two-sided: p=0.031 for both spB-cpB and spB-cpS.
[113] Wilcoxon signed ranks test, two-sided: p=0.031 (spS-cpB); p=0.094 (spS-cpS).
[114] Wilcoxon signed ranks test, two-sided: p=0.0031 for both spS-cpB and spS-cpS.
[115] Mann-Whitney U test, two-sided: p=0.020 (Chinese data); p=0.014 (German data).

relational contracting, as relations can only be formed if sellers accept private renewal offers, the incentives of sellers do not provide much ground for explaining why the level of relational contracting changes with the treatment conditions.

Result R4: *In both countries, the strategic value of forming relations, as seen from the buyers' perspective, seems to drive the level of relational contracting.*

8.2.3 When culture meets relations

The strong reaction of the level of relational contracting to buyers' incentives is not the only piece of evidence that speaks against a higher level of relational contracting being merely driven by cross-cultural differences in cooperativeness; behavioural differences between Chinese and Germans also vary over the four treatments studied. Rent-sharing is not more pronounced in our Chinese sessions when buyers have strategic power, but when sellers have strategic power. This coincides with a high level of relational contracting in both spS-treatments in China, whereas in all other treatments relations make up a relatively small part of the trades. Although there is a tendency towards more relational contracting in all Chinese treatments compared to the German counterparts, differences in relational contracting between China and Germany are considerably unequal between the treatments: we find significantly more relations in China in mixed-power treatments but not in concentrated-power treatments (see result C-RC2); among the concentrated-power treatments, there are a still somewhat more relations when sellers have strategic power (in spS-cpS) but no difference at all when buyers have strategic power (in spB-cpB). As we randomly allocated subjects to the different treatments, subject-differences alone, that is independent from treatment conditions, cannot plausibly explain why we find, compared to the corresponding German treatments, more relational contracting in the mixed-power treatments but not in the concentrated-power treatments; they can also not explain why relative inequality is lower in China than in Germany when sellers have strategic power but not when buyers have strategic power.

We also observe systematic behavioural differences between different phases of a relation: when sellers have strategic power, they provide significantly higher quality in the middle rounds of a relation than in the initial round and again provide less quality in the last round of a relation[116]; when buyers have strategic power, the same pattern holds for prices – albeit

[116] Wilcoxon singed ranks test for middle rounds vs. first round quality-level, two-sided: p=0.004 (German data); p<0.001 (Chinese data). Wilcoson signed ranks test for last round vs. middle rounds quality-level, two-sided: p=0.002 (German data); p<0.001 (Chinese data).

more clearly in the Chinese sessions, whereas in the German sessions only the drop in prices from the middle rounds to the last round of a relation can be observed[117].

The evidence, hence, strongly speaks against some unobserved differences in cooperativeness that may explain both different levels of relational contracting and gift-exchange between the Chinese and the German sessions. Therefore, we rather need an explanation for the variations in the level of relational contracting that is related to the interaction between the treatment conditions and the distinct backgrounds of the subjects in our German and Chinese sessions.

Based on our cross-cultural motivation (see section 6), we expected Chinese subjects do have more experience with relations as an instrument to enforce incomplete contracts; thereby, they may also have more experience in which contexts relational contracting is most beneficial for them. This may give us a clue in explaining the cross-cultural differences in the level of relational contracting. Taking result R3 as a starting point, we compare the strategic value of relations from the perspective of buyers in all four treatments in both countries with the level of relational contracting. In spB-cpB, neither German nor Chinese buyers have a strong reason to form relations as profitability of relations is low in both countries (difference in rents between trades within and outside relations is 2.74 in Germany and 7.39 in China); we consequently find the same low level of relational contracting in both countries. In spS-cpS, relations are profitable in both countries but yet significantly more so in Germany (rent-differences are 31.80 in Germany versus 19.61 in China); nevertheless, we find still somewhat more relations in China than in Germany. In spS-cpB, incentives to form relations seem to equally strong in both countries (rent-differences are 19.44 in Germany and 17.55 in China), but we find Chinese to rely significantly more on relations. In spB-cpS, incentives to be in relations are high in both countries (while rent-differences are only 9.70 in Germany and 5.39 in China, recall that two buyers will every round be excluded from concluding trades)[118]; yet, we find significantly more relations in China than in Germany. At the same time, forming relations may be more difficult in spB-cpS than in spB-cpB; as argued above, buyers may rather want to conclude any kind of trade than leaving the trading phase empty-handed because they have unsuccessfully tried to establish a relation. This may explain why there are not more relations in the spB-cpS treatments than in the spB-cpB treatments; in fact, there are

[117] Wilcoxon singed ranks test for middle rounds vs. first round price-level, two-sided: p=0.219 (German data); p=0.004 (Chinese data). Wilcoxon singed ranks test for last round vs. middle rounds price-level, two-sided: p=0.008 (German data); p=0.001 (Chinese data).

[118] Besides, also incentives for sellers to enter relations are relatively strong in both countries; differences in seller-rents between trades inside and outside relations are 10.12 in Germany and 15.49 in China.

significantly less relations in the German spB-cpS than in the German spB-cpB treatment, while there are about the same level of relations in the two corresponding Chinese treatments.

Of course, our data are too limited to give a full account on the cross-cultural determinants of relational contracting; nevertheless, comparing cross-cultural differences in the level of relational contracting with differences in profitability of relations suggests that Chinese buyers, possibly based on their experiences outside the laboratory, react more strongly to the strategic value of relations than German buyers.

Result R5: *Cross-cultural differences in the level of relational contracting may be explained by Chinese buyers reacting more strongly to the strategic value of relations than their German counterparts.*

Consequently, the higher is the strategic value of relational contracting for the side who initiates relations, the more relations we expect to be formed. If the strategic value is low, even a subject pool with a presumed good understanding of how to use relations to overcome cooperation problems employs it to a low degree. Consequently, the main cross-cultural differences between China and Germany emerge where relational contracting carries a high strategic value. In terms of average achievable gains from trade, the data seem to imply a trade-off between different sources of incentives for sellers. The same level of efficiency is achieved either when buyers have strategic power or when sellers have strategic power and subjects rely strongly on relational contracting, as in the respective Chinese sessions. When buyers have strategic power, sellers face strong static incentives to perform, whereas a high degree of relational contracting creates strong dynamic incentives for sellers to deliver high quality.

Result R6: *The dependence of relational contracting on its strategic value seems to imply a ceiling for achievable gains from trade: high efficiency may either be achieved by sellers having strong static incentives, when buyers have strategic power, or by strong dynamic incentives, arising from a high degree of relational contracting.*

9 Summary

In this paper, we analyse two sources of incentives to cooperate in repeated incomplete contracts environments: strategic and competitive power. While strategic power stems from the internal structure of contracts, competitive power results from the competitive market condition in which the contract is embedded. As dynamic incentives are critical for the relative strength of competitive and strategic power when contracts are incomplete, we also investigate the role of relational contracting for the identified effects. The experiments were therefore run in two culturally distinct places that we expected to display different degrees of relational contracting: Erfurt in Germany and Chengdu in China.

We find competitive and strategic power to have a robust effect on rent-sharing across distinct cultures and different degrees of relational contracting. Competitive power influences rent-sharing even if contract enforcement is entirely absent. If the strategically favoured side is not endowed with competitive power, it displays a more reciprocal behaviour. This can be explained by the incentive the side without competitive power has to enter a rent-generating relation by presenting themselves as a reciprocal transaction partner. The side without competitive power can earn rents within a relation by concluding more trades than on the public spot-market. On average, trades generate significant rents for both transaction partners. Interestingly, competitive power reinforces reciprocity even when the market is predominantly characterised by spot-market trading rather than the bilateral trading islands found by BFF (2004). Competitive power does not affect efficiency.

Strategic power has a larger impact on rent-sharing than competitive power. In Germany, strategic power even dominates competitive power in such a way that the strategically favoured side always gains a larger share of rents regardless of competitive power. In China, by contrast, strategic power only dominates competitive power when buyers have strategic power. Strategic power influences efficiency in case subjects rely weakly on relational contracting, as they do in our German sessions. In this case, efficiency is higher when buyers have strategic power as the structure of static incentives for sellers to provide quality drives efficiency. When buyers have strategic power, sellers maximise profits if they provide maximum quality because of buyers´ reciprocal behaviour. By contrast, when sellers have strategic power, providing above-minimum quality may only pay off dynamically, i.e. within a rent-generating relation. As relational contracting is used to a low degree in our German-sessions, efficiency suffers when sellers have strategic power.

This result is not replicated in China: efficiency is unaffected by strategic power in the Chinese sessions. In our Chinese sessions, subjects employ relational contracting more than in our German sessions. This difference is most pronounced when sellers have strategic power; in this case, efficiency is higher in China. When sellers have strategic power, efficiency is higher in China than in Germany because of the stronger reliance of subjects on relational contracting and because of somewhat higher quality levels outside relations. By contrast, efficiency levels are undistinguishable between the Chinese and the German sessions when buyers have strategic power; in this case, differences in relational contracting between our Chinese sessions and our German sessions are less pronounced. More importantly, the higher the quality sellers provide outside relations, the less buyers rely on relations when they have strategic power. As a result, when buyers have strategic power the stronger dynamic incentives in the Chinese sessions do not simply add up to the strong static incentives that we also find in the German sessions. As in Germany, competitive power has no bearing on efficiency in China.

Behaviour within relations, and when buyers have strategic power also outside relations, is astonishingly similar between China and Germany. Relational contracting is based on a high degree of trust of the side without strategic power and a high degree of reciprocity by the side with strategic power. Consequently, rent-sharing is more pronounced and efficiency higher within than outside relations. Efficiency-levels within relations are almost undistinguishable across treatments and cultures. Nevertheless, power still matters for rent-sharing also within relations. We therefore conclude that the culturally robust rent-sharing norms inherent in relations are based on a notion of fairness that includes elements of both equality and equity. Relations are therefore a buffer against the forces of competitive and strategic power that works imperfectly in terms of rent-sharing but almost perfectly in terms of efficiency.

In both countries, relational contracting is used more when sellers have strategic power, while competitive power does not influence the level of relational contracting. It therefore seems that the strategic value of forming relations, as seen from the perspective of the buyers, who are also the initiators of relations, drive the level of relational contracting. In order to explain cross-cultural differences in the level of relational contracting, it seems that the Chinese buyers react more strongly to the strategic value of relations. We conclude that the prevalence of relational contracting depends both on the strategic value of forming relations, which varies mainly with the contractual structure, as well as on the, possibly culturally influenced, strategic understanding of relational contracting as a contract enforcement device.

10 Discussion and outlook

We now want to highlight three directions to further think about the implications of the experimental result. First, we will assume the perspective of economic actors rather than of economic transactions by hypothetically asking whether players would rather be equipped with competitive or strategic power if they have to choose. Second, we want to ask how robust the experimental results are to the parameterisation and the specific form of strategic and competitive power. Third, we want to indicate possible future research on the nature of relational contracting.

First: We analyse incentives to cooperate when contracts are incomplete; consequently, this paper focuses, in terms of rent-sharing, on individual transactions rather than between economic actors. We may nevertheless gain further interesting insight into the effects of competitive power and strategic power by switching the perspective. By implementing excess demand or excess supply, players without competitive power could only make 5/7 of the number of trades that players with competitive power could conclude. In order to put ourselves in the perspective of economic actors, we first compare total rents of both sides in the mixed-power treatments, i.e. when competitive and strategic power are allocated to different sides. Interestingly, total rents of buyers and sellers are statistically indistinguishable in both mixed-power treatments in both countries[119]. The higher rents of the side with strategic power in each completed trade is on average eaten up by being restricted to less trades than the side with competitive power. If roles were fixed but players could choose to have either competitive or strategic power, in the German sessions both buyers and sellers would be better off if buyers had strategic and sellers competitive power[120]. In terms of expected rents, switching powers when sellers have strategic power and buyers have competitive power would therefore be a Pareto-improvement, which is driven by the higher gains from trade when buyers have strategic power. In China, total rents for both buyers and sellers are statistically indistinguishable between the two mixed-power treatments. Hence, looking at total rents, strategic power is no longer an advantage when it has to be traded for competitive power. It is left for future research to find out at which point competitive power even turns into an advantage when excess supply or excess demand are even more pronounced.

Second: As we used a specific implementation of strategic and competitive power, we may ask how generic the results of rent-sharing *within* each trade are; more precisely: did we give strategic power a better chance than competitive power to affect rent-sharing given that it implied full, and thereby an extreme form of, discretionary power? It, certainly, seems worth investigating whether a greater asymmetry of the market sides or a less pronounced form of strategic power may lead to a stronger role of competitive power. On the other hand, one may

[119] Recall that within each trade the side with strategic power earns a higher rent in all mixed-power treatments apart from the Chinese treatment in which sellers have strategic and buyers competitive power. Therein in which the difference in earnings is not statistically significant.

[120] Mann-Whitney U test, two-sided: p=0.065 (total buyer rents); p=0.093 (total seller rents).

argue that we already implemented an artificially strong form of competitive power in that either demand was strictly greater than supply or vice-versa. Excess demand and excess supply are extreme forms of market imperfections; in the field, we would only expect to observe them in the very short run. We therefore claim that we implemented extreme forms of both strategic and of competitive power. Future research may attempt to investigate forms of strategic and competitive power that are closer to what we may also find more frequently in the field.

Third: Still very little is known about the conditions under which relational contracting flourishes and about the role subjects' experience and culture play therein. Exogenous variations in relational contracting may shed further light on a causal relationship between the level of relational contracting and the relative effects of strategic and competitive power. Further cross-cultural evidence is also warranted in order to investigate whether subjects' understanding of the strategic value of relations indeed varies with culture or the institutional environment subjects are accustomed to outside the laboratory. We found relations to be an imperfect buffer against the forces stemming from competitive and strategic power; it is left for future research to find out whether relations, because of inherent fairness-concerns, may also buffer against *changes* in market conditions and contract structure.

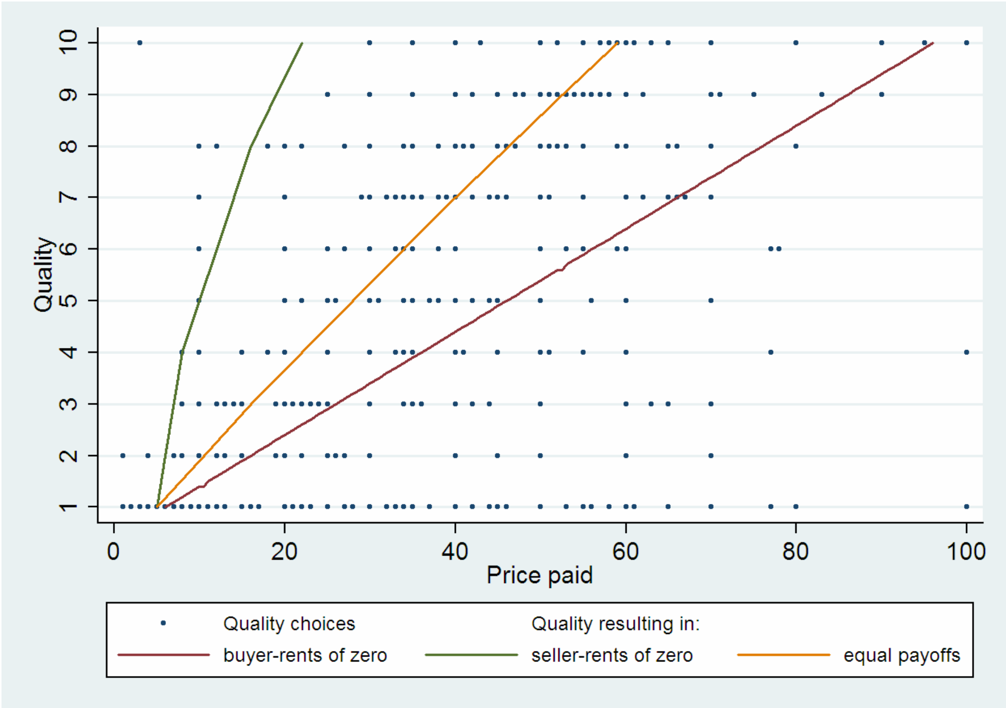


Figure 12: Quality-price schedule in German spS-treatments

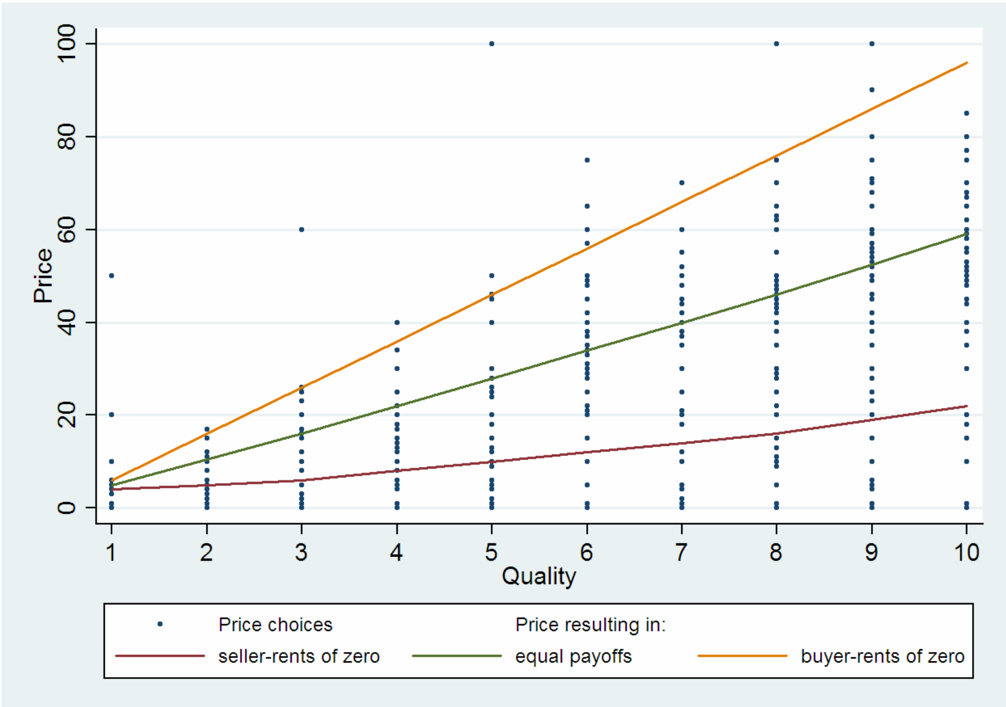


Figure 13: Price-quality schedule in German spB-treatments

Appendix: Experimental instruction

[The instruction provided here is the English translation of the original German and Chinese instructions of treatment spS-cpB. The instructions of treatments spB-cpS, spS-cpB, spS-cpS are similar, except for the following parts:

- *Treatment spB-cpS:*
  - *A team consists of 5 sellers and 7 buyers.*
  - *The trading phase will end when all sellers have made a deal or the trading time has elapsed.*
- *Treatment spB-cpB:*
  - *Each round is composed of two phases: the first phase is the trading phase; in the second phase, sellers decide on the quality of the goods.*
- *Treatment spS-cpS:*
  - *Each round is composed of two phases: the first phase is the trading phase; in the second phase, sellers decide on the quality of the goods*
  - *A team is consists of 5 sellers and 7 buyers.*
  - *The trading phase will end when: all sellers have made a deal or the trading time has elapsed.*

The original Chinese and German instructions are available from the authors upon request.]

Experimental instruction

General instruction

In today's experiment, you will interact with your team members. A team consists of **7 sellers** and **5 buyers**. You earn points by selling goods as a seller or buying goods as a buyer. The value of the good to a buyer and the cost of the good to a seller depend on the good´s quality. At the beginning of the game, you will be randomly assigned a role either as a buyer or a seller. Your role will not be changed during the game. All participants will also be given a random ID number, which stays the same during the entire experiment. You will be notified about your number at the beginning of the experiment. There are a total number of 15 rounds in the experiment.

In each round, you can earn points. At the end of the experiment, we will exchange the total number of points you earned into RMB/Euro and pay you off. At the beginning of the experiment, **each participant receives 100 points as his endowment**. All information about payoffs in this instruction is expressed in points. The exchange rate is: **8 points = 1 RMB / 35 points = 1 Euro.**

Please note:

Please do not communicate with other participants in any form throughout the experiment. Please keep your mobile phones switched off. In case you have any questions please raise your hand from behind the curtain. All decisions are made anonymously. That is to say no participants know which decisions during the experiment have been taken by which other participants. Payments will be made anonymously and immediately after the experiment.

Procedure:

Each round is composed of the following phases:

1. Trading between sellers and buyers

2. Sellers decide quality of the goods
3. Buyers decide actually price of the goods

Display of payoffs of the current round

Phase 1: Trading

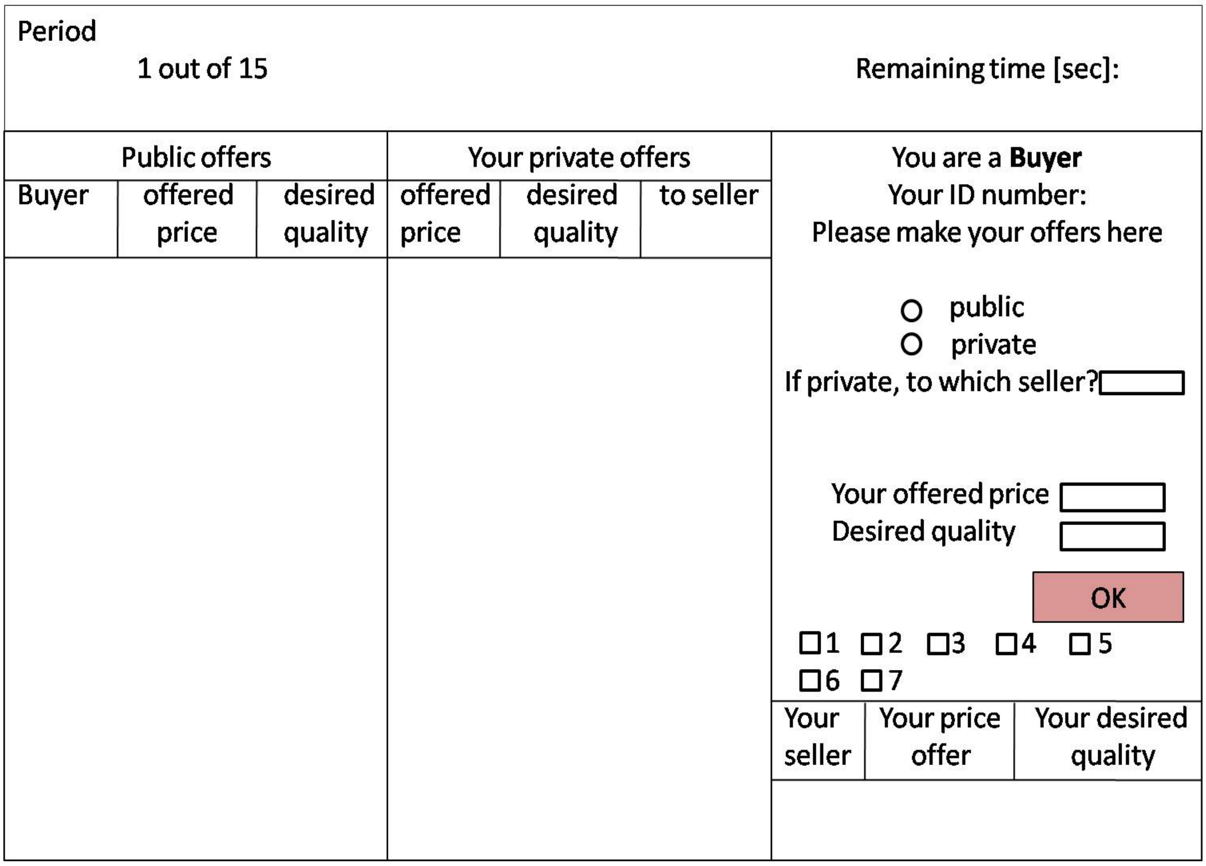
During the trading phase buyers can make offers to sellers.

When making an offer a buyer must deliver the following information:

1. **Price offer (between 0 and 100)**
2. **Desired quality of the good (between 1 and 10)**
3. **Receiver of the offer**

   Buyers can make either a **private** or a **public** offer. A private offer will only be sent to one particular seller and can only be viewed and accepted by this seller. In order to make a private offer a buyer must first select “private” and type in the ID number (1, 2, …, 7) of the desired seller in the region underneath. A public offer can be viewed by all sellers and can be accepted by any one seller. To make a public offer a buyer must first select “public”.[121]

A buyer can make as many offers as he wants until one of the offers is accepted by a seller. The check box in the lower-right corner of the screen shows to buyers which seller has already accepted an offer. In addition, all buyers and sellers see on screen which trade they made in the current round. **Each buyer and seller can make only one trade in each round.**



[121] Each buyer also sees the public offers from other buyers in the left frame.

Period	
1 out of 15	Remaining time [sec]:

You are a **Seller**
Your ID number:

Private offers to you			Public offers		
From buyer	offered price	desired quality	From buyer	offered price	desired quality
		accept			accept

Your buyer	offered price	desired quality

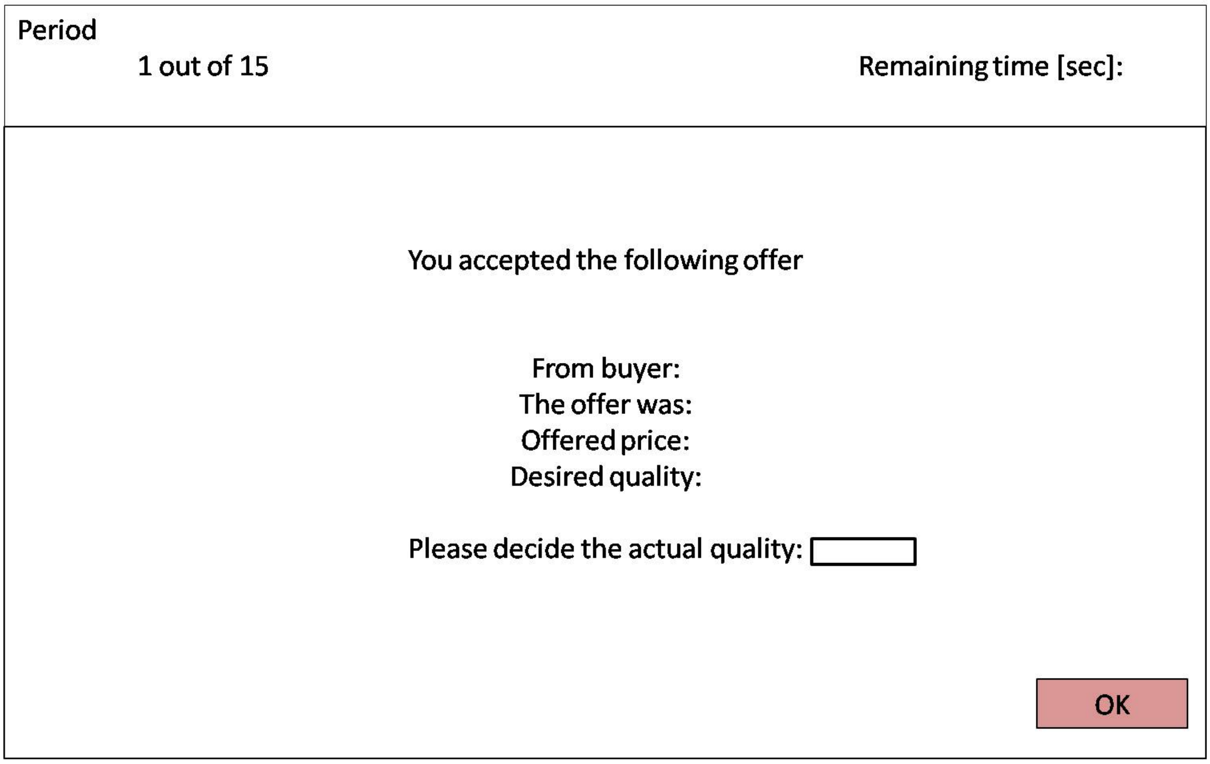
Sellers will see all public offers and those private offers that are sent to him.[122] At any time during the trading phase sellers can accept offers. All received private offers are shown on the left part of screen while all public offers are shown on the right. By clicking on an offer and pressing "accept" a seller can accept one of the listed offers.

No buyer has to make an offer while no seller has to accept an offer. The trading phase will end when all **buyers** have made a deal or the trading time has elapsed. The trading time is 3 minutes (=180 seconds).

Second Phase: deciding quality of the goods

After the trading phase each seller who has accepted an offer will decide the quality of the goods. A seller can select an integer between 1 and 10 as quality of the good. **The seller can deliver a quality equal to, higher or lower than what the buyer desires.**

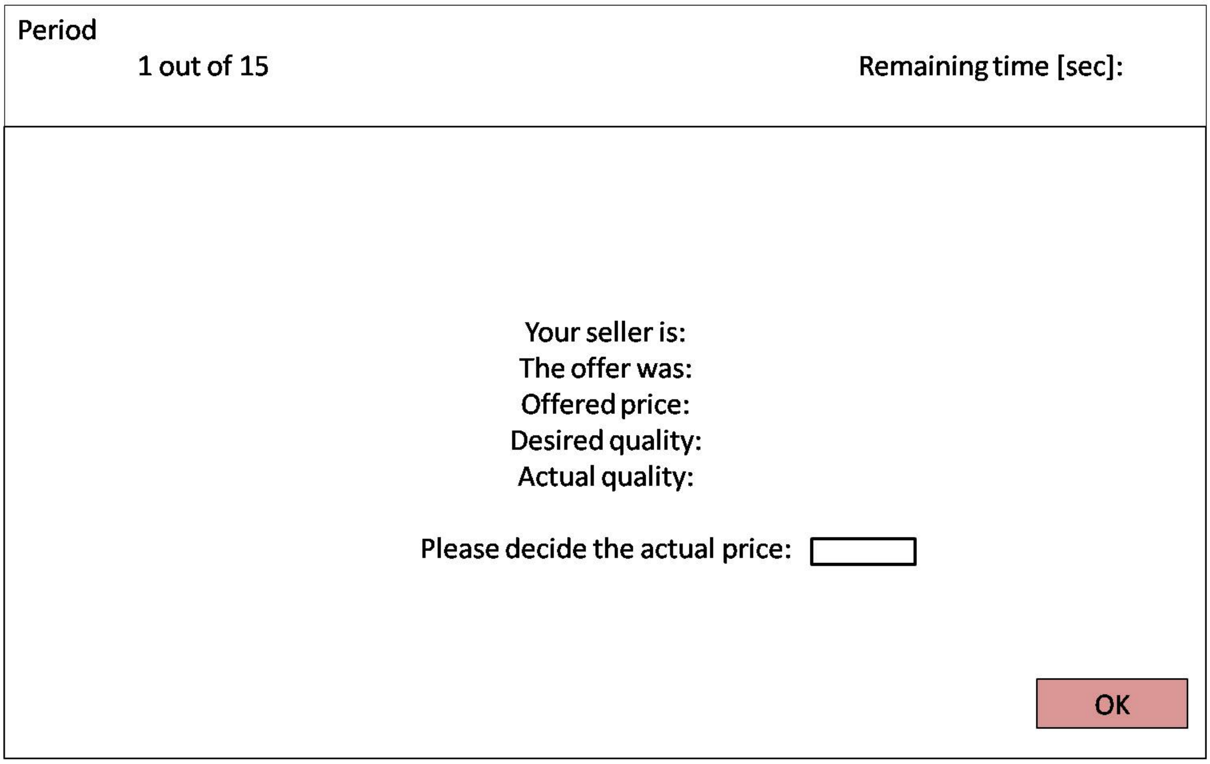
[122] All private offers which are directed to specific sellers are listed in the upper left frame. Public offers from all buyers are listed in the upper right frame.



During the time sellers decide on the quality of the goods each buyer who concluded a trade will estimate what quality of the good he will receive. This estimation will not be seen by any other participants and will not have any influence on the points earned.

Third phase: Deciding actual price

After sellers provide the quality of the goods every buyer who made a deal will decide on the **actual price** of the good. **The buyer can pay a price higher, lower or equal to his offered offer.**



During the time buyers decide on actual prices every seller who concluded a trade estimates the actual price of the goods. This estimation will not be seen by any other participants and will not have any influence on points earned.

Display of payoffs of the current round:

At the end of each round you will be informed of your payoff of this round. If you concluded a trade you will be given the following information: type of offer, ID number of your trading partner, offered price, desired quality of the good, actual quality of the good, actual price of the good, payoff of trading partner and your own payoff of the current round.

Please fill this information into the attached documentation sheet. Any of the 15 rounds ends with payoffs of the current round being displayed.

<u>Calculation of payoff of each round</u>

1. No trade made: payoff of this round = 4
2. Trade was concluded:

Buyer's payoff	=	**Value of the good**	–	**Actual price**
Seller's payoff	=	**Actual price**	–	**Cost of good**

3. Cost and value of the good are dependent of its quality, according to the following schedule:

Quality	1	2	3	4	5	6	7	8	9	10
To buyer value of the good is	10	20	30	40	50	60	70	80	90	100
To seller cost of the good is	0	1	2	4	6	8	10	12	15	18

The calculation applies to all buyers and sellers. Hence, all buyers can calculate their sellers' payoff and all sellers can calculate their buyers' payoff.

Advice for making inputs: after each round or to confirm your input please click „OK", „continue" or „accept". Your input will be valid only after you press these buttons.

The experiment will start after all participants correctly answered the test questions. The test questions make sure that all participants have understood the experiment correctly.

Good luck!

Chapter II
The Limited Power of Voting to Limit Power – A Stress-test in China and Germany[1]

1 Introduction

Voting is an important mechanism to allocate power and hierarchy. Despite a large variation in electoral systems between countries, elections are an essential ingredient of democratic systems in order to select public leaders. Elections also take place within organisations in order to determine organisational hierarchies. Power often comes with conflicts of interest between the holder of power and her electorate: public leaders may embezzle funds; executives of companies may opt for short-term window-dressing instead of long-term profit maximisation; both may spend money on pet-projects instead on good-value-for-money investments. Given the widespread use of elections, which difference do formal voting procedures themselves make to the exercise of power in a situation of conflicting interests?

Within this debate, the economics literature largely adopts an outcome-based view of voting. Its main focus is the potentially disciplinary role elections have on incumbent leaders by threatening dismissal from office (see for example Bardhan and Yang, 2004 and for experimental evidence Weiss, 2009). A second, albeit less developed, strand of literature analyses how elections may serve to select holders of power. Voters may want to give power to those whose preferences are supposedly most in line with their own (for a related model see Maskin and Tirole, 2004), who is up to the task in terms of her ability (see Carrillo and Mariotti, 2001) or who may even feel a public service motivation (Cooter, 2003); this motivation is intrinsic in nature[2] and related to the concept of civic duty (for a discussion and a related model see Besley, 2005). This approach already departs considerably from the assumption of homogenous candidates who are solely motivated by, pecuniary or non-pecuniary, benefits of power; yet, this approach still considers only the outcomes of elections to matter: winning candidates may either be 'good' or 'bad', but are otherwise unaffected by the procedure through which they assume power.

[1] based on: "The Limited Power of Voting to Limit Power – A Stress-test in China and Germany" by Hong Geng and Arne Robert Weiss (2009), Mimeo, University of Bonn.

[2] For an introduction of the concept of intrinsic motivation into economics see Frey (1997).

This, however, may not be the whole story. Possibly, being elected by her constituency triggers the leader to feel a stronger sense of duty to act in her voters' interests. Indeed, a vast amount of evidence on procedural justice in social psychology shows procedures to matter. However, the literature almost exclusively concentrates on the procedural determinants of behavioural responses of subordinates to power (see e.g. Thibaut and Walker, 1993 and 1998; Tyler, 1989; Tyler and Lind, 1992; Tyler and Blader, 2003; Tyler, 2006), rather than on the effects of procedures for the behaviour of those who hold power. In psychological economics and related fields in political philosophy, the utility consequences of participatory procedures are investigated. According to research on procedural utility (e.g. Frey, Benz and Stutzer, 2004), which draws on earlier work by Sen (1995), people may have a preference for participation independent of outcomes and yield substantially higher life satisfaction from participatory rights.

Our research question consequently takes up an understudied area. It departs from the outcome-based view of voting in economics on the one hand and from the utility-consequences of participation in psychological economics on the other hand. Rather, we follow recent research that analyse the behavioural consequences of voting procedures. The largest body of evidence points to ballot voting (Ostrom, Walker and Gardner, 1992; Maier-Rigaud and Apesteguia, 2003; Tyran and Feld, 2006; Dal Bó, Foster and Putterman, 2008) or foot voting (Gürerk, Irlenbusch and Rockenbach, 2006) enhancing cooperation within groups. It has also been shown that merely choosing one´s transaction partner can mitigate the moral hazard problem: agents behave more reciprocally if they have accepted an offer by a principal that was only targeted at him instead of an offer that was open to all agents in a group (Brown, Falk and Fehr, 2004). The behavioural consequences of voting procedures for the exercise of power in situations of conflict of interest have only very recently been investigated.

Experimental evidence so far shows that holders of power[3] to behave more pro-social if they have been elected compared to being selected randomly. Corazzini, Kube and Maréchal (2007) find elected allocators to send more to the recipients than randomly drawn allocators if their approval rates are higher than what is minimally required to win the election. Weiss (2009) shows elected allocators to send back considerably more than randomly drawn allocators; this effect is found even in the last election period when re-election cannot motivate incumbents

[3] In their experimental instructions, the holder of power is formulated either as "the winner" in Corazzini, Kube and Maréchal (2007) or as "the president" in Weiss (2009) and Walkowitz and Weiss (2009).

anymore. Walkowitz and Weiss (2009) find a less pronounced but qualitatively same effect on return on investments even if reliable reputation building is ruled out. Besides the voting procedure itself, however, commitment to promises (Ellingsen and Johannesson, 2004; Vanberg, 2008), dynamic considerations or interaction with investments may also play a role in explaining the effects of voting.

In the first series of experiments reported in this paper, we rule these factors out and thereby provide a *stress-test* for the power of voting to limit power. Our research goal is to concentrate on whether the mere procedure of voting matters. In our design, recipients elect one candidate to be, effectively, a dictator based on positively connoted, personal descriptions that candidates choose prior to the election. The elected dictator decides how to split an endowment between himself and the voters. In the control treatment, the dictator is selected by a random draw. The experiment is only played once, which is known to all subjects. Apart from voting, recipients do not make any further decisions. We thereby stress-test whether dictators exploit their power less if they have been elected by their group instead of chosen by a random mechanism.

The experiment was first conducted in Chengdu, China. The results show that voting has no effect on dictators' transfer decisions. In order to test whether the results are different if subjects are more used to formal voting as a mechanism to determine hierarchy, we re-ran the experiment in Erfurt, Germany. The German results show that voting does not have an effect on the dictators' choices either. The results of the stress-testing experiment therefore send a cautionary note as they imply that the power of voting to limit power shown in previous experiments may be context-specific and dependent on, possibly, an interaction with specific promises, monetary reciprocity or dynamic considerations. In order to test the role of promises, we ran a new experiment in Chengdu, China, with the same subject pool as in the first experiment. We altered the treatments by implementing number promises instead of personal characteristics as statements prior to the selection of the dictators. In this case, voting indeed matters: elected dictators transfer more to their recipients than dictators that are randomly drawn.

The paper is organized as follows: In section 2 we introduce our experimental design and describe the experimental procedure. Section 3 provides a game theoretic solution and our intra- and cross-cultural hypotheses. In section 4, we first present intra-cultural results and subsequently cross-cultural results. We discuss and put the experimental results into a wider context in section 5. Section 6 provides an outlook and first experimental results on a follow-up research question that is immediately derived from the discussion in section 5. Section 7 concludes our findings and provides an outlook for future research.

2 Experimental design and procedure

2.1 Experimental implementation

As already mentioned in the title of our paper, we want to stress-test the voting effect on asymmetric power distribution, e.g. to test whether power can be limited merely because the decision-maker is elected by voters. To serve this goal, we need to rule out other interacting effects and dynamic considerations and at the same time keep two fundamental elements in our experimental design: a voting process and an asymmetric power distribution.

Based on these considerations, we choose a simple design. Each experimental group consists of five players. Two of them are candidates and the other three are recipients. The candidates first choose a personal description that can be seen by all three recipients. In the experimental voting treatment (VOT), the recipients elect their dictator. The candidate who receives at least two votes becomes the dictator and decides on how to distribute 100 points among his recipients and himself. The dictator can choose any amount $s \in \{0,1,...,100\}$ to allocate to his recipients. The remaining of the 100 points ($100 - s$) is the dictator's payoff. As her payoff, each recipient gets exactly the amount s transferred by the dictator[4]. The experiment is only played once. In the control treatment (RAN), the procedure stays the same but the dictator is selected randomly. Figure 1.a and figure 1.b illustrate the experimental process in treatment VOT and in treatment RAN respectively. An experimental instruction of each treatment is provided in the appendix.

[4] As each recipient receives s but the dictator pays only once, our design implies a multiplicator of 3 for the amount s transferred by the dictator.

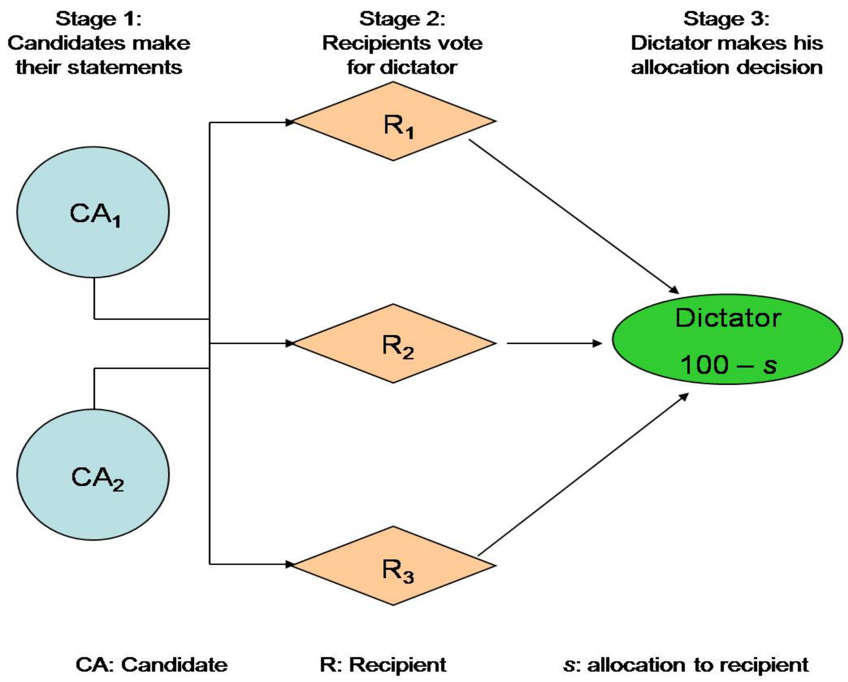


Figure 1.a: Experimental procedure of treatment VOT

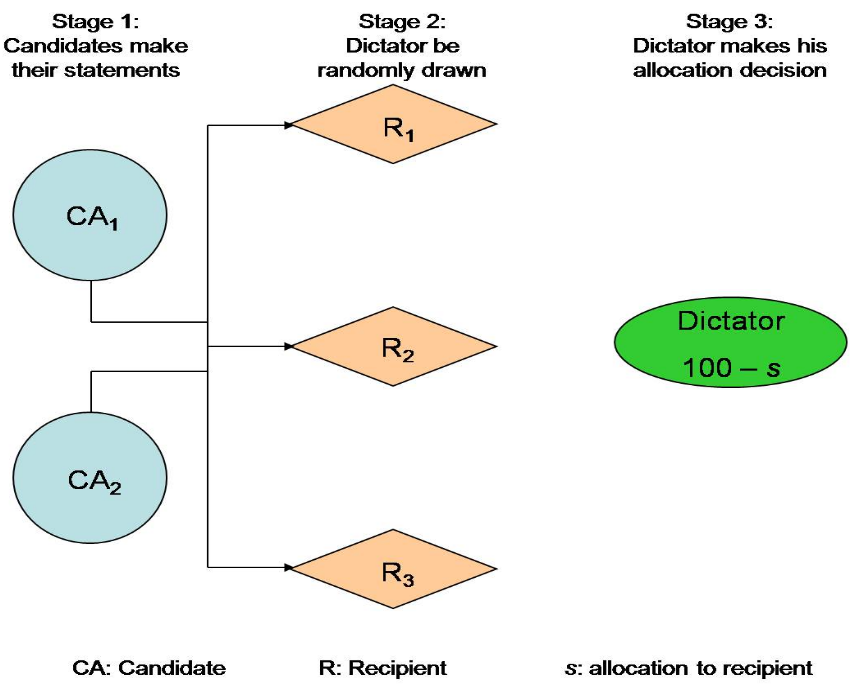


Figure 1.b: Experimental procedure of treatment RAN

The power entitled to the dictators in our experiment is comparable to the power a dictator is endowed with in a dictator game: they both have full power to decide on how to split the endowment between themselves and the recipients.

Compared to Corazzini, Kube and Maréchal (2007) as well as Walkowitz and Weiss (2009), we rule out any interaction with competition for votes and commitment to promises. In both Corazzini, Kube and Maréchal (2007) as well as Walkowitz and Weiss (2009), candidates announce promises on how they will behave in case they win the election. Winning an election based on a promise sends a message from the voters of what they expect from the winning candidate, which matters to allocators who want to keep their promises or exhibit guilt-aversion. In addition, the voting procedure in Corazzini, Kube and Maréchal (2007) also triggers higher promises compared to their control treatment, in which the computer votes. Since second-order beliefs of selected allocators are likely to correlate with the promises they gave, the more group-oriented behaviour of elected allocators may be explained by a combination of commitment to promises or guilt-aversion and competition for votes, rather than the procedure of voting itself. In our design, election takes place based on personal descriptions unrelated to the later task of the winning candidate. Electoral success is therefore not clearly attributable to voter expectations. Therefore, guilt-aversion may not lead to elected allocators behaving more group-oriented than randomly selected allocators.

Compared to Walkowitz and Weiss (2009), which is our experimental starting point, we also rule out any sort of dynamic considerations. The experiment by Walkowitz and Weiss (2009) is based on a repeated investment game with five election periods. Reliable reputation building is ruled out in order to test for voting effects that are unrelated to strategic incentives; nevertheless, it remains unclear how strong the role of dynamic incentives is in explaining the voting effect on allocator behaviour in penultimate election periods. If dictators can be re-elected, dynamic considerations may differ between VOT and RAN if the incumbent believes the probability to be elected in the next round may be influenced by her transfer decision in the current round. Even if reputational building is effectively ruled out, dictators may still believe that their re-election probability is not independent of their transfer behaviour (as it clearly is in the control treatment). As a consequence, they may, for example, strategically choose higher transfers in earlier rounds. As the one-shot play is common knowledge among the subjects, we rule out any sort of dynamic considerations and, consequently, any possible interaction between dynamic considerations and voting.

The experimental design also rules out monetary reciprocity. A behavioural effect of voting on the elected person may also possibly be different if the voters also trust him by sending investments (as in Walkowitz and Weiss, 2009). By singling out the behaviour of the elected person, any interaction between reciprocity to investments and voting is also ruled out. In other words, if an elected dictator wants to reward his recipients, he will reward them because he is elected by the recipients but not because recipients invested in the first place.

2.2 Experimental design in more details

2.2.1 Framed text and separated roles

As the power asymmetry between players is very important in the experimental design, players' roles are framed with terms that create power distance between subjects: the dictator is framed as "president" and recipients as "citizens". In order to keep the hierarchy between president and citizen stable throughout the experiment, we strictly separate players' role in the experiment. Each player plays either as a candidate or as a citizen. Only citizens vote. The unelected candidate does not turn into a recipient of the president because we want to concentrate strictly on the relationship between the elected president and the voters (citizens): how will the president treat his constituency?

2.2.2 Statements of candidates

The biggest challenge we face in the experimental design is how to create a meaningful election but at the same time control for unwanted differences between the treatments. In order to test for the effects of voting, the voting procedure needs to be perceived differently than the random draw by the dictators. At the same time, we want to rule out a selection of more group-oriented candidates through voting and the interaction of competition for votes and commitment to promises. We therefore refrain from using quantitative promises as in other voting experiments (Corazzini, Kube and Maréchal, 2007; Walkowitz and Weiss, 2009) and use non-strategic, positively connoted personal statements.

In order to avoid the selection of different types of candidates through the voting procedure, we largely rule out signalling opportunities by only using positively connoted terms. In RAN the selection of candidates is independent of their statements while in VOT recipients are expected to vote for candidates they consider to be trustworthy. If candidates faced positive as well as negative terms, we would expect elected dictators in VOT to be self-characterised purely by positive terms, whereas this may not be the case in RAN. In this way, the personal characteristics of dictators may differ between the treatments, which may have unwanted repercussions for dictators' transfer behaviour.

Therefore, candidates are asked to choose from eight positively connoted terms that are not directly related to the experimental decision of the dictator: “optimistic”, “erudite”, “creative”, “musical”, “sportive”, “lively”, “diligent”, and “fond of travelling”. Candidates select three of these eight terms and are asked to rank them according to a decreasing order of conformity to their personality. These ranked terms are candidates’ statement prior to either the voting procedure or the random draw.

2.2.3 Payoff for non-selected candidate

The payoff of the non-selected candidate is chosen in a way as to minimise confounding effects on candidates’ attitude towards the election or on dictators’ decision. If the payoff for the non-selected candidate is too high, candidates may not want to be elected as dictator. If the payoff for non-selected candidate is a fixed number, the dictator may take it as a focal point for his transfer decision. Therefore the non-selected candidate receives a random payoff from the same interval as the dictator’s decision range [0, 100]. The expected payoff for a risk-neutral non-selected candidate is 50, which is also the equal-split solution, i.e. when the dictator sends half of the 100 points to his recipients.[5]

2.2.4 Belief and hypothetical decision

In order to gain further insight into the relevance of the voting procedure, we also collect hypothetical transfers from the unselected candidates as well as recipients’ expectations on the dictator’s transfer. In order to elicit hypothetical decisions as closely to actual decisions as possible but without actually employing the strategy method (Selten, 1967)[6], we let the non-selected candidates state directly before announcing the election result how much they will transfer in case they have been elected as dictator. The wording of the question is chosen in a way as to blur its hypothetical nature[7]. For the same reason, the timing for the elicitation of recipients’ beliefs is after announcing the winner of the election or the random draw. As soon as they are informed about the result of the selection, recipients are asked to estimate how

[5] We cannot exclude the effect which the payoff range for the non-selected candidate [0,100] may have on the dictator’s decision. If the dictator is risk-averse, he may ask for less than 50 points.

[6] We opted against employing the strategy method for eliciting decisions from both the selected and the un-selected candidate as we want to stay as closely as possible to a real-world voting context. Besides, as possible influences of the strategy method on the effects of voting procedures are untested, we want to refrain from discussing whether either observed or unobserved treatment differences may be an artifact of having employed the strategy method.

[7] The text non-selected candidates see on their screen is “It will be announced soon which candidate has been elected / randomly drawn as president. Please insert how many points you will transfer to the citizens if you have been elected / drawn as president”.

many points they will get from the selected dictator. In the German sessions, recipients afterwards have to make a second estimation on how many points the non-selected candidate would have transferred if he had been selected as dictator.

2.3 Procedure

The Chinese sessions of the experiment were run in November 2007 at the Herbert A. Simon & Reinhard Selten behavioral decision research lab of the Southwest Jiaotong University in Chengdu, China. The German sessions of the experiment were run in January 2008 at the Laboratory for Experimental Economics (eLab) at the University of Erfurt. For each treatment, we collected 15 independent observations in China and 10 in Germany. Therefore, 150 students participated in the Chinese sessions and 100 students in the German sessions. Test questions made sure that subjects are aware of the one-shot play, the power asymmetry in their group as well as of how profits are calculated. The experiment started only when all subjects in the session correctly answered all test questions. Each experimental session lasted about one hour including instructions and payments. On average, Chinese students earned about 42.5 RMB (approximately 4 Euro), and German students earned about 11.5 Euros. The exchange rates between points and cash / local currency were set according to local standards.

Experimenter effects are a sensitive issue in running cross-cultural experiment (see for example Roth, Prasnikar, Okuno-Fujiwara and Zamir, 1991). In order not to let subjects wonder about cross-cultural research questions (if they faced a foreign experimenter), we kept the nationality of the experimenter constant to the subject pool. We also kept the gender of the experimenter constant across experimental sessions. One of the authors (Weiss) was present both in China and in Germany. The other author (Geng) was the experimenter in China. Another female German experimenter ran the German sessions. The experimental instructions were originally written in Chinese. The translation of instructions and computer screens into German was done applying the back-translation method [8]. The experiment was programmed in z-Tree (Fischbacher, 2007).

[8] See Brislin (1970) and Eco (2000) for the method of back-translation.

3 Theoretic solutions and behavioural hypotheses

3.1 Payoffs and game-theoretic solution

Payoff functions for dictator (π_D), recipients (π_R) and the non-selected candidate (π_L):

$$\pi_D = 100 - s$$

$$\pi_R = s$$

$$\pi_L = random\ interger\ number \in [0, \dots, 100] \text{ with } E(\pi_L) = 50 \text{ for risk-neutral players}$$

The unique game theoretic prediction based on money-maximising rationality is the dictator, regardless of whether he is elected in VOT or randomly drawn in RAN, keeping the entire 100 points for himself, i.e. s = 0. No recipients will therefore receive anything from the dictator.

3.2 Behavioural hypotheses

3.2.1 Intra-cultural hypotheses

In our design, only a direct behavioural response to being elected, rather than being randomly drawn, may constitute an effect of voting on power. By electing a dictator, considerable power is transferred. The dictator is completely unrestricted to decide on her own payoff as well on the payoff of the three recipients. If the dictator decides to send less than 50% of the 100 points, the transfer of power also comes with an expected positive material gift (as the un-elected candidate has an expected payoff of 50).

The elected dictator may consider the transfer of power as an act of trust and the implied potentially profitable position as a material gift. Previous research has shown that distrust can be self-fulfilling in that it lowers trustworthiness (Fehr and Rockenbach, 2003; Falk and Kosfeld, 2006). By the same idea, trust may be considered a kind act to be positively reciprocated (Falk and Fischbacher, 2006). Gift-exchange, i.e. the reciprocation of a material gift with another gift, has been found in many experimental studies (e.g. Fehr, Kirchsteiger and Riedl, 1993 and 1998; Fehr, Kirchsteiger and Riedl, 1998; van der Heijden, Nelissen, Potters and Verbon, 2001; Charness, 2004), that may be explained by inequity aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) or by reciprocity utility (Falk and Fischbacher, 2006). Hence, based on previous research on trust and gift-exchange, we expect dictators to show a reciprocal response to being elected. Within our design, reciprocity by a dictator means a transfer s that is higher than what the dictator would send in the absence of reciprocity considerations. As dictators have no reason to consider their selection in RAN as a gift from their recipients, we consequently predict

Hypothesis H1: *Transfers are higher in VOT than in RAN.*

The voting procedure may also affect recipients' expectations of the behaviour of the chosen dictator. Firstly, voters may expect elected dictators to behave reciprocal and group-oriented because of the voting procedure. Secondly, as the recipients in VOT decide who will be elected as the dictator, they may feel less vulnerable towards the dictator than recipients who have to be content with the outcome of a random draw. Research in social psychology on the "illusion of control" (Langer, 1975) has shown that participatory procedures may turn people more confident about personal success in uncertain situations even when the objective probabilities of success have not changed. Translated into our design, recipients may trust more in the group-oriented behaviour of the dictator in VOT than in RAN merely because they are able to choose. We therefore predict

Hypothesis H2: *Recipients expect higher transfers in VOT than in RAN.*

3.2.2 Cross-cultural hypothesis

Democratic procedures are considerably more common in Germany than in China. In Germany, already school kids use ballot voting in order to determine hierarchy and to take decisions of collective importance. From about the age of 10, school kids vote for their class presidents and may use voting to decide on issues such as in which colour their class room may be painted. Later in their school life, pupils may vote directly for the pupil´s president, and their representatives are increasingly involved to vote on issues of collective importance. The typical mechanism is voting with a simple majority rule and universal suffrage. The outcomes of voting procedures are generally well respected. German recipients are typically awarded the right to vote in political elections at the age of 18 and in some local elections even at the age of 16. The electoral system in Germany is generally a mixture of direct and proportional elements. All German participants have likely participated in political elections besides elections in school and at university. In the World Bank´s Governance Indicators, Germany is consequently ranked in the top bracket in terms of "Voice and Accountability" with a percentile rank of 94.7 (Kaufmann, Kraay and Mastruzzi, 2008). Furthermore, in the democracy index of World Audit, an international organization providing global geopolitical perspectives, Germany is ranked at place 10 out of 150 countries, where place 1 means the best overall democracy and place 150 the worst overall democracy[9]. In China, by contrast, direct elections only occur for village councils in designated rural areas and for the local people's congress. Young people rarely get a chance to participate in formal voting procedures. From school to university, class representatives are usually assigned by teachers. In few cases voting is used, but the candidates are nominated by teachers. Chinese pupils have therefore no experience with free and contested elections. According to the World Bank´s Governance Indicators, China consequently scores low on "Voice and Accountability": China is ranked in the bottom bracket with a percentile rank of 5.8 (Kaufmann, Kraay and Mastruzzi, 2008). In the democracy index of World Audit, China's rank is 120 out of all 150 investigated countries. Although voting procedures are rarely used in political and organisational contexts, ballot voting is nevertheless known even to city dwellers; it is increasingly used for example in popular TV-shows. Here, however, the content of voting is questions of taste, rather than issues with conflicting interests, which are typical of political contexts. As the Chinese media also reports on elections taking place outside China, our Chinese subjects should be familiar with the voting mechanism we employ.

Because of large differences in personal experiences with democratic procedures we, nevertheless, expect Germans subjects to be more influenced by the voting procedure than Chinese subjects. We therefore predict

Hypothesis H3: *The difference in transfers between VOT and RAN ($s_{VOT} - s_{RAN}$) is larger in the German sessions than in the Chinese sessions.*

4 Results

4.1 Results of the Chinese sessions

Table 1: Descriptive summary statistics of Chinese sessions (standard deviation in parentheses)

	C-VOT	C-RAN
Mean transfer of dictators	29.93	34.80
	(17.248)	(17.877)
Mean hypothetical transfer of non-selected candidates	33.73	36.80
	(23.864)	(21.301)
Mean expected transfer of recipients	41.20	39.62
	(7.578)	(14.796)

[9] See www.worldaudit.org/democracy.htm.

Table 1 shows the summary statistics of VOT and RAN in the Chinese sessions. Surprisingly, both average values of transfers (29.93) and hypothetical transfers (33.73) are lower in VOT than in RAN (34.80 and 36.80 respectively). Significance tests do not deliver evidence that actual transfers are significantly different between the two treatments[10]. The same holds true when we compare the hypothetical transfers of un-elected candidates in VOT to either the hypothetical transfers of unselected candidates in RAN[11] or to the actual transfers in RAN[12]: differences are statistically insignificant. Even when we merge dictators' actual transfers and non-selected candidates' hypothetical transfers[13], still no significant difference emerges between VOT and RAN[14]. As significance tests do not reveal a significant difference in either actual or hypothetical transfers and as average values are lower in VOT than in RAN, the results from our Chinese sessions do not support hypothesis H1. The average beliefs of recipients are very similar in VOT (41.20) and RAN (39.62). A significance test[15] cannot deliver evidence for hypothesis H2, which predicts recipients to expect higher transfers in VOT than in RAN. Hence hypothesis H2 does not hold for the Chinese data either.

Result C-1: *In the Chinese sessions, neither dictators' transfers nor recipients' expectations are significantly different between VOT and RAN.*

Since the average transfers are lower in VOT than in RAN, one may ask the question that to what extent voting may have a negative effect on subjects' behaviour. In order to gain a better insight into this question, we multiply the observations of VOT by a shift $\lambda \in [0,1]$ and compare the modified observations of VOT with the original observations of RAN. The λ which yields a 10% one-sided significance level that the modified transfers of VOT are

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[10] Mann-Whitney U test, two-sided: p=0.624.
[11] Mann-Whitney U test, two-sided: p=0.775.
[12] Mann-Whitney U test, two-sided: p=0.766.
[13] If by merging actual transfers and hypothetical transfers we still do not find significant differences, the evidence to reject H1 is even stronger. While the purist may not view actual and hypothetical transfers as statistically independent, we may still treat them as independent observations – in order to get further insight into the results – as there was no direct informational exchange between the candidates. Only the winning candidate in VOT is aware of being favoured by the voters but still has no information on the other candidate. Hence, while actual transfer decisions may, for a number of reasons, be different from hypothetical transfer decisions, candidates within one group should not influence each other. In fact, statistical tests reveal no difference between actual and hypothetical transfers, according to both two-sided Mann-Whitney U tests, p=0.845 for VOT and p=0.976 for RAN and two-sample Kolmogorow-Smirnow tests, p=0.999 for both VOT and RAN; there is also no significant difference in variances according to the Two-Sample Randomization Test for Differences in Variances: p=0.591 (two-sided) for VOT and p=0.731 (two-sided) for RAN.
[14] Mann-Whitney U test, two-sided: p=0.547.
[15] Mann-Whitney U test, two-sided: p=0.992.

significantly smaller than the original transfers of RAN is 0.857[16]. Hence, we can exclude with 90% confidence limit that voting has a higher than 14.3% negative effect on the dictators' transfer decisions in the Chinese sessions.

Result C-2: *In the Chinese sessions, we can exclude with 90% confidence limit that voting has a higher than 14.3% negative effect on the transfers of dictators.*

4.2 Results of the German sessions

Table 2: Descriptive summary statistics of German sessions (standard deviation in parentheses)

	G-VOT	G-RAN
Mean transfer of dictators	38.30	49.70
	(13.873)	(24.221)
Mean hypothetical transfer of unselected candidates	40.40	36.80
	(12.518)	(17.479)
Mean expected transfer of recipients	35.03	40.53
	(8.972)	(4.11)
Mean of recipients' expected transfer of unselected candidates	34.27	42.27
	(7.694)	(3.042)

Table 2 shows the summary statistics of VOT and RAN in the German sessions. The average transfer in RAN (49.79) is very close to the equal split of 50, while the average transfers in VOT (38.30) are 11.40 points lower. Significance tests, however, do not show a difference[17].The hypothetical transfers of non-selected candidates in VOT (40.40 on average) also do not significantly differ both compared to the actual[18] (49.70 on average) and the hypothetical[19] transfers (36.80 on average) in RAN. The merged data of actual and hypothetical transfers[20] do not show a difference between VOT and RAN either[21]. Thus hypothesis H1 does not receive support from our German sessions too.

Result G-1: *In the German sessions, dictators' transfers are not significantly different between VOT and RAN.*

[16] Mann-Whitney U test, one-sided: p=0.098.
[17] Mann-Whitney U test, two-sided: p=0.424.
[18] Mann-Whitney U test, two-sided: p=0.424.
[19] Mann-Whitney U test, two-sided: p=0.565.
[20] As in China (see footnote 13 on merging actual and hypothetical transfers), both types of transfers are not statistically different according to both two-sided Mann-Whitney U tests, p=0.811 for G-VOT and p=0.159 for G- RAN, and two-sample Kolmogorov-Smirnov tests, p=0.976 (G-VOT) and p=0.294 (G-RAN); there is also no significant difference in variances according to the Two-Sample Randomization Test for Differences in Variances: p=0.702 (two-sided) for VOT and p=0.461 (two-sided) for RAN.
[21] Mann-Whitney U test, two-sided: p=0.877.

We also calculate the λ for the German VOT observations. The λ which yields an 8% one-sided significance levels between the modified transfers of G-VOT and the original transfers of G-RAN is 0.979[22]. Thus we come to the conclusion that in the German sessions we can exclude with 92% confidence limit that voting has a higher than 2% negative effect on the dictators' transfer decisions.

Result G-2: *In the German sessions, we can exclude with 92% confidence limit that voting has a higher than 2% negative effect on the transfers of dictators.*

Surprisingly and contrary to our hypothesis H2, German recipients' expectation on their dictators' transfer choices is weakly significantly higher in RAN (40.53 on average) than in VOT (35.03 on average)[23]. Furthermore, recipients' expectations on the hypothetical transfers of the unselected candidates are also significantly higher in RAN (42.27 on average) than in VOT (34.27 on average)[24].

Result G-3: *In the German sessions, recipients expect to get higher transfers in RAN than in VOT.*

4.3 Cross-cultural results

Although the German transfers (see table 2) are on average higher than the Chinese transfers (see table 1) in both VOT and RAN, neither difference reaches a significant level[25]. The same holds true for the hypothetical transfers of the unselected candidates. The German unselected candidates would not behave differently than the Chinese unselected candidates, neither in VOT[26] nor in RAN[27]. However, when we compare the merged data of actual and hypothetical transfers, we find that they are (weakly) significantly higher in German VOT than in Chinese VOT[28], whereas there is no significant difference between the two RAN treatments[29]. This finding suggests the German dictators may care more about the welfare of their group than the Chinese dictators if they are put into a voting environment. The reason for not directly observing cross-cultural differences in actual transfers, neither in VOT nor in RAN, may be

[22] Mann-Whitney U test, one-sided: p=0.080. Due to the methodology of discrete statistic test, this is the closest significance level to 10%.
[23] Mann-Whitney U test, two-sided: p=.069.
[24] Mann-Whitney U test, two-sided: p=.007.
[25] Mann-Whitney U test, two-sided: p=0.229 (VOT); p=0.115 (RAN).
[26] Mann-Whitney U test, two-sided: p=0.180.
[27] Mann-Whitney U test, two-sided: p=0.946.
[28] Mann-Whitney U test, two-sided: p=0.072.
[29] Mann-Whitney U test, two-sided: p=0.238.

due to the small number of observations (10 German observations vs. 15 Chinese observations).

Result CG-1: *In VOT, German candidates tend to transfer more than Chinese candidates. In RAN, the transfers are not different between German and Chinese sessions.*

The average difference between the transfers in VOT and RAN is larger in the German sessions (11.40) than in the Chinese sessions (4.87), yet in another direction than stipulated in hypothesis H3: in both countries, transfers are higher in RAN than in VOT. Hence, dictators' transfers are not raised more due to the voting procedure in Germany than in China and do not deliver support for hypothesis H3. Nevertheless, German dictators may still be more affected by the voting procedure, albeit in a different direction than hypothesised. In order to test whether the treatment effect between VOT and RAN is different in the two countries, we use the Monte-Carlo approximation of a two-sided permutation test with 50.000 draws. The result fails to reach a significant level[30].

Result CG-2: *The difference in transfers between VOT and RAN does not differ between the German and the Chinese sessions.*

5 Discussion of the stress-testing experiment

Hence, based on our experimental setup, voting fails to limit power. Or put differently: the voting effect identified by Walkowitz and Weiss (2009) and Corazzini, Kube and Maréchal (2007) do not survive the stress-test of our experiment. Based on our results, merely being elected instead of randomly drawn does not lead to power being exploited less. The implications of our results are therefore not in line with Walkowitz and Weiss (2009) and Corazzini, Kube and Maréchal (2007). The average values of our results even go contrary what has been previously observed. The power of voting to limit power therefore seems to be highly context-specific: Voting leads to considerable less morally hazardous behaviour by a powerful allocator in an investment context when voting is based on promises and reputation building is possible (Weiss, 2009) and even when reliable reputation building is ruled out – albeit to a lesser extent (Walkowitz and Weiss, 2009). Voting also leads to higher transfers when elections are based on promises and approval rates are higher than what is minimally required to win the election (Corazzini, Kube and Maréchal, 2007). By contrast, when

[30] p=0.560, two-sided.

promises are absent and power is unconditional (as in our design) voting seems to have no capacity to limit power. Our experimental results therefore send a cautionary note not to put too much trust into voting or participatory mechanisms *by themselves* limiting power.

The question is: How pessimistic should we be about the power of voting to limit power? How may we reconcile the results by us on the one hand with the results by Corazzini, Kube and Maréchal (2007) as well as Walkowitz and Weiss (2009) on the other hand? In order to think about both questions and indicate directions for future research, we will in the remainder of this section evaluate features of our design and compare it to previous voting experiments. As our results do not yield significant behavioural differences between the elected dictators and the randomly drawn dictators, we will think in two directions. We will first consider aspects of our stress-test that may lead to elected dictators not behaving more group-oriented. We will then discuss whether a hitherto unconsidered behavioural mechanism, namely the creation of entitlements, may lead to voting causing less group-oriented behaviour.

In deriving the hypotheses of section 3 the candidates' perceptions of the voting procedure are critical. For the dictator to respond to the transfer of power and the potential material gift of being selected differently by being elected than by being randomly drawn, she has to view the voting procedure as something different than a random draw. If dictators consider being elected as a deliberate act of trust by the voters, we would expect at least some dictators to reciprocate this trust by sending more than a randomly drawn dictator. If, however, the dictator sees the voting mechanism as merely a substitute for some random mechanism, we would not expect him to behave any different by being elected than by being randomly drawn. We will separate this question into two elements: the perception of the voting mechanism itself and the role of intentionality.

5.1 Is the voting mechanism perceived as meaningful?

Candidates' statements, which voters base their decisions on, are unrelated to the later task of the elected candidate and leave little room for differences in taste. By contrast, in Walkowitz and Weiss (2009) candidates state a non-binding back-transfer-strategy that is directly related to the decisions elected candidates would later take. We purposefully refrained from using such kind of information in order to avoid unwanted differences between the statements of elected and randomly drawn candidates[31]. Possibly, however, the differences between the treatments thereby become so subtle that elected dictators consider their electoral success as just as much an outcome of chance as their randomly drawn counterparts.

In order to investigate the significance of the method we employed let us look first at the selection of terms. Evidence for candidates perceiving the term selection procedure as meaningful would be candidates deliberately choosing instead of randomly picking terms to describe themselves. Note that the distribution of selected terms should be uniform if subjects choose them randomly. Looking at Figure 2 and Figure 3, the distributions of selected terms in VOT are clearly not uniformly distributed[32]. Even in the Chinese random treatment, where the chosen terms are irrelevant for the selection of the dictator, candidates did not randomly pick terms[33]. Only in the German random treatment the distribution of terms is not distinguishable from being randomly selected[34].

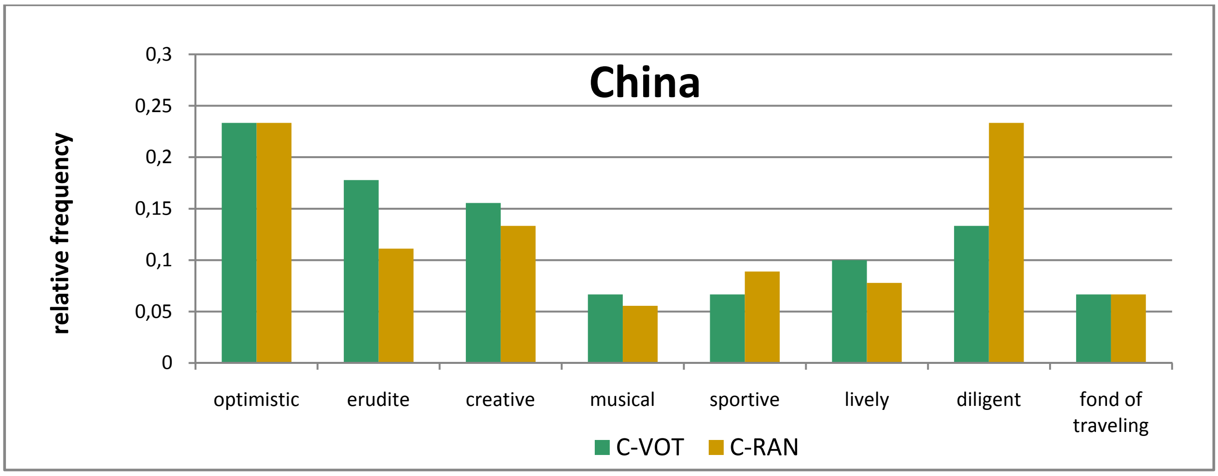

Figure 2: Distribution of chosen terms in Chinese sessions

[31] Recall from section 3 that candidates in VOT have an incentive to promise high transfers (or generally to present themselves in such a way as to receive votes) while candidates in RAN have no such incentive at all. If dictators are guilt averse (cf. e.g. Charness and Dufwenberg, 2005), higher transfers in VOT than in RAN may be due to a confound of guilt aversion and the voting effect we are interested in.

[32] We used two-sided Chi square Goodness-of-Fit test to test whether the distribution of chosen terms may be drawn from a uniform distribution of terms: p=0.008 for both C-VOT and G-VOT.

[33] Chi square Goodness-of-Fit test, two-sided: p=0.001.

[34] Chi square Goodness-of-Fit test, two-sided: p=0.494. Note that the test can nevertheless not show that candidates did not care about which terms to choose. The distribution of self-described characteristics may also be uniform.

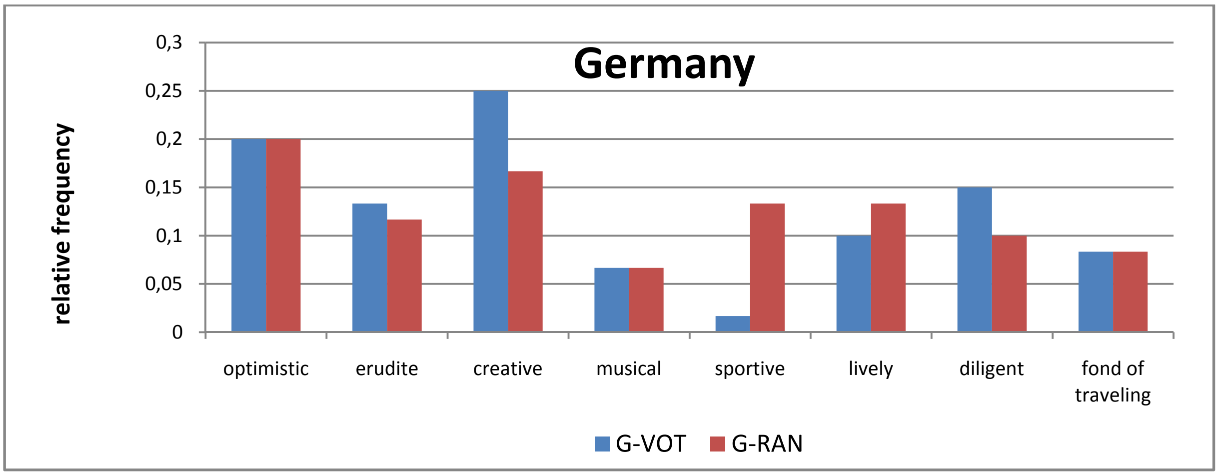

Figure 3: Distribution of chosen terms in German sessions

Dictators may perceive the voting procedure as meaningful if they attribute their electoral success as a result of a deliberate act by the voters based on the chosen terms of the candidates. While we have no direct access to dictators' perceptions, we can nevertheless look at the voting procedure from the perspective of the recipients – assuming that dictators' expectations of voters' behaviour are not systematically false.

First, did recipients have a real choice? In only one case (out of 50) the two candidates in one group chose the same three terms. But even in this case the ordering of terms was not the same. Recipients therefore always had a choice in that the two options were not equal. Second, did recipients perceive the candidates´ statements as important for their voting decision? 87% of all German recipients affirmed this question when being asked after they took their voting decision[35]. As we have shown in section 5, recipients' voting decisions were in line with the transfers they expected from the respective candidates. Hence, candidates expected the personal statements of the candidates to be informative.

To conclude, the voting procedure we employed in VOT is certainly different from the random device in RAN. This is also reflected in the views of the subjects: significantly more subjects favoured the voting mechanism to a random mechanism in all treatments in both countries based on a hypothetical question in a questionnaire after subjects received feedback on their dictator's decision[36]. As recipients were neither better off in VOT than in RAN nor expected to receive more in VOT than in RAN, this seems to be in line with the notion of procedural utility (compare e.g. Frey, Benz and Stutzer, 2004): recipients seem to have a taste

[35] Chinese citizens were not asked about their perception of the candidates' statements.
[36] Chi square Goodness-of-Fit test, two-sided: $p<0.001$ (for C-VOT, C-RAN and G-VOT), $p=0.034$ (G-RAN).

for having a say. While the voting procedure therefore worked differently and was perceived differently than the random mechanism, we cannot rule out that the differences between the mechanisms in VOT and RAN were insufficient to lead to more group-oriented behaviour of dictators in VOT than in RAN.

5.2 Is intentionality critical?

Elected dictators, even if they feel to be elected purposefully, may not view the transfer of power as an intentional act of trust. Intentions have received considerable attention in the study of reciprocity (see for example Falk and Fischbacher, 2006). A common view seems to emerge that sees intentions as at least enhancing reciprocity (see e.g. McCabe, Rigdon and Smith, 2003; Charness, 2004). The basic experimental structure we employed is based on the power-distribution of a dictator game. The advantage of a dictator game for our research question may at the same time limit the power of voting: a dictator's power is unconditional. In our design, the recipients have no choice but to transfer power to one of the two candidates. Even if they deliberately choose one candidate over the other, they have no choice but to put their material well-being in the hands of the elected candidate. In other words: the transfer of power itself is not an act of choice. By contrast, in Walkowitz and Weiss (2009) voters made two choices: they transferred power by giving a candidate their votes and they decided through their investments to which degree they would trust the elected candidate. Possibly, for voting to be reciprocated, the intentional element of trust needs to be more visible.

5.3 Does voting lead to greater entitlements for elected dictators?

It has been shown that giving-behaviour in dictator game reacts sensitively to how entitled the dictator feels to keeping the amount to be distributed between herself and the recipient. Dictators give considerably less if they feel they deserve to have received the endowment, e.g. by first passing a test (Cherry, Frykblom and Shogren, 2002). Even subtle forms of entitlements seem to matter: in Bolle and Vogel (2007) provision allocations (by the experimenter) between the dictator and the recipient influence the final allocation of the dictator as long as the provisional allocation is privately and socially acceptable. Possibly, also the voting procedure we employed may create entitlements.

Candidates in VOT may consider the voting procedures as a contest between the candidates. Winning this contest may be attributed to being the "better" candidate in two possible dimensions: dictators who described themselves truthfully may consider their personality more appreciated by the voters while those dictators choosing terms strategically may consider themselves better in guessing voters' preferences[37]. As either the more appreciated or smarter candidate, the dictator in VOT may feel entitled to a monetary prize. In RAN, by contrast, the selection of the dictator is unrelated to their personal characteristics and only a matter of chance. Dictators in RAN may therefore feel less entitled to a reward. Consequently, the stronger sense of entitlement in VOT compared to RAN would lead to dictators, contrary to our hypothesis H1, sending less if they are elected than randomly drawn. Entitlements have hitherto not been explicitly considered in voting experiments. Entitlements may indeed be more important in our design than in previous voting studies. In Walkowitz and Weiss (2009) and Corazzini, Kube and Maréchal (2007) allocators were elected based on specific promises on their behaviour in the position of the allocator. No personal information was given about the candidates. Hence, elected allocators should be aware that their electoral success was due to their promises and not to them being the "better" candidate for intrinsic reasons. Furthermore, in Corazzini, Kube and Maréchal (2007), elected allocators knew before taking their transfer decisions that first and second-order beliefs on the transferred amounts would be elicited[38]. Thereby, the transfer expectations of the voters may have become salient both in attributing electoral success and in creating guilt-aversion. The voting procedure also triggered higher promises in their voting treatment compared to their random treatment (compare table 2 and table 3 in their appendix) so that elected allocators for whom their promises matter would send more simply because of the higher promises they gave rather than the procedure by which they were selected into their position. In Walkowitz and Weiss (2009), in addition to the promised-based elections the separate and repeated investment decisions may have made clear that trust is conditional. Electoral success would therefore not justify exploiting the position of the allocator.

6 Testing the role of promises

In order to test the role of promises in explaining differences between us on the one hand and Walkowitz and Weiss (2009) and Corrazini et al. (2007) on the other hand, we ran two new treatments. These were run in January 2009[39] also at the Herbert A. Simon & Reinhard Selten

[37] The game played by strategic candidates may be seen as a sort of beauty contest.
[38] Compare the instruction in Corazzini et al. (2007).
[39] We thank Peng Cheng for running the experiment for us. The experiment will be presented and analysed in more depth in his diploma thesis, supervised by Prof. Dr. Armin Falk of the University of Bonn.

behavioral decision research lab of the Southwest Jiaotong University in Chengdu, China. We altered the treatments VOT and RAN by implementing number promises instead of personal characteristics as statements prior to the selection of the dictators. 20 independent observations were collected for both treatments, which we call VOT-P and RAN-P. We also elicited second order beliefs of the selected dictators in order to test for guilt-aversion as a possible driver of treatment differences[40]. We elicited second order beliefs after dictators chose how many points to transfer to their group; in this way, number promises instead of personal characteristics are the only difference between the new treatments VOT-P and RAN-P and the original treatments VOT and RAN.

Table 3: Descriptive summary statistics of number promises treatments
(standard deviation in parentheses)

	VOT-P	RAN-P
Mean transfer of dictators	34.00	25.30
	(17.592)	(20.303)
Mean hypothetical transfer of unselected candidates	37.55	29.35
	(21.132)	(18.004)
Mean promises of dictators	52.25	49.75
	(7.887)	(13.396)
Mean expected transfers of recipients	40.99	34.13
	(9.954)	(10.095)
Mean second order belief of dictators	40.75	36.70
	(11.616)	(16.658)

Table 3 summarizes the results of the new treatments. We find actual transfers to be weakly significantly higher[41] in VOT-P (34.00 on average) than in RAN-P (25.30 on average); a joint comparison of actual and hypothetical transfers[42] even imply significantly higher[43] transfer decisions in VOT-P compared to RAN-P.

Result CP-1: *Dictators' transfers are weakly significantly higher in VOT-P than in RAN-P.*

[40] The text dictators see on their screen is "The three citizens in your group are estimating how many points you will allocate to them. Please estimate the average point of the estimation of the three citizens."
[41] Mann-Whitney U test, two-sided: p=0.098.
[42] As in previous sections (see footnote 13 and 20 on merging actual and hypothetical transfers), both two-sided Mann-Whitney U tests (p=0.738 for VOT-P and p=0.341 for RAN-P) and Kolmogorov-Smirnov tests (p=0.933 for VOT-P and p=0.699 for RAN-P) do not reveal significant difference between actual and hypothetical transfer decisions in both VOT-P and RAN-P treatments. There is also no significant difference in variances according to the two-sample Randomization test for differences in variances: p=0.682 (two-sided) for VOT-P and p=0.776 (two-sided) for RAN-P.
[43] Mann-Whitney U test, two-sided: p=0.050.

The average expected transfers of recipients are, on average, higher in VOT-P (40.99) than in RAN-P (34.13). The statistical test rejects the null hypothesis that recipients have the same expectations in the two treatments at a weakly significant level[44].

Result CP-2: *Recipients' expectations on the transfers are weakly significantly higher in VOT-P than in RAN-P.*

Second order beliefs of dictators and promises are strongly and highly significantly correlated in VOT-P but only marginally significantly in RAN-P[45]. Transfers are weakly significantly correlated with second order beliefs of dictators in VOT-P but again only at marginal significance in RAN-P[46]. Surprisingly, elected dictators did not give significantly higher promises[47] than randomly selected dictators (on average 52.25 in VOT-P vs. 49.75 in RAN-P); second order beliefs of dictators are only somewhat higher[48] in VOT-P than in RAN-P (on average 40.75 in VOT-P vs. 36.70 in RAN-P). Interestingly, transfers are not positively correlated with promises in RAN-P, while they are, at a weakly significant level, in VOT-P[49]. Although average values of commitment to promises are considerably different (on average, actual transfers fall short of promises by 18.25 in VOT-P and 24.45 in RAN-P), tests reveal no significant difference between the treatments[50].

7 Conclusions and outlook

The results of the stress-testing experiment using personal descriptions as statements strongly reject our hypotheses of more group-oriented behaviour of the elected dictators both in the Chinese as well as in the German sessions. Average transfers tend to be higher in RAN than in VOT in both countries – albeit not at any reasonable significance level. In our German sessions, recipients even expect to receive more from the randomly drawn than from the elected dictators. Albeit not significant either, the tendency of differences in expected transfers goes into the same direction in China. The experimental results fail to support our basic hypotheses on the effects of voting. .

[44] Mann-Whitney U test, two-sided: p=0.081.
[45] VOT-P: Spearman's rho = 0.662, p=0.001 (two-tailed); RAN-P: Spearman's rho = 0.372, p=0.106 (two-tailed).
[46] VOT-P: Spearman's rho = 0.408, p=0.074 (two-tailed); RAN-P: Spearman's rho = 0.374, p=0.104 (two-tailed).
[47] Mann-Whitney U, two-sided: p=0.297.
[48] Mann-Whitney U, two-sided: p=0.158.
[49] VOT-P: Spearman's rho = 0.404, p=0.078 (two-tailed); RAN-P: Spearman's rho = -0.072, p=0.763 (two-tailed).
[50] Mann-Whitney U, two-sided: p=0.427.

In case voting is based on explicit promises, as in VOT-P, elected dictators transfer more to their group than randomly drawn dictators, while they do not if voting is based on un-strategic personal characteristics, as in VOT. The results thereby also show that an intentional transfer of power is not, contrary to the discussion in section 5.2, necessary for voting to affect behaviour. Part of the results is consistent with guilt-aversion: both dictators seem to be motivated by guilt-aversion, more clearly so in VOT-P than in RAN-P; as Corrazini et al. (2007) also argue, promise-based voting sends a clear message about voter expectations; consequently, second order beliefs of dictators are considerably stronger correlated with promises in VOT-P than in RAN-P, while transfers are not even positively correlated with promises in RAN-P. Nevertheless, guilt-aversion cannot be the whole story: the difference between VOT-P and RAN-P is higher in transfers (on average, 8.70) than in second order beliefs of dictators (on average, 4.05), the latter difference not even reaching a significant level. Direct commitment to promises, as a mechanism independent of guilt-aversion, does not provide any explanatory power in order to account for the effects of voting as no significant difference can be found between the treatments. Rather, it seems that at least the basis for the behavioural hypothesis in section 3 seems to be vindicated: motives other than commitment to promises or guilt-aversion, such as reciprocity, may lend an additional role for voting procedures limiting the exercise of power. More research is nevertheless needed to clearly attribute the effects of voting to behavioural mechanisms. More experimental data are also warranted to find out more about whether voting procedures create entitlements. Possibly, depending on procedural details, voting can lead to an either more or less group-oriented exercise of power.

Appendices: Instructions

Appendix A: General instruction

Welcome to our experiment!

Please read through the instructions carefully. You are not allowed to communicate with other participants by any means during the experiment.

If you are not clear about the experiment, please read through the instructions once again. For any further questions please raise your hand and we will come to answer your questions individually.

Your payoff will be expressed in points. The amount of the points depends on the decisions made by you and the other participants. After the experiment, we will exchange the points into RMB/Euro according to the following exchange rate:

100 Points = 75 RMB / 18.75 Euro

Besides, each participant will receive 10 RMB / 4 Euro for participating in the experiment.

During the whole experiment, please make the decision on your own. Be sure not to communicate with other participants in any way, or else you have to be ruled out of the experiment.

All the data and answers will be analyzed anonymously. To ensure anonymity, you have been instructed to choose a code number. Please find your seat in the cabinet with the corresponding number and make your own decision during the experiment. We can match decisions only to code numbers, but not to persons.

Appendix B: Instruction for treatment VOT

There are 25 participants in the experiment.

This Experiment has only one round!

At the beginning of the experiment, each participant is allocated randomly into a five person group. The decisions within each group are independent of the other groups, that is to say, your decision only influences your own group members. There are two types of players in each group: 3 participants are citizens and the other 2 are candidates. One of the 2 candidates will be elected by the 3 citizens to be the president.

The elected president decides on how to distribute 100 points among the citizens in his group and himself. He can arbitrarily distribute the 100 points between him and the citizens of his group. The president decides how many of the 100 points he will transfer to **each** citizen in his group. The payoff for the president is the difference between the 100 points and the amount he transferred to the citizens. The amount transferred to each citizen is identical, that is, each citizen receives the same amount from the president.

The citizens vote for the president in the following way:

The two candidates choose among 8 descriptive adjectives those 3 that best represent his personality and rank them according to how well the adjectives describe his personality. (The first one is the adjective that best explains his personality, the second one is the second suitable adjective for his personality and the third one represents the third adjective matching his personality.) These 3 ranked adjectives are the personality statements of the candidates.

The citizens see the personality statements of the 2 candidates and elect one of them to become the president. Each citizen has only one vote and the voting result is determined by majority rule, that is, the candidate with at least 2 votes becomes the president. The elected president then makes the allocation decision as described above.

The Steps of the Experiment:

1. At the beginning of the experiment, each participant is randomly allocated to a five person group and receives the role either as a citizen or as a candidate.
2. Each candidate chooses 3 descriptive adjectives that best represent his personality among the 8 descriptive adjectives and rank them according to the conformity of the adjectives with his personality.
3. Citizens see the personality statement of the candidates and elect one of them to become the president.
4. The elected president makes the transfer decision.
5. Each group member is informed on his own payoff.
6. The experiment ends.

How to calculate your payoff in the experiment

1. Citizen: Citizen's payoff = Amount transferred by the president.

   The payoff of each citizen is equal to the amount transferred by the president. The higher the amount is, the higher will be his payoff; the lower this amount is, the lower will be his payoff.

2. President: President's payoff = 100 – Amount transferred to the citizens

   The payoff of the president is equal to the difference between 100 points and the amount he transferred to the citizens. The higher this amount is, the lower will be his payoff; the lower the amount is, the higher will be his payoff.

3. Payoff of the unelected candidate: The computer draws one number from the interval [0, …, 100]. This number is the payoff of the unelected candidate.

Appendix C: Instruction for the treatment RAN

There are 25 participants in the experiment.

This Experiment has only one round!

At the beginning of the experiment, each participant is allocated randomly into a five person group. The decisions within each group are independent of the other groups, that is to say, your decision only influences your own group members. There are two types of players in each group: 3 participants are citizens and the other 2 are candidates. One of the 2 candidates will be randomly selected to be the president.

The randomly selected president decides on how to distribute 100 points among the citizens in his group and himself. He can arbitrarily distribute the 100 points between him and the citizens of his group. The president decides how many of the 100 points he will transfer to **each** citizen in his group. The payoff for the president is the difference between the 100 points and the amount he transferred to the citizens. The amount transferred to each citizen is identical, that is, each citizen receives the same amount from the president.

How to randomly select the president

Computer program select randomly one of the two candidates to be president. The two candidates choose among 8 descriptive adjectives those 3 that best represent his personality and rank them according to how well the adjectives describe his personality. (The first one is the adjective that best explains his personality, the second one is the second suitable adjective for his personality and the third one represents the third adjective matching his personality.) These 3 ranked adjectives are the personality statements of the candidates.

The citizens see the personality statements of the 2 candidates and know which one of them is randomly selected to be the president.

The Steps of the Experiment:

1. At the beginning of the experiment, each participant is randomly allocated to a five person group and receives the role either as a citizen or as a candidate.
2. Each candidate chooses 3 descriptive adjectives that best represent his personality among the 8 descriptive adjectives and rank them according to the conformity of the adjectives with his personality.
3. Citizens see the personality statement of the candidates and know which one of them is randomly selected to be the president.
4. The randomly selected president makes the transfer decision.
5. Each group member is informed on his own payoff.
6. The experiment ends.

How to calculate your payoff in the experiment

1. Citizen: Citizen's payoff = Amount transferred by the president.

   The payoff of each citizen is equal to the amount transferred by the president. The higher the amount is, the higher will be his payoff; the lower this amount is, the lower will be his payoff.

2. President: President's payoff = 100 – Amount transferred to the citizens

   The payoff of the president is equal to the difference between 100 points and the amount he transferred to the citizens. The higher this amount is, the lower will be his payoff; the lower the amount is, the higher will be his payoff.

3. Payoff of the unselected candidate: The computer draws one number from the interval [0, …, 100]. This number is the payoff of the unselected candidate.

Appendix D: Instruction for the treatment VOT-P

There are 25 participants in the experiment.

This Experiment has only one round!

At the beginning of the experiment, each participant is allocated randomly into a five person group. The decisions within each group are independent of the other groups, that is to say, your decision only influences your own group members. There are two types of players in each group: 3 participants are citizens and the other 2 are candidates. One of the 2 candidates will be elected by the citizens to be president.

The elected president decides on how to distribute 100 points among the citizens in his group and himself. He can arbitrarily distribute the 100 points between him and the citizens of his group. The president decides how many of the 100 points he will transfer to **each** citizen in his group. The payoff for the president is the difference between the 100 points and the amount he transferred to the citizens. The amount transferred to each citizen is identical, that is, each citizen receives the same amount from the president.

The citizens vote for the president in the following way:

The two candidates make promises about how many points they are going to distribute to the citizens if they win the election.

The citizens see the promises of the 2 candidates and elect one of them to become the president. Each citizen has only one vote and the voting result is determined by majority rule, that is, the candidate with at least 2 votes becomes the president. The elected president then makes the allocation decision as described above.

The Steps of the Experiment:

1. At the beginning of the experiment, each participant is allocated to a five person group at random and receives the role either as a citizen or as a candidate.
2. Each candidate makes a promise about how many points he will allocate to the citizens if he wins the election.
3. Each citizen sees the promises of the candidates and elect one of them to become the president.
4. The elected president makes the transfer decision.
5. Each group member is informed of his own payoff.
6. The experiment ends.

How to calculate your payoff in the experiment?

1. Citizen: Citizen's payoff = Amount transferred by the president.

The payoff of each citizen is equal to the amount transferred by the president. The higher the amount is, the higher will be his payoff; the lower this amount is, the lower will be his payoff.

2. President: President's payoff = 100 – Amount transferred to the citizens

The payoff of the president is equal to the difference between 100 points and the amount he transferred to the citizens. The higher this amount is, the lower will be his payoff; the lower the amount is, the higher will be his payoff.

3. Payoff of the unelected candidate: The computer draws one number within the interval [0, …, 100]. This number is the payoff of the unelected candidate.

Appendix E: Instruction for the treatment RAN-P

There are 25 participants in the experiment.

This Experiment has only one round!

At the beginning of the experiment, each participant is allocated randomly into a five person group. The decisions within each group are independent of the other groups, that is to say, your decision only influences your own group members. There are two types of players in each group: 3 participants are citizens and the other 2 are candidates. One of the 2 candidates will be selected to be president randomly by computer.

The randomly selected president decides on how to distribute 100 points among the citizens in his group and himself. He can arbitrarily distribute the 100 points between him and the citizens of his group. The president decides how many of the 100 points he will transfer to **each** citizen in his group. The payoff for the president is the difference between the 100 points and the amount he transferred to the citizens. The amount transferred to each citizen is identical, that is, each citizen receives the same amount from the president.

How to randomly select the president

Computer program select randomly one of the two candidates to be president. The two candidates make promises about how many points they are going to distribute to the citizens.

The citizens see the promises of the 2 candidates and know which one of them is randomly selected to be the president.

The Steps of the Experiment:

1. At the beginning of the experiment, each participant is allocated to a five person group at random and receives the role either as a citizen or as a candidate.
2. Each candidate makes a promise about how many points he will allocate to the citizens if he becomes the president.
3. Each citizen sees the promises of the candidates and knows which one of them is randomly selected to be the president.
4. The selected president makes the transfer decision.
5. Each group member is informed of his own payoff.
6. The experiment ends.

How to calculate your payoff in the experiment?

1. Citizen: Citizen's payoff = Amount transferred by the president.

The payoff of each citizen is equal to the amount transferred by the president. The higher the amount is, the higher will be his payoff; the lower this amount is, the lower will be his payoff.

2. President's payoff = 100 – Amount transferred to the citizens

The payoff of the president is equal to the difference between 100 points and the amount he transferred to the citizens. The higher this amount is, the lower will be his payoff; the lower the amount is, the higher will be his payoff.

3. Payoff of the unselected candidate: The computer draws one number within the interval [0, …, 100]. This number is the payoff of the unselected candidate.

Chapter III
Sensitivity to Corruption – An Experimental Investigation in China[1]

1 Introduction

Corruption by now is widely accepted to be a major economic problem around the world. In particular, corruption has huge negative welfare effects. Corruption and anti-corruption is hence an important political issue for every country. As defined by Transparency International, corruption is “the misuse of entrusted power for private gain. It hurts everyone whose life, livelihood or happiness depends on the integrity of people in a position of authority”[2]. The entrusted power mentioned here is usually understood as the authority an officer is endowed with by the state. In an environment where power is extremely asymmetrically distributed among different social classes and officers have enormous such kind of authority, corruption is easier to grow. Husted (1999) found corruption to be significantly correlated to power distance. A higher level of asymmetric power distribution most probably will lead to a higher level of corruption.

Developing countries with high growth rate may offer corruption a greenhouse to live in. Field evidence shows very few developing countries to have low levels of corruption (Khan 2006). It has been argued that such countries can bear the costs of corruption, or a certain degree of corruption can even speed up economic growth for these countries (Leff 1964; Bardhan 1997; He 2000). Yet, as Rose-Ackerman (2006) pointed out it is not only economic growth that matters. If corruption is indulged, it is very likely that social fairness and social harmony would reach a critical unbalance and lead to a series of social problems. These factors play a not ignorable role for the further economic growth. Recent research also shows the other side of the relationship between corruption and growth. Mauro (1995, 1997) and Mo (2001) find that corruption can also lower economic growth. Therefore, anti-corruption policy is of special importance for countries developing at a high rate.

Most anti-corruption researches so far suggest institutional policies as the main method against corruption. Ades and Di Tella (1999) propose that policies should make markets more

[1] based on: “Sensitivity to Corruption – An Experimental Investigation in China” by Hong Geng (2009), Mimeo, University of Bonn.
[2] See the home page of Transparency International http://www.transparency.org (date of download: 10.01.2009). Transparency International is a global civil society organization leading the fight against corruption.

competitive in order to control corruption more efficiently. Yang (2006) suggests using private firms as anti-corruption monitors in some public areas like taxation. In fact, many such institutions work efficiently against corruption, as e.g. the Independent Commission Against Corruption (ICAC) of Hong Kong.

Besides the attempts to adjust current anti-corruption policies or to construct new organizations, one more basic and general task in fighting corruption is to increase people's sensitivity to corruption. Or more precisely, all anti-corruption attempts' ultimate aim is not only to reduce current corrupt activities, but also to decrease the incentives to engage in corruption. Rousso and Steves (2003) show perceptions of corruption are positively correlated with the intensity of anti-corruption programs. Hauk and Saez-Marti (2002) suggest that educating the young generation is a key element in reducing corruption successfully since it changes young people's values and thus lowers their perception of corruption.

The focus of our research is to study whether people's sensitivity to corruption can be affected by directing people's attention to the bribery context. We study this issue by using the bribery game developed by Abbink, Irlenbusch and Renner (2002) in a neutral and a loaded scenario as applied by Abbink and Hennig-Schmidt (2006) (hereafter AHS). The framing aspect is analyzed presenting the task either as an interaction between a firm and a public official, where the firm can make private payments to get permission for running a plant. The firm's activity causes negative consequences to the public. In the neutral setup, the game is presented in a completely neutral way. If the loaded instruction can transfer the corruption message correctly to the subjects, we may observe a causal lower perception of corruption.

In their experiment conducted at the *Laboratorium für experimentelle Wirtschaftsforschung* (*B*onn*E*con*L*ab) at the University of Bonn, Germany, AHS (2006) did not find evidence supporting the hypothesis that the level of corruption is lower when subjects were guided by the loaded instruction. This finding, however, may be different in another culture of corruption. We deliberately choose to re-run the experiment in China. Compared to Germany, the Chinese corruption culture differentiates in the following way. First, the status quo of corruption is strait. According to the annual Corruption Perception Index (CPI) of Transparency International from 2003 to 2008, China's average score for the last six years is 3.43 out of a no-corruption score of 10 and ranks usually around place 70[3]. Second, the power distance and human inequality among different social groups is great (see Hofstede 1984 &

[3] Germany's average CPI score is 7.87 and belongs to the 20 most uncorrupted countries.

2001). The Chinese officers, who are often treated as a privileged class, have more power than citizens. Third, the economic growth rate is high. China is experiencing its transition period and has had persistent double-digit growth rates since 20 years.

As Schramm and Taube (2002) pointed out that from different perspectives "provision of certain goods or the implementation of transactions" may have different interpretation: from the perspective of a legal system which is independent of personal relationship, it is corruption; but from the perspective of a relationship, it appears as normal and even necessary within the network. Since Chinese are famous for their dependence on personal relationship network (Schramm and Taube, 2002), the strong trust-reciprocity design of the experiment suits particularly to study whether a private relationship which has negative public consequences can be restricted by bribery context.

We cannot reproduce AHS' results in the Chinese sessions. The experimental results show that although firms are not affected by the different corruption context, public officers, however, both accept fewer bribes and grant less permissions when they are facing the loaded instruction.

The reminder of this paper is organized as follows. In section 2, we introduce the experimental design and procedure. We present the results of our experiment in section 3, conclude and discuss our findings in section 4.

2 Experimental design and procedure

2.1 Experimental design

In our experiment, we use the design of Abbink and Hennig-Schmidt (2006) as a workhorse. The design catches three basic features of a bribery scenario: (i) trust and reciprocity between briber and bribee, (ii) negative externalities on society, (iii) a certain but low chance that a bribery action will be detected.

The wording in the loaded treatment describes the following bribery story. A firm wants to run a plant which causes negative consequences to the public. In order to run the plant, the firm needs to get a permission from a public official. Before the public official makes her choice on whether to grant the permission, the firm can offer her a private payment. The public official can accept or reject the private payment.

18 players, 9 firms and 9 officers, attend one session. Each firm is matched to a public official. As bribery in related real-life situations is usually a long-term relationship, the matching is fixed for the whole experiment. The experiment consists of 30 rounds. In each round, the firm first chooses whether to offer his matched public official a private payment. If he does, he has to pay 2 points as sunk transaction costs. Further, he has to choose a transfer amount from the interval [1, 2, … , 9]. As the marginal utility of the transfer is assumed to be larger for the public official than for the firm, the private transfer is tripled in case of acceptance by the public official. Moreover, a number from the interval [0, 1, … , 999] is randomly drawn if the public official accepts the private transfer, e.g. when a bribery action is completed. In case the random number is 0, 1 or 2 (equaling a probability of 0.3%), the corrupt action of this pair's firm and officer is detected. As punishment, these two players are disqualified for the remainder of the experiment and their accounts are cleared. They were paid the lump-sum show-up fee of RMB 10Yuan only. If the bribery action is not detected, i.e. the random number is bigger than 2 (equaling a probability of 99.7%), the public official must decide whether to grant the permission in this round. If she does, she receives 30 points and the matched firm receives 56 points as round payoffs. The remaining society consisting of all other 16 players are affected because granting the project has negative consequences for the public: Every permission reduces the payoff of each member of the remaining society by 3 points. If the public official does not permit the project, she and her matched firm both receive 36 points. The remaining society is not affected. After all decisions of a round have been made, the subjects were informed about their payoffs resulting from their own pair's decisions. They were reminded that their payoffs would also be influenced by the decisions of all other pairs in the experiment. An overview of the experimental procedure of one round is provided in figure 1.

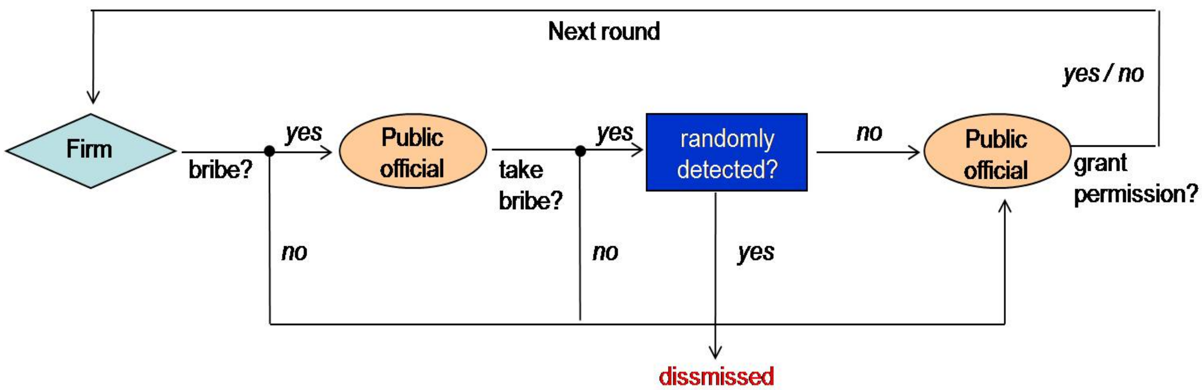

Figure 1: Sequence of actions in one round

In the control treatment, the instruction is written in neutral framing. The main differences in wording used in the loaded and neutral treatments are listed in table 1. In addition to the replacement of expressions, the paragraph describing the real-life situation modeled in the experiment was deleted.[4]

Table 1: Wording used in the loaded and the neutral treatment[a)]

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

[a)] Source: AHS (2006)

The original instructions are written in German[5]. The translation of instructions and into Chinese was done by native speakers applying the back-translation method (Brislin 1970; Eco and McEwen 2000). An experimental instruction of each treatment is provided in the appendix.

2.2 Payoff functions and game-theoretic solution

Payoff functions for firm (π_F) and public official (π_{PO}) vary in different cases:

- In case no bribe is offered:

$$\pi_F = \pi_{PO} = 36, \text{ permission is not granted}$$

$$\pi_F = 56,\ \pi_{PO} = 30, \text{ permission is granted}$$

- In case a bribe offer is denied:

$$\pi_F = 36 - 2 = 34,\ \pi_{PO} = 36, \text{ permission is not granted}$$

$$\pi_F = 56 - 2 = 54,\ \pi_{PO} = 30, \text{ permission is granted}$$

- In case a bribe offer is accepted being not detected:

$$\pi_F = 36 - 2 - \text{bribe offer},\ \pi_{PO} = 36 + 3*\text{bribe offer}, \text{ permission is not granted}$$

$$\pi_F = 56 - 2 - \text{bribe offer},\ \pi_{PO} = 30 + 3*\text{bribe offer}, \text{ permission is granted}$$

The standard game-theoretical solution for the bribery game is that the firm will never choose to bribe the public official and the public official will never grant the permission[6]. This translates directly into a corruption-free environment in our experiment: no bribe and no negative externality!

[4] See payoff tables and instructions of the loaded and the neutral treatment in English in Appendix A.1 to A.3. Instructions in German and Chinese are provided by the author upon request.

[5] We thank Klaus Abbink and Heike Hennig-Schmidt for providing their experimental instruction.

[6] See Abbink (2000) for proof.

2.3 Experimental procedure

The experiment was conducted at *Laboratory for Economic Research* of Nankai University, Tianjin, China. Student participants were almost all undergraduate students majoring in various disciplines who were recruited by campus advertisements promising a monetary reward for participation in a decision-making task. Every subject could participate in one session of the bribery experiment only. Overall 144 subjects participated in our experiment.

All sessions were computerized, using the software *zTree* (Fischbacher 2007). The design of the experimental software was the same in all treatments, except that in the neutral instructions condition the bribery story has been replaced by the corresponding neutral wording. For each treatment, we conducted two sessions. 18 subjects participated in each session. Since each session comprises nine statistically independent observations, we obtain 18 independent observations in each treatment.

After arrival, subjects were allocated to their computer terminals by random draw. They were seated in cubicles, visually separated from one another by curtains. The terminal numbers also determined the role of a subject as being firm or public official. After the subjects had been seated, each session began with an introductory talk. The instructions were read aloud by the experimenter and were explained in detail. Payoff tables were handed out to increase the transparency of the game. After the introduction, subjects were encouraged to ask questions that were answered in private. Then the experiment started. Every experimental session lasted for about 1.5 hours including instructions.

Immediately after the session, subjects were paid anonymously in cash. The exchange rate is RMB 0.045Yuan per point. In addition, a show-up fee of RMB 10.00Yuan was paid. The monetary reward was calculated to equal the hourly wage in a typical students' job. The total earnings in points ranged from 726 to 1489 with an average of 928.50. Calculated in the real currency, subjects gained an average payoff of RMB 53.43Yuan (approximately 5.34€).

3 Results

3.1 Choices of firms

We measure firms' level of corruption with two variables. The relative bribe frequency $f_B = \frac{\sum(rounds\ of\ offering\ bribe)}{\#\ of\ rounds\ played}$ measures how often firms intend to make bribe offers. The average bribe transfer $T_B = \frac{\sum(bribe\ transfers)}{\#of\ rounds\ played}$ mirrors how much firms are ready to pay to get the permission.

Table 2 presents f_B and T_B of each firm in both treatments ordered according to f_B. The game theoretic prediction of no bribe cannot be observed in the data. Out of all 36 firms, 17 (94.44%) firms in the neutral and 16 firms (88.88%) in the loaded treatment bribed at least once. On average, the f_B is 12% lower in the loaded treatment (35%) than in the neutral treatment (47%). But the Mann-Whitney U test does not deliver evidence rejecting the hypothesis of equal bribe frequencies in the two treatments (p=0.194, two-sided).

Figure 2 shows the distribution of bribe offers. In the neutral treatment, the distribution is obvious two-peak formed with a higher peak at 0 (53%), i.e. no bribe, and a smaller peak at 6 (21%), i.e. the equal-split bribe offer[7]. None of the other eight bribe offers is chosen more than 10%. In the loaded treatment, although bribe offer 0 and 6 are still the two most chosen bribes, the distribution is rather changed into a right-skewed distribution with the single peak at 0 (65%). The relative frequency of choosing 6 as bribe offers is only 8% which is not significant different from other bribe offers like 5 or 9 (both are chosen by 5%). The equal-split bribe offer hence loses its prominence in the loaded treatment. The average bribe offer drops from 2.51 in the neutral treatment to 1.94 in the loaded treatment. The Mann-Whitney U test, however, does not show that the individual T_B are statistically different between these two treatments (p=0.297, two-sided).

Figure 3 shows the average bribe offers for all 30 rounds. In both treatments, we cannot find a positive tendency of bribe offers over time. For each firm, we calculate a linear regression coefficient using bribe offers as dependent variable and the number of round as independent variable[8]. 10 positive and seven negative coefficients emerge in the neutral treatment, while six positive and 10 negative coefficients are found in the loaded treatment. The binomial tests do not reject the null hypothesis that the probability of positive and negative coefficients is equal, neither in the neutral (p=0.629) nor in the loaded treatment (p=0.454). The Mann-Whitney U test cannot detect different trends of bribe offers over time between the two treatments (p=0.214, two-sided).

Result 1: *The firms' level of corruption, neither the bribe frequency f_B nor the bribe offer T_B, is significantly different between the neutral and the loaded treatments.*

[7] When public officials accept a bribe offer of 6 points and then grant the permission to the firm, then both parties receive an equal payoff of 48 points.

[8] The linear regression coefficient is 0 for firms who do not bribe at least once.

Table 2. Bribe frequencies (f_B) and average bribe offers (T_B)

neutral			loaded		
Pair	f_B	T_B	Pair	f_B	T_B
13	0.00	0.00	6	0.00	0.00
14	0.07	0.07	13	0.00	0.00
16	0.07	0.10	3	0.03	0.03
6	0.10	0.60	8	0.07	0.30
7	0.13	0.20	12	0.10	0.27
1	0.20	0.70	2	0.13	0.90
5	0.27	1.80	1	0.27	0.80
15	0.47	1.90	17	0.30	1.77
17	0.50	2.33	18	0.37	1.47
10	0.60	2.07	10	0.40	0.60
2	0.67	4.03	5	0.43	2.40
3	0.67	4.80	16	0.43	3.50
11	0.73	3.60	7	0.47	2.63
9	0.77	4.30	9	0.50	4.37
4	0.80	4.63	11	0.53	3.27
18	0.80	4.73	15	0.57	3.97
12	0.83	4.53	14	0.80	4.07
8	0.87	4.70	4	0.90	4.57
Mean	0.47	2.51	Mean	0.35	1.94
SD	0.31	1.91	SD	0.26	1.67

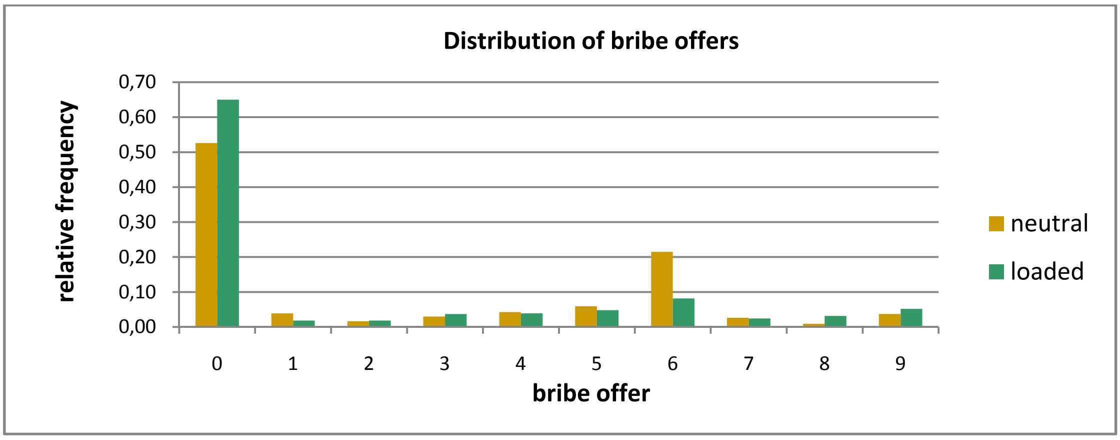

Figure 2: Distribution of bribe offers

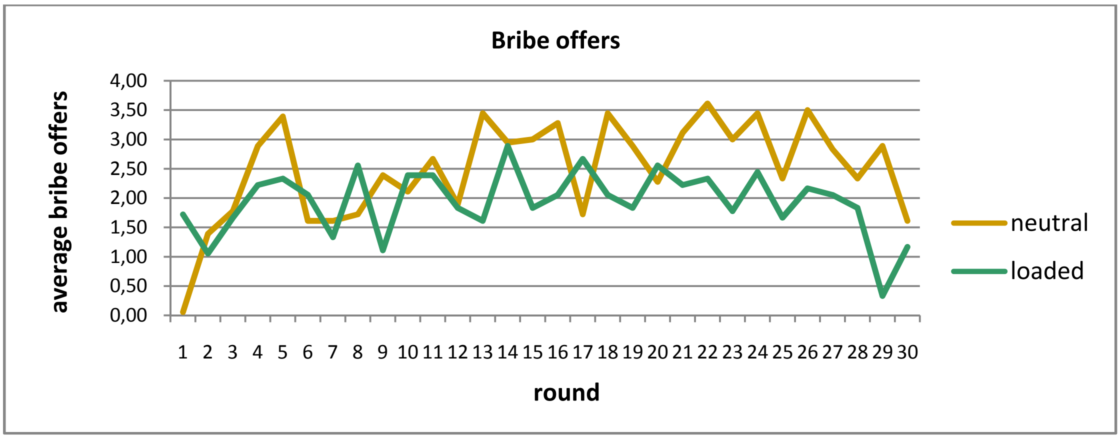

Figure 3: Bribe offers

3.2 Choices of public officials

We measure public officials' level of corruption with two variables. Public officials' acceptance level of bribe offers, defined as the ratio of accepted bribes over the bribe offers $A = \frac{\sum(accepted\ bribes)}{\sum(bribe\ offers)}$, measures their corruptibility. The relative frequency of permissions $f_P = \frac{\sum(rounds\ of\ permission)}{\#\ of\ rounds\ played}$ measures "the extent to which decisions have been manipulated by bribery" (AHS, 2006).

Figure 4 shows the aggregated acceptance frequencies on bribe offers. Except on bribe offer 1, the acceptance frequencies are lower in the loaded treatment than in the neutral treatment. Tabel 3 shows the A and f_P of each public official in both neutral and loaded treatments ordered according to f_P. On average, 78% bribe offers are accepted in the neutral treatment. The acceptance level declines 19% in the loaded treatment (59%). The Mann-Whitney U test rejects the null hypothesis that the acceptance levels of bribe offers are equal in the two treatments (p=0.047, two-sided). Figure 5 shows the relative permission frequencies f_P for all 30 rounds. In only two rounds (round 21 and 30), the f_P are higher in the loaded treatments than in the neutral treatments. In 25 rounds, the f_P are lower in the loaded treatment. In the remaining rounds, the f_P are equal in the two treatments. On average, the f_P is 20% less in the loaded treatment (21%) than in the neutral treatment (41%). The Mann-Whitney U test rejects the null hypothesis that the permission frequencies are equal in the two treatments with a weak significance level (p=0.064, two-sided).

Result 2: *Both public officials' acceptance levels of bribe offers and their permission frequencies are significantly lower in the loaded treatment than in the neutral treatment.*

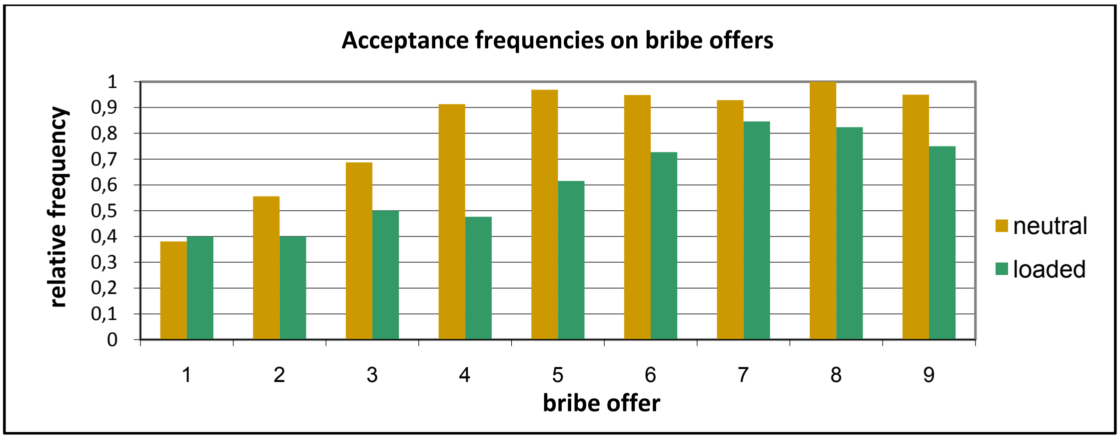

Figure 4: Acceptance frequencies on bribe offers

Table 3. Acceptance level of bribes (A) offers and permission frequencies (f_P)

neutral			loaded		
Pair	A	f_P	Pair	A	f_P
13	–[a)]	0.00	6	–[a)]	0.00
16	1.00	0.00	13	–[a)]	0.00
1	0.00	0.03	8	0.00	0.00
14	0.50	0.03	18	0.00	0.00
6	0.67	0.10	17	0.43	0.03
7	0.00	0.13	12	0.75	0.03
10	0.94	0.23	1	0.92	0.07
5	0.67	0.30	3	0.00	0.10
9	0.90	0.50	16	0.55	0.10
17	0.93	0.50	2	1.00	0.10
4	0.94	0.53	5	0.42	0.20
15	0.88	0.63	11	0.43	0.20
12	0.95	0.63	10	0.67	0.20
11	0.97	0.63	15	0.71	0.33
2	1.00	0.67	7	0.78	0.40
3	0.99	0.77	4	0.88	0.40
8	1.00	0.77	14	0.98	0.77
18	1.00	0.83	9	0.95	0.90
Mean	0.78	0.41	Mean	0.59	0.21
SD	0.33	0.30	SD	0.35	0.26

[a)] – : the matched firm does not offer a bribe.

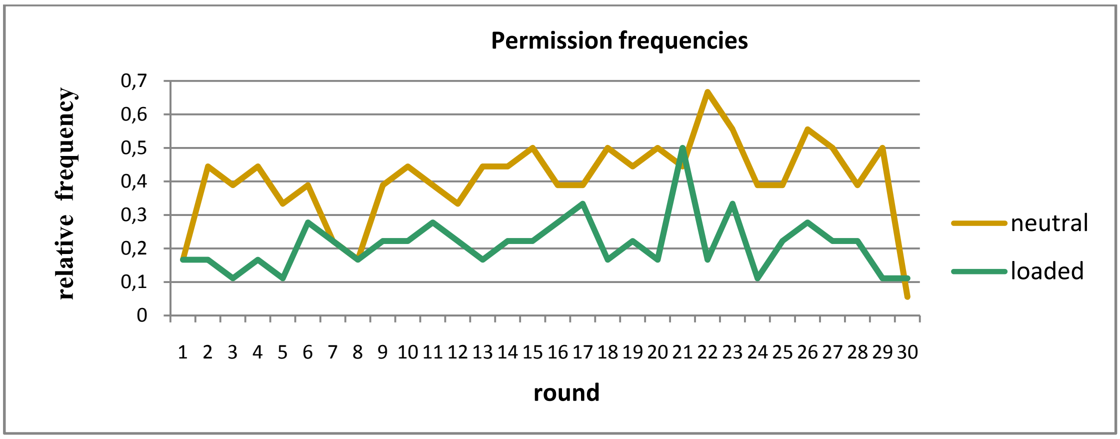

Figure 5: Permission frequencies

4 Conclusions and discussions

In this paper, we aim to exam sensitivity to corruption in China using a bribery experiment by AHS (2006). The treatments vary in the experimental instructions with either neutral framing or context-loaded framing of a bribery scenario. Contrary to their findings of AHS (2006) that German subjects are not affected by different contexts used in the experiment, we find that Chinese subjects' behaviour is not stable across different treatments. While firms still do not behave differently in the two treatments, public officials accept fewer bribe offers *and* consequently grant less permissions.

Many studies on corruption point to cultural causes for corruption and hence suggest country-depending treatment against corruption (see Husted 1999; Treisman 2000; Fisman and Gatti 2002; Kaufmann, Kraay and Mastruzzi 2006; Lambsdorff 2006). Abbink (2006) also encourages that more cross-cultural corruption experiments should be done to study the impact of culture on corruption. Our findings deliver support for the encouragement. More precisely, our findings suggest a model in which anti-corruption sensitivity is depending on culture and leave for future research to find out what is the most effective strategy to fight against corruption in different countries.

Two questions aroused after analyzing our experimental results: What is the reason of behavioral deviation in the Chinese treatments? Why such deviation is only observed by public officials? We now try to give some plausible explications to these questions.

4.1 Is the framing effect caused by monitoring?

To our best knowledge, our experiment is one of the first experiments conducted at the Laboratory for Economic Research of Nankai University. At the time of our experiment, experimental economics was a new discipline in China and subjects were not familiar with this method. As subjects had to make their decisions in isolated cubicles without any communication with other participants, they may find the environment odd. We may also not forget that our subjects are from a centralized country. They care about how they would be judged by others. Hence we cannot exclude possible monitoring effect that subjects may feel being watched by the experimenters in the experiment.[9] However, we take this issue for another proof for the framing effect. In our experiment, subjects, both in loaded and in neutral treatments, made their decisions under *identical* physical conditions. They faced the same experimenter and used the same laboratory. If they felt being observed, they should have such feeling in both treatments. Yet, the loaded instruction may trigger a stronger reaction of subjects that somebody is watching whether they are doing the right thing. For example, the loaded text may let subjects fear that the experiment is actually a hidden government test with possible career consequences. Anticipating this purpose, subjects may behave more morally in the loaded treatment. Future research can use double-blind process to study how subjects behave when the concern of monitoring is removed.

[9] We owe this point to Prof. Dr. Dr. h.c. mult. Reinhard Selten.

4.2 Do real-life experiences play a role?

We suspect that the different responses to the bribery contexts of German subjects as in AHS' experiment on the one hand and that of Chinese subjects as in our experiment on the other hand may due to subjects' different experiences of current anti-corruption publicities of Germany and China. The anti-corruption campaign is often case-based in Germany (for example the Siemens bribery scandal in 2006) while it is a hot topic in the daily life of Chinese people. In China, people every day get lots of anti-corruption information through all media channels. People's tendency to engage in corruptions may be imperceptibly reduced by learning these messages repeatedly. When they once meet comparable scenes, as in the loaded treatment of our experiment, their instinct tells them not to step into it. A neurobiological experiment may reveal more details in this suggestion. In the experiment of De Martino, Kumaran, Seymour and Dolan (2006), they found that subjects' brain activities are different when they face "Gain frame" of a trust game versus when they face "Loss frame" of the same game. Dolan, Fink, Rolls, Booth, Holmes, Frackowiak and Friston (1997) proved that "perception is a conjoint function of current sensory input interacting with memory and possibly attentional processes" (p.599). We hope future neurobiological researches can provide further evidence that an implicit learning process may influence subjects' corruption decision-making when they are stimulated by real-life based guide.

4.3 Why only public officials are affected by the bribery context?

As we mentioned before, the experimental design reflects the natural relationship of trust and reciprocity in a bribery scenario. Other than game theory predicts, firms and public officials have dynamic incentives to cooperate through the bribery action for higher payoffs. Firms can get better paid off if the permission is granted. Public officials can gain a higher payoff through firms' bribe offers.

The additional profit a firm benefits from a successful bribery is:

$$56 - 36 - (T_B + 2) = 18 - T_B$$

The additional profit a public official benefits from a bribe offer is:

$$30 + 3 * T_B - 36 = 3 * T_B - 6$$

As $T_B \in [0, \dots, 9]$, firms always have a positive additional profit if they get the permission. But for public officials, it is only then profitable for them to take the bribe offer and grant the permission when the bribe offer is greater than 2.

Now let us review the data of our experiment. The average bribe offer is 2.51 in the neutral treatment and 1.94 in the loaded treatment. That is to say, public officials can have, on average, an additional gain of 1.53 in the neutral treatment. But in the loaded treatment, it is not worth for the public officials to accept the bribe offers as they would have an average loss of 0.18 through their reciprocal bahaviour of granting the permission.

This finding is in accordance with the motivation of bribers and bribees in real life: When a firm decides to bribe a public official, he knows *ex ante* that his potential benefit is much higher than the bribe costs he pays. For public officials, the private gain must be high enough so that she would abuse her power.

The findings in our experiment may have some important meanings for policy-makers in China. In order to lower the level of corruption, it is easier to control public officials than to control private firms. As firms are always motivated to offering bribes for higher corporate gain, public officials' intention to corruption may be reduced sharply by receiving correct messages.

Appendix A: Experimental instructions (original in German and Chinese)

Appendix A.1: Instruction Loaded Treatment

Welcome to our experiment

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 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.**

Also ranomly, pairs of participants are formed; one firm and one public official are matched to one another. **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.

One Pair's 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 [in China: points]**, and the play is continued with stage 2. If the firm does not want to offer a private payment, then his credit remains unchanged, and the play is continued with stage 4.

Stage 2: The amount to be offered

The firm has to decide 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 [points]**. 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** of the accepted private payment. If a private payment is made and accepted, then this can be discovered with a certain probability. This is decided by randomly drawing a number out of the range from 0 to 999.
    - ⇨ If the 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 experiment ends for these two participants, and they do not receive any payment for whole experiment, i.e. also the talers [points] that have been earned in the past are cleared from their accounts.** (In the end of the experiment, both participants receive the show up fee, see below). The two disqualified participants have to wait until the experiment has ended. For the other participants, the experiment is continued normally.
    - ⇨ If the number is **3, 4, ..., 998, or 999**, then the private payment is not discovered, and the experiment is continued with stage 4.
- If the public official rejects the transfer, then the credits remain unchanged (Attention: Even if the public official rejets the private payment, the offer costs from stage 1 have to be paid). 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 grants the permission, then the firm's credit is increased by **56 talers [points],** whereas the public official's credit is increased by **30 talers [points]**. The credit of each of the 16 other participants is **decreased** by **3 talers [points]** by this decision.
- 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 [points]** each. The credits of the 16 other participants are **not changed** by this decision.

<u>Attention:</u> By each of the eight other pairs, in which a permission is granted for the firm, the payoff for **the firm** as well as for **the public official** is decreased by **3 talers [points]**, i.e. at maximum eight times 3 and at minimum no talers [points] 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.

Possible deductions by decisions of other pairs are not included in the round payoffs. They are considered only at the end of the experiment.

The payoffs

You receive your payoff at the end of the experiment, where the exchange rate is Euro 2.00 [RMB 4.50] for 100 talers [points]. In addition, you receive a show up fee of Euro 4.00 [RMB 10.00].

Appendix A.2: Instruction Neutral Treatment (original in German and Chinese)

Welcome to our experiment

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

Also ranomly, pairs of participants are formed; one player A and one player B are matched to one another. **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.

One Pair's Decision Situation in a Round

Stage 1: Offer of a transfer

First, the plyer A decides whether or not he wants to offer a transfer to the player B. If he does, then the credit of the player A is reduced by offer costs of **2 talers [in China: points]**, and the play is continued with stage 2. If the player A does not want to offer a transfer, then his credit remains unchanged, and the play is continued with stage 4.

Stage 2: The amount to be offered

The player A has to decide on the amount to be transferred to the player B. The player A can choose between **1, 2, 3, 4, 5, 6, 7, 8 or 9 talers [points]**. The play is continued with stage 3.

Stage 3: Acceptance or Rejection of the transfer

The player B decides on whether he **accepts** or **rejects** the proposed transfer.

- If the player B accepts the transfer, then the credit of the player A is reduced by the amount he proposed. The player B's credit is increased by the tripled amount of the accepted transfer. Then a random number out of the range from 0 to 999 will be drawn.
  - ⇨ If the number is **0, 1, or 2**, then the player B and the player A with whom the player B is matched are disqualified. That means: **The experiment ends for these two participants, and they do not receive any payment for whole experiment, i.e. also the talers [points] that have been earned in the past are cleared from their accounts.** (In the end of the experiment, both participants receive the show up fee, see below). The two disqualified participants have to wait until the experiment has ended. For the other participants, the experiment is continued normally.
  - ⇨ If the number is **3, 4, ..., 998, or 999**, the experiment is continued with stage 4.
- If the player B rejects the transfer, then the credits remain unchanged (Attention: Even if the player B rejects the transfer, the offer costs from stage 1 have to be paid). The play is continued with stage 4.

Stage 4: Decision on X or Y

The public official chooses X or Y**.**

- If the player B chooses X, then the player A's credit is increased by **56 talers [points],** whereas the player B's credit is increased by **30 talers [points]**. The credit of each of the 16 other participants is **decreased** by **3 talers [points]** by this decision.
- If the player B chooses Y, then his credit and the credit of the player A matched with him are increased by **36 talers [points]** each. The credits of the 16 other participants are **not changed** by this decision.

Attention: By each of the eight other pairs, in which player B chooses X, the payoff for **the player A** as well as for **the player B** is decreased by **3 talers [points]**, i.e. at maximum eight times 3 and at minimum no talers [points] are deducted from the player A's and the player B'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.

Possible deductions by decisions of other pairs are not included in the round payoffs. They are considered only at the end of the experiment.

The payoffs

You receive your payoff at the end of the experiment, where the exchange rate is Euro 2.00 [RMB 4.50] for 100 talers [points]. In addition, you receive a show up fee of Euro 4.00 [RMB 10.00].

Appendix A.3: The Payoff Tables

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

Round payoff if (player B) [the public official] **accepts (**a transfer) [a private payment] and the randomly drawn number is bigger than 2

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

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

(transferred amount) [private payment]	1,...,9	
(Player B's got) [public officer got]	0	
(Player B's decision) [permission granted?]	(X) [yes]	(Y) [no]
Payoff (... player A) [... firm]	54	34
(... Player B) [...public official]	30	36
... each of the other 16 participants	–3	0

Round payoff if (player A) [the firm] **does not** (transfer an amount) [offer a private payment]

(transferred amount) [private payment]	0	
(Player B's got) [public officer got]	0	
(Player B's decision) [permission granted?]	(X) [yes]	(Y) [no]
Payoff (... player A) [... firm]	56	36
(... Player B) [...public official]	30	36
... each of the other 16 participants	–3	0

Each of the 8 other pairs in which (X is chosen) [a permission is given] decreases the payoff for (player A and player B) [the firm and the public official] by another 3 talers each.

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