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Distributive fairness in an intercultural ultimatum game*

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Abstract

Does geographic or (perceived) social distance between subjects significantly affect proposer and responder behavior in ultimatum bargaining? To answer this question, subjects once play an ultimatum game with three players (proposer, responder, and dummy player) and asymmetric information (only the proposer knows what can be distributed). Treatments differ in their geographical scope in that they involve either one or three subject pools which, in the latter case, structurally differ in their between-subject pool heterogeneity. Observed choice behavior corroborates several stylized facts of this class of ultimatum games which are primarily explained by strategic play and other-regarding preferences. While the extent of self-interested allocation behavior in proposers significantly varies across sites, neither proposers nor responders meaningfully condition their choices on their co-players' provenance or affiliation. Altogether, we do not discern articulate discriminative behavior based on geographic or social distance.

JEL classification C70, C91, D63

Keywords Equity; fairness; social preferences; ultimatum bargaining; redistribution; cross-national experiment

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1 Introduction

Allocation behavior at the workplace, in markets, and in private life has long been known to frequently be affected by considerations of both procedural (Bies and Shapiro, 1988; Bolton et al., 2005) and allocational fairness (Homans, 1961; Bolton and Ockenfels, 2004).¹ In particular, the latter type of fairness has attracted substantial attention among economists and has been captured by models of inequity aversion that allow for a smooth substitution between equity and own material success (cf., Bazerman et al., 1989; Bolton, 1991; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). In light of the accumulated empirical evidence, there is now a broad consensus that most people are by varying extent, influenced by fairness concerns in situations where they can easily compare the material success of the interacting parties. Specifically, it is well acknowledged that people exhibit significant differences in their willingness to act pro-socially. People differ in how to share economic benefits with others and in reacting to others' proposals (cf., Andersen et al., 2005; Burlando and Guala, 2005; Fischbacher and Gächter, 2006). Similarly, bargaining behavior has been found to depend on socio-demographic characteristics (like age, gender, education level, and income; see, e.g., Botelho et al., 2000), and on the medium of participation (e.g., by mail, fax, or internet; see Güth et al., 2007a).²

In spite of the extensive research to discern and characterize the determinants of equitable distribution behavior, only few studies have tried to relate bargaining behavior to measures of geographical and social distance between the interacting participants.³ Usually, this literature has explored the effects of geographical, social, or cultural affiliation by independently repeating given experiments in several regions or countries (see Roth et al., 1991; Henrich, 2000, 2005, for distinct studies and Oosterbeek et al., 2004, for a recent meta-analysis). In most of these experiments, participants, however, did not directly interact with one another across regions, countries, or even cultures (Tse et al., 1994; Willinger et al., 2003; Ferraro and Cummings, 2007, are rare exceptions). These cross-national and cross-cultural studies were unable - and did not intend - to identify and describe behavioral patterns that result from the direct interaction of participants from different geographical and social

¹ For a general overview of the various and, at times, conflicting fairness concepts, see Konow (2003).

² There is self-selection in participants in the sense that people who respond to the survey via the internet act significantly more selfishly than the more socially-minded users of mail and fax.

³ In a similar vein, i.e., also using the concept of social distance, Buchan and Croson (2004) investigate the boundaries of trust and trustworthiness in interactions between US and Chinese citizens and report this factor as being significant.

provenances.⁴ By now several empirical studies indicated that negotiation processes and outcomes in cross-cultural interactions structurally differ from inter-cultural ones (e.g., Adler and Graham, 1989), but - to the best of our knowledge - there exist only relatively few inter-cultural studies on strategic bargaining behavior.

Our primary concern is to elicit whether, and if so, how decision behavior is affected when people from different geographic provenances and/or distinct social backgrounds are placed in direct interaction with each other. Unfortunately, this question immediately brings up another crucial issue; the one of discrimination. Although discrimination has, at least in its popular understanding, a largely negative connotation, it need not be unconditionally stigmatized. Under certain conditions, discrimination may be justifiable and, hence, valid; namely if the differential treatment is based on generally acceptable grounds and aims at improving social integration and coherence (e.g., special treatment for handicapped people and disadvantaged minorities). In the setting of our experiment, however, discrimination does not seem warranted. In addressing this issue, we will mainly draw on the rich sociological literature on discrimination of individuals and minority groups (Becker, 1971; Akerlof, 2005) which is, occasionally, also discussed as stigmatization (Kurzban and Leary, 2001; Link and Phelan, 2001).

When using the term discrimination, we think of the “treatment or consideration of others based on class or category rather than individual merit” whereby individual merit, in turn, relates to the “demonstrated excellence in an ability or achievement.”⁵ In the domain of economics, individual merit may be thought of as being closest in connotation to the concepts of productivity and efficiency. According to the above definition, discrimination thus is only legitimate if it is applied at the individual level and if any unequal treatment is exclusively based on individual performance differences. Any generalizing form of discrimination, which does not take the individuals’ particular characteristics into account, hence must be considered as illegitimate. Altogether, we deem it important to better understand those factors that influence individuals’ bargaining choices and, ultimately, shape their economic success. If the measure of geographic and social distance between the interaction partners actually were to be behaviorally significant - while it morally should not be - then we would in fact observe unjustified discrimination as a regular feature in intercultural bargaining.

⁴ We distinguish between a “cross-cultural” and an “intercultural” study as follows: While a cross-cultural study involves participants from distinct subject pools, but does not make these subjects to directly interact, an intercultural study, to the contrary, pursues exactly that objective.

⁵ Stigmatization, in contrast, refers to the act of disapproving or condemning and, hence, seems to more strongly rely on previously having observed negative deeds of the individual or group under consideration, respectively on being able to inquire the former’s (negative) history.

Bearing in mind that unwarranted discrimination typically is inefficient, it implies an economic cost. More specifically, the cost of economic discrimination is twofold: On the one side, it reduces the incomes of the affected minorities that are being discriminated against (e.g., pay differences for workers with the same ability but of different ethnic groups or gender). On the other side, unwarranted discrimination may also hurt the perpetrators themselves in that the latter miss potential synergies and profit opportunities. In particular, discrimination in otherwise competitive environments such as labor and financial markets may have strongly negative implications (see, e.g., Becker, 1971). If the former is merely based on demographic characteristics such as age, gender, and ethnicity, it may backfire if qualified labor is only insufficiently available and, as a result, vacant positions cannot be optimally filled (Arrow, 1998; Darity and Mason, 1998). In the same vein, discrimination in (mortgage) lending can likewise lead to economically unsatisfactory results (Ladd, 1998; Han, 2004).

If the propensity of economic agents to act fairly, in the sense of sharing surplus equitably, significantly depends on coarse measures of geographical, social, or cultural provenance, this would have important economic implications. Once, it would suggest that, when bargaining in unfamiliar social environments, a large fraction of people make use of categorical and, hence, largely undifferentiated decision rules (prejudices) which may be inspired by own and others' images of the respective other (stereotypes) (see Devine, 1989; Brewer, 1999). Further, if categorical discrimination already were to be regularly observed in a relatively neutral laboratory setting, it may be even more pronounced in practical economic interactions in which cues for triggering prejudiced behavior are more frequent. Finally, if any systematic discrimination were to be observed, sensitizing people and making them aware of the economically detrimental effects of stereotyping and undifferentiated discrimination may open up avenues for reaching more efficient interaction outcomes that are also individually preferable (e.g., Petty et al., 1997).

Our study intends to explore behavioral regularities in a three-person ultimatum bargaining experiment (featuring a proposer, responder, and dummy player) in which participants interact at various levels of geographic and social proximity, i.e., either locally, between regions, or internationally. Ultimately, this paper strives to (1) improve our general understanding of the effect(s) of geographical and social distance in bargaining and to (2) quantify these factors in the context of distribution behavior. More specifically, we conduct an experiment in which the participants' geographical and social distance constitutes a treatment variable which is systematically varied.

The remainder of this paper is organized as follows: In the following section, we selectively survey the recent economic literature on social preferences and point out some influential experimental and theoretical findings on location effects and related factors of social identity. In section 3 we introduce the experimental design and procedure. Section 4 comprises the data analysis and highlights our central findings. In section 5, these results are then related to the literature and a conclusion is drawn.

2 Related literature

By now, many studies have made clear that most people exhibit at least some degree of non-selfish preferences, and that the latter's influence on distribution choices is meaningful and consistent (e.g., Güth et al., 1982; Dufwenberg and Gneezy, 2000; Fong, 2001; Charness and Rabin, 2002). In the following, we just focus on how group identity and discrimination may depend on and influence such behavior.⁶

(1) Subjects tend to pay attention to the efficiency of bargaining outcomes and, to a certain extent, give up own payoff if social welfare can be substantially augmented in return, i.e., they make a helpful sacrifice. For this motive it is, of course, necessary that the interaction setting grants subjects the discretion to influence the level of efficiency. In ultimatum games, efficiency concerns may guide both proposer and responder choices.⁷ (2) Subjects may derive a psychological benefit from raising the payoff of the least earning subject in their reference group, a concept which is also known as “maximin preferences” (Rawls, 1971; Charness and Rabin, 2002; Engelmann and Strobel, 2004, cf.,). In ultimatum bargaining, this may induce proposers to take an active interest in the relative payoffs of their co-players. (3) Subjects may suffer a psychological loss from an uneven income distribution within their reference group (cf., Bazerman et al., 1989; Bolton, 1991; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) which may induce the former to share rather equitably in the first place. (4) Subjects tend to appreciate reciprocity which induces them to reward (sanction) acts of others which they perceive as (un)kind (cf., Rabin, 1993; Young, 1996; Dufwenberg and Kirchsteiger, 2004; Falk and Kosfeld, 2006). Given

⁶ All the subsequently mentioned pro-social traits in people can be captured by formal social preference models which concede that the former are inherently self-interested, but also, at least partially, care for the payoffs of others (cf., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Falk and Kosfeld, 2006).

⁷ While efficiency concerns may prompt the proposer to offer significantly positive amounts to both the responder and dummy (because she believes that any lower offer would cause the responder to reject), efficiency considerations may effectively prevent responders from rejecting even disadvantageous offers (since rejecting an offer invariably leads to the least efficient interaction outcome).

the simultaneous-move structure in our model we, however, may at most refer to 'indirect reciprocity' in the sense of "I help you and someone else will help me" (cf., Bolle, 2002; Nowak and Sigmund, 2005). In our setup, it is by definition impossible for the participants to react on the preceding actions of their co-players. And finally, (5) subjects may not feel morally entitled to exploit their superior bargaining position which could, again, invoke the equitable sharing norm (see Hoffman, 1985, on the effects of (lacking) entitlement).

Myopic or narrow self-interest, which corresponds to fully rational behavior, typically is seen as the primary motivator of inequitable distribution proposals. According to the standard mindset of neoclassical economics, a higher income for oneself is universally preferred over a lower one. Nonetheless, we think that there are further determinants - apart from the dominant profit maximization scheme - which may induce even not exclusively selfish people to deliberately avoid equitable outcomes. In our view, the locational and social distance between interacting participants are two such factors. While the former concept is straightforward - as it equates to the geographic distance between the interaction partners - the latter one requires some clarification. We think of social distance as a measure of the "closeness" or "demographic similarity" between players in a strategic interaction and, hence, in the same connotation as in Buchan and Croson (2004) and Buchan et al. (2006). In the literature on social preferences, social distance is commonly linked to minimal groups (in the sense of Tajfel and Turner, 1979) and is thought to be systematically and negatively related to pro-social behavior. In fact, several empirical studies brought up evidence in support of the above assertion (e.g., Hoffman et al., 1996).

Let us therefore briefly illustrate why we think that our experimental approach - which hinges on the concepts of locational and social distance - is relevant when it comes to characterize regular bargaining behavior between more or less heterogeneous agents.

In recent years, an increasing number of laboratory and field studies sought to explore whether aspects of participants' social identity and group association significantly influence their bargaining behavior and, ultimately, the outcomes of their interactions. In this context, people's group association may be discerned at various levels, be it by their geographical provenance (from town to country) or social identity (from social group to ethnicity and culture). In particular, social identity theory (cf., Tajfel and Turner, 1979; Brown, 2000) is now seen as one of the central theories in this field. The theory suggests several important effects at the group level which are based on processes of group categorization, identification, and comparison, and may significantly alter the way in which individuals engage in

bargaining.

Picturing oneself as a member of a specific group tends to motivate people to primarily care for the well-being of their (in-) group. Under certain conditions, this goal may be pursued even at the expense of others, namely the out-group. Along these lines, ingroup favoritism may induce discriminatory and, potentially, even hostile behavior toward the outgroup. This conjecture, however, is controversially discussed in the literature (Brewer, 1999). A recurrent argument in this debate is that members of one group do not purposefully hurt members of the other group, but rather concentrate on furthering their own groups' interests. Hence, outright sabotage does only rarely take place.

At present, accumulated laboratory and field evidence has produced mixed results and thus remains ambiguous. In some studies, social identity-related factors were reported to be significant. In their article on bargaining and market experiments in several countries with heterogeneous cultural backgrounds, Roth et al. (1991) observed significant behavioral differences between the national subject pools (Tokyo, Jerusalem, Ljubljana, and Pittsburgh). After controlling for demographic variables, the authors interpreted these differences as being culturally determined. Similar experimental evidence was provided in an extensive survey on bargaining games (see Roth, 1995). In yet another international study (see Henrich, 2000), bargaining regularities in small-scale (Machiguenga) and Western societies (U.S.) were contrasted and significant differences were revealed. In other studies, however, it is argued that any reported significance of the cultural factor may as well simply result from imperfectly controlling for demographic participant characteristics (e.g., Eckel and Grossman, 2001; List, 2004).⁸ In the study, we therefore strive to answer as unambiguously as possible whether a geographical and social association scheme significantly contributes to structuring the observed data. With help of the two above mentioned distance concepts, we think to be able to rationalize the existence of discrimination in ultimatum bargaining between geographically and socially distant participants. The initial step in this argumentation must be to verify whether discrimination actually takes place and can be systematically observed in our controlled bargaining experiment.⁹ Only if this is the case, it makes sense to start the

⁸ Considering this caveat, we paid close attention to the demographic characteristics of the various subject pools and, for each session, explicitly invited groups of participants with approximately matching socio-demographic characteristics to the respective participating sites.

⁹ In the framework of our experiment, we classify the behavior of proposers and responders from a given location as discriminatory if they systematically vary their offer and response strategies contingent on the identity of their interaction partners. If, for instance, Palestinian proposers, on average, assign a significantly larger share to Israeli responders than to German ones, then this - in our understanding - would constitute discriminatory behavior.

discussion on location- and distance-based discrimination. By establishing a sound empirical dataset in which the geographical and social distance between interaction partners is used as the treatment variable, we are in a position to systematically evaluate the factor's impact on choices in simple bargaining games. At the end of the day, understanding the distance effect in bargaining environments may prove to be very useful in designing effective coordination mechanisms and institutions that govern the economic interplay of agents with differing geographic or cultural backgrounds in the global economy.

3 Experimental model and protocol

3.1 The model

The three players (proposer X, responder Y, and dummy player Z) are asked to distribute a positive monetary amount A of stochastic size among themselves. To this end, X proposes an offer vector (x, y, z) with $x + y + z = A$, $\underline{A} = 80$, and $\overline{A} = 120$. Modeling the pie size as being stochastic is a useful technique to evaluate the extent of self-serving preferences in proposers, taking into account that X may exploit Y's (and Z's) ignorance about the actual realization of A to increase her own payoff while being able to conceal her greedy intention.¹⁰ The game has the following sequential structure:

- X suggests a distribution of \underline{A} and \overline{A} , namely $(\underline{x}, \underline{y}, \underline{z})$ with $\underline{x} + \underline{y} + \underline{z} = \underline{A}$ and $\overline{x} + \overline{y} + \overline{z} = \overline{A}$. The sets of all offer vectors given \underline{A} or \overline{A} , that were available to the participants, are shown in Table 1.
- Nature selects $A = \underline{A}$ with $p = 0.25$ and $A = \overline{A}$ with $(1 - p) = 0.75$. These a priori probabilities are announced in the instructions. While X always gets informed about the realization of A at the end of the experiment, Y never learns about A 's realization and only receives limited information on X's offer vector, i.e., only the fragment (x, y) of the entire offer vector (x, y, z) is disclosed to her. Lastly, dummy Z only learns about z . If, to the contrary, Y and Z were informed about either the realization of A or the amount x , at least Y could unambiguously assess X's true level of generosity. Effectively, this would deprive the proposer of the possibility to hide her greed behind the stochastic pie size.

¹⁰ Our model is partly inspired by the works of Mitzkewitz and Nagel (1993) and Güth et al. (1996) who employ the same technique to explore the implications of incomplete information in several variations of the ultimatum game.

Table 1: Set of available offer vectors

Pie size	Offer vector component	Set of available offer vectors															
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Large (A=120)	x	40	40	40	40	40	60	60	60	60	80	80	80	100	100	100	120
	y	80	60	40	20	0	60	40	20	0	40	20	0	20	10	0	0
	z	0	20	40	60	80	0	20	40	60	0	20	40	0	10	20	0
		1	2	3	4	5	6	7	8	9							
Small (A=80)	x	20	20	40	40	40	60	60	60	80							
	y	40	20	40	20	0	20	10	0	0							
	z	20	40	0	20	40	0	10	20	0							

Note: Proposer must select one offer vector per pie size.

Table 2: Set of conditional responses

Offer vector component	Set of conditional responses															
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
y	0	0	0	0	0	10	20	20	20	20	40	40	40	60	60	80
z	0	20	40	60	80	10	0	20	40	60	0	20	40	0	40	0

Note: Responder must denote her choice (to accept or reject) for each conditional response.

- Next, Y states her response strategy (via the strategy method) to accept or reject the distinct variants of X's reduced offer vector (y, z) .¹¹ Table 2 shows the entire set of response choices. If Y accepts, she earns y and Z earns z while X collects the residual $A - y - z$. Otherwise, all three players earn nothing.

The benchmark solution for a monetary payoff maximizer in this game is straightforward:

- Y should accept any non-negative offer ($y > 0$) and may only be indifferent between accepting and rejecting at the zero offer ($y = 0$).
- Anticipating Y's rational response, X should offer the minimal amount to Y and Z. From the set of possible offer vectors (see Table 1), X is indifferent between ceding the minimal amount exclusively to Y ($y = 20$ and $z = 0$) or splitting it equally between Y and Z ($y = 10$ and $z = 10$). Consequently, the following offer vectors denote undominated equilibria: #6, 7, 9 for \underline{A} and #13,

¹¹ Several offer vectors that are distinct in their complete form (x, y, z) become indistinguishable in their reduced form (y, z) and thereby hide the actual size of A based on which they were deduced.

14, 16 for \bar{A} .¹²

- If Y is known to reject all offers with $\tilde{y} < y$ and $\tilde{z} < z$, but would accept (y, z) , the optimal reply for X then is to offer exactly y and z . This shows that any proposal (x, y, z) with $y, z > 0$ is an equilibrium outcome. Hence, deriving the benchmark solution requires applying sequential rationality and not only the equilibrium property.

Altogether, we conducted three treatments which are distinguishable by the overall geographical scope of the participating subject pools. Details concerning the matching procedure are presented in Table 3.

- In the *Local treatment* (LOC), the three players X, Y, and Z, who constitute one interaction group, are students from the same university (University of Jena, Germany). Because all participants in this subject pool are affiliated to the same institution and share numerous aspects of their daily lives, we presume them to be rather homogeneous in their attitudes, values, and beliefs, i.e., to exhibit little social distance.
- In the *Interregional treatment* (REG), the game is played by groups of three participants who each attend a different university in Germany (Humboldt University of Berlin, University of Bonn, and University of Jena). Although the three subject pools still share many common characteristics (i.e., citizenship, language, age, lifestyle, knowledge about social norms and traditions), we do not expect them to be as homogeneous as the participants in (LOC).
- Lastly, in the *International treatment* (INT), participants from three distinct sites (Germany and Israel as countries and Palestine as a geographical region) are linked to play the ultimatum game. In this treatment, we think of the three subject pools as featuring a maximal level of social distance or heterogeneity between the subject pools, surpassing the one of both the (LOC) and the (REG) treatment.

3.2 Laboratory protocol

The computerized experiment was conducted in three stages at the laboratories of the following institutions: Max Planck Institute of Economics, Jena (Germany), Humboldt University, Berlin (Germany), University of Bonn (Germany), Hebrew

¹² The relative abundance of undominated equilibria is due to the fact that X's choice set is discrete rather than continuous.

Table 3: Matching design

Treatment	X	Role Y	Z	Number of observations
(LOC)	Jena	Jena	Jena	27
(REG)	Berlin	Bonn	Jena	18
	Berlin	Jena	Bonn	18
	Bonn	Berlin	Jena	18
	Bonn	Jena	Berlin	18
	Jena	Bonn	Berlin	18
	Jena	Berlin	Bonn	18
(INT)	Germany	Israel	Palestine	18
	Germany	Palestine	Israel	18
	Israel	Germany	Palestine	18
	Israel	Palestine	Germany	18
	Palestine	Germany	Israel	18
	Palestine	Israel	Germany	18

University (Israel), and Bethlehem University (Palestine) between December 2004 and February 2006. The experiment was implemented as a web application, linking the geographically remote participating sites via the internet in quasi real time.¹³ Altogether, we distinguished three treatments and ran five sessions with a total of 243 participants, all of which were undergraduate students at the various universities. 27 of them took part in the local treatment (LOC), and 108 participants each joined the interregional (REG) and international treatment (INT).

In each treatment, participants in a first stage had to state how they would decide assuming that they once were the proposer and once the responder. Subjects thus made their choices behind the veil of ignorance (Rawls, 1971), i.e., without knowing their eventual role (of being the proposer, responder, or dummy) in the experiment. In the former case, they were asked to state their preferred offer vector (x, y, z) conditional on the applicable pie size (\underline{A} and \overline{A}), thus altogether reporting two offer vectors $((\underline{x}, \underline{y}, \underline{z} | \underline{A})$ and $(\overline{x}, \overline{y}, \overline{z} | \overline{A}))$. In the latter case, they had to state their response strategy for all possible reduced offer vectors (y, z) , yielding a total of 16 choices (i.e., the strategy-vector method was applied; see Selten, 1967). In either case, they could condition their strategies on the respective identities of their two interaction partners.

¹³ To receive a copy of the software package and the technical documentation, please contact the corresponding author (Torsten Weiland).

In a second stage, which was not announced in the initial instructions (see Appendix A), participants were asked for their beliefs about their fellow participants' preferred choices in both the proposer and responder role. Evidently, the above concept of a "fellow participant" needs to be clarified. We consider a group of people to be "fellows" if they participate in the same session of the experiment and do so at the same location, i.e., the same computer lab. Additionally, they must face - depending on their own role - either a proposer or a responder from an identical other location.¹⁴ For instance, all Palestinian participants at the computer lab at Bethlehem University who take part in the ultimatum bargaining in the responder role and face a proposer from Germany (and consequently a dummy player from Israel) are considered as fellow participants according to our definition. If, to the contrary, for some of the above participants, the assigned proposer came from Israel and not from Germany, these participants would constitute a different group of fellow participants.

Prior to stating her choices and beliefs, each participant was informed about the university affiliation of the respective two other players.¹⁵ A proposer from Israel may, for instance, learn that her assigned responder (dummy) is from Palestine (Germany). In the end, the elicited beliefs denote the participants' expectation about the most likely (or modal) offer and response behavior of their fellow participants. Hence, we do not ask participants for their first-order beliefs (or stereotypes) concerning their co-player's most likely behavior, but rather request them to express their beliefs about the predominant behavior of their in-group, i.e., those people whom they are closest to and most familiar with. While the former set of beliefs would have been instructive in evaluating proposers' genuine fairness motives, the latter one is very indicative as it prompts participants to provide a "self-evaluation" of their ingroup's predominant attitude toward distinct outgroups.¹⁶ As a side effect, this approach also allows us to assess the degree of actual and perceived behavioral conformity within particular groups of participants.

Both sets of beliefs, i.e., those about the proposer's two offer vectors (given \underline{A} and \overline{A}) and those about the responders' 16 expected response choices conditional on the 16 distinct reduced offer vectors (y, z) , were each incentivized with a monetary bonus of €20.00 that was awarded to the subject who most accurately predicted the predominant (or modal) behavior of her fellow participants.

¹⁴ As a consequence, fellow participants may never interact with one another.

¹⁵ Choices and beliefs were always elicited for the same constellation of interaction partners.

¹⁶ We arbitrarily decided to only elicit the latter type of beliefs to keep the complexity of the experiment within reasonable limits. In retrospective, eliciting both sets of beliefs may have been worthwhile.

After completing both stages, proposer, responder, and dummy roles were assigned by random draw, the two former's choices were matched and participants were paid according to the realized outcomes. Each session took about 50 minutes. Finally, payoffs in the experiment were converted into euros (in Germany) or USD (in Israel and Palestine) at the exchange rate of 1 ECU = 0.20 USD and 1 USD = €0.78, resulting in earnings of, on average, €5.19 per subject (SD 3.79). In addition to their income from the experiment, participants received a fixed show-up bonus of €4.00. Statistical testing reveals that the regional scope of the treatments, on average, did not significantly affect payoffs ($p = 0.613$, Kruskal-Wallis test). For all further considerations, let us now turn to the results section.

4 Results

In reporting our results, we proceed as follows. First, we present a descriptive overview of proposers' elicited offers across the three treatments. In a second step, we provide a likewise summary of the responders' choices. In a third step, we then address our central research question and evaluate the significance and practical implication of geographic and social distance on individuals' offer and acceptance behavior in the featured ultimatum bargaining task.

4.1 Proposer behavior

Figure 1 provides descriptive statistics on the relative frequencies at which the distinct offer vectors were chosen. Frequency statistics are reported separately for each treatment (i.e., (LOC), (REG), and (INT)) and for each participating site (i.e., Bonn, Berlin, and Jena, respectively Germany, Israel, and Palestine). A third distinction is made between offer vectors that condition on the available pie being large (the leftmost 13 bars) or small (the rightmost 9 bars). Moreover, the figure allows contrasting the subjects' actual choices with their beliefs about the respective predominant (or modal) choices in their own reference group, i.e., in the group of their fellow participants.¹⁷

We observe that proposals are neither uniformly distributed over the entire range of possible offer vectors, nor do they coincide in a single dominant offer for each of the two possible pie sizes \underline{A} and \overline{A} .¹⁸ Nonetheless, an easily discernible pattern emerges; most subjects clearly reveal by their choices that they dislike (excessively)

¹⁷ See section 3.2 for the definition of the group of fellow participants and an illustration thereof.

¹⁸ While not yet coinciding, proposals under \underline{A} evidently are closer to being uni-modal (40, 20, 20) than those under \overline{A} .

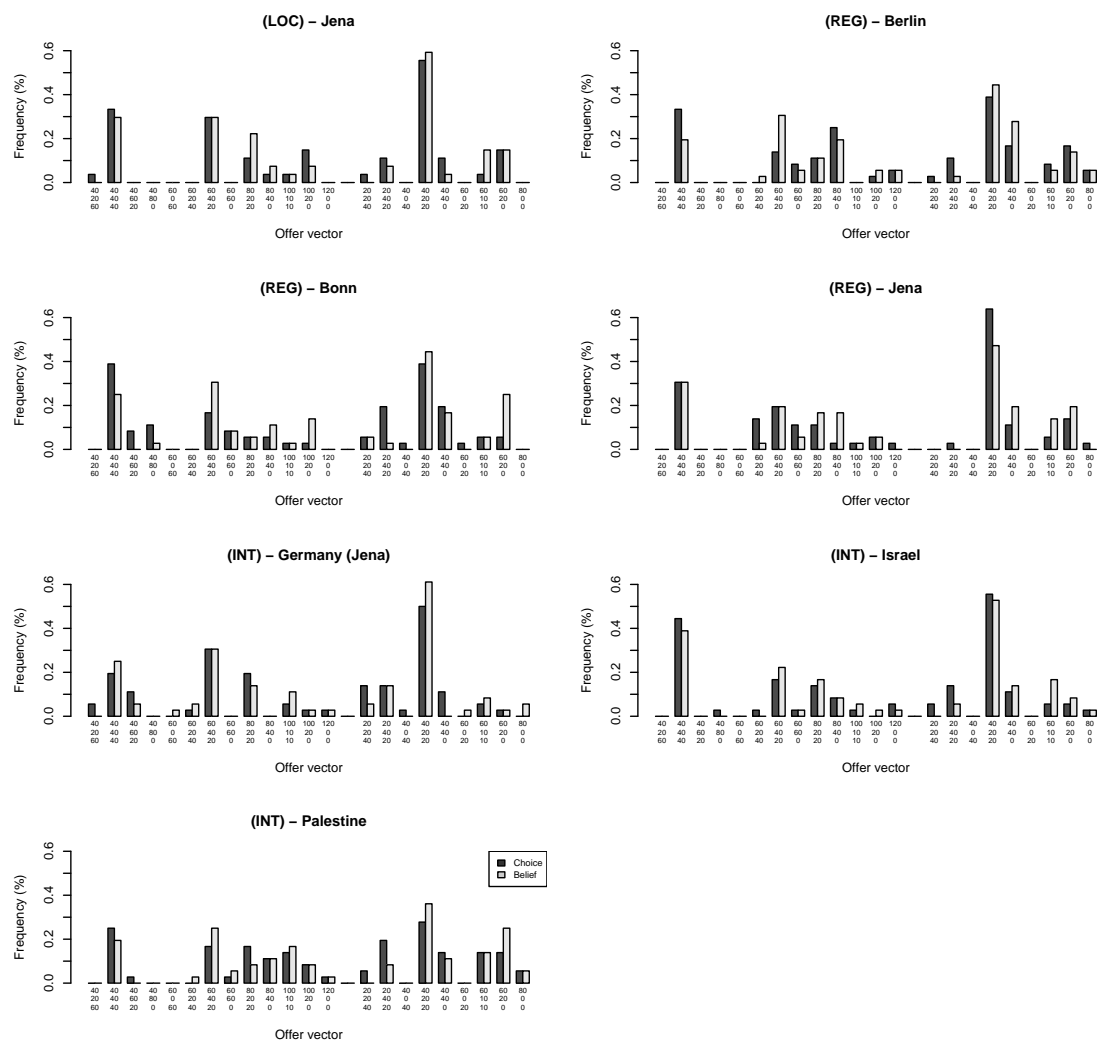


Figure 1: Actual and expected frequencies of proposals

inequitable distribution outcomes. As a result, economically rational offer behavior - which equates to maximally selfish behavior - only is observed in a minority of subjects. In any treatment and for any participating site, (largely) selfish offers are outweighed by (largely) equitable ones. While 32% (65%) of all subjects make a perfectly (nearly-) equitable offer when disposing of the large (small) pie ($x = y = z = 40$, respectively $x, y, z \geq 20$), only 12% (20%) of the population behave in an extremely selfish manner ($x \geq 100$, respectively $x \geq 60$).

In light of the existing empirical evidence on ultimatum bargaining (cf., Güth and van Damme, 1998; Charness and Rabin, 2002; Camerer, 2003), the latter finding is unsurprising; the theoretical benchmark based on monetary payoff maximization is generally a poor predictor of actual proposer (and also responder) behavior. Nonetheless, it would be equally unreasonable to declare that proposers, on average, act pro-socially out of an altruistic conviction. This is the case because, in ultimatum games, one cannot unambiguously impute proposer generosity to unselfish, pro-social motives: generous offer may merely result from strategic considerations based on the fear that a greedy offer may be rejected.¹⁹ In terms of evaluating the extent and impact of social preferences in people, the behavior of responders actually is more informative. We will therefore spend some time on discussing the prominence of other-regarding preferences in the ensuing section on responder behavior.

With respect to prominent and frequently chosen offers, in particular two focal points in the case of \overline{A} stick out; the “equal split” (40, 40, 40) and the “power hierarchy”²⁰ (60, 40, 20). While the former distribution scheme is practically omnipresent in the empirical social preference literature (e.g., Eckel and Grossman, 2001; Güth et al., 2001), the latter one also denotes a robust stylized fact in three-person ultimatum bargaining (cf., Güth et al., 2003; Bosco and Marcheselli, 2006; Güth and Napel, 2006). Instances of equal sharing between proposer and responder while omitting the dummy, i.e., (60, 60, 0) - a distribution pattern that is also dubbed as the “power coalition” - are far less frequently observed. In the case of \underline{A} , the majority choice and belief is even more evident; 47% of all subjects suggest to split the pie in the form of (40, 20, 0) and even slightly more of them (49%) expect their fellow participants to act in this way. After all, this finding is not very surpris-

¹⁹ Since we chose to inquire participants concerning their beliefs about the predominant behavior in their in-group - rather than their first-order beliefs about their co-players’ likely behavior - we cannot disentangle genuine pro-social behavior in proposers from mere strategic play.

²⁰ The power hierarchy is a regularly observed distribution pattern in three-person ultimatum games according to which the pie is shared in function of the players’ bargaining power. Proposers (responders) are entitled to the largest (a lesser) share due to their ultimatum power and information privilege (their veto power). The dummy, to the contrary, is deprived of any active strategic influence.

Table 4: Summary statistics - distribution proposals

Treatment	Location	Offer vector		
		x	y	z
(LOC)	Jena	0.52 (0.17)	0.29 (0.10)	0.20 (0.13)
(REG)	Berlin	0.55 (0.19)	0.30 (0.13)	0.15 (0.14)
	Bonn	0.46 (0.15)	0.35 (0.15)	0.19 (0.14)
	Jena	0.53 (0.15)	0.28 (0.11)	0.19 (0.12)
(INT)	Germany	0.49 (0.18)	0.30 (0.12)	0.21 (0.13)
	Israel	0.56 (0.21)	0.28 (0.14)	0.16 (0.13)
	Palestine	0.48 (0.16)	0.29 (0.13)	0.23 (0.13)

Note: Standard deviations in parentheses.

ing. Already in preceding studies, it had been pointed out that the high frequency of choices at the equal split may, at least in part, be attributable to the discretization and, hence, the more or less reduction of the proposer's choice set (e.g., Güth et al., 2001). In line with this rationale, proposers were found to actually prefer slightly, but not excessively self-favoring distributions. If, however, these distribution options were not available, proposers would opt for the perfectly equal split rather than any other, considerably more selfish allocation. Our results corroborate this claim (see also Table 4 for a tabulated overview of mean offers across sites and treatments).

In the following, we present the most prominent behavioral regularities that were encountered in analyzing proposers' choices and beliefs. In particular, we will focus on several measures of consistency that are central in characterizing the elicited bargaining behavior.

When comparing proposers' offer vectors under \underline{A} and \overline{A} in relative terms, i.e., as fractions between zero and one, it becomes apparent that participants are largely consistent in their distribution preferences. Those subject who prefer to split the pie equitably in case of \overline{A} will regularly behave likewise in case of \underline{A} . Moreover, as behavioral consistency is found to work in both ways, selfish proposers are, in most cases, not affected in their distribution choices by the size of A , either. To statistically underline this assertion, we transformed subjects' respective offer vectors into one-dimensional statistics of income concentration.²¹ Statistical testing yields

²¹ More precisely, we computed the Gini-coefficient of each of a subject's two offer vectors, i.e., in case of \underline{A} and \overline{A} and, in a second step, assessed their correlation and statistically tested their structural similarity by means of a paired, two-sided Wilcoxon signed rank test.

that the two offer vectors given \underline{A} and \overline{A} , as expressed by their Gini-coefficients, are highly correlated ($\rho = 0.66$, $p < 0.001$, Pearson correlation) and do not structurally differ ($p < 0.001$, paired two-sided Wilcoxon signed rank test).

Apart from being largely consistent in their social preferences when distributing monetary amounts of varying sizes, participants also demonstrate substantial consistency in aligning their own proposals (given \underline{A} and \overline{A}) with their expectations about others' predominant choices. More specifically, we find that own behavior and the expected behavior of members of one's reference group coincide to a large extent, both at the aggregate and the individual level. In other words, we discern a large extent of - at least perceived - behavioral conformity among in-group members. A series of binomial tests comparing the observed frequencies of expected and actual choices reveals that, in most cases (89%), the two frequencies do not differ statistically ($p \leq 0.05$).²²

Yet, one particularity is striking; subjects, on average, predict the most likely choice in their reference group to be slightly more selfishly - in the sense of being more economically attractive for the proposer - than their own choice. On average, proposers claim, by their own choices, a share of 52% of the available pie²³ for themselves whereas they anticipate their reference group at large to demand a significantly larger share of 55% ($p = 0.003$, paired Wilcoxon signed rank test). This seemingly kind and pro-social behavior is, in fact, well documented in the sociological literature. Therein, such conduct is thought of as a means to boost one's self image and to maintain or enhance one's self-esteem (e.g., Hogg, 1993; Sheldon et al., 2001), even if this act of generous giving cannot be observed by others.

Expect for the relative similarity between actual and expected proposer claims, the far-reaching accordance between own and expected offers also holds true for the entire offer vector. More specifically, the correspondence between actual and expected offers, after being transformed to one-dimensional Gini-coefficients, turns out to be highly significant ($p = 0.003$, paired Wilcoxon signed rank test, respectively $\rho = 0.485$, $p < 0.001$, Pearson product-moment correlation). In all, participants thus entertain largely realistic beliefs about their reference group's predominant behavior and systematically associate their choices and beliefs.²⁴

²² For each of the 22 offer vectors, the corresponding actual and expected frequencies of use were contrasted. Predictive accuracy was found to differ across sites and treatments, with participants in (LOC) and participants from Bonn in (REG) achieving the best (worst) correspondence (for 7% (17%) of all offer vectors, actual and expected frequencies of use differ significantly).

²³ For the test, the means of proposers' claimed shares given \underline{A} and \overline{A} were compared with the corresponding mean expectations concerning the behavior of the reference group.

²⁴ We refrain from making any imputations with respect to the causal relation between proposers' beliefs and choices, as this question cannot be reasonably answered on the basis of our experiment design.

Due to the particular matching design that was applied in the experiment, we are able to conduct yet another test of behavioral consistency: Do people meaningfully change their distribution proposals in the ultimatum bargaining task when they are matched with increasingly unfamiliar, i.e., geographically remote and socially distant, interaction partners? To answer this question, we compare the distributions of the relative amounts that proposers from Jena - who constitute the only subject pool that is present in all three treatments - intend to keep for themselves (x), respectively to pass on to the responder (y) across the three treatments. Statistical testing reveals that these two components of the proposer's offer vector, are not significantly affected by the treatment variable ($p = 0.359$ ($p = 0.656$) for comparison of x (y) across treatments, Kruskal-Wallis tests).²⁵ The analogous statement holds true for beliefs ($p = 0.755$ for x and $p = 0.732$ for y). We conclude from these results that proposers' beliefs and their eventual offers are not significantly affected by a mere increase or decrease in the geographical and social distance between the bargaining partners. Hence, if anything, the treatment variable must interact with other, yet omitted factors to cause a substantial change in proposer behavior.

Lastly, we want to evaluate our initial hypothesis of whether proposers systematically exploit their information advantage, i.e., the responder's ignorance of the actual pie size. To this end, let us first identify those offers from which the responder cannot unambiguously deduce whether the available amount is of size \underline{A} or \bar{A} . The following (reduced) offer vectors feature this property: #1-3, 6-9, and 11-12 (see Table 2). In the following, we concentrate our attention on offers that were made while assuming that the large pie (\bar{A}) applies, because only in this case proposers may exploit the responders' uncertainty about the pie's actual realization. Figure 2 shows the five most frequently chosen offer vectors in descending order for each treatment. The solid gray lines indicate the extent of income concentration and bar colors allow to distinguish between unambiguous and ambiguous offers.

Strikingly, proposers in all three treatments most frequently (in 32% of all cases) choose the equal split which, by our design, denotes an unambiguous offer (light gray). Although subsequent, i.e., less frequently selected offer vectors typically are ambiguous (dark gray) and become increasingly self-centered²⁶, it nevertheless would be inappropriate to conclude that proposers systematically prefer ambiguous over unambiguous offers to further their own material benefit. In most cases (>50%) and irrespective of the applicable treatment, X actually grants at least half of the pie

²⁵ Bearing in mind that the amount to the dummy (z) simply is the complement of x and y , i.e., $z = 1 - x - y$, it follows that z is not significantly affected by the treatment variable, either.

²⁶ The solid gray line indicates the degree of income concentration - as measured by the Gini coefficient - for the various offer vectors.

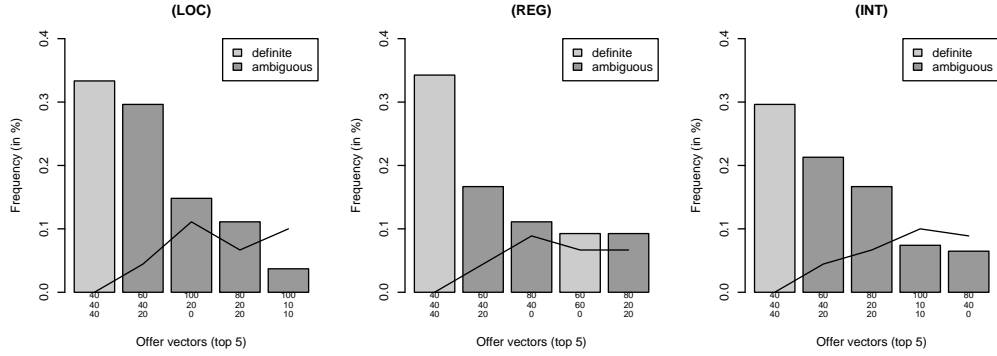


Figure 2: Frequency of ambiguous proposals

Note: Solid gray lines connect the Gini coefficients of the various offer vectors. For better visualization, Gini coefficients are scaled up by factor five.

(≥ 60) to Y and Z.

Before we turn to the regression analysis to answer our main research question, we continue our descriptive analysis, this time exploring the choices and beliefs of responders. Having developed a better understanding of their behavioral regularities will enable us to advance more pointedly in the concluding micro-econometrics section.

4.2 Responder behavior

The responders' reactions to the various possible offers of X are illustrated in Figure 3. Analogously to Figure 1, the frequencies of actual and expected responses are separately reported while observations are further grouped by treatment and location. Several regularities immediately can be immediately observed. Responders almost uniformly (in, on average, 84% of all cases) reject offers that leave them empty handed ($y = 0$). Once the responder is included in sharing the pie, her propensity to reject X's offer generally decreases in function of the transferred amount y . Besides, Y's response behavior seemingly also is influenced by welfare concerns for dummy Z; if Z is omitted from the distribution of the pie, responders seem to be less willing to accept the offer. We leave this observation pending for now, but will return to and more closely evaluate it in the subsequent section.

As a general pattern, we find that responders exhibit similar response patterns across treatments and locations. The degree of behavioral conformity is particularly strong for the three participant groups from Germany ($p > 0.05$, series of pairwise binomial tests, i.e., 11 tests per site comparison) and is most evident for both zero offers ($y = 0$) and very generous offers ($y \geq 40$). Israeli and Palestinian participants,

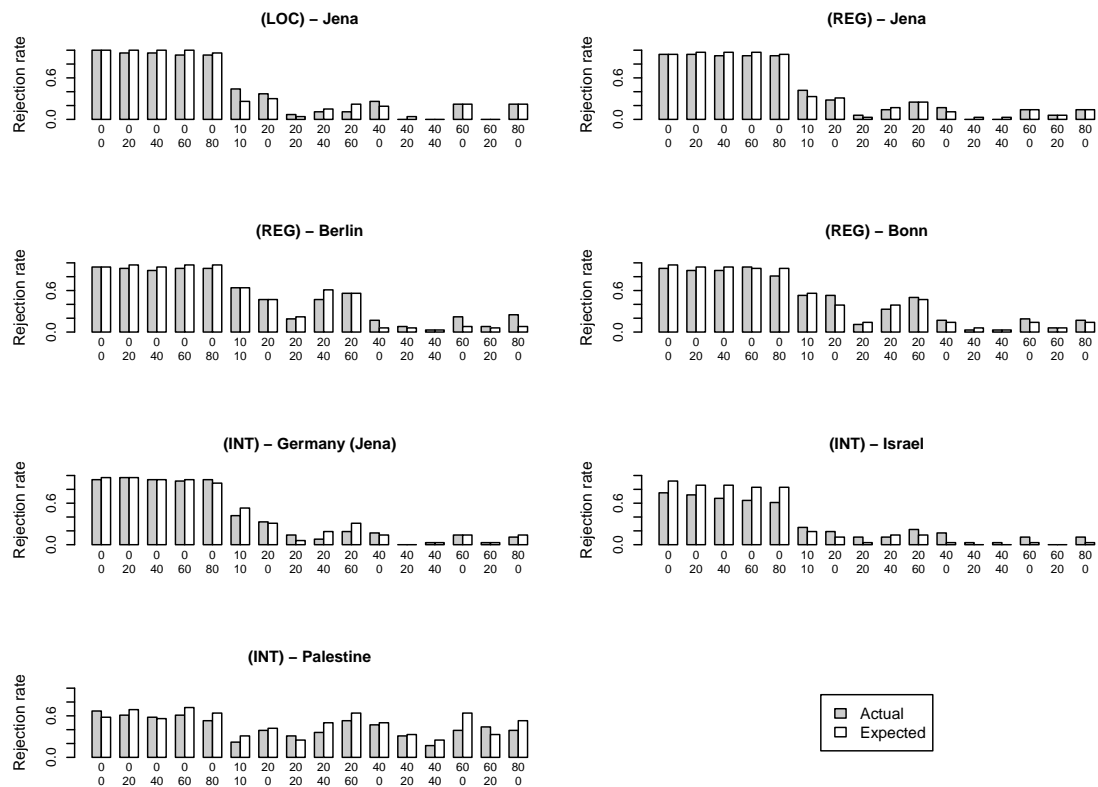


Figure 3: Actual and expected rejection rates

Note: Solid gray lines connect the Gini coefficients of the various offer vectors. For better visualization, Gini coefficients are scaled up by factor five.

by contrast, exhibit at least partly diverging response patterns. Moreover, while subjects of the latter two groups coincide in accepting zero offers with a relatively larger probability ($p \geq 0.453$ in a series of pairwise tests for the different offer vectors with $y = 0$, same test setup as above), they profoundly differ in their reaction to more generous offers ($y \geq 40$) ($p \leq 0.014$ in five out of six pairwise tests between the two groups, $p = 0.112$ in one case). For the range in between ($10 \leq y \leq 40$), response behavior structurally also differs within the treatments (REG) and (INT). In both cases, responders from the distinct subject pools clearly deviate in their propensity to reject given offers. In particular, participants from Jena in (REG) and Israel in (INT) reject offers from X significantly less often than at least one group of participants from the other two corresponding locations ($p \leq 0.10$ in three out of five pairwise comparisons in (REG), i.e., Jena vs. Berlin and Jena vs. Bonn, respectively $p \leq 0.10$ in three out of five comparisons in (INT), i.e., Israel vs. Palestine, series of binomial tests).

Furthermore, we observe an unexpected, yet very pronounced particularity in the behavior of Palestinian responders. Unlike any other group of participants, they, on average, reject equitable and even exceptionally favorable offers from X with surprising rigor. For instance, even offers with $y \geq 60$ are rejected at rates between 33 and 64 per cent. Although this stark rejection behavior may be rationalized by strong fairness concerns in responders (cf., Camerer, 2003), we remain skeptical that this conjecture truly explains the above regularity. If the conjecture were to be correct, we would at least discern a major inconsistency between a relatively inequitable offer behavior and a pronouncedly inequity-averse response behavior in Palestinian participants. With this in mind, we may also consider several other explanations:

First, Palestinian participants may have been unfamiliar with acting in a laboratory environment which could have led them to behave rather erratically and to make economically insensible choices (see Smith, 1994; Binmore, 1999, for critical views on the possibilities and limits of the experimental methodology). Considering that the experiment is, in our estimation, only moderately difficult and control questions were asked prior to the experiment start, we deem this explanation as rather unlikely. Second, the pecuniary incentives in the experiment may have been insufficient to encourage some meaningful cognitive effort on the part of the participants (also known as the “Harrison criticism,” see Harrison, 1989). Again, this explanation is unlikely since Palestinian participants actually earned relatively more in terms of purchasing power than both Germans and Israelis. If anything, the latter two groups should have demonstrated an inferior willingness to exert the necessary

cognitive effort. And lastly, Palestinian participants may have acted erratically because they, possibly, simply did not believe in the authenticity of the experiment (see Schram, 2005; Eckel and Wilson, 2006). Should they have mistaken the serious experiment for a mere entertainment game, this would at least partially explain their individually irrational response behavior. Ultimately, and given our data, we cannot exactly pinpoint the reason for this divergent behavior. For this reason, we encourage other scholars to either replicate this experimental design or to run slightly modified variations of it to scrutinize this puzzling finding.²⁷

With regard to responders' beliefs about the predominant response behavior in their group of fellow participants, we come to the following realizations. Actual and expected responder choices coincide to a large extent and, consequently, are highly correlated. The mean correlation between responders' vectors of actual and expected choices equals $\rho = 0.672$, with German (Palestinian) participants exhibiting the strongest (weakest) correspondence between the two ($mean(\rho) = 0.786$ vs. $mean(\rho) = 0.161$, Pearson correlation coefficients). In most cases, responders' actual choices and their expectations of the modal choices of their in-group members are identical (the gray and white bars, respectively), whereby we intentionally refrain from imputing any causal relationship between the two. In certain subject pools, responders show a tendency to expect their fellow participants to reject a given offer but accept themselves or vice versa. Both variants of contrarian behavior are most frequently observed in Palestinian participants (in 51% and 38% of all choices). This finding is only logical when the low correlation between actual and expected responses in the former group is considered. On the whole, however, we can rightfully state that there is a robust correspondence between actual and expected responder choices.

Knowing that responders' choices and beliefs are highly correlated, it also appears worthwhile to evaluate yet another form of intrapersonally consistent behavior: Would responders generally accept their own offers or, alternatively, does a sizable group of them state excessive claims in the proposer role which they, themselves, would not tolerate in the responder role? Again, we find proposer and responder choices to be largely consistent; only a minority of participants (11% and 8% given \underline{A} and \overline{A}) would indeed reject their own offers. Apparently, this form of consistency is independent of the applicable pie size ($p = 0.375$, binomial test).

Concluding our series of consistency checks, we briefly assess the economic rationality of proposer and responder choices. More specifically, we explore whether

²⁷ As yet another explanation, erratic choice behavior may also result from being dissatisfied about one's (stochastically) assigned role. However, since roles were only reported after beliefs had already been elicited, this rationale cannot apply in this study.

proposers configure their offers as best replies to imputed responder choices and whether responders universally accept all offers which entitle them to a positive, even if only marginal payoff. Let us first evaluate the proposers' choices. On average, they claim a share of 51% of the pie (irrespective of whether \underline{A} or \overline{A} applies) and, due to occasional rejections by responders, eventually appropriate 43% of the pie. Although these shares already clearly surpass the ones of responders (28%) and dummies (19%) (see also the outcome statistics in Table 5), a combinatorial exercise²⁸ yields that, given responders' elicited choice behavior, proposers could have, at least theoretically, earned up to 53% (62%) of the pie \underline{A} (\overline{A}) and, hence, could have done substantially better.

Likewise, responders could have substantially improved their payoff, too. By uniformly accepting all offers that yield a positive amount to themselves, they could have slightly raised their mean payoff from 28% to 31%. However, despite the economic incentive, only a fraction of responders actually implemented the theoretical benchmark of uniformly accepting all offers (5%) or of accepting all offers with $y > 0$ (33%). In all, we thus observe that responders systematically, and to their own detriment, deviate from equilibrium play. We moreover garner indicative evidence that the same assertion also applies to proposers. Candidate explanations for these systematic violations of equilibrium behavior are, among others, false beliefs in proposers about the preferences and acceptance thresholds of responders²⁹ (with respect to both the responder and the dummy) and, arguably more likely, a non-negligible degree of social preferences in proposers and responders.

In the subsequent part of the analysis, we intend to shed more light on the prevalence (or absence) of social preferences in responders. For this purpose, we turn our attention to the following central question: Do responders systematically condition their choices to accept or reject given offers on the dummy's assigned share of the pie? Figure 4 provides some first descriptive insights. In the figure, rejection rates are related to the amounts y and z that are assigned to the responder and the dummy. At first glance, the propensity to reject a given offer significantly decreases

²⁸ First, each possible offer given \underline{A} and \overline{A} is matched with each responder's elicited choice vector (matching is limited to responders from the same treatment). Next, the payoffs for each matching are computed and aggregated. The two offer vectors (given \underline{A} and \overline{A}) that yield the highest mean payoff to the proposer then denote the best response for the respective pie size (being (80, 20, 20) and (60, 20, 0) in (LOC) and (REG) and (100, 10, 10) and (60, 10, 10) in (INT)).

²⁹ Since we intentionally limited the elicitation of responder beliefs to expectations about the predominant behavior of their fellow responders, i.e., the members of their in-group, we can neither conclusively confirm nor reject this possibility. The belief elicitation stage was limited for two reasons; to avoid eliciting mere stereotypes and to prevent overburdening participants from repeatedly having to state entire choice vectors via the strategy method which is known as being cognitively rather challenging (see Roth, 1995, for a discussion of this method).

Table 5: Summary statistics - payoffs and efficiency

Treatment	Economic efficiency	Role	Payoff	Share (in %)
(LOC)	1.00	X	57.78 (29.06)	0.52
		Y	28.89 (10.54)	0.26
		Z	24.44 (19.44)	0.22
(REG)	0.86	X	45.56 (23.23)	0.48
		Y	30.00 (18.21)	0.32
		Z	18.89 (15.82)	0.20
(INT)	0.92	X	46.11 (21.28)	0.45
		Y	33.06 (16.87)	0.32
		Z	23.06 (15.64)	0.23

Note: Standard deviations in parentheses.

in y and increases in z . Yet, in qualifying this statement one, at the same time, needs to point out that the increase (decrease) in the rejection rate in function of y and z is non-linear in either domain. While larger transfers to Y principally decrease the responder's willingness to reject the corresponding offer, we observe that generous offers ($y \geq 40$) actually are rejected more often than merely equitable ones ($y = 40$) (17% vs. 4%, $p < 0.001$, binomial test).³⁰ Similarly, we find that, on average, the responders' empirical rejection rate is minimal (29%) at $z = 20$ and increases with deviations in either direction.

Primarily, two effects and their interaction may explain this regularity. Once, participants may generally be self-serving, but are simultaneously inequity averse (see, e.g., Fehr and Schmidt, 1999). This set of preferences is compatible with appropriating a slightly more than equitable share of the pie, but does not support excessive self-enrichment. To avoid the anticipated anguish from advantageous inequality, responders may actually prefer to altogether forego their certain income rather than to obtain the latter and, as a side effect, having to cope with the above mentioned incommoding sensation. In this case, the responder's perspective is exclusively intrapersonal. Alternatively, participants may also exhibit social - and as such interpersonal - preferences with respect to the dummy. In that case, responders may want to punish inequitable proposers, at times even at a substantial cost to themselves, for having violated a perceived social norm. Yet, the shape of the rejection curve in function of the transfer to the dummy (the dashed line) suggests

³⁰ We already appealed to this issue when talking about the distinct response behavior of Palestinian participants. While rejection behavior turns out to be non-monotonic across all sites, the peculiar response behavior of the above group nevertheless sticks out.

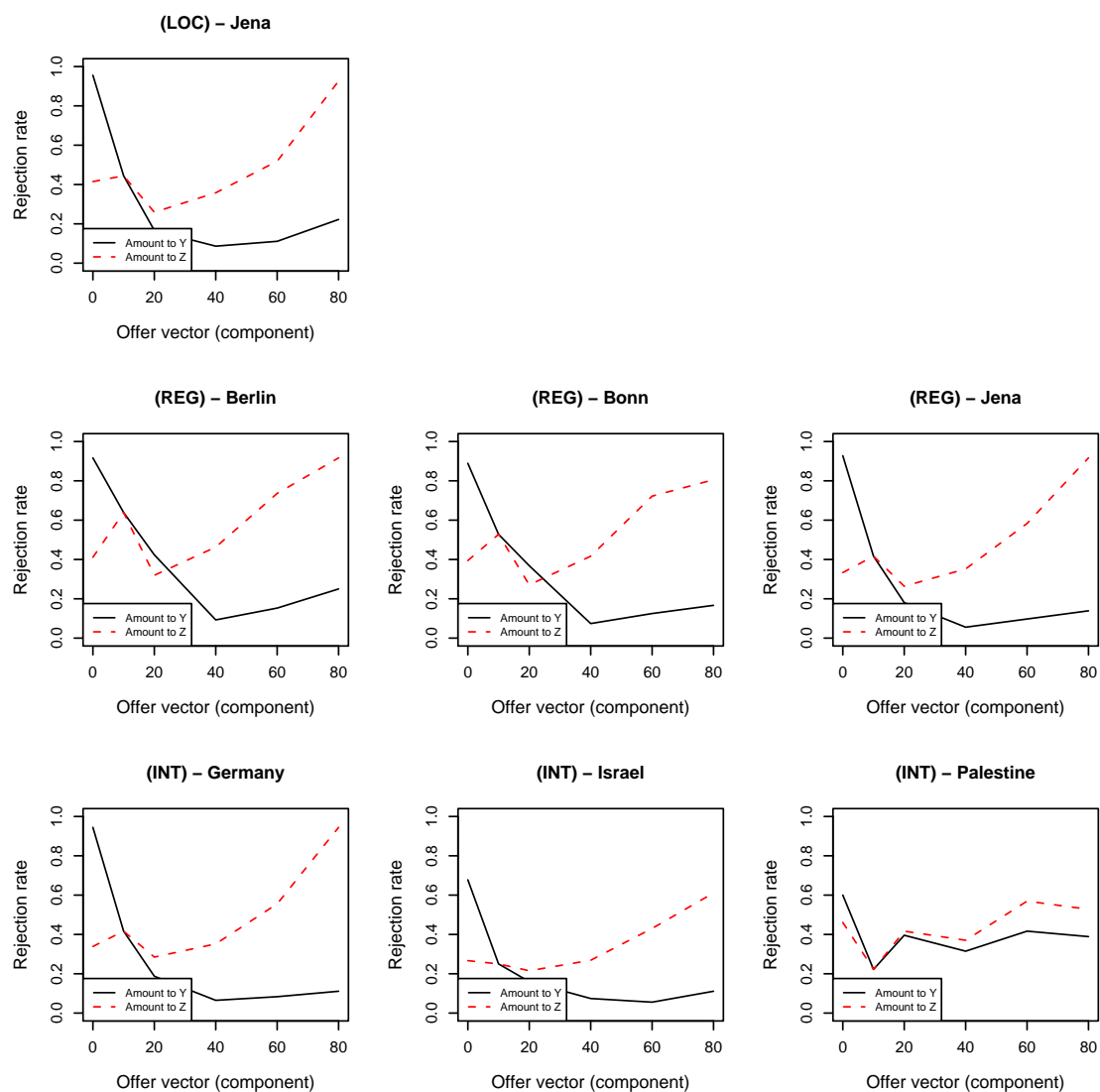


Figure 4: Partial influence of responder and dummy shares on rejection rate

that any aspiration to support the dummy is simultaneously undermined by the responder's preference to prevent bargaining outcomes in which she would suffer from disadvantageous inequity.

The above descriptive analysis is limited in its generalizability, since only the aggregate rejection rates at different levels of y and z were considered. In this approach, we only evaluated the mean effects - but not the actual partial effects - of transfers to the responder and dummy. Further, we did not control for the respective other transfer and further influential factors in a *ceteris paribus* manner. As a result, we cannot yet make assertions concerning the significance and magnitude of the partial effects of the set of influential factors that shape people's individual offer and response behavior. In view of the practical relevance of a better understanding of these contingencies, we shall close this gap in the following section.

4.3 Regression analysis

In a first step, we analyze the behavior of proposers at the individual level to identify and quantify those factors which primary and systematically shape their decisions. To this end, we fit six separate linear mixed-effects regressions.³¹ Per treatment, two regression models are estimated, one to explain the retained share by the proposer (x) and one to explain the transferred amount to the responder (y). To allow for the elicited choice data given \underline{A} and \overline{A} to be pooled, the distributed amounts were transformed into relative shares (with $0 \leq x \leq 1$ and $0 \leq y \leq 1$). The proposer's transfer to the dummy is not explicitly modeled, but may be easily ascertained as the complement of the former two shares ($z = 1 - x - y$). The following covariates were used in the series of regressions: a dummy for pie size (equals one if \overline{A} applies), two dummies for the provenance of the proposer, and another two dummies for the provenance of the responder (representing sites 2 and 3 in treatments (REG) and (INT)).

The following insights can be derived from the regression analysis (see Table 6):

1. *Power hierarchy.* Observing the coinciding intercepts for x and y in all three treatments ($x \approx 0.50$ and $y \approx 0.30$) immediately reminds us of the stylized fact of the power hierarchy which advocates to split the given amount in decreasing fractions according to the players' bargaining power. Confirming previous findings, we discern the distribution pattern (50%, 30%, and 20%) as pivotal

³¹ Pinheiro and Bates (2002) provide an overview of the theory and application of linear mixed-effects modeling, the approach that we will subsequently apply, including grouped data and repeated measures.

Table 6: Linear mixed-effects regression explaining allocation shares

Dependent variable		x			y		
Treatment	Independent variable	Coefficient	Std. Error	p-value	Coefficient	Std. Error	p-value
(LOC)	Intercept	0.509	0.033	< 0.001	0.301	0.019	< 0.001
	LargePie	0.015	0.022	0.490	-0.026	0.020	0.204
		# obs. (groups): 54 (27) Pseudo- R^2 : 0.089			Pseudo- R^2 : 0.045		
(REG)	Independent variable	Coefficient	Std. Error	p-value	Coefficient	Std. Error	p-value
	Intercept	0.529	0.045	< 0.001	0.300	0.032	< 0.001
	LargePie	-0.029	0.012	0.018	0.025	0.014	0.076
	X-Bonn	-0.069	0.042	0.104	0.047	0.030	0.114
	X-Jena	-0.003	0.042	0.940	-0.031	0.030	0.300
	Y-Bonn	0.048	0.042	0.256	-0.009	0.030	0.766
	Y-Jena	0.022	0.042	0.608	-0.017	0.030	0.578
		# obs. (groups): 216 (108) Pseudo- R^2 : 0.053			Pseudo- R^2 : 0.061		
(INT)	Independent variable	Coefficient	Std. Error	p-value	Coefficient	Std. Error	p-value
	Intercept	0.454	0.049	< 0.001	0.326	0.032	< 0.001
	LargePie	0.039	0.016	0.015	-0.024	0.013	0.074
	X-Israel	0.068	0.045	0.137	-0.009	0.030	0.758
	X-Palestine	0.008	0.045	0.852	-0.034	0.030	0.265
	Y-Israel	0.001	0.045	0.974	0.015	0.030	0.616
	Y-Palestine	0.040	0.045	0.379	-0.051	0.030	0.091
		# obs. (groups): 216 (108) Pseudo- R^2 : 0.088			Pseudo- R^2 : 0.069		

in the three-person variant of the ultimatum game (compare with Table 4).³²

2. *Conditioning on pie size.* Proposers in (REG) and (INT) systematically condition the size of their retained share on the size of the applicable pie ($p \leq 0.018$). This finding is not self-evident, considering that we already transformed the dependent variable into relative statistics ($0 \leq x, y \leq 1$) to accommodate for the pie size (\underline{A} and \overline{A}). Proposers thus not only adjust their claims in absolute terms, but also do so in relative ones. Nonetheless, the relevance of this effect should not be overstated, either. In the end, the factor's economic significance is rather limited, considering that it implies a shift in x of less than 4%. Moreover, in the treatment with the - supposedly - most homogenous pool of participants (LOC), the effect actually is entirely absent ($p = 0.490$). By the same token, no systematic effect of the pie size on the transferred amount to the responder (y) is observed ($p \geq 0.074$).
3. *Irrelevance of location.* Proposers do not systematically condition the composition of their offers on their own or their interaction partners' geographical provenance. Already among presumably rather homogeneous subjects, i.e., participants from the same location, some substantial within-group variance is observed. If anything, we detect a minor effect between the proposer's location and her retained share ($p = 0.104$ in (REG)), respectively between the assigned responder's location and the transfer to the latter ($p = 0.091$ in (INT)). Beyond, knowing the proposer's location or the ones of her co-players only marginally improves the fitted models' predictive accuracy (i.e., leads to adjustments in x and y of less than 7%).

Next, we fit another series of regressions to explain the rejection behavior of responders. In contrast to the above analysis, we are this time interested in explaining the binary choice of either accepting or rejecting a given offer. Therefore, we fit three separate logit mixed-effects models, i.e., one per treatment. The following covariates are comprised in all three models: the relative share to the responder (y) and the dummy (z), two dummies marking those instances in which $y = 0$ (yNil), respectively $z = 0$ (zNil), and two further dummies (Ambiguous and Inequitable) denoting ambiguous and disadvantageously inequitable offers.³³ While it is straightforward to tell whether an offer is ambiguous or not, assessing the (in)equity of an offer is slightly more complicated. To make this distinction, the following, arbitrary

³² This insight is supported by the relatively small improvement in the augmented models' fit over the one of their respective null models (see the Pseudo- R^2 statistics in Table 6).

³³ Disadvantageous inequity in our model holds the same meaning as in Fehr and Schmidt (1999).

rule is introduced: An offer is considered inequitable from the viewpoint of a selfish responder if she receives less than the equitable share of the presumed pie size. Since the latter is unknown to the responder, she applies the “in dubio pro reo” rule, i.e., if uncertain about the actual pie size, she always assumes the former to be small. In those treatments which involve more than one regional subject pool, four (in REG) and two (in INT) further dummies are added to denote the geographical provenance of the responder and, in case of (REG), the one of her assigned proposer.³⁴ The results from the series of regressions are shown in Table 7:

The following key findings were derived from the analysis:

1. *Quasi zero-slope of responder and dummy share.* Despite the fact that the coefficient estimates for both the responder (y) and dummy (z) share typically are statistically significant, they are nevertheless only of secondary importance in terms of their economic relevance. This means that the responder’s propensity to reject a given offer only marginally diminishes as the amount of y and z increases.
2. *Refusal of zero offers.* Responders are highly sensitive to zero offers by the proposer, irrespective of whether those are directed toward themselves (y_{Nil}) or toward the dummy (z_{Nil}). Thus, offering a zero amount to either responder or dummy or, even worse, to both of them, will substantially increase the responder’s propensity to reject the offer. Overall, we can therefore unambiguously conclude that responders, on average, do care about the economic well-being of the dummy, although they would not need to give any heed to the latter’s satisfaction from a strategic viewpoint. However, when comparing the effect of a zero offer to the responder and one to the dummy, the former one weighs significantly more heavily on the responder’s choice which can be taken as an indication of a self-serving tendency in responders.
3. *Ambiguous offers dissuade rejection.* Offers that could stem from both \underline{A} and \overline{A} are less frequently rejected than unambiguous offers in treatment (REG) ($p < 0.001$), but not in the other two treatments (LOC) and (INT) ($p \geq 0.094$). Although intuitively, one may have expected the converse effect, i.e., rendering responders suspicious and more prone to reject whenever offers cannot be unambiguously assigned to either \underline{A} or \overline{A} , the “in dubio pro reo” principle may help to explain this repeatedly observed phenomenon.

³⁴ In treatment (INT), the responder was not informed about the provenance of her assigned proposer and, hence, could not condition her response on this information. In retrospect, this design choice is unfavorable but not existential, as the results from treatment (REG) indicate.

Table 7: Logit mixed-effects regression explaining rejection rate

Treatment	Independent variable	Coefficient	Std. Error	p-value
(LOC)	Intercept	0.292	0.140	0.025
	y	-0.003	0.002	0.069
	z	-0.003	0.001	0.023
	yNil	0.702	0.052	< 0.001
	zNil	0.114	0.049	0.015
	Ambiguous	-0.076	0.056	0.094
	Inequitable	0.101	0.064	0.067
	# obs. (groups): 432 (27) Pseudo- R^2 : 0.617			
(REG)	Independent variable	Coefficient	Std. Error	p-value
	Intercept	0.340	0.096	< 0.001
	y	-0.003	0.001	0.013
	z	-0.002	0.001	0.011
	yNil	0.503	0.029	< 0.001
	zNil	0.073	0.028	0.005
	Ambiguous	-0.111	0.032	< 0.001
	Inequitable	0.260	0.036	< 0.001
	X-Bonn	-0.005	0.054	0.466
	X-Jena	-0.076	0.054	0.081
	Y-Bonn	-0.007	0.054	0.449
	Y-Jena	-0.113	0.054	0.020
	# obs. (groups): 1728 (108) Pseudo- R^2 : 0.475			
	Independent variable	Coefficient	Std. Error	p-value
(INT)	Intercept	0.256	0.095	0.004
	y	-0.001	0.001	0.178
	z	-0.001	0.001	0.107
	yNil	0.468	0.034	< 0.001
	zNil	0.064	0.032	0.025
	Ambiguous	-0.043	0.037	0.124
	Inequitable	0.096	0.043	0.013
	Y-Israel	-0.102	0.041	0.007
	Y-Palestine	0.038	0.041	0.180
	# obs. (groups): 1728 (108) Pseudo- R^2 : 0.241			

4. *Disadvantageous inequity furthers rejection.* In line with accumulated empirical evidence on the implication of disadvantageous inequity, responders reject offers significantly more frequently when they perceive to receive less than the equitable share of the pie ($p \leq 0.067$). The rejection rate linearly increases in the difference between the equitable and the responder's actually received share.
5. *Location (weakly) matters.* The geographical provenance of the proposer exercises a weakly significant influence on the responder's choice to accept or reject the former's offer ($p = 0.081$). Due to an unfavorable design modification in (INT), as a consequence of which responders' could no longer condition on the proposer's provenance, this effect could only be observed in treatment (REG). In particular, proposers from Jena faced significantly less rejection from responders than proposers from the two remaining subject pools. Bearing in mind that both the size of the effect and its significance are rather faint and, as a result, are not entirely conclusive, a replication or slight modification of this study using a larger sample seems worthwhile.

Strikingly, the provenance of the responder herself is far more important than the proposer's one in shaping the response decision. In this vein, responders from certain subject pools (participants from Jena in (REG) and Israeli participants in (INT)) turn out to be significantly more willing to accept (even unfair) offers than members of the respective other two subject pools. With regard to the latter group, i.e., the participants from Israel, likewise accounts have already been repeatedly given in the framework of bargaining experiments, emphasizing their above-average decision rationality in a wide range of strategic choice settings (e.g., Roth et al., 1991; Costa-Gomes and Zauner, 2001).

Summarizing, we find only weak experimental evidence indicating that people from different provenances and social and cultural backgrounds systematically and categorically discriminate between their bargaining partners. Instances of discriminatory behavior in proposers are almost absent and are only infrequently observed in responders. Nevertheless, since significance levels for factors of locational and social distance were found to be not entirely negligible, we cannot definitely reject the distance-based discrimination hypothesis. Given this finding, discrimination may, after all, constitute a behaviorally relevant phenomenon in cross-national and cross-cultural bargaining that deserves due consideration and justifies follow-up research.

5 Discussion

Our concern has been to explore whether people who, in a controlled laboratory experiment, engage in ultimatum bargaining with several (at least two) other interaction partners systematically discriminate between the latter on grounds of the geographical and social distance between them. To evaluate this question, we implemented a three-person ultimatum game similar to the ones of Mitzkewitz and Nagel (1993) and Güth et al. (2007b), linking a proposer, a responder, and a dummy player. Moreover, we incorporated two further specificities in our bargaining model, allowing us to garner supplementary evidence on proposers' and responders' social preferences. Once, the pie available to the proposer was stochastically determined and would eventually be revealed to the proposer alone (analogous to Mitzkewitz and Nagel, 1993). Via the strategy method, proposers were able to condition their offers (two three-dimensional vectors designating the proposer, responder, and dummy shares) conditional that the pie was either small or large. And second, responders were informed about the proposer's reduced offer vector (designating the responder and dummy shares only) and could, also via the strategic method, condition their response on the generosity of the former's offer.

Altogether, three treatments were distinguished in which the treatment variable, the geographic and social distance between the three interaction partners, was varied (in a between-subject design) between a local, an interregional, and an international pool of participants. As a result, our study structurally differs from most other thematically related experimental works on international or cross-national bargaining that purposefully involve participants from either different social groups and ethnicities or regions and countries. Unlike in our study, however, the interaction of participants in these studies typically is constraint in some way. Although bargaining experiments in this strand of research may actually involve participants from different nationalities, the latter may, for instance, only interact with their fellow citizens and may never truly mingle with foreigners (cf., Roth et al., 1991; Henrich, 2000; Oosterbeek et al., 2004). In other studies, the interaction between heterogeneous (local) groups may only take place within a single country (Fershtman and Gneezy, 2001) or may rely on highly specific samples of participants. Such studies did, for instance, focus on Ph.D. students alone (Bornhorst et al., 2004) or made heavy use of expatriates (cf., Boarini et al., 2002; Chuah et al., 2004).

Evidently, both types of studies address target groups that, essentially, are convenience samples. In our view, this approach is an entirely acceptable first step in opening up new avenues of research, but is invariably associated with the statistical problem of lacking subject pool representativeness. Thanks to our particular

experiment design, we manage to avoid some of the above constraints. Most importantly, though, we are able to directly examine and quantify people's propensity to discriminate related others based on factors of the latter's (perceived) geographical and social distance. To the best of our knowledge, there exist only few studies in which the authors took the same, truly cross-national approach and link geographically remote participants in quasi real-time (see, e.g., Anderhub et al., 2001; Eckel and Wilson, 2006).

The principal insights from this study on conditional ultimatum bargaining are the following: Participants, on average, were not found to discriminate - neither as proposers nor as responders - between their co-players on the basis of the latter's provenance, i.e., the latter's regional or national affiliation. This implies that factors of geographical and social distance are, if at all, only of secondary importance in determining the distribution of payoffs in ultimatum bargaining settings. Notwithstanding, we do discern systematic differences between the various groups in their overall extent of self-serving proposer choices and their willingness to accept largely inequitable offers. While, for instance, proposers from Berlin (Israel) in the interregional (international) treatment, on average, claimed a fraction of roughly 58% of the entire pie for themselves, participants from Bonn (Palestine) contented themselves with a significantly smaller fraction of the pie (about 46%). Similarly, responder behavior was also characterized by substantial between-group heterogeneity. While the behavior of responders from Israel may be classified as being largely rational, responders from Palestine, to the contrary, on average demonstrated a significantly higher propensity to reject both mediocre and, to our surprise, also relatively equitable offers.

More broadly, our findings replicate and, in doing so, corroborate a number of behavioral regularities that, by now, have attained the status of robust stylized facts. In this vein, we provide further empirical support suggesting that, most generally, people are ill-characterized as purely self-interested payoff maximizers. Numerous responders, for instance, would accept a payoff of zero for themselves if only this allowed the dummy to obtain a positive payoff. Likewise, a non-negligible share of responders would turn down a positive offer to themselves if the dummy had been intentionally neglected by the proposer. Further, we find that payoff outcomes in ultimatum bargaining clearly reflect the various parties' inherent bargaining power. In particular, outcomes in the three-person variant of the ultimatum game may be adequately explained via the power hierarchy distribution scheme (see Güth and Napel, 2006).

With respect to future research, subsequent studies may want to transfer the

research methodology that we applied in this study to other simple (e.g., dictator or trust game) or more complex bargaining scenarios (e.g., social dilemma or team games). Apart from being tested by further laboratory research, we would also appreciate to see our findings critically evaluated by corresponding field studies. As a special virtue, the latter approach would allow to ascertain whether the reported absence of geographic distance- and social identity-based discrimination actually is a structural feature of inter-group or intercultural bargaining behavior. Answering this question actually is not trivial, considering that the above effect may, alternatively, also be attributable to the neutral framing of the decision task³⁵ which does not provide the ecological stimuli typically found in practical negotiations among businessmen or consumers.

³⁵ See Chao and Bowles (2006) for a critical view on this issue.

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A Experimental instructions

The following instructions originally were written in German.

Welcome and thank you very much for participating in this experiment. For arriving in time, you receive a fixed amount of 4.00 USD. You will have the possibility to earn an additional amount of money during the experiment. This amount depends both on your own decisions as well as on the decisions of other participants. In the experiment, all amounts are stated in ECU (experimental currency units). At the end of the experiment, your accumulated amount of ECU will be converted into USD at the exchange rate of $1 \text{ ECU} = 0.20 \text{ USD}$ and will be paid to you in cash.

Please do not talk to any other participant in the room during the experiment. If you have questions, please raise your hand. An experimenter will then come to your place and answer your questions individually. It is very important that you respect these rules, because we otherwise would have to exclude you from the experiment and from any payoff.

Information on the participants

All participants are assigned to groups of three members each, i.e., you are together with two other participants in one group. Participants are randomly assigned to groups by the computer.

The following passage only appears in instructions for treatments (REG) and (INT):

The other two group members are **not** with you in the same room, but are located at similar computer labs at two other universities. More specifically, this experiment is jointly carried out by

- the Humboldt-University of Berlin, the University of Bonn, and the University of Jena (REG).
- the Hebrew University, Israel, the Bethlehem University, Palestine, and the University of Jena, Germany (INT).

You will be informed on your computer screen at which particular location the other two group members are located.

Each participant within a group is randomly assigned a certain role (X, Y, or Z). However, you will not be informed about your own role until the very end of the experiment.

Your task

It is the task of the three participants X, Y, and Z to distribute a positive monetary amount A . This amount can be either “small” or “large,” whereby the eventually realized amount is randomly determined. The amount will equal $A = 80$ ECU with a probability of 25% (“small amount”) and will equal $A = 120$ ECU (“large amount”) with a probability of 75%.

Initially, X proposes a distribution of the amount A among X, Y, and Z. The amount which X retains for herself is denoted by x and the amounts transferred to Y and Z are denoted by y and z . The amount x may assume the following values $x \in \{20, 40, 60, 80, 100, 120\}$ while the possible amounts for y and z are $y, z \in \{0, 10, 20, 40, 60, 80\}$. Moreover, the distribution of the amount A among X, Y, and Z is subject to certain constraints. Attached to this instruction sheet, you find a list that presents all available distribution proposals. As a general rule, the sum of the three amounts x , y , and z universally must add up to the amount A .

Since X does not yet know whether the available amount A is small or large, she must suggest a distribution conditional that A is “small” ($A = 80$) and another one conditional that it is “large” ($A = 120$). With a probability of 25% (75%), the eventual distribution proposal is the one chosen by X for $A = 80$ ($A = 120$).

In a second step, Y receives a list of all possible amounts y and z that X may propose to Y and Z. More precisely, Y is shown a table with 16 combinations of the amounts y (offered to herself) and z (offered to Z). Remember that while deciding, Y does not know whether the realized size of A actually is “small” or “large.” If Y accepts X’s distribution proposal, the participants’ respective payoffs are determined as follows: X receives x , Y obtains y , and Z collects z . If Y rejects X’s proposal, all three participants X, Y, and Z earn nothing.

Bearing in mind that you do not learn about your actual role till the end of the experiment, you must specify your decisions for the case you were assigned the role of X as well as for the case that you were assigned the role of Y. Since Z uniformly remains inactive, you do not have to make any decisions in the role of Z.

Information at the end of the period

After the experiment has finished, you will be informed about the role that was assigned to you and the ones that were assigned to the other two group members. If you were assigned the role of X by random draw, you learn about the actual amount A as well as about the amounts x , y and z that represent the participants’ respective payoffs. If you were assigned the role of Y, you only are informed about the amounts

y and z which X has passed on to you and to participant Z, respectively. Lastly, if you were assigned the role of Z, you only learn about the amount z that has been transferred to you by X. In addition, you always are informed about the response of Y, no matter which particular role was assigned to you.

Your payoff at the end of the experiment

Your final payoff is determined by your earnings in the experiment (that are converted into USD at the above exchange rate) to which the above stated fixed amount of 4.00 USD is added. The resulting sum then is paid to you in cash.

Miscellaneous

Before the experiments starts, we kindly ask you to fill in a control questionnaire on your computer screen. The questions therein are asked to ensure that you have fully understood the rules of the experiment. Also, please remain calmly seated at your place throughout the entire experiment.

If you have any questions, please raise your hand and an experimenter will come to your place and answer them.

Continuation

We now start with the second phase of the experiment. In this phase, you are asked to provide several accurate predictions of the decisions of those participants

- who are seated together with you in this room and
- who are interacting with the same constellation of co-players as you do (e.g., having a Y-player from the University of Bonn and a Z-player from the University of Jena).


The composition of your group remains identical to the one in the first phase. Also, with respect to their overall structure, the first and the second phase of the experiment are exactly alike.

However, in contrast to the first phase, you now do not state your own choices but predict how - according to your view - the largest number of participants (with characteristics corresponding to yours) have decided in the roles of X and Y. Hence, you do not specify your own actions but make several assumptions about the predominant behavior of others.

If you provide the most accurate prediction of all participants (with characteristics corresponding to yours), you receive an extra bonus of 20.00 USD in addition to your “regular” payoff from the experiment.

B Screenshot

Figure 5: Information on co-players' location

ID: Jena01 

Assume that you were assigned the role of X...

Y is located at a university in Bonn.
Z is located at a university in Berlin.

<p>Assume that the large amount has been selected.</p> <p>A=120 ECU</p> <p>Please specify your distribution proposal.</p> <p>x = <input type="text"/></p> <p>y = <input type="text"/></p> <p>z = <input type="text"/></p> <p>Sum = <input type="text"/> <input type="button" value="Compute"/></p>	<p>Assume that the small amount has been selected.</p> <p>A=80 ECU</p> <p>Please specify your distribution proposal.</p> <p>x = <input type="text"/></p> <p>y = <input type="text"/></p> <p>z = <input type="text"/></p> <p>Sum = <input type="text"/> <input type="button" value="Compute"/></p>
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