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A RADIUS OF TRUST? CONTRASTING INSIGHTS FROM EXPERIMENTS AND SURVEY DATA

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A Radius of trust?

Contrasting insights from experiments and survey data

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Abstract

A person's reach of efficient economic activities is strongly influenced by the extent to which she grants trust towards other people. The radius of trust has recently gained interest as a concept to elucidate the underlying principles of how far a person extends her trust. However, empirical research on the radius of trust has up to now only been grounded in survey data. In this paper we use an incentivized experiment, namely the trust game, and two sets of survey questions to i) identify and localize the radius of trust and ii) contrast experimental and survey results regarding the radius of trust. To do so, we measure trust layers of 394 semi-nomadic pastoralists in rural Kenya conditional on three levels of social distance: trust towards people from one's own village, trust towards people from a neighboring village, and trust towards city dwellers from the county capital. Experimental data suggest that city dwellers are excluded from the radius of trust and face particularly low trust levels, while people from one's own village and from neighboring villages are inside the radius of trust. Survey data do not suggest any clear-cut radius of trust. Implications for development practitioners and further research on the radius of trust are discussed.

Keywords: radius of trust, social distance, trust, field experiment, pastoralism, Kenya

JEL Classification: C93, D01, O12

1 Introduction

Trust is an essential part of economic interactions. Nearly all activities involving the exchange of goods or services require at least some degree of mutual confidence (Arrow 1972). Since trust is fundamental for productive cooperation, trust deficits can be severe barriers for economic development (Zak and Knack 2001; Algan and Cahuc 2010). Recent years have therefore seen trust taken center stage of economic research.

A person’s trust can be sensitive to the relationship between trustor and trustee (Karlan 2005; Buchan et al. 2006; Etang et al. 2011; Newton and Zmerli 2011; Burns 2012; Ansink et al. 2017; Gupta et al. 2018). If trust is seen as a property of a specific trustor-trustee interaction rather than a stable attribute or disposition, it follows that trustors can – and most likely do – exhibit different levels of trust depending on the target or object of trust. Accounting for such differentiation with respect to the object of trust is “crucial for uncovering the true role of trust as a civic force” (Delhey et al. 2011, p. 798).

One important differentiation of trust¹ is described through the radius of trust (Harrison 1985; Fukuyama 2000; Welch et al. 2007; Freitag and Traunmüller 2009; Etang 2010; Delhey et al. 2011; Reeskens 2013; Mattes and Moreno 2018). The radius of trust indicates “the circle of people among whom cooperative norms are operative” (Fukuyama 2000). Trust towards people within the radius of trust is generally described as “thick”, while people outside of the radius face “thin” trust (Mattes and Moreno 2018).

The radius of trust is an essential trust construct since it determines the inclusivity of the circle of cooperation for a given person or group. Civic cooperation is higher with people within the radius of trust than people outside of the radius of trust. Economic interaction between two agents can therefore be challenging if one is excluded from the other’s radius. As a result, the radius of trust can have a major influence on a person’s scope of economic activity. However, past economic re-

¹ The literature on trust generally distinguishes between social trust and political trust (Newton 2001). In this paper we focus solely on the former and use the term trust in the following to refer to social trust.

search has focused rather on overall levels or intensities of trust. Much less attention has been devoted to analyzing its radius (van Hoorn 2014).

The few studies that have looked at the construct of the radius of trust rely on survey questions (Welch et al. 2007; Etang 2010; Delhey et al. 2011; Reeskens 2013). The predictive power of such self-reported trust questions for actual economic behavior is however subject of a lively scientific debate (see Bauer and Freitag (2018) for a recent overview). Up to now it is unclear whether evidence for the radius of trust can also be drawn from experiments. We are not aware of any experimental study particularly aimed at analyzing the radius of trust. A comparison of these two methods concerning the radius of trust does ergo not exist as well.

The objective of this paper is twofold. First, we use an incentivized experiment, namely the trust game by Berg et al. (1995) to measure trust towards three different groups, each associated with an increasing length of interactive social distance. This helps revealing if people differentiate between thick and thin trust and more importantly approximating where the line between the two types of trust is drawn. Second, we use two sets of survey trust questions that relate to the same targets of trust as the experiment to examine whether survey results regarding the radius of trust coincide with experimental findings. To our knowledge we are the first using experiments to measure the radius of trust and the first contrasting findings of an experiment and survey questions in this matter.

Our analyses focus on pastoral communities in Northern Kenya. The context of rural and remote pastoralism is in itself an interesting case for research on the radius of trust. The region suffers from low institutional quality and little economic modernity. Both these factors are thought to have a negative influence on the radius of trust (Delhey et al. 2011). At the same time, pastoral communities in Eastern Africa have adapted to the challenging surroundings of the region² with close-knit networks and strong social ties (Davies and Bennett 2007), which could point towards a more inclusive and wider radius of trust. Despite this remarkable context, research on trust among

² Social challenges arise among others from frequent droughts, violent conflicts between tribes (Bond and Mkutu 2018) as well as political and social marginalization (Commission on Revenue Allocation 2012).

pastoralists is nearly non-existing. Taking a closer look at pastoralists' trust culture will therefore serve as an initial step in closing this research gap.

Given the context of rural pastoralism in northern Kenya, the radius of trust can have important implications for policy makers and development practitioners. Aid project or insurance programs that are targeted at rural populations and bear the potential to reduce poverty or risk exposure can suffer from lower uptake if providers of the service are not trusted (Takahashi et al. 2016) or might not even be accepted at all. Providers or program officers that are aware of where the radius of trust of the targeted population begins can identify suitable trusted agents that promote the program or service and facilitate successful collaboration more easily.

2 Theoretical framework

2.1 The radius of trust

A fundamental strand of trust research relates to the question if trust is unidimensional or if different dimensions of trust can be identified. While results are not entirely concordant, recent evidence converges to the proposition that (at least) two dimensions of trust exist. One type is called particular, thick, bonding, or specific trust and refers to a narrow circle of people that are socially close. The other type is called general, thin, bridging, diffuse, or abstract trust and refers to people that are more socially distant and unfamiliar (Putnam et al. 1993; Yamagishi and Yamagishi 1994; Stolle 2002; Freitag and Trauttmüller 2009). However, interpretations about the scope of the former type of trust vary. Some scholars construe particular trust in a narrow sense and only relate it to people that are well-known such as friends, family or close neighbors (Fafchamps 2006; Freitag and Trauttmüller 2009); others interpret particular trust more widely and also relate it to people that are not directly known but belong to the same identity group or clan (Putnam et al. 1993; Newton and Zmerli 2011; Mattes and Moreno 2018). In some cases, trust towards people that are not necessarily personally known, but share a common identity is referred to as identity-based-trust and treated as a third and independent trust dimension (Stolle 2002; Freitag and Bauer 2013).

The radius of trust emerges from this multidimensionality and can be understood as the border between two trust dimensions. Certain cooperative norms are active among people within the radius and less active or not active at all with people outside the radius (Fukuyama 2000). We adopt Gambetta’s (2000, p. 217) view that “trust [...] is a particular level of the subjective probability with which an agent assesses that another agent or group of agents will perform a particular action, both before he can monitor such action (or independently of his capacity ever to be able to monitor it) and in a context in which it affects his own action.” Let P_X and P_Y now be the subjective probability with which a certain agent (Z) assesses the likelihood of cooperation with persons X and Y respectively. The social distance that agent Z associates with persons X and Y is D_X and D_Y . The agent Z ’s radius of trust is denoted as R_Z . Combining Gambetta’s definition of trust with the concept of the radius of trust brings forth two deductions: First, if two persons lie within the radius of trust, they are assessed with a similar subjective probability of cooperation:

$$D_X \leq R_Z \wedge D_Y \leq R_Z \rightarrow P_X \approx P_Y \quad (1)$$

Second, if one person is inside the radius of trust and another person is not, the first person is assessed with a considerably larger probability of cooperation compared to the second person:

$$D_X \leq R_Z \wedge D_Y > R_Z \rightarrow P_X > P_Y \quad (2)$$

Several studies using both experimental and survey data present contexts in which people exhibit less trust towards people that are more socially distant (Buchan et al. 2006; Etang et al. 2011; Burns 2012; Binzel and Fehr 2013). The deduction presented in propositional expression (1) however suggests that even when a person associates two other persons with a different length of social distance, he or she might not discriminate between them in terms of trust, if both are inclusive his or her radius of trust. The second deduction implies that a third, even more socially distant person however is trusted less, if he or she is excluded from the radius of trust.

To analyze the radius of trust it is therefore necessary to have at least three different objects of trust that are associated with varying degrees of social distance. The more trust levels for different objects of trust are specified, the more precisely the radius of trust can be located. Most experimental studies that link social distance and trust however treat social distance as a binary charac-

teristic and define it as either zero or unity depending on whether two persons do or do not belong to the same group. While such binary comparisons are useful setups to point out ingroup/outgroup trust biases, they allow only limited conclusions on the radius of trust.

Propositions (1) and (2) consist of three elements: social distance, trust, and the radius of trust. Multiple techniques are available to measure the two former elements. The radius of trust however is unknown and also not directly measurable. It is therefore impossible to assess if the statements on the left-hand sides of propositions (1) and (2) are indeed true. Since we cannot assume logical equivalence, we can only infer with certainty from the statements on the left-hand side of propositions (1) and (2) to the statements of the right-hand side. In other words, observing a true statement on the right-hand side is not sufficient to infer that the statement on the left-hand side must be true as well. It is therefore important to note that empirical analysis cannot provide any prove for or against the existence of the radius of trust. We can however analyze if experimental and/or survey data match the model's predictions while ruling out alternative explanations. The first hypothesis therefore states:

H1: There is a radius of trust which leads to different trust levels for the in-group and out-group.

2.2 Measuring trust

The importance of trust for interpersonal relationships is widely recognized. There is however an expansive academic discussion on how trust can be measured adequately. Experiments and survey questions present the two most popular methods to do so. The former have the advantage of revealing actual behavior and – if incentivized – bearing real economic consequences for the respondent. The latter are cheaper, easier to collect, and allow for more design flexibility. Analyses concerned with more complex dimensions of trust such as time horizons of trust or more specific trust domains are thus easier done using surveys rather than experiments. Depending on what aspects and which targets of trust are of interest, survey questions might be more convenient. The relative rigidity of trust experiments compared to survey questions is presumably a reason for the scarcity

of experimental studies on more complex trust issues such as the radius of trust. Their findings should nevertheless coincide when both methods deal with the same target of trust.

Experimental data on trust is generally considered to be a decent proxy of actual trust (Bouma et al. 2008; Cardenas and Carpenter 2008). Correlations between experimental measurements and survey questions however remain widely disputed with contradicting results found in several studies (Glaeser et al. 2000; Karlan 2005; Danielson and Holm 2007; Etang et al. 2012). The divergence of results has led to the assumption that the informative value of survey questions hinges on the context in which they are used (Etang et al. 2012). This study presents two contexts, namely the sample of sub-Saharan pastoralists and the methodical focus on the radius of trust, for which a comparison of survey and experimental trust data does not exist up to now. The suitability of survey questions for either context is therefore unclear. To shed light on this aspect, we measure trust with incentivized experiments as well as survey questions and contrast the results of the two measuring methods. The second hypothesis consequently states:

H2: The measurement of the radius of trust is independent of the method used for elicitation.

3 Data

This study uses primary data collected from July to August 2018 in Turkana County, Northern Kenya. We followed a two-stage sampling approach. In the first stage, we selected five larger clusters of villages with approximately fifteen villages in each cluster. All villages lie within a one to two hours' drive from the county capital of Turkana County.³⁴ We randomly selected four to five villages from each cluster. In the second step, we obtained lists of all households living in each of the selected villages with the help of local area chiefs. Afterwards 20 to 26 households per village were randomly selected and the household head invited for participation. All but two of the invited households accepted the invitation and finished the interview. In case the household head was una-

³ Due to intertribal conflicts involving violent raids and road blocks, one group of villages was not safely accessible. The final sampled villages were drawn from the remaining four groups.

⁴ The county capital of Turkana County is Lodwar and has a population of approximately 48,000 people (Commission on Revenue Allocation 2013).

available, the second person in charge of that household was invited. The sample includes 402 households from 17 villages.

3.1 Trust

3.1.1 General Setup

We conduct a variant of the trust game by Berg et al. (1995) (BDM), which has become one of the most established experiments to measure trust. The BDM involves two players, here called player A and player B. Several variations of the BDM exist, but the game is usually set up in the following way: player A is endowed with some amount of money. She can send a share of this endowment to player B. The researchers then triple whatever amount is sent. Upon receiving the tripled amount, player B can choose to send any share of this money back to player A. This transfer concludes the game. There is usually no communication allowed between the two players, before, during, or after the game and both players remain anonymous. The share of the amount sent by player A is regarded to measure his/her trust in player B; the amount returned by player B is referred to as his/her trustworthiness.

The actual players' identities are usually kept secret during and beyond the game to ensure that individual choices cannot be traced back. Experimental trust studies on social distance therefore create exogenous variation by providing some general information, so-called *cues to identity* about player B, for instance the player's religion, gender, ethnicity, or surname. In the study at hand, player A received information on where player B lived.⁵ We conducted three treatments by letting the respondents play the trust game with a partner belonging to one out of three possible groups: respondents were either paired with another fellow villager, or with a villager from a neighboring village, or with a city dweller from the county capital. We assigned treatment randomly at the individual level.

⁵ Cues to identity about the approximate physical distance to player B's group are motivated by Karlan (2005). He finds that when people know the exact person with whom they are playing, more trust is placed in people who live close by.

The amount sent in the trust game is generally considered a valid measurement for player A’s trust in player B. To reduce confounding factors such as altruism that might also influence the transfers, we let respondents play both the role of the sender and the receiver (Schechter 2007). We endowed each respondent with 500 Kenyan Shillings (about 5 USD). This amount of money is substantial for most households in Turkana, since more than ninety percent of the people in that region live below the poverty line of 1.90 USD per day (Turkana County Government 2014). The high stakes of the experiment increased the financial consequences of each decision and the attractiveness of the survey. The threat of having a biased sample in which only more trusting individuals accept the invitation to participate, while lower trusting individuals do not participate is consequently small. All payouts were realized, since random payment has shown to bias behavior in the trust game (Johnson and Mislin 2011).

3.1.2 Specifics for measuring the radius of trust

Since player A’s total payoff depends on how much player B decides to return⁶, player B’s response conditional on player A’s choice needs to be known. If player A and player B are both part of the same experimental session, this sequentiality is unproblematic; player A and player B not participating in the same session, however, can cause complications. Past research has dealt with this issue in different ways. Etang et al. (2011) waited with all payouts until data collection was fully completed, causing a time lag of seven days between playing the game and receiving remuneration. Gupta et al. (2018) held experimental sessions simultaneously in different villages, but transmitted information on amounts sent and received via mobile phones. It is also possible to trick respondents into believing that they play with a certain counterpart (deception), when this counterpart actually does not exist – or at least does not participate in the game – as done for example by Tanis and Postmes (2005). If player A fully believes the researchers, simulated counterparts theoretically produce the same results as real counterparts. However, people behave differently in exchange situations, when they know that they are paired with a computer rather than an actual human being (Sanfey et al. 2003). Respondents that do not fully believe the researchers are there-

⁶ Except for the case in which player A sends nothing.

fore likely to alter their actions. Johnson and Mislin (2011) find evidence for this bias in the trust game.

Due to the long physical distance between the county capital and our target villages, we were unable to have joined experimental sessions with both pastoralists and city dwellers. Waiting to complete the payouts until all data were collected also proved difficult due to challenges in tracking down the semi-nomadic respondents and the generally low levels of trust among the target population. We therefore employed the strategy method, which is commonly used in trust game literature (Johnson and Mislin 2011; Sapienza et al. 2013).

The strategy method relies on asking player B how much she would like to return to a player A, given all potential amounts that player A might have sent. Player A had 500 KSh at her disposal and could make transfers in steps of hundred Kenyan Shillings. We therefore asked player B about her returns for all five possible positive amounts sent. Johnson and Mislin (2011) do not find that using the strategy method affects behavior of player A in the trust game. A comprehensive description of how players were matched is presented in the Appendix A2 and Table A1. Sending decisions of respondents paired with a neighboring villager made in the last session were not consequential, since there were no subsequent sessions in which the sending decision could have been used. To ensure incentive compatibility, we exclude these eight observations from the analysis (see Table A1).

Only people from the respective village were physically present in each experimental session, while city dwellers and pastoralists from neighboring villages were absent. This could result in a game-theoretical advantage of intra-village pairings over inter-village pairings and pairings with city dwellers. Several characteristics of our design ensured that this potential source of communication bias is kept to a minimum. First, we prohibited any questions or comments about the game during the explanation in the group.⁷ We also made it very explicit that violations of this rule would lead to exclusion from the game. Two assistants, one of which was a well-known and respected elder of

⁷ We explicitly encouraged respondents to ask questions or state any uncertainties about the game later in private.

the region, constantly supervised the respondents. Second, we told respondents if they were paired with a fellow villager or with a neighboring villager or with a person from the county capital in private just before they made their decision. Third, we kept all decisions anonymous. If player A and player B found some way to agree on a cooperation where player A's promise to send substantially and player B promise to return substantially, each player B still has strong incentives to deviate from this agreement since his/her player A has no way to punish a contract breach.

3.1.3 Experimental procedure

Since a large share of our sample is illiterate and unfamiliar with economic experiments, we opted for a single blind protocol giving respondents enough room for individual questions. Some of the respondents, especially the elderly, also suffered from reduced dexterity and required manual assistance in handling the banknotes. Our presence could have introduced some bias in the respondents' actions compared to a double blind procedure. Single blind protocols are however usually used in low income country contexts (Barr 2003; Schechter 2007; Karlan 2005; Etang et al. 2011; Johansson-Stenman et al. 2013). Johnson and Mislin (2011) do not find that deviating from a double blind protocol induces significant biases in player A's behavior. The script used during the experiment is shown in the Appendix A1. The sequence of all elements of the data collection is presented in Table A2.

With the exception of the corresponding author, all members of the research team directly involved in the data collection are native Turkana. One of the eight people in the team was born in the county capital, and all of them spent at least some period of their life in an urban environment. No member ever lived in one of the sampled villages.

We derive two trust variables from the experiment. The first measurement of trust is the amount sent in the trust game. As mentioned earlier, respondents could send money to player B in steps of 20% of their initial endowment. The share of the amount sent consequently has a discrete distribution, is left-censored at zero, and right-censored at 100%. Our second trust measurement based on the experiment is the expected return: after respondents chose how much they wanted to send, we

asked them how much money they expected to be returned by Player B. We define the expected return as the share of the amount that player A expected player B to return relative to the total amount received by player B. If player A sent nothing, we mapped her expected return to zero.

3.1.4 Survey questions

After playing the trust game, we conducted short surveys with each respondent, as part of which we also asked two sets of trust related questions. One set included context independent trust questions and another set focused on trust questions within a specific context. In the former set we asked three questions: “How much do you trust people living in [A: this village? B: neighboring villages? C: the county capital?]”. Respondents could choose between the answering options “Not; a little; somewhat; a lot” as done for example by Mattes and Moreno (2018). Due to the respondents’ unfamiliarity with percentages and self-evaluation on continuous numeric scales, we used discrete answering options with labels rather than continuous scales. Throughout the data collection we used the actual name of the county capital.

Our context dependent trust question depicts a concrete scenario in which respondents stated their belief regarding other people’s behavior. We asked three questions: “Assume that you are walking in [A: your village; B: a neighboring village, C: the county capital] and 1,000 KSh (approximately 10 USD) that are yours fall out of your pocket without you realizing it. Someone else from [the village/county capital] sees that the money fell out of your pocket. He or she can pick up the money and take it as his/her own without incurring any punishment or he/she can tell you that you lost the money. How likely do you think it is that he or she will tell you that you lost the money and return it to you?” The answering options were “Very unlikely; rather unlikely; rather likely; very likely”. The context dependent questions are similar to questions used by Sapienza et al. (2013) and Etang et al. (2012), who ask about the likelihood of a lost wallet being returned.

3.2 Social distance

Social distance has a fundamental influence on interpersonal relationships and individual economic decisions (Akerlof 1997). To conceptualize social distance it is first necessary to clarify which as-

pects of social distance are actually being targeted. This is particularly important when social distance and trust are jointly analyzed, because both concepts exhibit substantial overlap. Hoffman et al. (1996) for example define social distance as “the degree of reciprocity that subjects believe exist within a social interaction”, which closely resembles many definitions of trust. To allow for a more precise differentiation between trust and social distance, this paper focuses on the dimension of social distance that is based on the interaction frequency between two persons, namely interactive social distance (Kadushin 1962; Karakayali 2009).

The frequency of interaction between two persons can be measured on a continuous scale; interactive social distance is therefore also likely to follow a continuous – or at least step-wise – distribution. The gradual nature of interactive social distance is in contrast to forms of social distance based on binary group identity or group affiliations, which dominate the trust literature, and makes interactive social distance a well-suited dimension to capture the radius of trust.

4 Results

This section provides the results of our analyses with the overarching objective to a) analyze if the theoretical predictions of the radius of trust are supported by experimental behavior and b) contrast if survey trust suggests the same results as the experiment. Descriptive statistics first give an overview of the sampled population’s socioeconomic characteristics. Comparisons of behavior in the trust game and survey answers for the three treatments are then based on population means. Regression analyses afterwards corroborate if findings also hold conditional on the respondents’ characteristics.

4.1 Descriptive statistics

Table 1 reports summary statistics of the sample. We show characteristics that have been found to be relevant determinants of trust in previous studies, for example age, education, gender, and in-

come. Other characteristics are relevant particularly in a pastoral setting, such as the livestock owned by the households.⁸

Table 1: Summary statistics for total sample (N = 394)

Variable	Mean/share	Standard deviation	Range
Age [years]	39.64	14.43	[13;80]
Constant relative risk aversion coefficient (CRRA) ^a	2.18	2.00	[0;5]
Distance to the next paved road [walking minutes]	27.43	45.03	[1;420]
Education [years]	1.14	3.01	[0;14]
Female	0.51		
Herd size [TLU] ^b	32.80	35.81	[0;251.2]
Household size	8.11	3.86	[1;25]
Income per year [1000KSh] (1000 KSh \approx 10 USD)	5.55	11.45	[0;95]
Mobile phone user [1 = yes]	0.81		
Religion [1 = Christian]	0.98		
Village size [100 people]	5.63	3.94	[1.75;15.60]
Years lived in the village [percentage of lifetime]	0.77	0.32	[0.02;1]

Notes: CRRA = Constant relative risk aversion. TLU = Tropical Livestock Unit. KSh = Kenyan Shillings.

^a Elicitation method based on an incentivized Eckel and Grossman (2002) task (see Table A3)

^b One tropical livestock unit equals 1 head of cattle, or 0.7 of a camel, or 10 goats, or 10 sheep (Mburu et al. 2017).

Source: Own elaboration.

The sample is largely homogenous in terms of religion (Christianity), ethnicity (Turkana) and language (Turkana). This homogeneity is vital for trust research, since understandings and interpretations of experiment and survey trust can differ strongly depending on cultural backgrounds (Cronk 2007; Reeskens and Hooghe 2008; Torpe and Lolle 2011).

⁸ Treatment in the trust game was assigned randomly. Descriptive statistics differentiated by treatment are presented in Table B1 in the appendix.

Income and education levels in the sample are considerably low. More than 85% of the respondents never went to school. With 8.09 members, the average household is relatively large, which is due to several generations and other relatives often residing in one common homestead.

In this study, we measure trust towards three different groups: fellow villagers, people from a neighboring village, and city dwellers from the county capital. To ensure that the radius of trust lies within one's scope of analysis, it would be necessary to measure trust towards groups that are socially closest and would therefore undoubtedly lie within the circle of trust, that is spouses, best friends, or close family, as well as trust towards groups that are socially most distant and would therefore undoubtedly lie outside the circle of trust, that is unknown people with whom one shares minimal similarity. Extremely narrow or extremely wide radii might otherwise lie outside the range of analyzed trust levels. These corner solutions are however of little relevance from an economic perspective, when economic interactions with such groups occur at a low frequency.

Table 2 shows how often respondents in our sample interact with people from neighboring villages and the county capital. As expected, neighboring villages are visited considerably more often than the county capital. Only nine percent of the respondents never or at least extremely rarely visit the county capital. The other 91.12% visit the county capital at least once a year and are therefore likely to engage in situations in which trust towards city dwellers might be relevant.

Table 2: Frequency of interaction with people from neighboring villages and the county capital

	...neighboring villages [%]	...the county capital [%]
Visiting ...at least once a year	99.76	91.12
...at least once a week	93.91	20.81
...every day	75.38	5.58
Equal interaction frequency (Wilcoxon signed-rank test):		$z = 16.813^{***}$

Notes: N = 394. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Own elaboration.

The three groups analyzed in this study therefore cover a wide range of interactive social distance, but still represent groups of people with whom pastoralists in a rural setting actually engage in

some form of economic interaction e.g. ad hoc market interactions to purchase livestock, processed foods, or basic electronic devices. Trust towards a fellow villager thus relates to the closest possible platform of economic interaction for the respondents; trust towards a city dweller relates to the border of social distance that is still relevant for most respondents in our sample; trust towards a person from a neighboring village relates to a middle part between these two extremes, because interactive social distance is longer compared to one's own village but shorter compared to the county capital.

4.2 Comparison of means

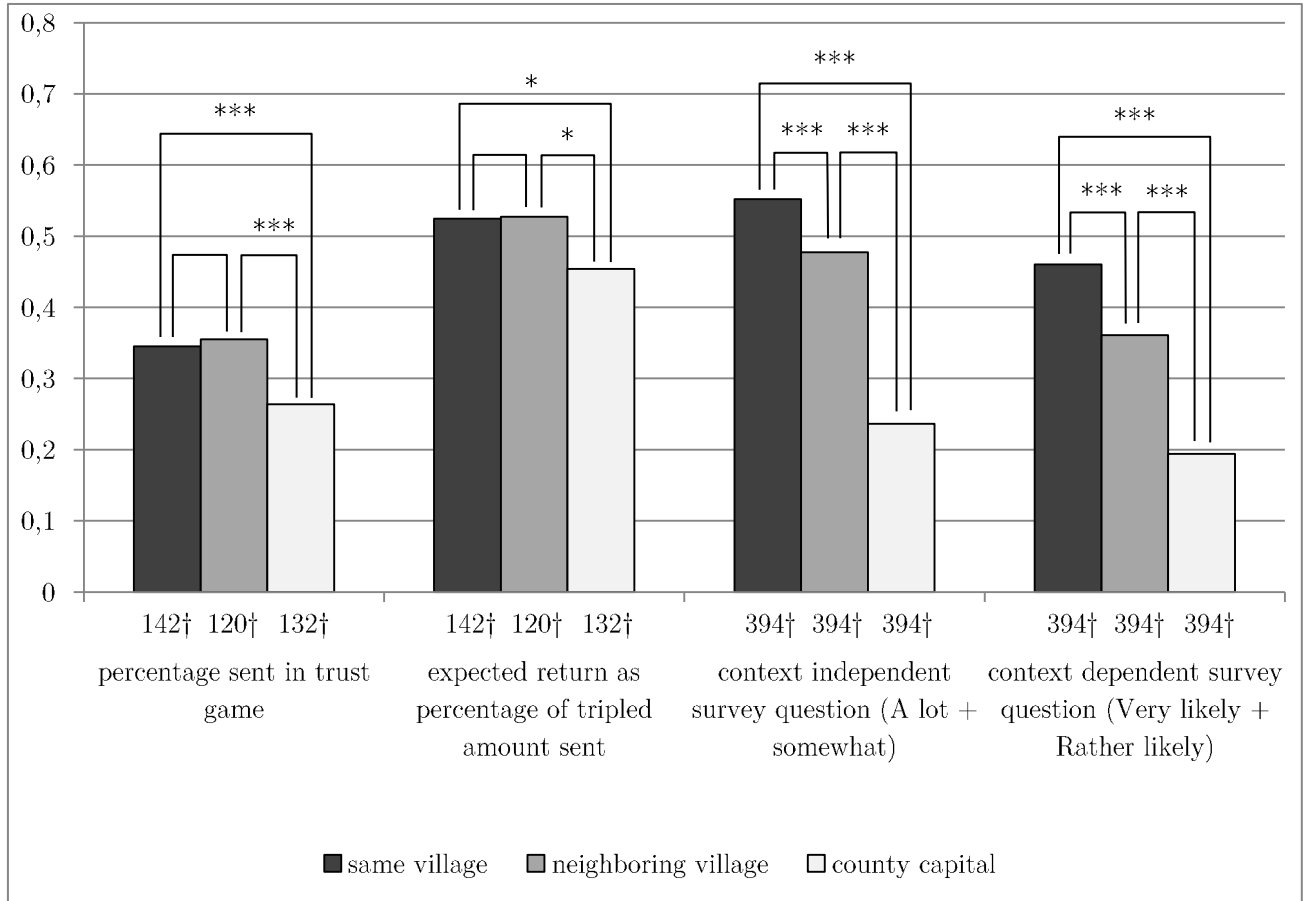
Figure 1 shows levels of trust towards fellow villagers, towards people from a neighboring village, and towards city dwellers for four different measurements of trust. The first three bars represent the share sent in the trust game. Respondents sent on average 34.5% to fellow villagers. Only slightly more was sent to people from a neighboring village, but the difference of 1.0 percentage points is not statistically significant ($p = 0.61$). City dwellers received only 26.4% on average. Mann-Whitney U tests prove the differences between the share sent to city dwellers and both fellow villagers and villagers from a neighboring village to be statistically significant at a 1% level.

The second bar triplet of Figure 1 shows the share that respondents expected player B to return. When paired with a fellow villager, respondents expected to receive back 52.5% of the amount that player B received. In pairings with a person from a neighboring village, respondents expected to receive 0.3 percentage points more compared to intra-village pairings. This difference is not statistically significant ($p = 0.96$). When paired with a city dweller from the county capital, respondents expected player B to send back only 45.4% of what they received. The difference between pairings with city dwellers and intra-village pairings is statistically significant at a 10% level according to a Mann-Whitney U test ($p = 0.07$). The same holds for the difference between pairings with city dwellers and inter-village pairings ($p = 0.07$).

The third set of bars of Figure 1 shows the share of respondents that answered the context independent survey questions with either “somewhat” or “a lot”. Since the survey questions were an-

swered by the entire sample, we have 394 observations for each dimension of interactive social distance. A little more than half (54.8%) of the respondents stated their trust towards fellow villagers. A little less than half of the respondents (47.8%) stated to trust people from a neighboring village and only 23.1 % said to trust city dwellers from the county capital. We use sign tests that incorporate all four possible answering options of the survey question. All differences are statistically significant at a 1% level.

Figure 1: Trust levels using experiments and survey questions



Notes: Differences between means are tested for statistical significance using the Mann-Whitney U test (bars 1 – 6) and the sign test (bars 7 – 12). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. † = sample size.

Source: Own elaboration.

The fourth bar triplet of Figure 1 shows the share of respondents that answered the context dependent survey question with either “very likely” or “rather likely”. The context dependent survey question regarding their own village was answered positively by 46.2%. The respective values concerning a neighboring village and the county capital are 36.3% and 19.5%. We again use sign tests

that incorporate all four possible answering options. Similar to the context independent survey question, all differences are statistically significant at a 1% level. Disaggregated survey responses are shown in Figure B1 and Figure B2 in the appendix.

4.3 Regression analyses

Since treatment was assigned randomly, the comparison of means represents an unbiased method to detect differences in trust across the three treatments. To corroborate the stylized facts and analyze if results change when socioeconomic characteristics are included, we also employ formal regression analyses.

Multivariate regression results for the four trust variables are presented in Table 3. Column (1) has the amount sent as the dependent variable and coefficients are based on a censored interval estimator. Column (2) has the share expected to be returned by player B as the dependent variable and coefficients are based on a tobit model. Trust towards fellow villagers is the reference point.

As mentioned earlier, each respondent answered six survey questions: a context dependent and a context independent question concerning trust towards a person from a) the same village, b) a neighboring village and c) the county capital each. To compare the answers for these three levels of social distance, we combine a), b), and c) in one regression. Consequently we have only two regressions instead of six: one for context dependent survey trust and one for context independent survey trust. Since each respondent now appears in the regression with three observations, the sample size is three times as large as the original sample size. Both context dependent and context independent questions have four ordinal answering options. While the order of the answering options is unambiguous, distances between adjacent answering options are unknown and might be inconsistent. The answer “*rather likely*” could for example be closer to the answer “*very likely*” than to the answer “*rather unlikely*”. Consequently we run ordered logistic regressions, but robustness checks show that results are robust for other estimation techniques such as ordered probit or techniques that do not account for potential differences in distances between answering options.

Table 3: Multivariate regression analyses on the radius of trust

Dependent Variables Independent Variables	Share sent in the trust game (1)	Share expected to be returned by player B (2)	Context inde- pendent survey question (3)	Context de- pendent survey question (4)
Trust towards people from the same village (SV)	BASELINE	BASELINE	BASELINE	BASELINE
Trust towards people from a neighboring village (NV)	1.167 (1.886)	0.197 (2.778)	-0.305*** (0.099)	-0.186** (0.082)
Trust towards city dwellers from the county capital (CD)	-7.889*** (2.742)	-8.361** (3.661)	-1.626*** (0.142)	-1.023*** (0.104)
Control variables?	YES	YES	YES	YES
N	394	394	1,182	1,182
Wald test: NV = CD	***	*	***	***

Notes: Estimates in column (1) are based on censored interval regression. Estimates in column (2) are based on a tobit model. Estimates in columns (3) and (4) are ordered log-odds estimates from ordered logistic regression. Standard errors shown in parentheses are robust and clustered at the village level for columns (1) and (2), and clustered at the individual level for columns (3) and (4). The control variables are: age, constant relative risk aversion coefficient, distance to the next paved road, education, gender, herd size, household size, income, mobile phone use, village size, and the years lived in the village. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The results overall confirm the stylized facts presented in Figure 1: Respondents place significantly less trust in city dwellers compared to fellow villagers and people from a neighboring village. This is consistent for both experimental and survey measurements of trust. While the survey questions also suggest a step-wise decline of trust with regards to people from neighboring villages, outcomes of the trust game do not. In fact, raw sending decisions of the trust game even show larger shares sent to people from a neighboring village compared to fellow villagers, albeit marginal and insignificant. Regression results for the control variables' coefficient estimates can be found in Table B2 in the appendix.

How do these findings relate to the theoretical implications of the radius of trust? As shown earlier, (interactive) social distance increases from the same village, to the neighboring village, up to the county capital. We first look at experimentally measured trust: city dwellers are trusted substantially less than fellow villagers or people from a neighboring village. Trust towards people from the same village is however very similar compared to trust towards people from neighboring villages: depending on the outcome variable, the difference between intra-village and inter-village trust ranges from -1.0 percentage points to -0.3 percentage points.

All logical statements presented in section 2 are true under these conditions, if the radius of trust lies between people from the neighboring village and city dwellers: social distance to fellow villagers is longer than social distance to neighboring villagers, but trust levels towards these two groups are similar. This is in accord with proposition (1); city dwellers are even more socially distant, and trust levels towards city dwellers are indeed substantially lower. This is in accord with proposition (2). The trust experiment therefore suggests that a) pastoralists do exhibit a radius of trust, and b) that this radius is drawn so that fellow villagers and people from a neighboring village are within the radius and city dwellers from the county capital are outside of the radius. We therefore confirm hypothesis 1.

Trust levels based on the survey questions manage to reproduce the disparity of trust towards fellow villagers and trust towards city dwellers that becomes evident through the experiment. However, survey trust deviates from the experiment in the sense that the difference between intra-village and inter-village trust is substantial and statistically significant. As a result, the survey questions do not support the claim that people from a neighboring village are within the radius of trust as defined in section 2. We thus do not find support for hypothesis 2.

5 Discussion

Our analyses are – as virtually all empirical studies that employ some form of statistical tests – based on testing a statistical model incorporating the hypothesis that a particular effect has a cer-

tain size. In the study at hand we test if differences in trust between treatment groups are zero. Two aspects regarding the limits of statistical hypothesis testing deserve further discussion here.

First, statistical significance does not per se imply economic significance: effects can be statistically significant, but when effect sizes are small in absolute terms, they might not be economically relevant. As shown in Figure 1, all statistically significant differences between treatments also have a substantial effect size. Transfers sent towards city dwellers for example are on average 23.6% lower than transfers to fellow villagers, and 25.8% lower than transfers to people from a neighboring village. The pitfall of a “sizeless stare” (McCloskey and Ziliak 1996; Hirschauer et al. 2018) can therefore be avoided in this study.

The second and far more intricate limitation is the difficulty to draw conclusions from statistically insignificant effects. A large p-value suggests that the null-hypothesis should not be rejected, but does not provide any evidence in favor of the null-hypothesis. As long as the p-value is not equal to one, there is always an alternative hypothesis that is more compatible to the data than the null-hypothesis (Greenland et al. 2016). This renders proving negatives, for example that a difference between two treatments is precisely zero, extremely difficult. Translated to our case, it means that there is no statistical certainty that experimental trust between fellow villagers and neighboring villagers is actually the same. In fact, it is rather unlikely that the difference is precisely zero. However, the theoretical framework on which we built our analysis does not necessarily require groups within the radius of trust to face strictly equal levels of trust. It is sufficient if trust towards groups within the radius is similar, thus allowing room for small potential differences.

The differences between intra-village and inter-village trust we find in the experiment are only marginal: the 95% confidence intervals for the differences in means between inter-village and intra-village pairings only range from -3.2 points to 5.2 points for the amount sent and -5.9 points to 6.4 points for the expected return. Trust towards fellow villagers is therefore sufficiently similar compared to trust towards people from a neighboring village, to support the claim that the radius of trust lies between people from a neighboring village and city dwellers from the county capital.

How can the deviation of measuring techniques regarding differences between intra-village and inter-village trust be explained? First, it is important to note that the general trends of the two measuring techniques presented here correspond to other existing studies. A monotonously falling relationship between social distance and survey trust is also seen in other studies based on survey trust (Etang 2010; Mattes and Moreno 2018). We are aware of only one other study that offers a comparison of experimentally measured trust for both intra-village and inter-village relations: Etang et al. (2011) conduct a trust experiment in two Cameroonian villages without a focus on the radius of trust and find that people send statistically significantly more to fellow villagers (74%) than to people from the other village (63%). We do not observe this discrimination in the study at hand. However, expected returns displayed in Etang et al. (2011) are actually very similar for intra-village pairings (46%) and inter-village pairings (47%). This resembles our finding that experimental trust towards fellow villagers and neighboring villages is rather similar.

An explanation for the deviation of survey trust and experimental trust could be based on an over-estimation of survey trust in fellow villagers relative to trust towards more socially distant groups. Respondents might for example purposefully overstate their trust towards fellow villagers, because they want to present themselves as a particularly connected, trusting, and trustworthy member of the community and hope for some reward in the future. In an incentivized game however, trust towards fellow villagers is shown to be somewhat lower than the orally reported trust. This form of social desirability bias would then support the claim that survey trust is not a sufficiently precise predictor of actual trust in some contexts. However, the survey questions do coincide with the experiment regarding statically significantly lower trust towards city dwellers compared to fellow villagers and people from a neighboring village. This can be interpreted as support for the validity of survey trust questions.

6 Conclusion

Trust is an important mechanism to overcome market failures such as imperfect contract enforcements. Economists have therefore long argued that trust deficiencies are responsible for much of the economic underdevelopment in the world (Arrow 1972). The radius of trust presents a key trust aspect because it indicates how far a person extends his or her trust. It thereby determines the reach of productive economic interaction within a society (van Hoorn 2014).

Up to now, the relatively scarce research on the radius of trust exclusively relied on survey questions. In this study we have used an incentivized experiment, namely the trust game by Berg et al. (1995) and two sets of survey questions to localize the radius of trust of pastoralists in Northern Kenya and contrast the results of both measuring methods.

We find that behavior in the trust game can be well explained by the concept of the radius of trust. Pastoralists' trust towards people from the same village closely resembles trust towards people from neighboring villages, even though social distance towards the latter group is considerably longer than to the former group. This suggests that both groups are included in the radius of trust. Trust towards people from the county capital is substantially lower compared to both trust towards fellow villagers and trust towards people from neighboring villages, which suggests that city dwellers are excluded from the radius of trust.

Survey questions in contrast do not fully coincide with the results of the incentivized experiment. This adds fuel to the fire concerning the discussion about the validity of survey trust questions. We do not give one-sided support for either side however: the survey questions manage to reproduce the disparity of trust towards fellow villagers and trust towards city dwellers that becomes evident through the experiment. This supports the validity of survey trust questions. However, the survey questions also indicate a trust difference between fellow villagers and people from a neighboring village. This difference is not statistically significant in the experiment. This deviation of survey trust and experimental trust could be based on an overestimation of survey trust in fellow villag-

ers. The existence of a clear-cut radius of trust can therefore not be drawn from survey questions in the context at hand.

Our results suggest that experimental measuring techniques are inevitable for further research on the radius of trust and should at least complement survey data. At the same time, it is methodology desirable to measure trust towards multiple different groups, in order to localize the radius of trust as precisely as possible. The three groups presented in this paper constitute the minimal number of groups needed to draw conclusion on the radius of trust and further research based on a larger number of groups can certainly help drawing a more detailed picture. In particular it might be worthwhile to use experiments to measure trust towards very close persons such as family members or close friends. The technique of matching players presented in this paper could be useful to reduce the logistical challenges that such an extensive research design would entail.

The political situation in the analyzed region caused some constraint to our sampling procedure. Areas of Turkana County that are physically close to neighboring tribes are particularly prone to raids and conflicts. Four of the 21 villages that were initially targeted for data collection were thus inaccessible for the research team during the time of data collection. The inability to include these communities might have reduced the representativeness of the data at hand for the rest of the county's population. These areas should be included in further follow up research, when intertribal tensions reduce.

The results of this study are relevant for policy makers and development practitioners. Kenya's national policies have recently shifted their focus more towards supporting pastoral livelihoods (Odhiambo, 2013). The relative lack of trust towards people from the county capital should concern any person that works with pastoralists, but does not come from a Turkana village, because such trust deficits could lead to lower cooperation of pastoralists and thus hamper the implementation of development projects. The involvement of trusted agents might help to overcome trust barriers and secure successful collaboration. Our results suggest that suitable agents do not necessarily need to come from each individual village, since people of neighboring villages seem to be similarly trusted.

7 References

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Appendix A: Game protocol

Appendix A1: Game protocol

The protocol used for the field experiment of this study is very closely related to a game protocol employed by Schechter (2007). She in turn builds up on the project “The Roots of Human Sociality: an Ethno-Experimental Exploration of the Foundations of Economic Norms in 16 Small-Scale Societies” by Barr, Barrett, Bolyanatz, Cardenas, de la Pena, Ensminger, Gil-White, Gurven, Gwako, Henrich, Johnson, Marlowe, McElreath, Lesorogol, Patton, and Tracer.

Appendix A1.1: Opening instructions

Thank you all for taking the time to come today. We are here with you today to play a game with you and to ask you some questions. We will first start with a game and afterwards conduct interviews with each one of you. We invited you here today, because you were randomly selected from this village to participate in this activity. Only those people that were selected can participate. The decision of who was to be selected was not made by us, but by chance. We did not influence the selection and cannot change it.

The game that we will be playing now and the interviews will take approximately 4 hours, so if you think you will not be able to stay that long without leaving please let us know now. Before we begin I want to make some general comments about what we are doing here today and explain some rules that we need to follow. We will be playing a game for real money that you can take home. You should understand that this is not our own money. It is money given to us by a university to use to do a research study. This research will eventually be part of a book. It is not part of a development project of any sort. There are many researchers who are carrying out the same kind of games all around the world.

Before we proceed any further, let me stress something that is very important. Many of you were invited here without understanding very much about what we are planning to do today. If at any

time you find that this is something that you do not wish to participate in for any reason, you are of course free to leave whether we have started the game or not.

We are about to begin the game. It is important that you listen as carefully as possible, because only people who understand the game will actually be able to play it. We will run through some examples here while we are all together. You cannot ask questions or talk about the game while we are here together. This is very important and please be sure that you obey this rule, because it is possible for one person to spoil the game for everyone, in which case we would not be able to play the game today. Do not worry if you do not completely understand the game as we go through the examples here in the group. Each of you will have a chance to ask questions in private with [Name of enumerator and field assistant] to be sure that you understand how to play.

This game is played by two people, which we call player 1 and player 2. All of you will first play this game as player 1. But each of you will have a different partner, with whom you play the game. For some of you player 2 will be someone from this village. For some of you player 2 will be a pastoralist from a neighboring village and for some of you player 2 will be a city dweller from Lodwar. We will later tell each of you individually if you will play the game with someone from your village, or with a pastoralist from a neighboring village, or with a city dweller from Lodwar. However, none of you will know the name of the person with whom you are playing. Only [name of corresponding author] knows who is to play with whom and he will never tell anyone else. You will only know if it is a person from your village, or a pastoralist from a neighboring village or a city dweller from Lodwar.

Let me explain the game. We will soon talk with each of you in private and we will give each one of you 500 Kenyan Shillings and an envelope to play with. Then we will tell you if you play this game with someone from this village or someone from a pastoralist from a neighboring village or a city dweller from Lodwar. You then have the opportunity to send any portion of that money to the person you are paired with by putting it into the envelope. Whatever amount you put into the envelope will be tripled by the research team and then the envelope will be passed to the other player. Whatever amount you do not put into the envelope can be kept by you. We triple the

amount of money that you put into the envelope and pass the envelope to the player 2. That player has the option of returning any portion of this tripled amount to you. You must know that all the other players also played this game before in the role of player 1. At the end of today, when everyone here played the game, you will receive the envelope back from your partner. At the end of the game you can keep all the money you did not put into the envelope plus anything that was returned to you by player 2.

Then, the game is over.

You see that there is no person here from Lodwar and no person from a neighboring village. So you will ask yourself, how we know if your partner wants to send something back to you and if yes, how much your partner wants to send back to you. So let me explain. We have played this game before in Lodwar and also in neighboring villages with other pastoralists and we have asked them to write down how much they want to send back to you. So we have a list where people told us if they want to send something back and if yes, how much they want to send to you. Only [name of corresponding author] has this list and he will not show it to anyone or talk with anyone about it.

Let us now go through some examples how the game could be played together:

1. Imagine that we start the game and it is your turn to play. You come to us and we tell you that you are playing with another pastoralist from this village. Remember, you will never know who exactly that person is. Now you can decide how much you want to send to that person. For example, you can decide to send all 500 shillings to the other player. We triple this amount, so that we will give 1500 shillings to the person with whom you play (3 times 500 shillings equals 1500 shillings). At this point, you have nothing and player 2 has 1500 shillings. Then we ask player 2 if he or she wishes to give anything back to you, and if so, how much. Suppose player 2 decides to return 200 shillings to you. At the end of the game you will go home with 200 shillings and the other player will go home with 1300 shillings.

2. Now let's try another example. Imagine that it is your turn and we tell you that you play with another pastoralist from a neighboring village. Again, you will never know who that person is.

Now for example you decide to send 200 shillings to that player 2, and keep the remaining 300 KES. We triple the amount so that we will give 600 shillings (3 times 200 shillings equals 600 shillings) to the player 2. At this point, you have 300 shillings and player 2 has 600 shillings. Then player 2 has to decide whether he wishes to give anything back you, and if so, how much. Suppose player 2 decides to return zero shillings to you. At the end of the game you will go home with 300 shillings and player 2 will go home with 600 shillings.

3. Now let's try another example. Imagine that you are paired with a city dweller from Lodwar and you decide to send 100 shillings to that player 2. Again, you will never know the exact identity of the player. We triple this amount, so player 2 gets 300 shillings (3 times 100 equals 300). At this point, you have 400 shillings and player 2 has 300 shillings. Then player 2 has to decide whether he wishes to give anything back to player 1, and if so, how much. Suppose player 2 decides to return 200 shillings to player 1. At the end of the game you will go home with 600 shillings and player 2 will go home with 100 shillings.

4. Now let's try another example. Imagine that you give 400 shillings to player 2. We triple this amount, so player 2 gets 1200 (3 times 400 equals 1200). At this point, you have 100 and player 2 has 1200. Then player 2 has to decide whether they wish to give anything back to you, and if so, how much. Suppose player 2 decides to return 500 shillings to you. At the end of the game you will go home with 600 and player 2 will go home with 700.

5. Now let's try another example. Imagine that you give nothing to player 2. There is nothing for us to triple. Player 2 has nothing to give back and the game ends here. You go home with 500 and player 2 goes home with 0.

Note that the larger the amount that you give to player 2, the greater the amount that can be taken away by you and player 2 together. However, it is entirely up to player 2 to decide what he should give back to you. You could end up with more than 500 shillings or less than 500 shillings as a result.

Remember that all of your actions are treated confidentially by us. Only we will know if and how much money you put back into the envelope and we promise to never tell anyone about this.

We will go through more examples with each of you individually when you come to play the game. In the meantime, do not talk to anyone about the game. Even if you are not sure that you understand the game, do not talk to anyone about it. This is very important. [Name of enumerator] will wait together with you and if we see that you are talking to anyone about the game while you are waiting to play, we must disqualify you from playing.

Appendix A1.2: Individual explanation

Hello, [name of the respondent] it is now your turn to play. Before you can make your decision, we want to ask you some questions, to make sure that you understood the game correctly. Let's suppose you decide to send 100 shillings to player 2. How much money will player 2 receive?

[Now we checked if the respondent gave the correct answer. If she/he did not, we again explained and physically showed the process of tripling the money.]

Let's assume that your player 2 decides to return 100 shillings. With how much money will he or she go home?

[We again checked if the respondent gave the correct answer. If she/he did not, we explained and physically showed what happens to the 300 KSh if 100 KSh are sent back.]

With how much money will you go home?

[We again checked if the respondent gave the correct answer. If she/he did not, we explained and physically showed what happens to the 400 KSh that were kept plus the 100 KSh that were returned by player 2.]

Now let's try another example. Let's suppose you decide to send 300 shillings to player 2. How much money will player 2 receive?

[Now we checked if the respondent gave the correct answer. If he did not, we again explained and physically showed the process of tripling the money.]

Let's assume that your player 2 decides to return 500 shillings. With how much money will he or she go home?

[We again checked if the respondent gave the correct answer. If he did not, we explained and physically showed what happens to the 900 KSh if 500 KSh are sent back.]

With how much money will you go home?

[We again checked if the respondent gave the correct answer. If he did not, we explained and physically showed what happens to the 200 KSh that were kept plus the 500 KSh that were returned by player 2.]

[We continued with these examples up to the point where respondents answered two scenarios in a row correctly. Most people needed three examples. Two respondents required five examples.]

[After the respondent answered two scenarios in a row correctly, we continued.]

Do you want us to give more examples about the game?

[If the respondent wanted more examples, we continued until the respondent did not ask for more examples.]

We have randomly chosen, that you will be playing with [a pastoralist from this village OR a pastoralist from a neighboring village OR a city dweller from Lodwar.] That means that you can now decide if and how much you want to send to [a pastoralist from this village OR a pastoralist from a neighboring village OR a city dweller from Lodwar]. On this table are your 500 shillings and the envelope. You can give that player 2 nothing, 100 shillings, 200 shillings, 300 shillings, 400 shillings, or 500 shillings. Simply put all the money that you want to send to him or her in the envelope.

We will triple this amount and send it to that player. Later today we will pass onto you whatever he or she decides to return.

Before you make your decision we want to ask you, if you have any questions about the game?

Do you want any assistance in handling the bank notes or the envelope? *[This question was only asked, when respondents showed signs of reduced dexterity.]*

Appendix A2: Assignment of strategies I

All 402 pastoralists in our sample also played the trust game in the role of player B, after they played the game as player A. 133 respondents played the game as player B and received money from a pastoralist from a neighboring village. We used the actual decisions of how much pastoralists sent to a pastoralist from a neighboring village revealed in previous sessions to determine how much that player B would have at her disposal. Her strategy could later be used in subsequent sessions to determine how much a player A that is paired with a pastoralist from a neighboring village would get back from a neighboring pastoralist. From having 133 strategies for returning money to a pastoralist from a neighboring village and 128 respondents that sent money to pastoralist from a neighboring village it follows that each inter-village return strategy was used a little less than once on average. However, returning strategies derived in early sessions are probabilistically used more than once in the course of the data collection, while strategies from later session are less likely to actually have been put into effect. The returning strategies for being paired with a pastoralist from a neighboring village derived in the last session were never employed, as there were no subsequent sessions. The returning strategies from the very first session were taken from the pretest.

From trust games played with city dwellers that are not further analyzed in this study, we derived 62 strategies of how much city dwellers would return to pastoralists living in Turkana villages. Out of the 402 pastoralists analyzed here, 132 were paired with a city dweller. Each of the 132 respondents paired with a city dweller was randomly matched with one of these 62 strategies. Each returning strategy from a city dweller was therefore used on average a little more than twice.

Table A1: Assignment of strategies II

Element	Days	Location	BDM players	Pairings with	SD used for ele- ments	RS taken from ele- ments	Part of analysis
A	1	pre-test in a village	pastoralists	SV	A	A	No
				NV	C, E, F	Simulated	No
				CD	B, D, G	Simulated	No
B	2-3	county capi- tal	city dwellers	CD	B	B	No
				NV	C, E, F	A	No
C	4-12	villages	pastoralists	SV	C	C	Yes
				NV	C*, E, F	A, C**	Yes
				CD	D, G	B	Yes
D	13-16	county capi- tal	city dwellers	CD	D	D	No
				NV	E, F	A, C	No
E	17-23	villages	pastoralists	SV	E	E	Yes
				NV	E*, F	A, C, E**	Yes
				CD	G	B, D	Yes
F	24	village	pastoralists	SV	F	F	Yes
				NV	n.a.	A, C, E	No
				CD	G	B, D	Yes
G	25	county capi- tal	city dwellers	CD	G	G	No
				NV	n.a.	A, C, E, F	No

Notes: BDM = Trust game by Berg et al. (1995). SV = pastoralists from the same village. NV = pastoralists from a neighboring village. CD = city dwellers from the county capital. SD = sending decision. RS = Returning strategy. * = sending decisions only used for subsequent sessions. ** = strategies only taken from preceding sessions.

Table A2: Sequence of data collection

Element	Element description	Conditional on element
1	Introduction and opening instructions for playing the game as player A	-
2	Random assignment of a player B	-
3	Individual instruction and decision as player A	2
4	Potential payout to player A of the money that was not sent to player B	2,3
5	Household survey including:	
5a	Expected return of player B	2,3
5b	Socio economic characteristics	-
5c	<i>Either</i> context independent trust question \rightarrow context dependent trust question <i>Or</i> context dependent trust question \rightarrow context independent trust question	-
6	Repeating general instructions for playing the game as player B	-
7	Random assignment of a player A	-
8	Individual instruction and decisions as player B	7
9	Potential payout to player B of the money not returned to player A	7,8
10	Non-incentivized Discrete Choice Experiment regarding livestock index insurance	-
11	Decision regarding incentivized Eckel Grossman Task	-
12	Payout of Eckel Grossman Task	-
13	Potential payout to player A of amount returned by player B	2,3

Table A3: Risk preference elicitation

Gamble	High	Low	Expected	Standard	Lower	Upper	Averages used
Choice	Payoff	Payoff	Payoff	Deviation	CRRA	CRRA	in analyses
				Payoff	range	range	
1	100	100	100	0	5	∞	5
2	120	90	105	15	1.64	5	3.32
3	140	80	110	30	1	1.64	1.32
4	160	70	115	45	0.72	1	0.86
5	180	60	120	60	0	0.72	0.36
6	200	40	120	80	$-\infty$	0	0

Notes: CRRA = Constant relative risk aversion.

Source: Method based on Eckel and Grossman (2002).

Appendix B: Additional statistical information

Table B1: Summary statistics by treatment

Variable	Pairing with someone from the same village (N=142)	Pairing with someone from a neighboring village (N=120)	Pairing with a city dweller from the county capital (N= 132)	Kruskal- Wallis test
Age [years]	40.345	40.512	38.076	
Constant relative risk aver- sion coefficient (CRRA)	2.221	2.092	2.214	
Distance to the next paved road [walking minutes]	27.732	25.75	28.644	
Education [years]	1.063	0.783	1.538	
Female	0.430	0.575	0.522	*
Herd size [TLU]	32.822	31.549	33.913	
Household size	8.401	8.333	7.583	
Income [1000 KSh]	5.575	5.608	5.475	
Mobile phone user	0.824	0.775	0.818	
Village size [100 people]	5.677	5.904	5.326	
Years lived in the village [percentage of lifetime]	0.765	0.759	0.771	

Notes: Mean values are shown. Variables are compared using the Kruskal-Wallis test. *** p<0.01, ** p<0.05, * p<0.1

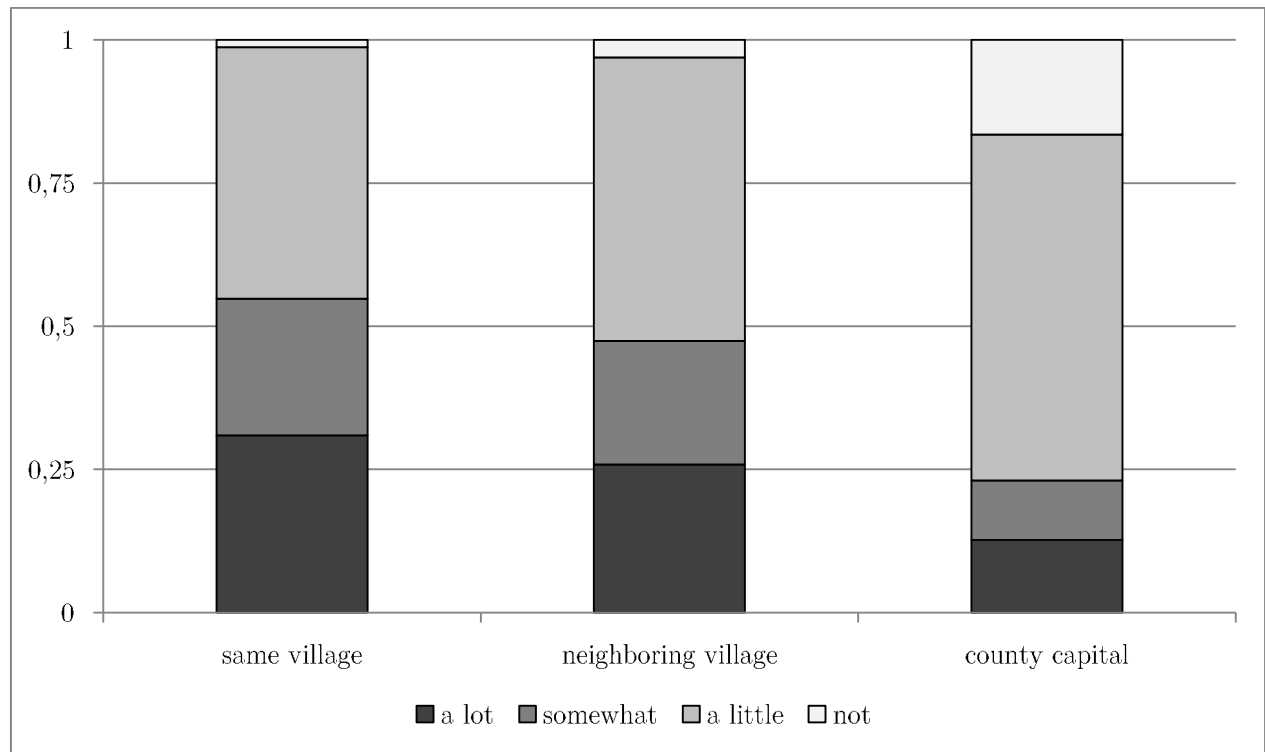
Table B2: Multivariate regression analyses on the radius of trust with explicit control variables

Dependent Variables	Share sent in the trust game	Share expected to be returned by player B	Context inde- pendent sur- vey question	Context de- pendent sur- vey question
Independent Variables	(1)	(2)	(3)	(4)
Trust towards people from a neighboring village	1.167 (1.886)	0.197 (2.778)	-0.305*** (0.099)	-0.186** (0.082)
Trust towards city dwellers from the county capital	-7.889*** (2.742)	-8.361** (3.661)	-1.626*** (0.142)	-1.023*** (0.104)
Age [years]	0.050 (0.077)	-0.143 (0.146)	0.022*** (0.006)	-0.017*** (0.006)
Constant relative risk aver- sion coefficient (CRRA)	-0.785 (0.541)	-0.010 (0.870)	-0.050 (0.035)	-0.046 (0.038)
Distance to the next paved road [log of walking minutes]	-0.743 (0.635)	0.290 (1.207)	-0.195*** (0.063)	-0.029 (0.068)
Education [years]	0.083 (0.387)	-1.081** (0.495)	-0.085*** (0.024)	-0.003 (0.029)
Female	0.455 (1.659)	-2.013 (3.892)	0.167 (0.158)	0.237 (0.157)
Herd size [TLU]	0.022 (0.050)	-0.024 (0.047)	0.008*** (0.002)	0.006*** (0.002)
Household size	0.299 (0.286)	0.267 (0.500)	-0.050*** (0.019)	0.037* (0.022)
Income [1000KSh]	-0.501** (0.214)	0.107 (0.335)	0.005 (0.005)	0.003 (0.009)
Mobile phone user	5.053* (2.646)	3.919 (3.853)	0.611*** (0.184)	-0.320 (0.195)
Village size	0.126 (0.342)	0.269 (0.515)	-0.041* (0.023)	-0.002 (0.020)
Years lived in the village [per- centage of lifetime]	-1.438 (2.923)	-5.026 (3.937)	0.762 (0.215)	0.322 (0.220)
Constant	31.089 (4.877)	56.776*** (8.571)		
Total Observations	394	394	1,182	1,182
Left-censored observations	34	46		

Uncensored observations	n.a.	320
Interval observations	359	n.a.
Right-censored observations	1	28

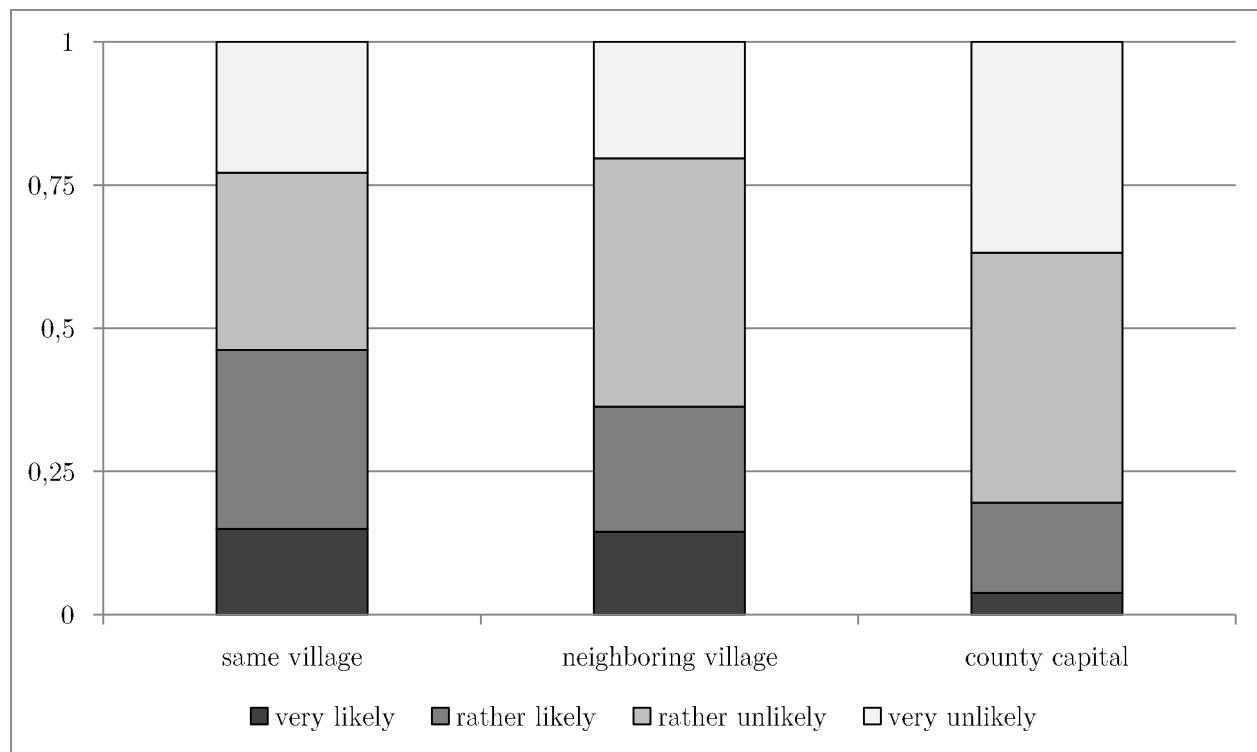
Notes: Estimates in column (1) are based on censored interval regression. Estimates in column (2) are based on a tobit model. Estimates in column (3) and (4) are ordered log-odds estimates from ordered logistic regression. Standard errors shown in parentheses are robust and clustered at the village level for columns (1) and (2), and clustered at the individual level for columns (3) and (4). *** p<0.01, ** p<0.05, * p<0.1.

Figure B1: Context independent survey trust (N = 394)



Source: Own elaboration.

Figure B2: Context dependent survey trust (N = 394)



Source: Own elaboration.