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Market Exposure, Civic Values, and Rules

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Abstract

Does markets exposure foster or erode civic values and rules necessary to constrain opportunistic behavior? Using a natural experiment on market location from Ethiopia, I compare individuals who are from the same clan and attend the same market but vary in their exposure to that market. I find a positive effect of market exposure on civic values and rule formation. This result arises because individuals trade primarily in livestock, which is prone to cooperation problem from asymmetric information and weak state capacity. I use vignette studies to show that societies develop different types of exchange structures to mitigate this problem, which then shapes civic values and rules. In societies far from markets, there is no need for civic values and rules, as individuals rarely attend markets and sell livestock eponymously within their social network. In societies near markets, ephemeral and impersonal nature of market exchange creates a demand for civic values and community sanctioning as lubricants to conclude exchange, otherwise individuals end up losing gains from trade. Exposure to markets without asymmetric information has no effect on civic values and rules, suggesting that prosperity and contact hypothesis are not the channels.

JEL: C93, D8, N97, Z13

Keywords: Markets, civic values, rules, cooperation, market failure, asymmetric information, Ethiopia

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I. Introduction

What happens to civic values and rules when societies transition from exchange based on community relations to that based on impersonal interactions in markets?[1] The link between market exposure and civic values is one of the most debated topics in economics (see Hirschman, 1982). While the *doux commerce* hypothesis argues market exposure fosters civility and cooperation (Montesquieu, 1748; Smith, 1763), the destructive hypothesis argues it gives rise to envy and exclusion (Marx, 1872; Veblen, 1899; Polanyi, 1944). In a seminal study, Henrich et al. (2010) shed light on this debate by documenting a positive link between calories purchased from markets and experimental measures of fairness. Subsequently, Enke (2022) found a positive link between market related concepts in a society's folklore and folklore-based measures of morality, and Agneman and Chevrot-Bianco (2022) between occupational choice and experimental measures of honesty.[2]

These impressive findings raise a number of important questions. We don't know what causes markets to have a positive rather than a negative association with civic values. Markets are not homogeneous but differ in their characteristics: some are more competitive, others are prone to asymmetric information. Do we expect these characteristics to have the same effect or are they expected to determine the direction of the market effect? For instance, Shleifer (2004) argues that high competition can undermine civic values, whereas Arrow (1970) and Bowles (1998) argue that market failure from asymmetric information can create a demand for civic values. Thus far, there is hardly any empirical evidence on mechanisms underlying the effect of market exposure. Crucially, transition to market exchange is not random but may itself depend on the so-called "feudal shackles" or pre-existing differences (Hirschman, 1982). Moreover, markets typically co-evolve with states and are usually located in urban areas, so their effect may be confounded with state capacity and urban amenities. There is rarely compelling evidence from exogenous variation in market exposure.

In this paper, I resolve these challenges using the context of rural markets from the homeland of the Arsi Oromo People in Ethiopia. These markets resemble a fair and attract a fluctuating population of thousands of buyers and sellers who gather weekly to trade in ephemeral and impersonal interactions. Trade occurs primarily in livestock, which is prone to asymmetric information. Weak state capacity means there are no impartial

[1] Civic value is the tendency to cooperate if others do the same even when defection would yield a higher payoff and there are no benefits to repeated interaction and reputation formation. Civic minded people derive utility from their own payoff and also the payoff of other people because of motives like inequality aversion, reciprocity, and efficiency (Fehr and Schmidt, 1999; Sobel, 2005; DellaVigna, 2009). Rules are formal written down constraints (regulations, laws) that limit opportunistic behavior (North, 1991; Alesina and Giuliano, 2015).

[2] A parallel literature studies how markets affect morals towards third parties, such as environment, child labor, and animals (see Sandel, 2013). These studies rely primarily on lab experiments. Falk and Szech (2013) find that markets erode moral values, whereas Bartling, Fehr and Özdemir (2020) find that this is not due to markets but repeated play.

third parties to remedy this situation. This context is not peculiar to Ethiopia. Livestock markets are widespread throughout Africa, providing livelihood to over 30 percent of its population and accounting for 35 percent of its agricultural GDP.[3] Moreover, asymmetric information and weak state capacity are pervasive in Africa, affecting many goods and services.(Fafchamps, 2003; Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2014). These features make the findings from this study of general interest.

The Arsi homeland offers plausibly exogenous variation in market emergence and location. Until 1890s, there were no markets or towns in the Arsi homeland, which is also confirmed by Italian colonial maps. Markets evolved inadvertently from military camps that emperor Menelik built in ca 1900 after defeating the Arsi. These camps brought hundreds of soldiers and their families, creating opportunities for market exchange. When Menelik died a decade later, the camps lost their military character and transformed into market places, without leaving a state behind. Accounts of British military generals affirm that Menelik chose camp locations on the basis of geographical suitability to defence, resulting in their placement in areas without link to trade, slavery, and coffee cultivation. The Arsi unlikely influenced these locations, as they surrendered unconditionally without waging a war after Menelik gained decisive victory in a battle fought 150 km away. Since it was not the intention of Menelik to build markets, their emergence was accidental and their locations were unrelated to potential for trade.

I capture market exposure as distance between markets and Arsi settlement units called 'groups'. Since there are no roads in the study area, I use average time (hours) individuals from a group take to access the nearest market on foot. I then compare individuals who belong to the same clan and attend the same market but vary in their distance to that market. This allows me to absorb time invariant unobserved clan and market specific differences. Within a clan, individuals are exposed to common patrilineal descent, as well as common religious and political leadership (Hassen, 1994; Gnamo, 2014). This mitigates the concern that pre-existing differences in culture and institutions drive changes in civic values and rules today. Market distance is unlikely affected by sorting. The Arsi settlements predate the markets and their locations were determined via a custom which forbade conquering of territories, thereby ensuring that settlement locations did not change after Menelik's invasion. Movement between the groups was also rare because of the belief that residing in the same settlement as one's ancestors offers protection from their spirit (Gnamo, 2014). To this end, I show there is no association of market distance with population, population density, migration, and geography.

Measuring civic values is difficult because of confounding motives like beliefs about others' behavior, reputation formation, repeated interaction, and social pressure (Bénabou and Tirole, 2006; Nunn, 2009; DellaVigna, List and Malmendier, 2012). I use a public goods game to measure civic values as conditional cooperation – propensity to cooperate

[3]FAO 2020. https://www.fao.org/faostat/en/#data/QV

if others do the same. The game is one-shot and anonymous, which rules out repeated interaction and reputation formation from playing a role. There are two players in the game and each is endowed with 6 Ethiopian Birr. The players take two decisions in which they can contribute in units of 1 Birr. In the first decision, players decided simultaneously on their contribution, so both civic values and beliefs play a role. In the second decision, I shut down beliefs by eliciting each players' contribution in response to each of the seven contribution decisions of the other player using the strategy method (Selten, 1967; Fischbacher, Gächter and Fehr, 2001). I use the Spearman rank correlation between self and other players' contribution in the second decision to measure conditional cooperation. Since there are very few altruists, higher Spearman implies stronger conditional cooperation, whereas zero implies full free riding. Laboratory and field evidence show conditional cooperation has implications for public goods provision (Fehr and Gächter, 2000; Rustagi, 2022*a*), commons management (Rustagi, Engel and Kosfeld, 2010; Kosfeld and Rustagi, 2015), and donations (Frey and Meier, 2004; Rustagi, 2022*a*).

I complement the experimental measure with two additional context specific measures. All groups are engaged in a program which allows them to manage their forest as a common property. The program encourages groups to engage in decentralized monitoring to prevent outsiders from depleting their forest. Since monitoring is individually costly, generates benefits for the entire group, and there is no monitoring of monitoring, it serves as a proxy for civic values. I use household surveys to collect data on time spent monitoring in a month. Next, I consider the presence of formal written down rules that restrict livestock grazing inside the group managed forest. These rules restrict browsing damage caused by livestock to young trees and are considered crucial for natural forest regeneration (Amente, 2006). I combine official records maintained by each group with household surveys to collect data on the presence and quality of formal grazing rules.

There has been no road development since Menelik's time to reduce the cost of market attendance. This means, market distance shapes civic values and rules directly through contemporaneous exposure and indirectly through the exposure of ancestors. I find that one standard deviation increase in market distance (1.1 hours) results in a decline in conditional cooperation by 0.17 points, time spent monitoring by 7 hours, and the likelihood of forming grazing rules by 21 percentage points. These effects are not only statistically significant at the 1 percent level but are also economically large (25-45 % of the mean). The results are robust to wide variety of controls including geography, forest condition, education, population density, and inequality. To mitigate the concern that market distance might be capturing access to urban amenities, I show that the main results hold when I control for distance to local administration, schools, and religious places. Data from the first decision of the public goods game yield similar results: both belief in other players' contribution and own contribution to the public good decline with market distance.

Why do these effects arise? I conduct vignette studies imitating local livestock ex-

change to shed light on plausible mechanisms. I find that personal relationships based on bilateral exchange is not the channel. Instead, the vignettes reveal that livestock exchange is prone to market failure from asymmetric information, which locks buyers and sellers in a cooperation problem. Groups near and far from markets differ in their opportunity cost of market attendance, so they develop different kinds of exchange structures to deal with this problem. Individuals acquire heuristics from exposure to these exchange structures and apply them across cooperation domains to avoid cognitive dissonance (Festinger, 1957). This explains differences in civic values and rules by market distance.

In groups far from markets, the opportunity cost of attending markets is high, so individuals rarely attend markets. Instead, they trade livestock locally with known individuals from their social network. While this exchange structure makes dishonest behavior costly, it teaches individuals to cooperate when own identity is eponymous but defect otherwise. Since the game is anonymous and there is no monitoring of monitoring, this explains weaker civic values among individuals far from markets. In contrast, in groups near markets, the lower opportunity cost of attending markets means individuals have the possibility to engage in ephemeral and impersonal livestock exchange. Achieving cooperation in this situation calls for new mechanisms. One possibility is that market failure creates a demand for civic values and rules to facilitate exchange, otherwise individuals lose gains from trade Arrow (1970); Bowles (1998). Consistent with these arguments, I find that one standard deviation increase in market distance leads to a decline with market attendance by 0.3 visits per week, trade with individuals outside the social network by 40 percentage points, and learning honesty by 37 percentage points.

Vignette studies further reveal the presence of a sanctioning mechanism that threatens defectors with exclusion from future exchange. Since markets exchange is impersonal, it is not possible to sanction individuals. Instead, communities use information on clan identity that the Oromo announce when they greet other Oromo people and ethnic markers in interactions with non-Oromo people to target the defector's community (see Greif, 2006; Deb, 2020). This exchange structure teaches individuals to cooperate even when own personal identity is anonymous, as in the game, resulting in stronger civic values.

The above results suggest (i) positive effect of market attendance on civic values and rules; this is exactly what I find. Instrumental variables regression that use market distance as an instrument for market attendance yield similar results; (ii) the effect of market distance should disappear if there is no asymmetric information, unless there is confounding with alternative channels like prosperity and contact hypothesis (see Allport, 1954). I construct a falsification test using distance from other periodical markets that trade primarily in products of verifiable quality (aluminium utensils, polyester textiles) and are as well attended as livestock markets. I find that prosperity declines with both distance from livestock and non-livestock markets. Yet, without or with controls, distance to non-livestock markets has no effect whatsoever on civic values and rules. These results

confirm the importance of asymmetric information in shaping civic values and rules.

Related Literature. This paper contributes to the literature on the determinants of civic values, culture, and pro-social behaviors. Previous studies focus on the role of slavery in eroding trust (Nunn and Wantchekon, 2011), trade in reducing religious riots (Jha, 2013), central institutions in crowding out honesty (Lowes et al., 2017), and self-governance in fostering pro-social behavior (Guiso, Sapienza and Zingales, 2016) and norms of cooperation (Rustagi, 2022*a*). This paper highlights the role of market exposure in shaping civic values and rules.

The paper distinguishes itself in several ways from previous studies (Henrich et al., 2010; Enke, 2022; Agneman and Chevrot-Bianco, 2022). First, it uses a natural experiment to study the effect of market exposure on civic values and rules. It then sheds light on the mechanism through which this effect unfolds. This step is crucial because without the mechanism, one cannot predict the direction of the market effect. The paper highlights the role of market failure from asymmetric information in understanding why the effect is positive and not negative. In doing so, the paper lends empirical support to the conjecture that market failure fosters civic values (Arrow, 1970; Bowles, 1998).

Second, the paper documents the emergence of different kinds of exchange structures and how these map on to civic values and rules. This offers empirical evidence in support of many influential studies suggesting the importance of economic organization for cultural change (Polanyi, 1944; Granovetter, 1985; North, 1991; Greif, 2006).[4] Since the exchange structures affect who cooperates with whom and under what conditions, the paper also contributes to the literature on the plausible origins of limited and generalized cooperation (Platteau, 2000; Tabellini, 2010; Enke, 2023).

Third, the paper provides evidence on self-governance in conclusion of market exchange when state capacity is weak (Ostrom, 1990; Fehr, Gächter and Kirchsteiger, 1997; Platteau, 2000; Greif, 2006; Dixit, 2009; Deb, 2020). Fourth, the paper highlights the importance of market exposure in shaping both cultural norms and formal written down rules. These findings contribute to the literature on the interplay between culture and institutions in sustaining collective action (Acemoglu and Robinson, 2019; Rustagi, 2022*b*; Bisin and Verdier, 2023). Finally, the paper builds on Rustagi, Engel and Kosfeld (2010) and (Kosfeld and Rustagi, 2015) who study the effect of civic values on successful commons management, but do not study the determinants of variation in civic values.

The paper is organized as follows. Section II discusses the natural experiment on market exposure, Section III the measures of civic values, rules, and market distance. Section IV presents the empirical strategy, Section V the main results, and Section VI the plausible channels. Section VII offers concluding remarks.

[4]The paper complement studies on social organization in Africa into kinship and age-sets (Moscona, Nunn and Robinson, 2017; Moscona and Seck, 2021). By leveraging variation within the Arsi clans, it highlight the importance of economic organization for cultural change holding social organization fixed.

II. Historical Background

The Arsi (Arusi) form the largest branch of the Oromo people. Their homeland is divided into two parts (*moieties*) separated by the river Wabe Shebele. Those who descended patrilineally from the ancestor named *Siko* live to the north of the river on a plateau, whereas those who descended patrilineally from the ancestor named *Mando* live to the south of the river in the Bale mountains. The study area lies entirely to the south of the river, so my focus is on the Mando Arsi.

Like many pastoral societies in sub-Saharan Africa, the Mando Arsi are divided into patrilineal kinship ties called *gosa* ($\sim$ clan) and live in clan-based settlements that have a well defined geographical territory (Gnamo, 2014). These settlement patterns have persisted, such that land ownership rights today can be traced to the ones in the past (Gnamo, 2014). In the mountains, land suitable for building homes is scarce, so the clan-based settlements were fragmented into hamlets that are far from each other. Since land suitable for homes is also where the clan boundaries often meet, hamlets from adjacent clans coalesced to form a shared settlement unit called "group" to reap benefits from economies of scale (see Figure A.2). It is this group which forms the unit of my analysis. Figure 1 shows the location of these groups as white polygons on a topographical map of the study area.

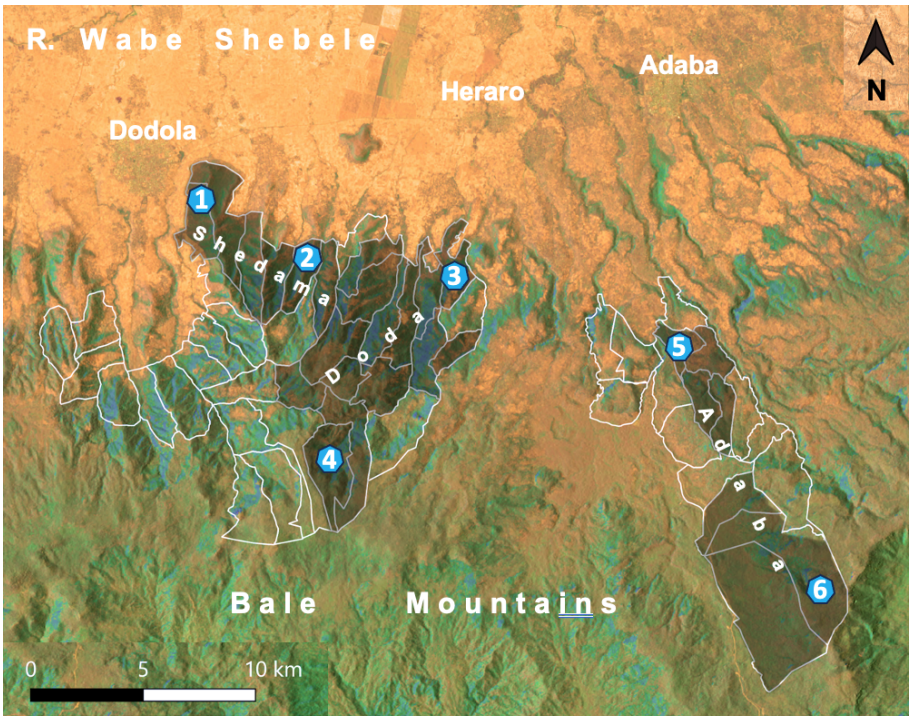

Figure 1: Location of Groups, Clans, and Markets

Notes: The figure shows the location of groups (white boundaries), market towns (Dodola, Heraro, Adaba), and major clans (Doda, Adaba, Shedama). For numbers on the map see text.

I capture market exposure as the average time individuals from a group take to access

the nearest market on foot. The further the groups are from markets the lower is their exposure because of higher opportunity cost of attending markets. I argue that this market distance is plausibly exogenous because of the natural experiment on the emergence and location of markets, use of clan and market fixed effects to absorb pre-existing differences, and no evidence on sorting by market distance. I discuss each in detail below.

II.A. Market Emergence and Location

Market Emergence.- Until the 19th century, the Arsi did not have a centralized state (Murdock, 1967; Gnamo, 2014) or trading centers or towns (Horvath, 1968). An Italian map from 1894 confirms this (see Figure 2). On the map, the study area "Monti degli Arussi" stands out as an empty space without any towns. It is unlikely this was due to an omission by Italian cartographers because they did include nearby towns like Arbegoma and Chevena, which are over 80 km away in the homeland of a different ethnic group – the *Sidamo* people. Balcha Safo, an Oromo military officer, also remarked that "the local people did not know how to build towns" (see Pankhurst, 1985). During this time, the Arsi traded livestock within their social network and exchange with outsiders was discouraged (Gnamo, 1982).

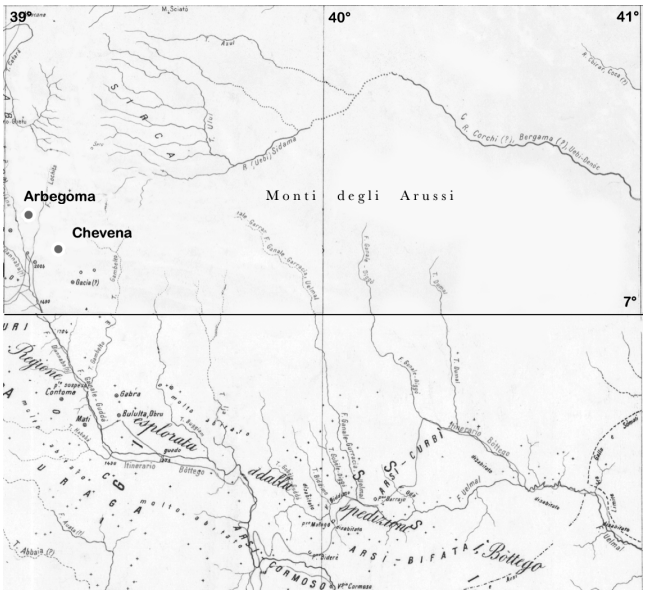

Figure 2: The Study Area Before the Conquest by Menelik

Notes: The map shows the study area "Monti degli Arussi" and its surroundings in 1894 before the invasion by Menelik. Source: Corpo di stato maggiore, Ministero della guerra, Italy, 1894.

In the late 1880s, Menelik, the king of Shoa (1865-1889) and the emperor of Ethiopia (1889-1913), began expanding the boundaries of his kingdom by waging wars against different ethnic groups including the Arsi Oromo. The spear wielding Arsi were no match for Menelik's army with superior European firearms and lost the war (Pankhurst, 1998;

Perham, 1948). To maintain his hold over subjugated lands, Menelik built military camps called *ketemas* which brought hundreds of soldiers and their families (see Fig A.1). This large influx of people created opportunities for exchange and gradually led to the emergence of market places. Since it was not the intention of Menelik to build centers of trade, the emergence of markets was unintended.

Menelik suffered from a stroke in 1909 and died in 1913, soon after the camps were built. After his death, the camps lost their military purpose and transformed fully into market places (Akalou, 1973). The Italian tourist guide to East Africa (1935-1941), written a few decades after the camps were built, described them as thriving market centers rather than as military establishments (Italiana et al., 1938, p464). It is likely that the camps disappeared over time because they were built of perishable materials, which the French explorer Jules Borelli described as "group of dwellings, usually surrounded by palisades" (p 230) and "walls made of bamboo" (p 286) (Borelli, 1890). The state Menelik erected was already weak and ruled subjugated areas indirectly through local clan leaders (see Gnamo, 2014, p162). Menelik's successor was considered incapable and was overthrown within four years. The subsequent rulers faced both internal challenges and from European powers. Together, these features imply that market emergence was not confounded with building of state capacity.

Market Location.- The camp locations were chosen strategically by Menelik, bearing in mind geographical suitability to defense (Wellby, 1901; Maud, 1904; Pankhurst, 1985). The British captain, M.S. Wellby noted that the camps were "cleverly placed...so that a force of several thousand rifles could be concentrated at any one point in a very short span of time." Figure 3 confirms this through a 3-D view of the wider geographical context in which the camps (black circles) are located.

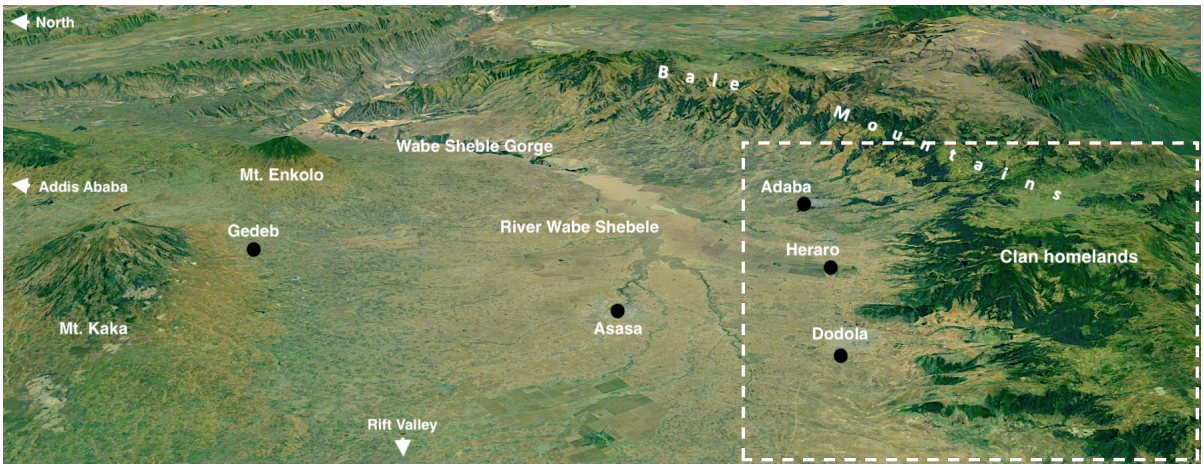

Figure 3: The Location of camps in the Study Area

Notes: The figure shows the geographical context in which the camps are located. The study area is enclosed by a dashed line and the camps are in black circles. Baseline map: Google Earth 2023.

There were five camps, of which three are in the study area (enclosed in a dashed line). All camps were built on a 2400m high plateau bounded to the west by the Rift Valley, to the east by the gorge of the river Wabe Shebele, to the south by the 4000m high Bale Mountains, and to the north by equally high Mt. Kaka and Mt. Enkolo. These geographical features offered the camps protection in the event of an attack, ensuring at the same time easy connection to Addis Ababa (Menelik's capital) through a well secured 3000m high saddle between Mt. Kaka and Mt. Enkolo (see Wellby's remark).

The camps were not placed in areas with potential for trade. Their locations did not intersect any trade routes (see Zewde, 2002, p23) or routes used for the Red Sea slave trade (see Shell, 2018, p66) or areas with coffee forest.[5]

The Mando Arsi are unlikely to have played a role in the location of camps. After Menelik gained decisive victory in a battle fought 150 km to the north of the study area in the Siko homeland (see Gnamo, 2014, p146), the remaining Arsi surrendered en masse without waging a war.

II.B. Clan and Market Fixed Effects

The setting allows me to mitigate the concern that pre-existing differences in culture and institutions drive changes in civic values and rules today. There is wide variation in distance to markets within clans. To illustrate this point, I superimpose the map in Figure 1 with areas in which three most prevalent clans are found. For example, in groups labeled 1 and 2, individuals from the *Shedama* clan attend the same market (Dodola) but are at a walking distance of 0.5 and 3 hours. Similarly, in groups labeled 3 and 4, individuals from the *Doda* clan attend the same market (Heraro) but are at a walking distance of 1.5 and 4 hours. Finally, in groups labeled 5 and 6, individuals from the *Adaba* clan attend the same market (Adaba) but are at a walking distance of 3 and 5 hours. This means: a) despite belonging to the same clan, some individuals are closer, but others are further away from markets, and b) individuals from different groups attend different markets.

I use clan and market fixed effects to absorb time invariant unobserved clan and market specific differences. Within a clan, individuals are culturally homogeneous from common patrilineal descent. Clan was and continues to be an important aspect of communal identity. This is also underscored by it's use in greeting. As Gnamo (2014, p63) writes "...when two individuals who do not know each other meet, the first question they ask each other is not about personal name, but about their membership in Gosa." Moreover, because religious institution (*Qallu*) and political leadership (*Gada*) among the Arsi operate at the clan level, individuals from the same clan are also homogeneous with respect

[5]The trade routes were far from the homeland of Mando Arsi. The altitude of the area is not suitable for coffee cultivation or for coffee forest. While coffee does grow naturally to the south of the Bale mountains in the Harenna forest, which is on a steep escarpment with a fall of over 1000 meters.

to exposure to common institutions (Hassen, 1994).

II.C. Sorting by Market Distance

In the post-market period, sorting may occur either through the shifting of clan boundaries or movement of people across groups within the clan homeland. I find no evidence in support of either of these two. The Mando Arsi formed settlements before the markets emerged on the basis of their clan. The locations of clan homelands were determined by a custom called *Baala Buusa* (leaf laying ceremony) through which clan leaders (*Abba baala*) declared communal property rights to land on a first-come first-served basis by placing a leaf. *Baala Buusa* forbade clans from conquering each others' territories and ensured that subsequent descendants inherited these rights. As Gnamo (2014, p33) writes "*Gosa* [clan], however strong and powerful it might have been, did not have the right to conquer and occupy the land belonging to the descendants of *Abba baala*." The Arsi clans also bonded over strong regional identity (*Arsooma*), friendship (*firooma*), marriage (*soddomaa*) and matrimonial alliances (*dhalooma*), which further prevented territorial take overs. Of special importance was the practice of clan exogamy, which requires the Arsi to marry only outside their clan, thereby cementing ties with other clans. As a result, clan homelands have remained stable over time and their locations were not disrupted when Menelik defeated the Arsi (Gnamo, 2014, p63).

Individuals rarely moved across groups within their clan homeland. As Gnamo (2014, P63) writes, "the residence rule is patrilocal among the Oromo, where men are expected to be born, grow up, live, bring their wives to their residence, and die." This preference to reside on ancestral land where one's forefathers once lived, died, and are buried is because of the belief that the spirit of their ancestors (*Ayyana Abba*) continues to live with them and could influence future outcomes. For this reason, the Arsi regularly visit the tombs of their ancestors and also carry out sacrifices there (Gnamo, 2014, p64). A household survey with the elderly from each group also shows that only 1.5 percent of the households have out-migrated in the past six decades.

If despite the narrative, sorting did happen then groups nears markets will be larger in size, have higher population density, and less out-migration. I test this in Table 1. There is no correlation of market distance with number of households, population, population density, and out-migration. The coefficients are very small in magnitude and are always statistically insignificant. Figure A.6 yields similar results on a wider set of covariates. These results alleviate concerns over sorting by market distance.

Table 1: Sorting and Migration by Market Distance

	Group size (1)	Population size (2)	Population density (3)	Migrant share (4)
Market distance	0.157	3.998	-1.468	-0.115
	(0.531)	(7.147)	(1.053)	(0.235)
Constant	25.860	169.795	18.174	1.802
	(1.648)	(18.375)	(2.938)	(0.790)
Obs.	52	52	52	52

Notes: OLS estimates with robust standard errors in parentheses. Group size is the number of households in a group. Population size is the number of individuals residing in a group. Population density is number of individuals per km^2. Migrant share is the percentage of households who have left the group in the past 55 years.

III. Data

Of the 56 groups in the study area, 52 are in the sample; three groups in which a pilot study was conducted were excluded and data were not available for the fourth group. I use community surveys to collect data on market distance and combine this with household level data on civic values and rules obtained from behavioral experiments and household surveys. I append this with data on group level characteristics obtained from the government office and data on market interactions obtained from vignettes studies imitating local livestock exchange. The experimental instructions and procedures are described in Appendix B. The vignette studies are discussed in Section V and Appendix C.

III.A. Groups

The groups are located on the north facing slope of the Bale Mountains, inside the Adaba-Dodola Forest Protection Area in the West Arsi administrative zone. They range in altitude from 2600m - 3500m, cover an average area of 5000 hectares, and have 16 to 30 households. The groups are heterogeneous in clan composition and the average Herfindahl index is 0.41 (s.d. 0.24), suggesting that the likelihood two randomly selected households will be from different clans is 40 percent. Overall, 80 percent of the households are from ten clans.[6] The main occupation is small-scale herding which involves both buying and selling of livestock. The households also practice forest gathering and subsistence agriculture, whereby the main crop is barley. All households follow Sunni Islam mixed with Oromo beliefs. The level of socio-economic development is poor: except for primary schools and mosques, roads, electricity, mobile towers, tap water, dispensary, irrigation, and veterinary care are completely lacking.

[6]These are Doda, Adaba, Shedama, Holbatana, Abena, Bidika, Angiso, Magda, Doyo, and Weqe.

The groups are engaged in a forest commons management program which was rolled out from 2000-2005 by the Oromia government with support from the German Development Cooperation. Under the program, each group was given property rights to manage the forest in its boundary as a common property. The program allows households to use their group managed forest to graze livestock, harvest timber and non-timber forest products, and retain farms and homesteads. In return for these benefits, the groups have to maintain the forest cover and prevent outsiders from accessing their forest, such that poor forest outcome entails a group level fine. The groups have the same organizational structure: an elected five-member executive committee headed by a leader.

In summary, groups are homogeneous with respect to being on the same side of the mountain, religion, occupation, organizational structure, and administration.

III.B. Market Distance and Interaction

There are three livestock markets in the study area that are on average 12 km apart from each other. Since there are no roads and transport connecting groups to markets, households walk to the market that is closest to their group. I measure market distance as the number of hours households take on average to cover a $one-way$ market trip on foot. Note that none of the groups are located adjacent to any market. Market distance typically ranges from 1 – 5 hours, the average being 2.85 hours (s.d. 1.11).

The markets are held on two days in a week called 'market days', which brings together thousands of buyers and sellers who are not known to each other. The markets are held on a large ground(s), which can be over 12 times the size of a standard football field. Sellers do not have a fixed location from which they operate; rather locations are taken on the basis of arrival. The first day of the market is reserved for livestock trade, which includes cattle, horse, donkey, pony, goat, and sheep. The number of individuals attending the market on this day is also larger than on the second day. For instance, the market in Dodola attracts as many as 5,000 people on the first day. Other prominent items sold in markets are butter, honey, fuel wood, charcoal, bamboo, rubber boots, synthetic textiles, candles, and utensils. Individuals from the study area attend markets to both buy and sell livestock. Exchange typically involves 1-2 animals.

Since livestock exchange forms the cornerstone of markets, several features are worth noting. First, market exchange occurs among strangers in the form of impersonal interactions that cease to exist once the exchange is over. Livestock is inspected on the spot and exchange takes place against instant payment in cash. There are no orders in advance and sellers do not offer credit. Second, livestock exchange is prone to asymmetric information, as the seller has more information about livestock quality than the buyer. This problem is further aggravated by the absence of quality guidelines, lack of veterinarian checks before purchase, and primitive patterns of production that yield highly uneven quality of

livestock. While there are some cues that the buyers may use to adjudge livestock quality, but the vignette studies show that these are far from perfect and the problem remains. Third, livestock exchange is riddled with informal agreements that cannot be verified and enforced by impartial third parties. As such, there are no impartial third parties in the form of courts or police. Even if such third parties did exist, the small size of transactions does not merit formal action. Informal enforcement agencies like mafias or private armies are absent. Fourth, there are no middlemen in the markets and trade happens directly between buyers and sellers. Fifth, markets attract a large fluctuating population of buyers and sellers who could be Oromo people from different clans, as well as non-Oromo people from different ethnicities (Amhara, Gurage, Sidamo, Somali).

These features imply that buyers and sellers are locked in a cooperation problem. If buyers do not trust sellers, they will not be willing to pay a high price. Knowing this, sellers have no incentive to produce high quality livestock. In these circumstances, markets may end up trading in poor quality livestock or may even fail to exist (Akerlof, 1970). However, individuals have clearly managed to avoid this fate as markets not only exist but are also thriving. This is not because of personalized relationships based on bilateral exchange, which the vignettes reveal to be rare (c.f. Vignette B10).

III.C. Civic Values and Rules

One way to measure civic values is to conduct a field experiment in which individuals are offered a product of unverifiable quality in a one-shot market exchange without third party enforcement. The number of recorded sales in a group can then be interpreted as reflecting trust. This approach, while appealing, has two drawbacks. First, it is difficult to find a product that everyone desires and is able to afford. This is important to separate rejection due to lack of trust from rejection because of lack of interest in the product or affordability. Second, it measures civic values in the same domain that engendered these values (market exchange). To the extent civic values acquired via market exchange become a generalized reason for behavior through a process called dissonance reduction, a stronger test lies in using a different domain than markets.

Following Camerer and Fehr (2004), I argue that behavioral experiments offer such a domain. As explained below, the game I use mimics livestock exchange in markets in being one-shot (ephemeral), anonymous (identity of the other player is not known), and prone to asymmetric information (player type is not known).[7] Moreover, the games are conducted with money, which is the medium of exchange everywhere. It is fair to assume that all individuals prefer money. My primary measure of civic value in the experiment

[7]The concern that behavioral experiments induce demand effect arises when experiments are used to assign treatments and participants are aware of this. There is no treatment manipulation in the experiments I used in this paper. All participants confronted the same decision situation regardless of their distance from market.

is propensity for conditional cooperation. I complement this measure with two additional measures in the context of forest commons management: time spent monitoring the forest and formation of rules regulating grazing inside the forest.

Propensity for Conditional Cooperation

Conditional cooperation is defined as the individual willingness to cooperate provided others do the same, even when the payoff maximizing strategy is to defect on others' cooperation (Elster, 1989; Bicchieri, 1990; Fehr and Schmidt, 1999). Measuring conditional cooperation is challenging due to confounding motives operating at the same time. Individuals might appear cooperative not because they have civic values but because of benefits from repeated interaction and reputation formation. Another concern is confounding of conditional cooperation with beliefs. Individuals with similar levels of conditional cooperation might behave differently because of differences in beliefs about others' cooperation. For instance, conditional cooperators with optimistic beliefs will cooperate, but those with pessimistic beliefs will defect. These concerns imply that observed cooperation behavior is a poor proxy for conditional cooperation. I overcome these concerns using a behavioral experiment.

The Experiment.– I use a one-shot anonymous public goods game in the strategy method (Selten, 1967; Fischbacher, Gächter and Fehr, 2001; Fischbacher and Gächter, 2010). This allows me to rule out repeated interaction, reputation formation, and beliefs from playing a role. During the game, two players were randomly paired. Each player received an endowment of six bills of one Ethiopian Birr and had to decide on his contribution to the public good in the units of 1 Birr. Any amount in the public good was multiplied by 1.5 and then distributed equally between the two players, regardless of their contribution. The payoff function of player i, where $i = (1, 2)$ is given by:

$$\pi_i = 6 - C_i + 0.75(C_i + C_i) \tag{1}$$

where C_i denotes the contribution of player i to the public good and $(C_1 + C_2)$ is the total value of the public good. Because the marginal per capita return from contributing one Birr to the public good was 1.5/2 or 0.75, it was in the self-interest of players to contribute nothing. Yet, if both players contributed their entire endowment, each player's earnings increased from 6 to 9 Birr; this created a cooperation dilemma.

Each player took two decisions: unconditional and conditional. In the unconditional decision, players contributed simultaneously and stated their beliefs about the other players' contribution. In the conditional decision, I used the strategy method to shut down beliefs: players reported their contribution in response to seven possible contributions by the other player. To ensure incentive compatibility, both decisions were made payoff relevant. A die was rolled to determine for which player the unconditional decision is

taken; this was matched with the other players' conditional decision to calculate payoffs.

This was the first time ever that individuals took part in an experiment, so I ensured they understood the game (see Appendix B for instructions and procedures). On average, each player earned 7.5 Birr, which was slightly over one day's wage in Dodola, the largest town. 720 individuals took part in the experiment, implying a response rate of 53 percent of the households. Given the importance of clan in the organization of the Arsi society, I test for sample representation using clan frequencies. Figure A.3 in Appendix A shows the proportion of ten major clans in the sample is the same as in the population (p-value = 0.45).

Measuring conditional cooperation.– The conditional decision allows me to obtain a revealed measure of civic values: (i) *free riders* are expected to always contribute zero regardless of the other players' contribution; (ii) *altruists* are expected to always contribute their full endowment regardless of what the other player does; and (iii) *conditional cooperators* are expected to increase their contribution in the increasing contribution of the other player. I find that a large fraction of individuals behave either as free riders or as conditional cooperators, but only a handful behave as altruists. Accordingly, I use the Spearman rank correlation between self and other players' contribution in the conditional decision as a measure of an individuals' propensity for conditional cooperation. The higher the Spearman ρ the higher is the propensity to cooperate conditionally, whereas zero correlation implies a tendency for free riding.[8] The average propensity to cooperate conditionally turns out to be 0.499 (s.d. 0.518). Note that since the game lacks explicit punishment, these measures suggest that individuals have internalized these civic values.

While conducting robustness checks, I use an indicator for conditional cooperator – individuals for whom the Spearman ρ is positive and statistically significant at p-value <0.05. I find that 47 percent of the individuals behave as conditional cooperators and 11 percent as free riders. Subsequently, I also use data on actual contribution to the public good (mean 2.05, s.d. 1.51) and beliefs about other players contribution (mean 2.67, s.d. 1.71) from the unconditional decision of the game.

Time Spent Monitoring

Maintaining forest commons requires groups to prevent outsiders from accessing their forest. This calls for monitoring the use of group managed forest. There is no tax funded third party like the police to conduct this monitoring, so individuals have to take turns to patrol the forest. Since there is no monitoring of monitors and so on so forth, this kind of decentralized monitoring is prone to a second order free rider problem: it is individually costly but generates group level benefits. Individuals are better off if others

[8]The Spearman ρ is also zero for altruists and flat contributors (individuals who contribute the same amount that is different from zero and full endowment). Only 1.8 percent of the individuals behave as altruists and 2.3 percent as flat contributors.

engage in monitoring while they can do something else in the meantime. This means time spent monitoring the group managed forest is a proxy for civic values. I use data from a household survey conducted in private to elicit time spent monitoring in a month. 508 individuals participated in the survey and the average turns out to be 27.80 hours (s.d. 15.48) per month. Figure A.4 shows the frequency of different clans in this sample is similar to that in the population, underscoring the representativeness of the sample (p-value = 0.38). Note that because there is no monitoring of monitoring, time spent monitoring is akin to cooperating in an anonymous interaction.

Rules on Resource Use

Rules regulating resource use are considered critical for the management of forest commons. In the study area, the single biggest cause of deforestation is the disappearance of young trees from the forest due to livestock browsing. To mitigate this deforestation, ecologists recommend rules regulating livestock grazing inside the forest (Amente, 2006). These rules when enforced are expected to put constraints on opportunistic behavior. I use household surveys conducted privately to assess whether a group has rules restricting grazing inside the forest and on the same spot, as well as the duration of these restrictions. 511 households took part in this survey, of which 44.6 percent (s.d. 49.8) reported having grazing rules in their group. Grazing was restricted, on average, for 1.76 months in a year (s.d. 1.698). Data from books maintained by groups also confirm these findings. Figure A.5 shows the proportion of different clans in this sample is the same as in the population (p-value = 0.44).

III.D. Descriptive Results

Figure 4 uses bin-scatter plots to show that both civic values and rules decline steeply with market distance (p-value < 0.001).

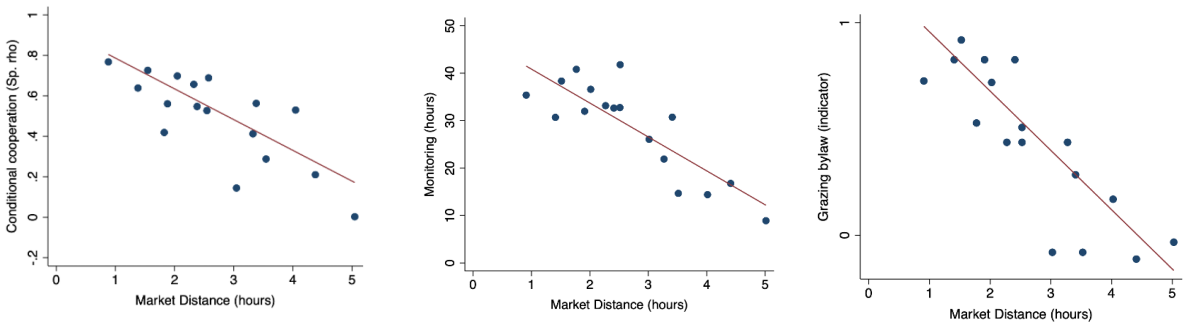

Figure 4: Civic Values and Rules by Market Distance

Notes: The bin-scatter plots show the association of civic values and rules with market distance after accounting for market fixed effects.

IV. Empirical Specification and Strategy

I study the effect of market exposure on civic values and rules using the following OLS specification:

$$y_{igcm} = \alpha_0 + \beta MD_g + \mathbf{X}_{gcm}\gamma + \mathbf{H}_{igcm}\delta + \theta_c + \eta_m + \epsilon_{igcm} \quad (2)$$

where y_{igcm} is the civic value or rule (conditional cooperation, monitoring, grazing bylaw) reported by individual i from group g, clan c, and attending market m. MD is one-way market distance measured in hours. $\mathbf{X}$ and $\mathbf{H}$ are vector of household and group specific characteristics that are expected to affect civic values and rules. These include altitude, group size, group fragmentation into hamlets, share of females in a group, Gini of cattle ownership, Gini of land ownership, years of education, and duration for which a group has been under the commons management program. Table A.1 reports the definition and summary statistics on these variables. θ_c is a fixed effect for the clan. I consider indicators for ten clans that account for over 80 percent of the households in the study area. η_m is a fixed effect for the market that the individual attends. I cluster standard errors at the group level. The results hold when I cluster standard errors on the group and the market, the group and the clan, or when I use spatial cutoffs at 2-10 km distance.[9] The coefficient of interest is β, which captures the effect of market distance on civic values and rules.

As outlined in section II, market distance is plausibly exogenous because: a) markets emerged accidentally from camps established by Menelik; b) Market locations had nothing to do with potential for trade and the Arsi played no role in their location; c) market emergence and locations were unlikely to be confounded with state capacity as Menelik died soon after the construction of camps, which themselves were made of perishable materials and disappeared over time. Moreover, Menelik's rule was indirect and relied on local leadership rather than introducing new forms of bureaucracy; and d) markets did not induce sorting either through shifting of clan boundaries or through movement across groups. A balance check in Figure A.6 shows that market distance is uncorrelated with a variety of geographical, forest, social, and economic variables. The coefficients are small in magnitude and are statistically insignificant. The only exception is latitude, where the difference through statistically significant at the 5-percent level is very small in magnitude (0.008 degrees). Such a small change is unlikely to induce major changes in climate.

One concern could be that market distance is picking the effect of urbanization like access to administration, schooling, and religious places. The field setting allows me to separate these effects. The administrative offices relevant to the households are not located in market towns. Also, households rarely have more than 3 years of education, so access to primary school is of utmost importance. Individuals attend primary schools in their vicinity and not the ones in market towns. Individuals do not rely on market

[9] Moran's I turns out to be statistically insignificant for these cutoffs.

places for mosque attendance. Instead, they attend local mosques dispersed throughout the study area. Also, Friday is the preferred day for mosques attendance, which does not coincide with market day(s). While reporting robustness checks, I control for distance to local administration, local primary school, and local mosque. Subsequently, I also control for population density per km^2 and trust in government. Table A.2 reports the definition and summary statistics on these variables.

While conducting robustness checks, I consider two additional sets of variables: a) *Forest type and condition*: the demand for civic values and rules may vary depending on forest type and initial forest condition. Accordingly, I control for share of plantation forest using data from the program office and forest condition at the start of the program using data on median tree cover in 2000 from Hansen et al. (2013); b) *Geography*: in addition to altitude, I include latitude and longitude which are expected to capture a host of geographical variables; c) *Border*: monitoring and rule formation could be higher in places where groups border many other groups, so I control for number of neighboring groups. Table A.2 reports the definition and summary statistics on these variables.

V. Results

V.A. Main Results

Table 2 presents results on the effect of market distance on conditional cooperation in Panel A, time spent monitoring in panel B, and formation of grazing rules in Panel C. Column 1 is without any controls and shows that market distance has a strong negative coefficient, which is statistically significant at the 1 percent level in all the three panels. It explains large variation in civic values and rules, which ranges from 12-51 percent. When I introduce clan and market fixed effects in column 2, the coefficient on market distance mostly retains its magnitude and remains statistically significant throughout, suggesting that market distance is not capturing unobserved clan and market specific differences. In column 3, I introduce the remaining control variables. The coefficient on market distance remains stable in magnitude in panel A, but declines in magnitude in panels B-C. Nonetheless, it remains statistically significant throughout at the 1 percent level. The controls variables and fixed effects are jointly statistically significant in all the panels and their inclusion leads to a jump in the R-squared by 6-18 percentage points.[10] According to the estimates in column 3, a one standard deviation increase in market distance (1.11 hours) leads to a decline in conditional cooperation by 0.17 points, time spent monitoring by 7 hours, and the likelihood of forming grazing rules by 21 percentage points. These are large effects in relation to the mean of the dependent variables. Results from a randomization inference test with 5000 repetitions show that these results remain

[10]Table A.3 reports the coefficients on covariates.

statistically significant (p-value < 0.001).

Table 2: Market Distance, Civic Values, and Rules

	No controls (1)	Fixed effects (2)	Control variables (3)	Median split (4)	Tercile split (5)
	Panel A: Conditional Cooperation (Spearman *rho*)				
Market distance	-0.158	-0.168	-0.156		
	(0.022)	(0.027)	(0.023)		
Above median market distance				-0.302	
				(0.085)	
Second tercile of market distance					-0.249
					(0.095)
Third tercile of market distance					-0.353
					(0.088)
R^2	0.12	0.14	0.18	0.16	0.16
	Panel B: Time Spent Monitoring (hours)				
Market distance	-8.527	-7.647	-6.106		
	(1.091)	(1.009)	(0.902)		
Above median market distance				-12.228	
				(3.342)	
Second tercile of market distance					-8.248
					(3.204)
Third tercile of market distance					-15.888
					(3.029)
R^2	0.39	0.49	0.57	0.54	0.56
	Panel C: Grazing Rules (indicator)				
Market distance	-0.317	-0.266	-0.195		
	(0.029)	(0.033)	(0.054)		
Above median market distance				-0.412	
				(0.152)	
Second tercile of market distance					-0.333
					(0.141)
Third tercile of market distance					-0.620
					(0.226)
R^2	0.51	0.60	0.68	0.66	0.67
Controls	No	No	Yes	Yes	Yes
Fixed Effects	No	Yes	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market. The number of observations is 720 in column 1, 508 in column 2, and 511 in column 3.

V.B. Robustness Checks

Alternative forms of clustering.– I show in Table A.4 that the main results are robust to clustering at different levels including market, clan, and spatial clustering of standard errors.

Alternative measures of market distance.– The effect of market distance may not be linear. To alleviate this concern, I consider an indicator variable for above median market distance in column 4 of Table 2. It enters with a large negative and statistically significant coefficient in all the panels. Similar results are obtained when I consider terciles of market distance in column 5. With respect to the first tercile as the benchmark category, civic values and rules decline in the second tercile and more steeply in the third tercile.

Alternative measures of civic values and rules.– Table A.5 shows that the results hold when I consider an indicator for conditional cooperator (column 1-2) and number of months grazing is forbidden in the group managed forest (column 3-4). One standard deviation increase in market distance leads to a decline in the share of conditional cooperators by 10 percentage points and grazing ban by 0.6 months.

Table A.6 reports results using data on beliefs about other players' contribution and actual contribution in the unconditional decision of the game. One standard deviation increase in market distance leads to a drop in beliefs by 0.42 Birr (columns 1-2) and a decline in contribution by 0.44 Birr (columns 3-4).

Distance from administration, school, and mosque.– In Table A.7, I control for distance to local administration in column 1, local primary school in column 2, and local mosque in column 3. These variables have a jointly statistically significant effect in all the panels (p-value < 0.01). Despite this, the coefficient on market distance retains its magnitude and significance. Market distance could be confounded with population density and trust in government, which may have been shaped by proximity to urbanization. Accordingly, I control for these variables in columns 4-5 of Table A.7. This does not lead to any major changes in the coefficient on market distance in panels A and B, but it declines in magnitude in panel C. Nonetheless, it always remains statistically significant at the 1-percent level.

Additional control variables.– In Table A.8, I start by introducing plantation forest share and median tree cover before the start of the forest management program in column 1, followed by latitude and longitude in column 2, and no of surrounding groups in column 3. This does not lead to any major changes in the magnitude of the coefficient on market distance, which remains statistically significant at the 1 percent level in all the panels. The additional controls are individually and jointly statistically insignificant, except for plantation forest in panel C, which has a negative and significant effect on rule formation.

All controls together.– There could an issue of over controlling when introducing simultaneously all additional and distance related controls. Nonetheless, the results hold when I carry out such an exercise in Table A.9.

Effects by age.– Since Menelik's time, there has been no infrastructural development affecting the opportunity cost of market attendance for individuals in groups located

further away from markets. This means, the results reflect the cumulative effect of markets operating directly through exposure today and indirectly through exposure of ancestors in the past, for instance, through cultural transmission (Bisin and Verdier, 2001). For this reason, I expect the effect of market distance to be similar across individuals from different age groups who differ in their length of direct exposure to markets. Figure A.7 and Table A.10 indeed show a negative and significant effect of market distance on civic values and rules across individuals from different age groups. The coefficient on market distance is statistically indistinguishable across the different age groups. The results hold even when I introduce control variables or restrict the sample to individuals 60 years old and above (see column 5).

VI. Plausible Channels

Why do civic values and rules decline with market distance? As discussed in section III.B, personalized relationships based on bilateral exchange is not the reason. Instead, I argue and provide evidence in support of the channel displayed in Figure 5:

Figure 5: Plausible Channel

- Livestock exchange is prone to market failure from asymmetric information, which locks buyers and sellers in a cooperation problem.
- Groups near and far from markets differ in their opportunity cost of market attendance, so they evolve very different kinds of exchange structures to deal with this problem.
- Individuals acquire heuristics from their exposure to these exchange structures and apply them across cooperation domains to avoid cognitive dissonance. This explains differences in civic values and rules by market distance.

I use data from vignette studies to shed light on this pathway. In the vignettes, I presented two scenarios imitating local livestock exchange, followed by a series of questions covering topics on the scope of asymmetric information in livestock exchange, third party enforcement, frequency of market attendance, exchange with outsiders, repeated interaction, and honesty. The scenarios are listed below and the exact questions are listed in Appendix C. The vignettes were very popular and lasted up to 2 hours.

Vignette 1: *One day an Oromo man called Ibsaa decided to sell his cows to earn money. He went to a market where he met Barentu, another Oromo man. Barentu wanted to buy Ibsaa's cows. The cows looked healthy from outside, but they were actually sick. No one except for Ibsaa knew that. Ibsaa decided not to tell this to anyone. Barentu bought the cows. Some months later, the cows died. This was a big loss to Barentu.*

Vignette 2: *One day an Oromo man called Ibsaa decided to sell his cows to earn money. He went to a market where he met Barentu, another Oromo man. Barentu wanted to buy Ibsaa's cows. Barentu bought the cows. Some months later, the cows died of sickness. This was a big loss to Barentu. Barentu thinks that Ibsaa cheated him by selling him sick cows.*

VI.A. Market distance, Attendance, and Exchange Structures

I start by documenting differences in market attendance and exchange structures by market distance.

Market distance and market attendance.– Column 1 of Table 3 shows that the coefficient on market distance has a negative sign and is statistically significantly at the 1-percent level. One hour increase in market distance leads to a drop in market attendance by 0.31 visits per week, which is economically large given the mean of attendance is 0.95 visits. This means once the one-way market distance crosses over 3 hours, market attendance declines to almost zero.

Exchange structure in groups far from markets.– The opportunity cost of attending markets is higher for individuals who live further away from markets, so they enter into livestock exchange eponymously with known individuals from their local social network. Indeed, column 2 shows that one hour increase in market distance leads to a statistically significant drop in trade with outsiders by 37 percentage points. Such an exchange structure has obvious advantages: it helps in mitigating asymmetric information associated with livestock exchange by making it easier for individuals to acquire information on traders' identity and past transaction history. In these situations, individuals don't have to "rely either on generalized morality or institutional arrangements to guard against trouble" (Granovetter, 1985). Thus, while exchange within the social network makes dishonest behavior costly for traders, it teaches individuals to cooperate parochially when own identity is known but defect otherwise. Since the game is anonymous and there is no monitoring of monitoring, this explains why individuals from groups further away from markets display weaker civic values. These results resonate with the findings of Banfield (1967), who documented similar patterns in southern Italy and called this behavior "amoral familism".

Table 3: Market Distance, Market Attendance, and Exchange Structures

	Market attendance	Trade with outsiders	Markets foster civic values
	(1)	(2)	(3)
Market distance	-0.306	-0.370	-0.343
	(0.061)	(0.050)	(0.048)
R^2	0.81	0.82	0.82
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Observations	52	52	52

Notes: OLS estimates with robust standard errors clustered on the group in column 1. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market.

Exchange structure in groups near markets.– In groups near markets, the lower opportunity cost of attending markets makes it possible to trade impersonally with individuals who are not part of the social network (see column 2 of Table 3). Since markets involve a fluctuating population of individuals who trade impersonally in ephemeral interactions, enforcing cooperation calls for a new set of solutions that go beyond trading with individuals from one's own social network. One possibility is that market failure creates a demand for civic values and rules to come in as lubricants and facilitate exchange, for in the absence of these traits trading parties would have to forego gains from exchange. Arrow (1970) writes, "Norms of social behavior, including ethical and moral codes [may be].....reactions of society to compensate for market failure." Similarly, Bowles (1998) notes that "when contracts are incompletely specified and costly to enforce, the ex-post terms of exchange may depend on normative commitments of the parties to exchange." Consistent with this argument, results in column 3 of Table 3 show that market distance is indeed negatively associated with decline in fostering honesty by 34 percentage points. These results are in line with evidence based on laboratory experiments. In a seminal study, Kollock (1994) randomly assigned individuals to trade in products of certain or uncertain quality. He found higher trust in strangers emerges among individuals exposed to uncertain product quality. Similarly, Fehr, Gächter and Kirchsteiger (1997) used a gift exchange game to show that labor markets with asymmetric information and absence of third party enforcement require civic values for contract enforcement.

Civic values are bolstered by a sanctioning mechanism which threatens dishonest traders with exclusion from future exchange. Targeting punishment requires information on identity and past transactions of individuals, but the impersonal and ephemeral nature of market exchange makes this difficult. So instead of targeting individuals, punishment is targeted at the group to which the individuals belong. The Oromo custom of announcing one's clan while meeting other Oromo people and use of ethnic markers (lan-

guages, accents) while meeting non-Oromo people generates information on trader's clan and ethnic group (Gnamo, 2014).[11] This information can play an important role in fostering cooperation through a credible threat in which the entire community of the defector is held responsible and faces punishment by the victim and his community (see Greif, 2006; Deb, 2020). The punishments take the form of graduated sanctioning, starting with a warning, followed by fines, and then expulsion from the community.[12] Importantly, to rule out moral hazard, the communities intervene only when the cow becomes sick or dies shortly after purchase but not otherwise. To quote Greif (2006), such "a credible threat of collective, multi-lateral punishment [could have] supported the beliefs that the short-run gain from cheating today was less than the long-run benefit of being honest." Thus the exchange structure in groups near markets teaches individuals to cooperate even in impersonal interactions even when own personal identity is not verifiable, as in the game and while monitoring.

Market Attendance, Civic Values, and Rules.– The above findings suggest that market distance operates via market attendance, that is, higher attendance is expected to be associated with higher civic values. Table 4 shows that this in indeed the case. The coefficient on market attendance turns out to be positive and is also statistically significant at the 1 percent level. One standard deviation increase in market attendance (0.57) is associated with an increase in conditional cooperation by 0.16 points, time spent monitoring by 5.73 hours, and formation of grazing rules by 30 percentage points. These effects are economically large.

Table 4: Market Attendance, Civic Values, and Rules

	Conditional cooperation	Time spent monitoring	Grazing rules
	(1)	(2)	(3)
Market attendance	0.274	9.795	0.510
	(0.073)	(2.984)	(0.082)
Control variables	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	720	508	511

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and markets.

[11] If individuals are from the same clan then they kiss on the cheek or hand, otherwise not. Individuals may have an incentive to lie about their clan, but this is clearly not happening. If lying about one's own clan were to be rampant than this custom would cease to exist.

[12] Ostrom (1990) notes that this kind of graduating sanctioning in prevalent among the users of commons. The Mando Arsi also use this kind of sanctioning mechanism while managing their forest as a common property resource. For example, violations of grazing rules start with a warning, followed by a small fine and then a higher fine, and then expulsion from the program.

Table A.11 show that the results also hold when I consider an instrumental variables regression in which I instrument market attendance with market distance. Panel B reports results from the first-stage and show that the instrument is relevant. Panel B shows that the IV coefficients are positive and statistically significant at the 1-percent level. Their magnitude is 1-2 times larger than the OLS counterparts. This is expected because of positive spillovers in civic values across generations and also because market attendance is aggregated at the group level.

VI.B. Falsification Test

The above results imply that if there were no market failure from asymmetric information, that is, if product quality is verifiable, then there should be no effect of market distance on civic values and rules, unless other channels confounded with market distance like prosperity and contact hypothesis Allport (1954) are at work. There is a possibility that market exposure makes individuals prosperous and this causes them to have higher civic values and form rules. Alternatively, since market exchange occurs at market places, it is possible that contact with outsiders shapes civic values and rules.

I construct a falsification test to separate these channels. For this purpose, I use distance from two alternative periodical markets. Three points are worth considering:

1) The alternative markets trade primarily in products of verifiable quality, such as aluminium utensils and polyester textiles.[13] I label distance to alternative markets as "Market Distance – No AI", as there is no asymmetric information. In contrast, distance to livestock markets is labelled "Market Distance – AI".

2) It is highly unlikely that the groups chose the location of alternative markets. The groups are very small to wield any influence on market location. Endogenous placement is possible only if groups coordinate with neighboring groups on providing a public good like market. In this case, Market Distance – No AI should also have a negative effect on civic values and rules. Crucially, there is no correlation between Market Distance – No AI and Market Distance – AI (r=0.09, p-value=0.51). To account for unobserved market specific differences, I include fixed effects for all markets in the analysis.

3) Third, I show that prosperity declines with distance from both livestock and alternative markets. Measuring prosperity in resource poor environments is difficult. Instead of relying on several proxies whose choice may be arbitrary, I follow (Tabellini, 2010) and extract the first principal component of several proxies as a summary measure of prosperity. These include self-reported financial rating, number of livestock units, and land

[13]Note that the sellers in local markets do not manufacture these items. Also, since Aluminium and polyester are already cheaper, it is difficult to replace them with even cheaper materials.

holding.[14] Fig 6 presents results from a regression of the principal component of prosperity on Market Distance – AI (grey circle) and Market Distance – No AI (black diamond). Both distances have negative and statistically significant coefficients which are comparable in magnitude and change little once fixed effects and controls are introduced. Moreover, survey data reveal that individuals attend both livestock and alternative markets.

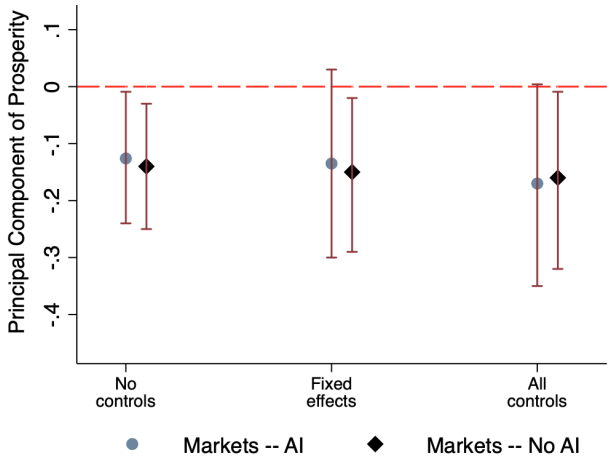

Figure 6: Market Distance and Prosperity

Notes: The figure plots the coefficient from a regression of the principal component of prosperity on market distance. The capped bars indicate 95 percent confidence bands.

If prosperity and contact hypothesis are driving the effect of markets then distance to alternative market should matter for civic values and rules even when there is no asymmetric information. Table 5 presents results from a falsification test. Column 1 is without any controls and includes only Market Distance – No AI. It enters mostly with a positive sign, but is always close to zero in magnitude and is never statistically significant in any of the three panels. When I introduce Market Distance – AI in column 2, the coefficient on market distance – No AI remains statistically and economically insignificant. In contrast, the coefficient on market distance – AI enters with a large negative sign and is always statistically significant at the 1-percent level. The results change little in magnitude and significance when I introduce fixed effects in column 3 and the remaining controls in column 4. The results remain unchanged when I additionally incorporate distance to a small market town called Bubisa in the construction of Market Distance – No AI (see Table A.13). These findings confirm that markets with asymmetric information foster civic values and rules, whereas prosperity and contact hypothesis are unlikely channels. After all, if product quality is verifiable then there is no need for civic values (Bowles, 1998).

[14]Self-reported financial rating was elicited through a survey which classified individuals into poor, middle class, rich. Livestock units uses weights based on market price: horse (1), cattle (0.8), donkey (0.7), sheep (0.2), goat (0.2). I do not consider housing quality as all households live in dwellings of mud floor and thatched roofs.

Table 5: Market Distance, Civic Values, and Rules:
Alternative Market Distance

	No controls	Market distance	Fixed effects	Controls and FE
	(1)	(2)	(3)	(4)
	Panel A: Conditional Cooperation (Spearman *rho*)			
Market distance – No AI	0.014	0.030	0.025	0.069
	(0.039)	(0.036)	(0.043)	(0.050)
Market distance – AI		−0.160	−0.181	−0.171
		(0.023)	(0.030)	(0.026)
R^2	0.00	0.12	0.15	0.19
	Panel B: Monitoring (hours)			
Market distance – No AI	0.668	1.312	1.213	2.047
	(1.775)	(1.061)	(1.490)	(1.423)
Market distance – AI		−8.603	−8.145	−6.592
		(1.125)	(1.076)	(0.940)
R^2	0.00	0.39	0.50	0.60
	Panel C: Grazing Rules (indicator)			
Market distance – No AI	−0.012	0.014	0.078	0.051
	(0.074)	(0.044)	(0.046)	(0.036)
Market distance – AI		−0.318	−0.264	−0.201
		(0.029)	(0.036)	(0.054)
R^2	0.00	0.51	0.62	0.68
Controls	No	No	No	Yes
Fixed effects	No	No	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Market distance – No AI is distance to markets that trade mostly in products of verifiable quality and hence without asymmetric information. Market distance – AI is the distance to livestock markets and is therefore with asymmetric information. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan, livestock markets, and alternative markets. The number of observations is 720 in Panel A, 508 in Panel B, and 511 in Panel C. FE stands for fixed effects in column 4

I carry out additional tests that yield similar results. I investigate whether the effect of market distance on civic values and rules holds once I introduce the principal component of prosperity as an additional control, acknowledging that it is endogenous. Table A.12 shows that this does not lead to any changes in the magnitude and significance of the coefficient on market distance. In contrast, the coefficient on the principal component of prosperity enters with a small and statistically insignificant coefficient. Following Algan et al. (2022), I use naming patterns of individuals as a proxy for contact hypothesis. In the study area, individuals have either an Oromo (Cushitic) or an Arabic (Semitic) name.[15] I test if the proportion of individuals with an Arabic name differs by market distance. Using a unique dataset on the name of each household head (available for 27 groups), I show in Table A.14 that there is no difference in the proportion of Arabic names by market distance.

[15]Individuals rarely give their children Amharic or Tigray sounding names.

VII. Conclusions

I study a long standing question in economics on whether market exposure fosters or erodes civic values and rules. Major challenges in conducting this study lies in identifying mechanisms and in establishing causality. I resolve these challenges using the context of livestock markets from rural Ethiopia that are prone to market failure from asymmetric information and absence of third party verification. I exploit a natural experiment on the emergence and location of markets, which mitigates concerns over pre-existing differences and sorting.

Using market distance as a proxy for market exposure, I show a strong negative effect of market distance on experimental and survey measures of civic values and rules. The further the individuals are from markets, the lower is their propensity to cooperate conditionally, time spent monitoring their forest, and formation of rules regulating grazing inside their forest. These results are robust to controlling for a variety of geographical, economic, forest, and clan specific variables, as well as distance from local administration, schools, and religious places, population density, and trust in government.

The results arise because livestock exchange is prone to market failure from asymmetric information and lack of third party verification from weak state capacity - a characteristic that is common across markets in sub-Saharan Africa and other developing economies. These features lock buyers and sellers in a cooperation problem. Depending on their distance from markets, groups developed different kinds of exchange structures to mitigate this problem. Individuals acquire and internalize heuristics from these exchange structures and apply them across cooperation domains to reduce cognitive dissonance. To this end, I show that individuals from groups located close to markets display higher trust and cooperation even when repeated interaction, reputation, and punishment are absent. A placebo test shows that distance from markets that are not prone to asymmetric information has no effect on civic values and rules. The placebo further allows me to rule out other channels like prosperity and contact hypothesis.

These results fill an important gap in the literature and highlight the importance of asymmetric information and market distance in the economic organization of societies which ultimately shapes cultural values. Future studies can use randomized control trials to experimentally vary the opportunistic cost of market attendance through infrastructure development and then test how this affects civic values and rules. The findings could be useful in studying if infrastructure projects contribute to growth and development (see Donaldson, 2018) through shaping of civic values and rules.

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ONLINE APPENDIX

Market Exposure, Civic Values, and Rules

Devesh Rustagi

Appendix A

I. Historical Setting

Figure A.1 shows the picture of a camp taken by Borelli (1890). It is evident from the picture that the camps were made of perishable materials.

Figure A.1: Military Camp of Menelik

Notes: Source: Borelli (1890).

Figure A.2 shows a stylized organization of a group with households from two different hamlets, one from clan 1 and another from clan 2. The picture to the right shows an actual group with similar organization – two hamlets from two different clans.

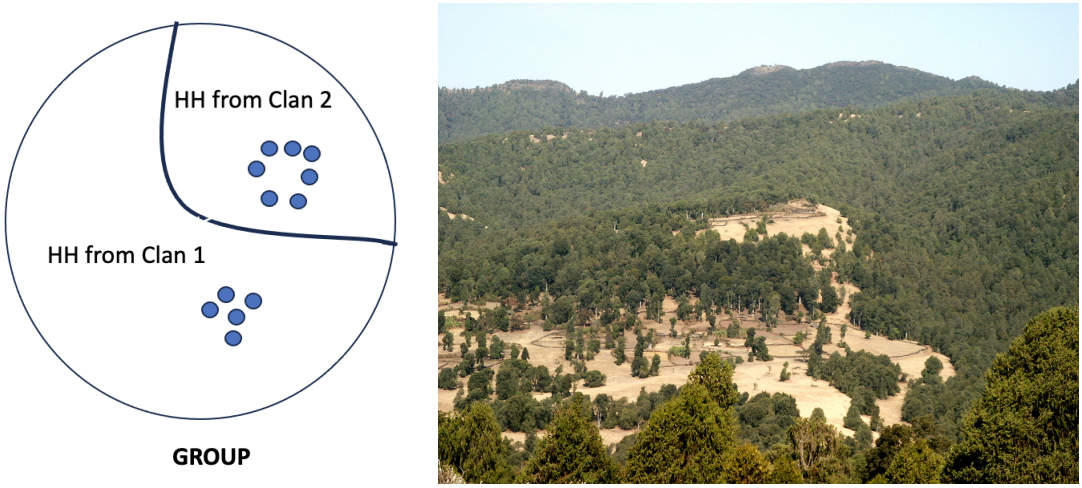

Figure A.2: Group Organization and Hamlets

Notes: The picture shows a group with households from two hamlets from two different clans

II. Data

I test the representativeness of the experimental and survey samples. The figures below show that the frequency of clans observed in the different samples is the same as the frequency of clans in the population.

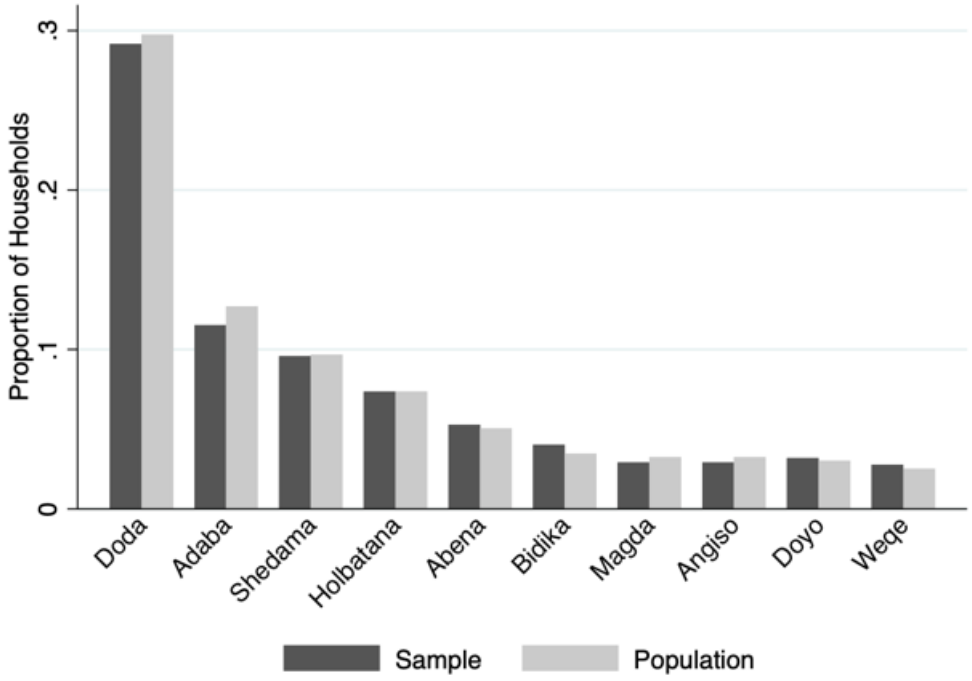

Figure A.3: Proportion of Households from Main Clans in the Experimental Sample and the Population

Notes: The bar graph shows the proportion of households from ten main clans in the experimental sample used to measure conditional cooperation and the population of interest.

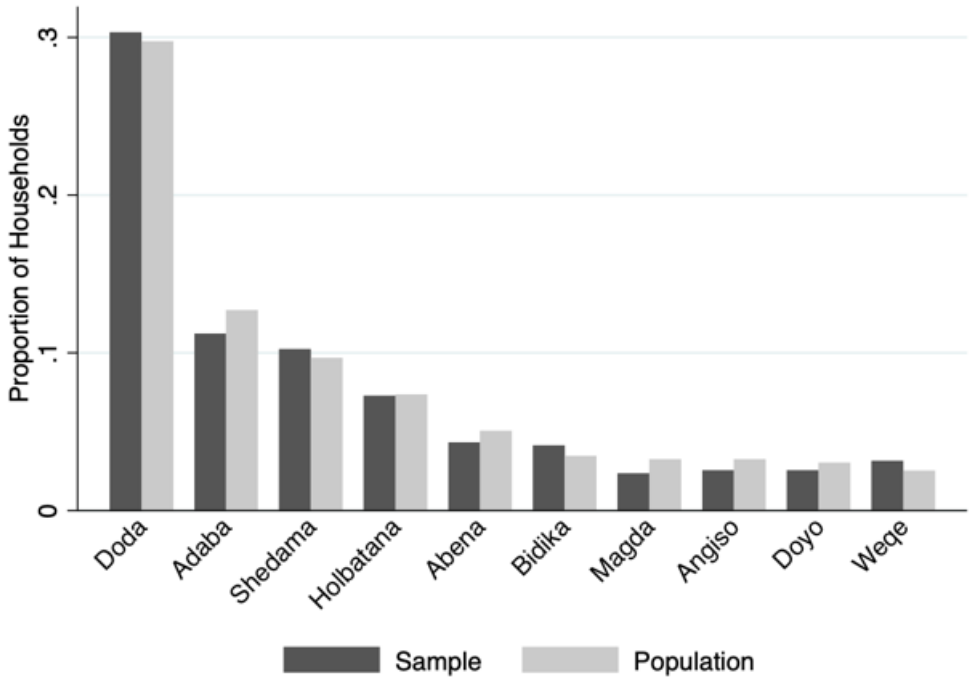

Figure A.4: Proportion of Households from Main Clans in the Monitoring Survey Sample and the Population

Notes: The bar graph shows the proportion of households from ten main clans in the sample used to measure monitoring and the population of interest.

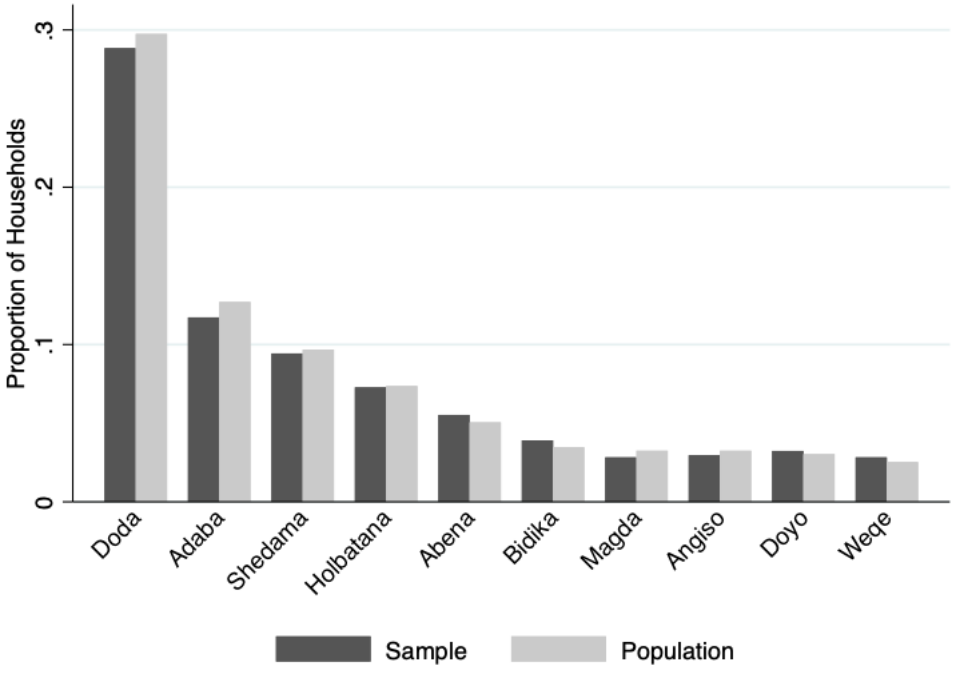

Figure A.5: Proportion of Households from Main Clans in the Grazing Rules Survey Sample and the Population

Notes: The bar graph shows the proportion of households from ten main clans in the sample used to measure grazing rules and the population of interest.

III. Summary Statistics

Table A.1 reports summary statistics of main control variables. Table A.2 reports summary statistics of control variables used for testing the robustness of the main results and also for testing the channels through which markets affect civic values and rules.

Table A.1: Summary Statistics

	Measurement unit	Obs.	Mean	Standard deviation
	A: Civic Values and Rules			
Main dependent variables				
Conditional cooperation (Spearman *rho*)	individual	720	0.499	0.518
Monitoring (hours per month)	individual	508	27.804	15.480
Grazing Rules (indicator)	individual	511	0.446	0.498
Alternative dependent variables				
Conditional cooperator (indicator)	individual	720	0.468	0.499
Grazing ban (months in a year)	individual	509	1.472	1.761
Unconditional contribution	individual	704	2.054	1.511
Belief about others' contribution	individual	704	2.675	1.710
	B: Market Distance			
Market distance (hours)	group	52	2.851	1.111
Market attendance (visits per week)	group	52	0.92	0.565
Alternative market distance (hours)	group	52	2.495	1.015
	C: Main Covariates and Fixed Effects			
Altitude	group	52	0.231	0.425
Group size	group	52	26.308	4.625
Group fragmentation	group	52	0.489	0.246
Female share	group	52	0.202	0.113
Gini of Cattle ownership	group	52	0.334	0.083
Gini of Land ownership	group	52	0.298	0.077
Program duration (months)	group	52	23.000	19.623
Education (years)	individual	742	3.315	3.062

Notes: Main dependent variables: Conditional cooperation is the Spearman *rho* between own and other players' contribution in the conditional decision of the public goods game. Monitoring is time spent in hours per month by individuals while managing their forest commons. Grazing rules is an indicator which equals 1 if a group has grazing rules, otherwise 0. *Alternative dependent variables*: conditional cooperator is an indicator for an individual who behaves as a conditional cooperator in the conditional decision of the public goods game, otherwise 0. Grazing ban is the number of months grazing is forbidden in the group managed forest. Actual contribution is from the first decision of the public goods game when players decide simultaneously on their contribution. Belief is about other player's contribution to the public good in the first decision of the public goods game. *Market distance* is one-way walking distance to market (hours on foot). Market attendance is the number of market visit per week. Alternative market distance is distance to markets that mainly trade in products of verifiable quality. *Main Covariates*: Altitude is an indicator for groups above 3100 meters, as it is beyond this altitude that forest type changes from a mix of broadleaf and coniferous to Erica heather. Group size is the number of households in a group. Group fragmentation is a Herfindahl index – the probability that two persons selected randomly from a group will not be from the same hamlet. Female share is the share of female household heads in a group. Gini of cattle and land are the Gini indices. Program duration is the number of months a group has been under the commons management program. Education is years of schooling. Measurement unit indicates the level at which the data were collected.

Table A.2: Summary Statistics: Additional Control Variables

	Measurement unit	Obs.	Mean	Standard deviation
Forest				
Plantation forest	group	52	0.085	0.353
Median tree cover in 2000 (%)	group	52	39.701	9.788
Geography				
Latitude	group	52	6.896	0.033
Longitude	group	52	39.299	0.083
Border				
Number of neighbors	group	52	3.737	1.624
Other				
Population density	group	52	13.988	8.738
Trust in Government	individual	503	0.406	0.491
Distances				
Administration distance	group	52	1.649	1.110
School distance	group	52	0.822	0.378
Mosque distance	group	52	0.702	0.462

Notes: Forest: plantation forest is the ratio of area under plantation forest to area under natural forest. Median tree cover is the forest cover in percentage in a group in 2000 from (Hansen et al., 2013). *Geography*: Latitude and longitude are measured in degrees. *Border*: is the number of neighboring groups. *Other*: Population density is number of persons per km^2. Trust in government is a binary variable, where 1 implies the government is cooperative and 0 otherwise. It was collected using a household survey in which individuals were asked to rate the district government on a 5 point scale: very cooperative, cooperative, neutral, dominating, very dominating. Most respondents chose either cooperative or dominating. *Distances*: Administration, school and mosque distance are local distances measured in hours.

IV. Empirical Strategy

Figure A.6 reports balance check by market distance.

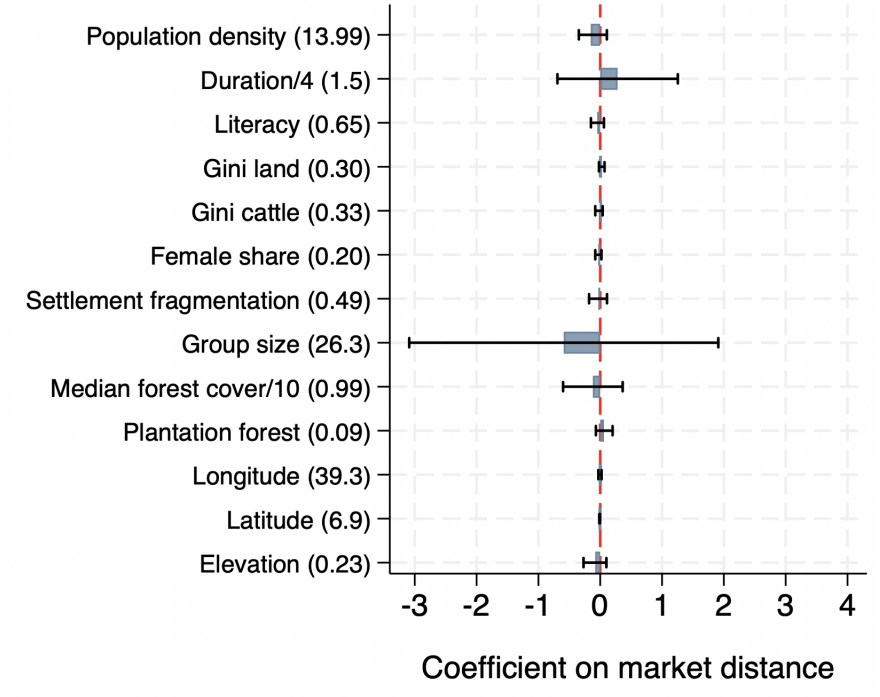

Figure A.6: Balance check

Notes: The figure plots the coefficient on market distance from a regression of the row variable on market distance and controls.

V. Main Results

Table A.3 shows the coefficients on control variables. Table A.4 shows the coefficient on market distance is robust to using different kinds of clustering of standard errors. Table A.5 shows the results are robust to using alternative dependent variables. Table A.8 and Table A.6 shows the results are robust to controlling for additional forest and geography related controls. Table A.7 shows the results are robust to controlling for distance from administration, school, and mosque. Table A.9 shows the coefficient on market distance is robust to simultaneously controlling for both additional controls and those related to distance and urbanization. Figure A.7 and Table A.10 shows the effect of market distance is similar by age-groups.

Table A.3: Market Distance, Civic Values, and Rules: Coefficients on Control Variables

	Dependent variable:		
	Conditional cooperation	Monitoring	Grazing rules
	(1)	(2)	(3)
Market distance	-0.156	-6.106	-0.195
	(0.023)	(0.902)	(0.054)
Altitude	-0.114	-5.201	-0.090
	(0.075)	(2.542)	(0.148)
Group size	-0.002	-0.472	0.001
	(0.005)	(0.216)	(0.007)
Group fragmentation	0.273	6.177	-0.089
	(0.094)	(3.576)	(0.123)
Female share	-0.152	6.060	-0.332
	(0.239)	(10.543)	(0.249)
Gini of cattle ownership	0.594	1.418	-0.926
	(0.345)	(11.361)	(0.514)
Gini of land ownership	0.159	5.349	0.537
	(0.345)	(11.998)	(0.422)
Education	-0.004	0.287	0.004
	(0.007)	(0.161)	(0.005)
Program duration	-0.001	0.269	0.012
	(0.004)	(0.091)	(0.005)
R^2	0.18	0.57	0.68
Fixed effects	Yes	Yes	Yes
Observations	720	508	511

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Fixed effects are for clan and markets.

Table A.4: Market Distance, Civic Values, and Rules:
Clustering standard errors on markets, village, clans, and space

	Standard errors are clustered by			
	group & market	group & admin	group & clan	space
	(1)	(2)	(3)	(4)
	Panel A: Conditional Cooperation			
Market distance	-0.156	-0.156	-0.156	-0.156
	(0.025)	(0.027)	(0.024)	(0.020)
	Panel B: Monitoring			
Market distance	-6.106	-6.106	-6.106	-6.106
	(2.000)	(1.580)	(1.124)	(1.180)
	Panel C: Grazing Rules			
Market distance	-0.195	-0.195	-0.195	-0.195
	(0.034)	(0.037)	(0.040)	(0.052)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors in parentheses clustered on the group and market in column 1, the group and administration in column 2, the group and clan in column 3, and on space using a cut-off of 5 km. The results hold when other cutoffs, such as 4, 5, and 7 km are chosen. Control variables include altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land ownership, education, and program duration. Fixed effects are for clan and market.

Table A.5: Market Distance, Civic Values, and Rules:
Alternative Dependent Variables

	Indicator for conditional cooperator		Grazing ban in months	
	No Controls	Controls and fixed effects	No Controls	Controls and fixed effects
	(1)	(2)	(3)	(4)
Market distance	-0.122	-0.097	-1.098	-0.614
	(0.017)	(0.029)	(0.120)	(0.163)
R^2	0.07	0.12	0.49	0.67
Controls	No	Yes	No	Yes
Fixed effects	No	Yes	No	Yes
Observations	720	720	509	509

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. The dependent variable in column 1 is an indicator for conditional cooperator and in column 2 the number of months grazing is banned inside the group managed forest. Control variables include altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market.

Table A.6: Market Distance, Beliefs about Others' Contribution, and Own Contribution

	Beliefs about others' contribution		Own contribution	
	No Controls	Controls and fixed effects	No Controls	Controls and fixed effects
	(1)	(2)	(3)	(4)
Market distance	-0.419	-0.382	-0.451	-0.404
	(0.074)	(0.128)	(0.100)	(0.175)
R^2	0.07	0.13	0.10	0.20
Controls	No	Yes	No	Yes
Fixed effects	No	Yes	No	Yes
Observations	704	704	704	704

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market. Beliefs and own contribution are from the first decision of the public goods game. This data is missing from one group.

Table A.7: Market Distance, Civic Values, and Rules: Urban Amenities

	Admin--inistration (1)	Primary school (2)	Local mosque (3)	Population density (4)	Trust in government (5)
	Panel A: Conditional Cooperation (Spearman)				
Market distance	-0.195	-0.200	-0.152	-0.186	-0.188
	(0.034)	(0.037)	(0.035)	(0.036)	(0.040)
Administration distance	0.070	0.066	0.028	0.030	0.026
	(0.047)	(0.048)	(0.042)	(0.042)	(0.044)
School distance		0.053	-0.047	-0.011	-0.032
		(0.093)	(0.090)	(0.080)	(0.084)
Mosque distance			-0.312	-0.284	-0.280
			(0.058)	(0.054)	(0.068)
Population density				-0.012	-0.013
				(0.003)	(0.004)
Trust in government					-0.056
					(0.045)
	Panel B: Monitoring (hours)				
Market distance	-8.501	-9.431	-9.513	-8.618	-8.515
	(1.443)	(1.300)	(1.370)	(1.500)	(1.411)
Administration distance	4.088	3.812	3.870	3.886	4.493
	(1.565)	(1.500)	(1.551)	(1.493)	(1.441)
School distance		7.393	7.561	6.477	4.835
		(3.265)	(3.560)	(3.706)	(3.487)
Mosque distance			0.570	-0.149	-0.008
			(2.429)	(2.498)	(2.337)
Population density				0.313	0.334
				(0.150)	(0.136)
Trust in government					4.909
					(1.489)
	Panel C: Grazing Rules (indicator)				
Market distance	-0.127	-0.188	-0.178	-0.141	-0.139
	(0.060)	(0.041)	(0.045)	(0.049)	(0.049)
Administration distance	-0.117	-0.134	-0.142	-0.141	-0.132
	(0.047)	(0.047)	(0.043)	(0.042)	(0.042)
School distance		0.484	0.462	0.419	0.398
		(0.120)	(0.130)	(0.119)	(0.115)
Mosque distance			-0.072	-0.102	-0.096
			(0.115)	(0.108)	(0.108)
Population density				0.013	0.014
				(0.006)	(0.006)
Trust in government					0.079
					(0.043)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market. In columns 1-4, the number of observations is 720 in Panel A, 508 in Panel B, and 511 in Panel C. In column 5, the no. of observations are 482 in Panel A, 500 in panel B, and 503 in panel C.

Table A.8: Market Distance, Civic Values, and Rules: Robustness Checks

	Forest condition	Latitude and longitude	Number of bordering grous
	(1)	(2)	(3)
	Panel A: Conditional Cooperation (Spearman *rho*)		
Market distance	-0.155	-0.180	-0.175
	(0.024)	(0.037)	(0.038)
Plantation forest	0.029	0.047	0.038
	(0.049)	(0.054)	(0.058)
Median tree cover	0.001	-0.000	0.000
	(0.003)	(0.004)	(0.004)
Latitude		-2.401	-2.261
		(2.159)	(2.176)
Longitude		-0.593	-0.547
		(1.137)	(1.155)
Border			-0.012
			(0.020)
	Panel B: Time Spent Monitoring (hours)		
Market distance	-5.977	-5.601	-5.895
	(0.901)	(1.242)	(1.284)
Plantation forest	0.316	0.109	0.569
	(2.714)	(2.790)	(2.788)
Median tree cover	-0.083	-0.129	-0.158
	(0.128)	(0.128)	(0.121)
Latitude		-25.950	-32.585
		(63.086)	(62.951)
Longitude		-61.518	-64.103
		(36.759)	(35.897)
Border			0.694
			(0.701)
	Panel C: Grazing Rules (indicator)		
Market distance	-0.196	-0.161	-0.172
	(0.045)	(0.051)	(0.057)
Plantation forest	-0.151	-0.176	-0.160
	(0.081)	(0.083)	(0.081)
Median tree cover	-0.009	-0.008	-0.009
	(0.005)	(0.006)	(0.006)
Latitude		2.627	2.387
		(1.944)	(1.941)
Longitude		-0.162	-0.256
		(1.026)	(1.046)
Border			0.026
			(0.038)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market. The number of observations is 720 in Panel A, 508 in Panel B, and 511 in Panel C.

Table A.9: Market Distance, Civic Values, and Rules: All Controls

	Dependent variable:		
	Conditional cooperation	Monitoring	Grazing rules
	(1)	(2)	(3)
Market distance	-0.194	-8.313	-0.118
	(0.050)	(1.639)	(0.061)
R^2	0.21	0.63	0.77
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market. Additional controls include plantation forest, median tree cover in 2000, latitude, longitude, and border. Distance related controls include distance to administration, primary school, and mosque, as well as population density and trust in government.

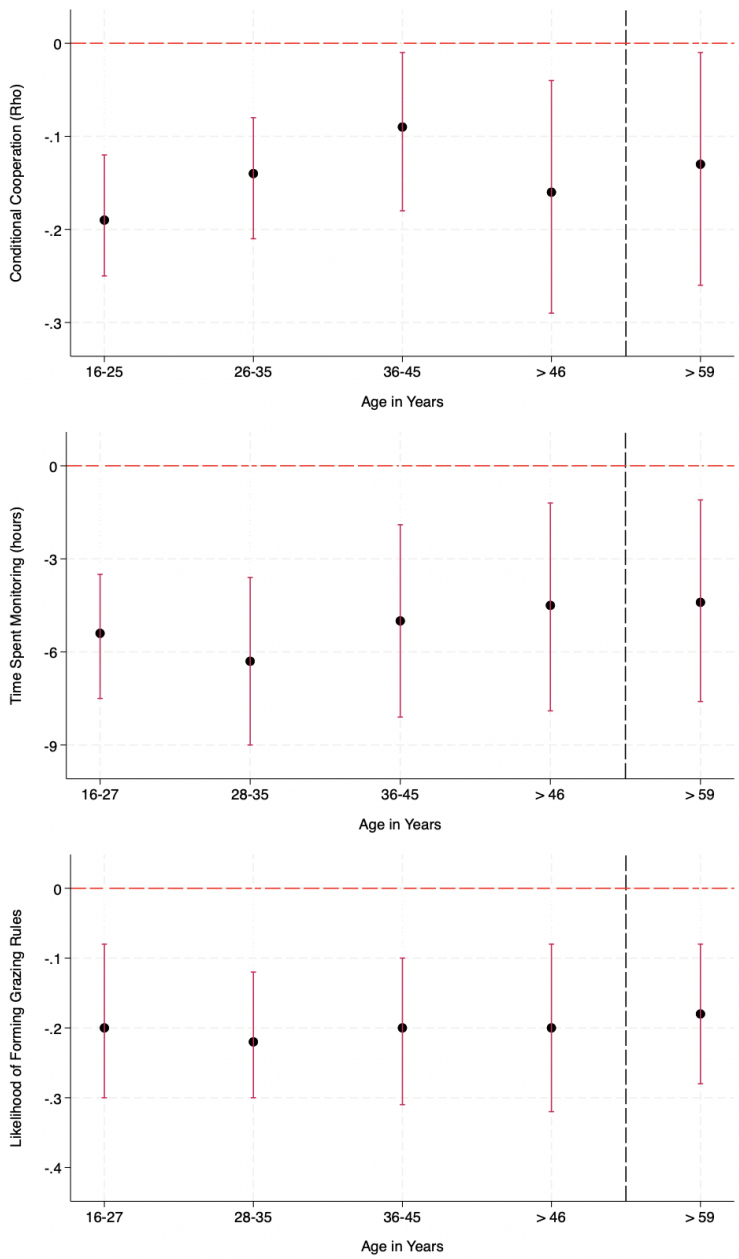

Figure A.7: Effect of Market Distance on Civic Values and Rules by Age

Notes: The figure plots the coefficient on market distance by quartiles of age and in a sample of individuals over 59 years old. The dotted red line indicates zero difference. The capped bars indicate 95 percent confidence bands.

Table A.10: Market Distance, Civic Values, and Rules:
Effects by Age Quartile

	First Quartile	Second Quartile	Third Quartile	Fourth Quartile	60 and above
	(1)	(2)	(3)	4	5
	Panel A: Conditional Cooperation (Spearman *rho*)				
Market distance	-0.190	-0.140	-0.094	-0.165	-0.131
	(0.034)	(0.032)	(0.043)	(0.064)	(0.064)
Obs.	177	188	170	154	82
	Panel B: Monitoring (hours)				
Market distance	-5.470	-6.298	-4.994	-4.512	-4.394
	(0.988)	(1.358)	(1.538)	(1.663)	(1.581)
Obs.	131	137	130	110	36
	Panel C: Grazing Rules (indicator)				
Market distance	-0.203	-0.216	-0.204	-0.203	-0.182
	(0.062)	(0.049)	(0.050)	(0.060)	(0.048)
Obs.	132	138	132	109	36
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market.

VI. Plausible Channels

Table A.11 shows the IV estimates of the effect of market attendance on civic values and rules. Table A.12 shows the main results are robust to controlling for the first principal component of prosperity. Table A.13 shows there is no effect of market distance - No AI even when I consider the third market. Table A.14 shows there is no effect of market distance on the likelihood of having an Arabic name.

Table A.11: Market Attendance, Civic Values, and Rules: IV Estimation

	Conditional cooperation	Time spent Monitoring	Grazing rules
	(1)	(2)	(3)
	Panel A: IV estimates second-stage		
Market attendance	0.529	20.675	0.662
	(0.107)	(3.823)	(0.150)
	Panel B: IV estimates first-stage		
Market distance	-0.295	-0.295	-0.295
	(0.041)	(0.041)	(0.041)
$F - Statistics$	51.30	50.66	50.72
Control variables	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	720	508	511

Notes: IV estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and markets.

Table A.12: Market Distance, Civic Values, and Rules: Controlling for Prosperity

	Dependent variable:		
	Conditional cooperation	Monitoring	Grazing rules
	(1)	(2)	(3)
Market distance	-0.164	-6.133	-0.185
	(0.023)	(0.964)	(0.054)
PC Prosperity	0.003	-0.047	0.019
	(0.015)	(0.454)	(0.008)
R^2	0.16	0.57	0.68
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Control variables include altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, education, and program duration. Fixed effects are for clan and market. PC Prosperity is the first principal component of three proxies of prosperity: self-reported financial rating, livestock units, and land holding.

Table A.13: Market Distance, Civic Values, and Rules:
Alternative Market Distance II

	No controls	Market distance	Fixed effects	Controls and FE
	(1)	(2)	(3)	4
	Panel A: Conditional Cooperation (Spearman *rho*)			
Market distance II - No AI	0.062	0.032	0.016	0.034
	(0.034)	(0.035)	(0.046)	(0.052)
Market distance - AI		−0.153	−0.170	−0.174
		(0.024)	(0.030)	(0.027)
R^2	0.01	0.12	0.15	0.18
	Panel B: Monitoring (hours)			
Market distance II - No AI	5.043	3.263	2.680	2.370
	(1.483)	(1.161)	(1.574)	(1.524)
Market distance- AI		−7.945	−6.662	−5.876
		(1.100)	(0.763)	(0.812)
R^2	0.11	0.43	0.54	0.60
	Panel C: Grazing Rules (indicator)			
Market distance II - No AI	0.116	0.049	0.081	0.029
	(0.054)	(0.039)	(0.053)	(0.043)
Market distance- AI		−0.309	−0.235	−0.179
		(0.032)	(0.044)	(0.060)
R^2	0.05	0.52	0.62	0.68
Controls	No	No	Yes	Yes
Other market FE	No	No	Yes	Yes
Clan FE	No	No	Yes	Yes
Market FE	No	No	Yes	Yes

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. Market distance II - No AI is the distance to three markets that do not trade in livestock but in products of verifiable quality and hence are not prone to asymmetric information. Market distance - AI is the distance to markets that trade in livestock and are therefore prone to asymmetric information. Control variables include altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, education, and program duration. Other market FE includes fixed effects for non-livestock markets, clan FE includes fixed effects for clans, and market FE includes fixed effects for livestock markets prone to asymmetric information. The number of observations is 720 in Panel A, 508 in Panel B, and 511 in Panel C.

Table A.14: Market Distance and Exposure to the Outside World:
Frequency of Arabic Names

	Dependent variable: Indicator for Arabic Name	
	(1)	(2)
Market distance	0.017	0.016
	(0.018)	(0.017)
R^2	0.001	0.004
Market FE	No	Yes
Obs.	735	735

Notes: OLS estimates with robust standard errors clustered on the group in parentheses. FE stands for fixed effects. Source: Adada-Dodola Forest Management Office.

Appendix B Experimental Procedures and Instructions

I. Experimental Procedures

I conducted three experiments, of which the paper uses data from the experiment that was conducted first. The household and community surveys were carried out after the experiments were conducted. I adopted several procedures to ensure that experimental measures are comparable across different groups and were not affected by contagion and contamination. Wherever possible, I followed the approach pioneered by Henrich et al. (see 2001, 2010) to conducting experiments in the field. This involved the following steps:

First, I used fixed written instructions, set of examples, and control test questions to conduct the experiments. This ensured uniformity in procedures and verbal explanations across groups. The written instructions were tested in a pilot study in the Yayu-Metu region of Ethiopia, which is linguistically similar but 400 km away from the study area. Based on the feedback, the instructions were fine-tuned and tested again in three groups that were excluded from the sample.

Second, the participants in experiments are familiar with money, especially small currency bills. I used actual bills of one Ethiopian Birr while delivering instructions, examples, testing, and for playing the actual game. This also made it easier for the participants to make simple calculations.

Third, the scope of contagion and contamination is larger if there is a wide gap separating the dates on which experiments are held in neighboring groups, as this leaves more time to discuss with future participants. To mitigate these concerns, whenever possible, I invited individuals from neighboring groups either at the same time or just the day after. The experiments were conducted right after the invitations were sent and were run each day, without a break, till all groups from a village had taken part. Once this was achieved, I moved to the next village and followed similar procedures there. Because the experiments lasted an entire day, individuals were left with less time to communicate with those yet to take part. Absence of electricity and mobile reception were of further help in achieving this. I find no difference in behavior when two groups were invited to take part in the experiment the same day or when one group was invited. The share of conditional cooperators or free riders is similar across these two situations.

Fourth, all experiments were run by me together with the main assistant. When two groups were invited at the same time, one group was guided through the experiment by me and the other group by the main assistant.I do not find any evidence of an experimenter effect on either the share of conditional cooperators (P-value > 0.7) or free riders (P-value > 0.6).

Fifth, participants were ensured that their decisions will be anonymous to each other as well as to the research assistants. Moreover, even though the main assistant is a native speaker, he is not from the study area and hence was not personally known to any of the participants.

Sixth, we employed community mobilizers, one from each village, to organize appointments with groups on our behalf. The community mobilizers knew nothing about the experiments or the surveys. They were informed: (a) to invite group members to arrive at an appointed date to play some games in which they could earn some money, (b) participation in the games was entirely voluntary, (c) the games could last an entire day. The mobilizers were given strict instructions to invite 15-25 households from each group. In addition, group leader, vice leader, and group committee members were highly encouraged to take part in the experiments. In some cases, household heads could not show up and sent a representative from their household. Representatives above 18 years old were allowed to take part in the experiments. Most experiments took place at administrative offices and camps. This did not affect any behavior in the experiment.

Seventh, once enough participants had arrived, we invited them to a room and requested them to sit according to their group affiliation. Participants were not allowed to enter the room if they were late by 15 or more minutes. Two local field assistants made sure that latecomers were sent back with a show up fee. After this, we requested the leaders or vice leader to check if: (i) all participants were from the invited group/s, (ii) did not belong to the same household, and (iii) were at least 18 years old. Participants who did not meet these criteria were sent back. In addition, nursing mothers with babies, sick participants, very old, and those with other health problems were also sent back with a show up fee.

Eight, before the experiments began, the main author and the main assistant introduced themselves in the local language, Afaan Oromo. After this, the main assistant took over the task of reading out the written instructions to the entire group. Because most of our subjects had limited literacy, we designed instructions didactically. During the instructions, the participants were given lots of opportunities to ask questions. Any unexpected question, for which we did not have a script, was answered first by the main author and then translated by the main assistant. This was followed by a fixed set of examples, which were illustrated by me. This had many advantages: (a) participants got used to me speaking in Afaan Oromo. I consider this important because when two groups were invited, I took over the responsibility of testing and engaging members from one of the two invited groups in the game; (ii) it gave some respite to the main assistant; and (iii) I gained participants' trust. The main assistant made sure that participants understood my accent; none of the participants complained about this. Moreover, the participants were very happy that I could speak their language.

ninth, before the actual game, the participants were tested one by one for their game

comprehension using a fixed set of control questions. These questions focused on testing a participant's basic understanding of the game, such as addition, multiplication, division, and other skills. While the main assistant tested this for households from one group, I did so simultaneously for households from another group. All participants rejected by me were tested again by the main assistant. Those who could not answer the questions correctly were given a show up fee and sent back. Further, two local assistants made sure that the rejected participants and uninvited persons had left the experimental venue.

tenth, depending on their group, the selected participants were given plastic identity cards bearing names of Swiss Cantons and German States. Before the actual game began, the selected participants were given another opportunity to ask further questions. During the actual game, participants were called one by one to a room / secluded area. Inside the room, subjects were given six bills of one Birr and then asked to make an unconditional decision to contribute to the public good. Once all participants had taken part in this decision, they were called one by one again to take the conditional decisions. In the meanwhile, the local assistants made sure that no one discussed the game. As in the experiments, anonymity was also ensured during the interviews. We were able to match a participant's behavior in the game with the interview through his/ her experimental identity (Swiss Cantons or German States).

II. Experimental Instructions

Introduction

Greetings and welcome to all of you. My name is Devesh Rustagi and I am a student from a university in Switzerland. I am here for a research concerning livelihood improvement through forest conservation. For my research, I would like to play a few games with you. Depending on the decisions made by you and other players in these games, you can win some money. The payment that you receive from these games is not from my own pocket, but sponsored by the German government. Before we proceed with the games, I would like to tell you some important things.

General instructions

1. In all games, your identity will be kept anonymous. This means that except for me and my assistant, no one will come to know of your identity. I am interested only in the decisions made by you in these games and not your identity. This is the reason that we will not ask your name in any of the games. We will identify your decision in the game with an identity card like this (show plastic cards). Please do not lose this card.

2. All games will be played for one round only. This means that after you have played the first game, we will begin with the second game. Likewise, after the second game is

over, we will begin with the third game. This means that after a game is over, there is no subsequent interaction.

3. We will play three games with you, but you will receive your money only in the end. We will keep a record of your earnings in all the games on a sheet like this (show payoff sheets for clarity) to make sure that you receive the correct amount.

4. We will give you separate instructions and examples on how to play each of these games. The instructions for each game will be given before we play the game. For instance, before we play the first game, we will give you the instructions on how to play the first game. Likewise, when we play the second game, we will give you the instructions for the second game, and so on. It is very important that you listen to these instructions carefully. In case you do not understand the game, please stop us and ask us. We will be happy to help you.

5. Before we play the actual game, we will check if you have understood the game or not. In case you do not understand the game, we will give the instructions again. However, if you are still not able to understand the game, we will have no choice but to request you to leave the venue. In this case, you will receive five Birr from us. Therefore, it is important that you listen to the instructions carefully.

6. We would like to keep the game anonymous, therefore, please do not discuss the game with each other. But you may discuss about politics, rainfall, market, cattle, WAJIBS, and other such things. In case we find that you are discussing the game with other players, we will exclude you immediately from the game. In this case, you will not receive any money.

7. We also request you to not to discuss the game with other WAJIB members as this will spoil my study.

8. (Read this only when two societies are invited) You will play the games only with the members of your own society.

9. I repeat again, please do not hesitate to ask any questions. We encourage you to ask as many questions concerning the games as possible. In case you have any questions at this stage, you may ask them now. Otherwise, we will begin with the instructions for the first game.

Experimental instructions

We will now give you instructions and examples for the first game. There are two parts in this game. We will now give you instructions for the first part. This is followed by a test in which we will check if you have understood the game or not. Once we are

sure that you have understood the game, we will begin playing the game.

In this game, we will divide you into groups of two players. You will not come to know to which group you belong. Likewise, you will not come to know the identity of the other (partner) player in your group. Similarly, the other player will not come to know your identity.

At the beginning of the game, each player will receive six Birr from us. Now you have to decide how many from the six Birr to put into your pocket and how many into a project. You may put any amount between 0 and 6 Birr into the project.

Now we will show you how this is done. Please note that since this is an example, we will tell the player how many Birr to put into the project. But when we play the actual game, you will have to decide this on your own, without any help from us. (Randomly select a player and give him six bills of one Birr each. Please make sure that each time YOU tell the person on how much he should put into the project. Do not allow the player to take a decision because this may influence the decision of other potential players). Suppose you are a player in this game. As mentioned before, you receive an endowment of six Birr from us. Now let us assume that out of six Birr, you put zero Birr into the project. Please put zero birr into the project. Ask the group: Can you tell me how many Birr there are in the project? How many Birr does the player have in his pocket? Have you understood this?

Now, let us assume that out of six, you put one Birr into the project. Please put one Birr. How many Birr are in the project? How many Birr does the player have in his pocket? (Carry on this procedure till 6 Ethiopian Birr). Have you understood this part? Do you need additional examples? (If yes, select another person and repeat the examples in the same order).

Any amount in the project will be increased by the same number of Shillingis as the number of Birr in the project. For example, if you put 0 Birr into the project, the project amount will be increased by 0 Shillingis. Now, the final amount of money in the project is 0 Birr. If you put 1 Birr into the project, the project amount will be increased by 1 Shillingi. Now, the final amount of money in the project is 1.5 Birr (Carry on till 6 Birr). I repeat, the project amount will be increased by the same number of Shillingis as the number of Birr in the project. Have you understood this? Do you need additional examples? (If yes, select another person and repeat the examples in the same order).

After the project money has increased, it will be divided equally between you and your partner player, irrespective of how much you have put into the project (Please repeat this again). For example, if the project contains 0 Birr, it will be increased by 0 Birr and then divided equally between you and your partner player. However, since zero does not

increase, both you and your partner will get zero Birr from the project. For example, if the project contains 1 Birr, it will be increased by 1 Shillingi. Now the total value of the project is 1.5 Birr, and both you and your partner player get 0.75 Birr each from the project (carry on till 6 Birr). Have you understood this part? Do you need additional examples? (If yes, select another person and repeat the examples in the same order).

Please remember that any money that you put into the project is first increased and then divided equally among the players in your group. Any amount that you put in your pocket remains the same. If you put 1 Birr in your pocket, it remains 1 Birr. It neither increases nor is it divided.

Your final earning from the game is the sum of the amount you have in your pocket and the amount you receive from the project.

We will now give you three examples. Please note that since now we are learning how to play this game, you can see the identity of each player as well as the decisions made by them. When we play the actual game, you will not come to know of this. Do you understand this? We will now select two people and tell them to take the following decisions in the game. You are player I and you are player II (look for participants with weak comprehension and always give them a chance to act as player I and II). We give you 6 Birr each at the start of the game.

Example 1: Now we will see what happens if both players put zero Birr into the project. Player I and II: Please put zero Birr into the project. Now, can you tell me how many Birr did player I put into the project? How many Birr does he have in his pocket? How many Birr did player II put into the project? How many Birr does he have in his pocket? How many Birr are in the project? We have zero Birr in the project. Since zero Birr does not increase and cannot be divided, each player gets zero Birr back from the project.

Player I has put zero Birr into the project, so he has six Birr in his pocket. He gets zero Birr from the project. Can you tell me, what is his income? Since player I has six Birr in his pocket and he gets zero Birr from the project, his final income is six Birr. (Please repeat the procedure to calculate the income of the second player.)

Example 2: Now we will show you the second example. You are player I and you are player II. You get six Birr from us at the beginning of the game. Now we will see what happens if both players put six Birr into the project. Player I and II please put six Birr into the project. Now, can you tell me how many Birr did player I put into the project? How many Birr does he have in his pocket? How many Birr did player II put into the project? How many Birr does he have in his pocket? How many Birr are in the project?

We have 12 Birr in the project. The project amount will now be increased by 12

Shillings. The final amount in the project is 12 Birr + 12 Shillings = 18 Birr. Now 18 Birr is divided equally among both the players. So, each player gets 9 Birr.

Now, can you tell me, how many Birr does player I have in his pocket? How many Birr does he get from the project? What is his final income? We repeat, since player I has zero Birr in his pocket and he gets nine Birr from the project, his final income is nine Birr. (Please repeat the procedure to calculate the income of the second player.)

Example 3: Now we will show you the third example. You are player I and you are player II. We will see what happens if player I puts zero Birr into the project and Player II puts six Birr into the project. Player I, please put zero Birr into the project and Player II, please put six Birr into the project. Now can you tell me how many Birr did player I put into the project? How many Birr does he have in his pocket? How many Birr did player II put into the project? How many Birr does he have in his pocket? How many Birr are in the project? We have six Birr in the project. The project amount will be increased by 6 Shillings. So the final amount in the project is 6 Birr + 6 Shillings = 9 Birr. Now 9 Birr is divided equally among both the players. So, each player gets 4.5 Birr. Now, how many Birr does player I have in his pocket? How many Birr does he get from the project? So, what is his final income? We repeat, since player I has 6 Birr in his pocket and he gets 4.5 Birr from the project, his final income is 10.5 Birr. How many Birr did player II put into the project? How many Birr does he get from the project? So, what is his final income? I repeat, since player II has zero Birr in his pocket and he gets 4.5 Birr from the project, his final income is 4.5 Birr.

We will now summarize the key results from these examples:

a) If both players put zero Birr into the project, they both earn 6 Birr.

b) If both players put 6 Birr into the project, they both earn 9 Birr.

c) If one player puts zero and the other player puts six Birr into the project, the player who puts zero Birr earns 10.5 Birr, while the player who puts 6 Birr, earns 4.5 Birr.

d) If you and your partner player put the same amount into the project, you both earn the same income.

e) If you put less than what your partner puts into the project, you earn a higher income.

f) If you put more into the project than your partner, you earn a lower income.

If you have any questions, you may ask them now. Otherwise, we will call you one by one and ask six questions to check if you have understood the game or not. Please note that if you answer these questions wrong, we will give you 5 Birr and request you to leave

the game venue. Therefore, please tell us if we need to repeat the examples or not (If yes, repeat the examples in the same order).

Control questions

1. How much money do you get at the start of the game? / What decision do you have to take in the game?

2. Suppose, you decide to put X Birr into the project, how much is left in your pocket?

3. What happens to the money in the project?

4. If you put X Birr into the project, by how much will this increase? What happens after the money is increased?

5. If you put X Birr into the project and your partner also puts X Birr into the project, who earns more?

6. If you put Y Birr into the project and your partner puts Z Birr into the project, who earns more?

(For those who answer 5-6 questions correctly, ask them to sit back in the room. Pay the remaining players 5 Birr and request them to leave. After this, repeat the control questions and let the selected players answer in a chorus. Ask again, if everyone understands. If yes, give them the identity cards).

Actual Game

We will now call you one by one to enter this room and play the game. Please remember that you will not come to know the identity of your partner player or the amount they put in the project.

We will also ask you a question: How many Birr do you believe your partner player will put into the project? This is an important question, so please think before you answer this question.

While you wait for your turn, two assistants will conduct interviews with some of you. They will also check if you discuss the game with each other or not. If they find you discussing the game, we will have to expel you from the game.

When entering the room, please keep your identity card ready.

Unconditional decision

Hello! Have a seat please. I hope you have understood the game. Your identity card, please? Here are your six Birr. Now you have to decide out of six Birr how much you

would like to put into the project. Please put the amount here on the table. How many Birr do you believe your partner player will put into the project? Thank you. Please do not discuss this with the other players.

Additional experimental instructions for the conditional decision

We will now give you instructions to play a slightly different version of the decision that you just played. In the first decision, you did not know the amount your partner player puts into the project. But in this game, we will tell you how many Birr your partner player puts into the project. After you have seen this, you can decide on how many Birr you would like to put into the project. There are seven decisions to be made in this game. Each decision is independent of the other. Please note that you will get a fresh endowment of six Birr at the start of each decision. We will now give you illustrations on how this game is played. Please listen carefully. While we give examples, no one is allowed to speak.

Decision 1: Your partner player in the game puts out of six - zero Birr into the project (put no money on the table). Now, out of six Birr, how much would you like to put into the project? After you have made your decision, the decision is over.

Decision 2: Your partner player in the game puts out of six - one Birr into the project (put one Birr on the table). Now, out of six Birr, how much would you like to put into the project? After you have made your decision, the decision is over. (Carry on till 6 Birr.)

Do you have any questions?

There are seven decisions to be taken in this game. Please watch our fingers for why there are seven decisions. Your partner player puts 0, how much would you like to put; Your partner player puts 1, how much would you like to put; Your partner player puts 2, how much would you like to put; Your partner player puts 3; how much would you like to put; Your partner player puts 4, how much would you like to put; Your partner player puts 5, how much would you like to put; Your partner player puts 6, how much would you like to put. Can you count our fingers now? How many decisions do you have to take in this game? At the beginning of each decision, you will get 6 Birr, just like in the examples you saw. Each decision is independent of the other. A very important point is that we will pick only one of these seven decisions to decide your earnings. So please take all the decisions seriously. Do you have any questions?

We will now call you one by one to play this game. As usual, please keep your identity

card ready.

Conditional decision

Hello! Please take a seat. I hope you have understood the game. Your identity card, please? Here are your six Birr. Now we will show you one by one how much your partner player puts into the project. After you have seen this, you can decide how many Birr you would like to put into the project. Please put the amount here on the table.

Decision 1: Your partner player in the game puts out of six - zero Birr into the project (put no money on the table). Now, out of six Birr, how much would you like into put in the project? Now this decision is over. Please return all the money you have in your hand to me.

Decision 2: Here are your six Birr. How many Birr do you have in your hand? Your partner player in the game puts out of six - one Birr into the project (put one Birr on the table). Now, out of six Birr, how much would you like to put into the project? Now this decision is over. Please return all the money you have in your hand to me.

(And so on till 6 Birr.)

Appendix C Vignette Study

Vignette A: Market Exchange 1

One day an Oromo man called Ibsaa decided to sell his cows to earn money. He went to a market where he met Barentu, another Oromo man. Barentu wanted to buy Ibsaa's cows. The cows looked healthy from outside, but they were actually sick. No one except for Ibsaa knew that. Ibsaa decided not to tell this to anyone. Barentu bought the cows. Some months later, the cows died. This was a big loss to Barentu.

1) In your opinion, what Ibsaa did to Barentu was right or wrong?

2) Are you pleased or displeased with Ibsaa?

3) Will you say anything to Ibsaa if you met him? What will you say?

4) What should Barentu do?

a) find Ibsaa and get his money back
b) go to the police
c) stop buying from Ibsaa in the future
d) tell everyone that Ibsaa cheated so that everybody is aware
e) contact the leader of the group to which Ibsaa belongs

f) contact the leader of the group to which Barentu belongs
g) Other

5) What will happen to Ibsaa? Will Ibsaa be punished?

6) What happens if Ibsaa says he did not know that his cows were sick? After all, there is no proof of this. Will you agree with him?

7) If you go to a market, will you buy cows from Ibsaa? Why / Why not?

8) Do you think other people will buy cows from Ibsaa?

9) Will you buy other products from Ibsaa?

10) If Barentu told everyone that Ibsaa cheated, will Ibsaa take revenge?

2B: Market Exchange 2

One day an Oromo man called Ibsaa decided to sell his cows to earn money. He went to a market where he met Barentu, another Oromo man. Barentu wanted to buy Ibsaa's cows. Barentu bought the cows. Some months later, the cows died of sickness. This was a big loss to Barentu. Barentu thinks that Ibsaa cheated him by selling him sick cows.

1) Do you think Ibsaa cheated Barentu or was Barentu just unlucky?

2) Whom are you going to support? Why?

3) Do you think Ibsaa knew his cows were sick and did not tell this to Barentu? After all, it is difficult to prove that Ibsaa knew it.

4) Does Barentu have any option to get justice? What should Barentu do?

5) After this incident, will other people buy cows from Ibsaa?

6) Will people buy other products from Ibsaa?

7) Would it have been possible to avoid this situation? How?

8) How easy is it for people to sell sick cows in the market?

9) Do you have a mechanism to detect people who cheat in the market? How does it work?

10) Do you think that people who buy livestock mostly buy from the same seller/s? Why?

11) Does market exchange (buying-selling) teaches you not to cheat? How?

12) Do you implement the social norm of not to cheat in others areas of your life, outside the market exchange. For example, in dealing with people from your group or people

from other group? How?

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