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# **Market Exposure, Civic Values, and Rules**

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Devesh Rustagi

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Devesh Rustagi\*

## Abstract

Does exposure to markets foster or erode civic values and rules necessary to constrain opportunistic behavior for collective action? What mechanisms drive this effect? I investigate these questions using a natural experiment in Ethiopia, where markets evolved accidentally from military camps and their locations were determined by geographical suitability to defence rather than prospect for trade. Using market distance as a proxy for market exposure, I compare individuals who are from the same clan and attend the same market but vary in their distance to that market. I find a large negative effect of market distance on seven different experimental and survey measures of civic values and rules. Vignette studies reveal that these results arise because trade occurs primarily in livestock, which is prone to asymmetric information. In areas near markets, impersonal interactions create a demand for civic values and rules to foster exchange. In areas farther from markets repeated interactions foster exchange and there is no need for civic values and rules. Falsification tests show that when the scope of asymmetric information is limited, market exposure has no effect on civic values and rules.

**JEL:** C93, D8, N97, Z13

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

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\*Corresponding author: Devesh Rustagi, [devesh.rustagi@warwick.ac.uk](mailto:devesh.rustagi@warwick.ac.uk). Department of Economics, University of Warwick. I am especially thankful to Dave Donaldson, James Fenske, Bob Gibbons, Joe Henrich, Stelios Michalopoulos, Nathan Nunn, Awa Ambra Seck, Silvia Sonderegger, Hans Joachim Voth, and Noam Yuchtman for detailed feedback and discussions on the paper. I am also thankful to Klaus Desmet, Andrew Foster, Simon Gächter, Oded Galor, Ed Glaeser, Claudia Goldin, Saumitra Jha, Dilip Mukherjee, Elias Papaioannou, Matthias Schief, Uwe Sunde, David Weil, and Ekaterina Zhuravskaya for helpful discussions and comments. I am thankful to seminar participants at Boston University, Brown University, ERINN, Harvard University, London Business School, MIT Sloan, NBER Culture and Institutions, Northwestern, University of Chicago, University of Nottingham, University of Manchester, University of Strathclyde, and University of York. Alazar Ruffo and Nathan Waller provided excellent research assistance for this paper. The study was financially supported by the German Federal Ministry of Education and Research (BMBF) (BioTEAM 01LM0201), and the German Federal Ministry of Economic Cooperation and Development (BMZ) via the Centre for Development Research, Bonn. I also acknowledge kind support from the German Organization for International Cooperation (GIZ) and the Oromia State Forest Enterprises Agency. The author declares that he has no relevant or material financial interests that relate to the research described in the paper.

# I. Introduction

What happens to civic values and rules when societies transition from community-based exchange to that based on impersonal interactions in markets?<sup>1</sup> This question lies at the heart of a long-standing debate 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 corruption (Marx, 1872; Veblen, 1899). Shedding light on this debate, a seminal study by Henrich et al. (2010) found a positive association between calories purchased from markets and experimental measures of fairness.

In this paper, I contribute to this agenda in two crucial ways. First, I leverage a natural experiment on the emergence and location of markets to provide causal estimates of the effect of market exposure on civic values and rules. This step is crucial because transition to market exchange is not random. As Hirschman (1982) points out, it is poorer in places with “feudal shackles”, thereby raising concerns over pre-existing differences. Markets typically co-evolve with states and are usually located in urban areas, so their effect may be confounded with state capacity and access to urban amenities. There is rarely compelling evidence from exogenous variation in market exposure. Second, I provide evidence on mechanisms underlying the effect of market exposure on civic values and rules. Thus far, there is hard any evidence on mechanisms, which limits our understanding of the channels through which market exposure operates and the direction this effect could take.

The study takes place in rural markets of Ethiopia. I use variation in exposure to markets *within* the Arsi Oromo homeland, where a historical accident brought market exposure to some areas but not the others. Historical accounts of Amhara, British, French and Italian explorers as well as military generals suggest that markets in this region emerged accidentally and their locations were unrelated to potential for trade. Prior to 1900, there were no trading centers in the Arsi homeland. Markets emerged from military camps built by Emperor Menelik, following his victory over the Arsi. These camps brought hundreds of soldiers together with their families, creating opportunities for market exchange. When Menelik died a decade later, the camps lost their military purpose and transformed fully into market places. Menelik’s brief and indirect rule did not bring administrative reforms and state capacity. Historical accounts further reveal that Menelik chose camp locations based on geographical suitability to defense rather than economic considerations or proximity to Arsi settlements. This resulted in the placement of camps in areas without link to trade, slavery, and coffee cultivation. Thus, the primary

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<sup>1</sup>Civic value is the willingness of individuals to cooperate if others do the same even when defection would yield a higher payoff. 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 (Elster, 1989; Fehr and Schmidt, 1999; Sobel, 2005; DellaVigna, 2009). Rules are formal written down constraints (regulations, laws) that limit opportunistic behavior (North, 1991). See also reviews by Guiso, Sapienza and Zingales (2011) and Alesina and Giuliano (2015)

legacy of military camps appears to be their role in the coordination of market places.

I use walking distance between markets and Arsi settlement groups as a proxy for market exposure.<sup>2</sup> The groups farther from markets provide a valid counterfactual for groups near markets for three reasons. First, some groups happen to be closer to markets than others because of the natural experiment on the emergence and location of markets. To this end, I show that many group specific characteristics are balanced by market distance. Second, the absence of road and transport infrastructure development since Menelik's time has left the walking distances between markets and groups unchanged. Third, I use clan and market fixed effects to absorb time invariant unobserved differences across clans and markets. This renders a powerful identification strategy: I compare individuals who share kinship ties from belonging to the *same clan* and attend the *same market*, but vary in their distance to that market.

There could be concerns about sorting by market distance, so I take steps to assuage its scope. The Arsi settlements predate the markets and were determined by customs that prohibited territorial conquest, ensuring that settlement boundaries did not change after markets developed (Gnamo, 2014). Movement between groups was rare due to the belief that residing in one's ancestral group offers spiritual protection. Since the Arsi rely on forest gathering, they were reluctant to share these resources, which further discouraged migration (Gnamo, 2014). As such, migration towards groups near markets would have resulted in changes in population density, settlement patterns, and other group characteristics, but I find no evidence in support of this.

The markets in the study area resemble a fair, whereby thousands of individuals gather every week on a large ground. Trade occurs primarily in livestock, which is prone to asymmetric information, as it is difficult to verify livestock quality at the time of purchase. Weak state capacity means there are no impartial third parties to remedy this problem. In these situations, sellers have the option to *cheat* by selling poor quality livestock for the price of a good quality, or *reciprocate* by selling livestock quality commensurate with price. This offers an ideal setting to study whether market exposure shapes civic values and rules. Note that such markets are not unique to Ethiopia but are widespread throughout Africa and share characteristics with 21 sub-Saharan countries (Wijk et al., 2019). Livestock trade provides livelihood to over 30 percent of Africa's population and accounts for 35 percent of agricultural GDP.<sup>3</sup> 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.

I measure civic values and rules using three proxies. My primary measure is the

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<sup>2</sup>I verify that individuals from groups closer to markets visit markets more frequently, indicating greater market exposure.

<sup>3</sup>FAO 2020. <https://www.fao.org/faostat/en/#data/QV>

individual propensity for reciprocity or conditional cooperation. It is defined as the 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 using observational data is difficult because of confounding motives like reputation formation, repeated interaction, and beliefs about others' behavior (Fischbacher, Gächter and Fehr, 2001; Bénabou and Tirole, 2006; Nunn, 2009). I use a one-shot, anonymous public goods game, 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). I measure propensity to cooperate conditionally as the Spearman rank correlation between self and other players' contribution in the second decision (Fischbacher, Gächter and Fehr, 2001; Rustagi, 2024). Since there are very few altruists, higher Spearman  $\rho$  implies stronger conditional cooperation, whereas zero implies free riding.

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 and are considered crucial for natural forest regeneration (Amente, 2006). I combine household surveys with official records maintained by each group to collect data on the presence and quality of formal grazing rules.

I find that one standard deviation increase in market distance (1.1 hours) results in a decline in conditional cooperation by 0.18 points, time spent monitoring by 6 hours per month, and grazing ban by 0.8 months per year. These effects are economically large (20-50 % of the mean) and statistically significant at the 1 percent level. The results are robust to a wide variety of controls including latitude, altitude, group characteristics, forest condition, and distance to local administration, schools, and religious places. Alternative measures of civic values like an indicator for conditional cooperators, actual contribution to the public good, beliefs about other player's contribution, and likelihood of forming grazing rules yield similar results.

Why do these effects arise? One possibility is that when the scope of reputation is limited, asymmetric information may create a demand for civic values and rules as lubri-

cants to allow trading parties to benefit from gains of trade (Arrow, 1970; Bowles, 1998). I use vignette studies imitating local livestock exchange to show that in groups far from markets, individuals rarely visit markets. Instead, they trade livestock in personalized and repeated interactions, where reputation alone deters dishonest behavior and there is no need for civic values and rules. In contrast, in groups near markets, individuals trade livestock in impersonal market-based interactions, where high search costs limit the scope of repeated interaction. In these contexts, individuals rely on trust and reciprocity to foster exchange. To this end, I find that one standard deviation increase in market distance reduces trade in impersonal interactions and stated support for markets teaching civic values by 40 percentage points. Moreover, civic values are bolstered by sanctioning of the defector's clan, which is made possible by the Oromo custom of announcing one's clan when meeting other people (see Greif, 2006; Deb, 2020). Thus, in groups far from markets, cooperation relies on known identities and repeated interaction, limiting the development of civic values. In contrast, proximity to markets encourages reliance on trust, reciprocity, and clan-based sanctions, promoting civic norms that support impersonal exchange.

Another possibility is modernization and contact hypothesis: market distance affects prosperity and outside exposure, resulting in a broader societal change including civic values and rules (Inglehart and Baker, 2000; Allport, 1954). I find that prosperity and market visits decline with market distance, but there is no effect when I use naming patterns as a proxy for outside exposure (see Algan et al., 2022; Cantoni, Mohr and Weigand, 2024).

I construct two falsification tests to separate the role of asymmetric information and modernization hypotheses. I examine what happens to the effect of market distance on civic values and rules when the scope of asymmetric information is limited but modernization is allowed to play a role. First, I use distance to alternative markets that trade primarily in products whose quality is more easy to verify than livestock. As with market distance, prosperity and visits to alternative markets decline with alternative market distance. Yet, alternative market distance has no effect on civic values and rules. Second, I leverage the variation in items sold on two days the livestock markets are held. On the first day trade occurs mostly in livestock which is prone to asymmetric information, but on the second day trade occurs mostly in items whose quality is more readily verifiable. I find that while market visits on both days decline with market distance, civic values and rules are positively and significantly associated only with market visit on the first day but not the second day. These results suggest that asymmetric information is the likely mechanism underlying the effect of market exposure.

**Related Literature.** This paper contributes to the literature on the determinants of culture (see Guiso, Sapienza and Zingales, 2011; Alesina and Giuliano, 2015). Previous studies focus on the role of slavery in eroding trust (Nunn and Wantchekon, 2011), central institutions in crowding out honesty (Lowe et al., 2017), and self-governance in foster-

ing pro-social behavior (Guiso, Sapienza and Zingales, 2016) and norms of cooperation (Rustagi, 2024). However, these studies do not investigate the role of market exposure in shaping cultural traits.

Second, the paper relates to previous studies showing the long-run effect of trade on reducing religious riots in India (Jha, 2013) and in the spread of religion (Michalopoulos, Naghavi and Prarolo, 2018), as well as short-term effect of trade in Israeli and Palestinian financial assets on progressive political attitudes (Jha and Shayo, 2019) and trust (Jha, Shayo and Weiss, 2025). It also connects to studies linking occupational choice to aggregate measures of honesty in Greenland (Agneman and Chevrot-Bianco, 2022), market-related concepts and moral themes in a society's folklore (Enke, 2022), and stylized laboratory evidence on market exchange and indirect reciprocity (Dufwenberg et al., 2022). This paper goes beyond by integrating a natural experiment to study the effect of market exposure on civic values and rules with vignette studies and falsification tests to elucidate the underlying mechanisms. Moreover, the study uses a state of the art behavioral experiment to separate civic values from confounding motives and beliefs, a concern to which economists have paid little attention (Alesina and Giuliano, 2015; Rustagi, 2024).<sup>4</sup>

Third, the paper highlights the role of asymmetric information as a mechanism underlying the effect of market exposure. This lends empirical support to the hypothesis that market failure fosters civic values (Arrow, 1970; Bowles, 1998), thereby underscoring the importance of market characteristics.<sup>5</sup>

Fourth, the paper shows that societies develop different kinds of exchange structures and mechanisms to counteract the effect of asymmetric information. This offers empirical evidence in support of many influential studies insinuating the importance of economic organization for cultural change (Polanyi, 1944; Granovetter, 1985; North, 1991; Greif, 2006). By leveraging variation within the Arsi clans, the paper shows the importance of economic organization for cultural change, which goes beyond social organization (Moscona, Nunn and Robinson, 2017; Moscona and Seck, 2024).

Fifth, the findings from the paper suggest that market exchange is possible even when state capacity is weak, thereby suggesting the importance of self-governance (Ostrom, 1990; Fehr, Gächter and Kirchsteiger, 1997; Platteau, 2000; Greif, 2006; Dixit, 2009; Deb, 2020). It also 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

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<sup>4</sup>A parallel literature examines how markets affect morals towards third parties, such as child labor and animals (see Sandel, 2013). Falk and Szech (2013) use a laboratory experiment to show that markets erode moral values in these contexts, but Bartling, Fehr and Özdemir (2020) show that this was due to repeated play.

<sup>5</sup>Other studies have presented arguments for and against the role of market competition in shaping values (see Shleifer, 2004; Dewatripont and Tirole, 2024).

Robinson, 2019; Rustagi, 2022; 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.

## II. Historical Background

The study takes place in the homeland of the Arsi (Arusi) people, who form the largest branch of the Oromo ethnic group. I discuss below the events related to the emergence and location of markets in the Arsi homeland.

**Market Emergence.**— Until the 19th century, the Arsi homeland did not have trading centers or towns (Horvath, 1968). Balcha Safo, an Ethiopian military commander, noted that “the local people did not know how to build towns” (see Pankhurst, 1985). An Italian map from 1894 in Figure 1 confirms this.

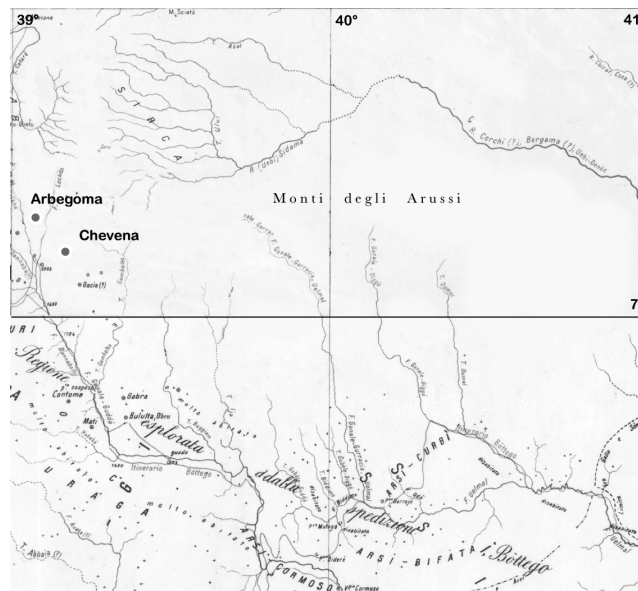


Figure 1: The Study Area in 1894

*Notes:* The map shows the Arsi Homeland “Monti degli Arussi” before the invasion by Menelik. Source: Corpo di stato maggiore, Ministero della guerra, Italy, 1894.

On the map, the study area “Monti degli Arussi” stands out as an empty space without any towns. This was not because the Italian cartographers overlooked small towns in the Arsi homeland; they included small towns like Arbegoma and Chevena,

located approximated 100 km away in the homeland of the *Sidamo* people. During this early period, the Arsi people traded livestock within their community (Gnamo, 1982).

In the late 1880s, Menelik, king of Shoa (1865-1889) and emperor of Ethiopia (1889-1913), started expanding his kingdom through military campaigns against different ethnic groups, including the Arsi Oromo. The spear-wielding Arsi were no match for Menelik's army with access to European firearms (Pankhurst, 1998; Perham, 1948). After losing a decisive battle fought 150 km north of the study area, the remaining Arsi surrendered unconditionally (see Gnamo, 2014, p146). To maintain control over the Arsi homeland, Menelik built military camps called *ketemas* which brought hundreds of soldiers and their families. This influx of people created opportunities for exchange, resulting gradually in the emergence of market places. Menelik suffered a stroke in 1909 and died in 1913. Following his death, the camps lost their military purpose and transformed into market places (Akalou, 1973). The Italian tourist guide to East Africa (1935-1941), published a few decades later, described these sites as thriving market centers rather than as military camps (Italiana et al., 1938, p464). It is likely that the military camps disappeared because they were made from perishable materials (see Figure A.1). The French explorer, Jules Borelli, described them as: "group of dwellings, usually surrounded by palisades" (p230) and "walls made of bamboo" (p286) (Borelli, 1890). The British military captain Maud (1904) also noted that the camps are "stockaded". Menelik's brief rule was fragile and administered the Arsi homeland indirectly through local clan leaders (see Gnamo, 2014, p162). His successor, deemed ineffective, was ousted just four later.

**Market Location.**— Menelik chose the location of his military camps strategically, taking into account geographical suitability to defense (Pankhurst, 1985). The historical records by British military captains corroborate this view. For example, Captain Wellby (1901, p157) remarked that the military 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." Captain Maud (1904, p558) wrote that the "Abyssinians [in Arsi area] are very fond of high ground".

Figure 2 presents a 3-D view of the geographical region in which the military camps (black circles) are located. The region covers an area of 5,000<sup>2</sup> and lies 300 km to the south of Addis Ababa, Menelik's capital. There are five camps in the region, of which three are in the study area (enclosed by a dashed line). All camps are situated on a 2,400 meter high plateau, bordered to the east by the steep gorge of the River Wabe Shebele, to the south by the 4,000 meter high Bale Mountains, to the north by equally high Mount Kaka and Mount Enkolo, and to the west by the Rift Valley. This setting clearly suggests that the camps were placed in an area with little prospect for trade, as they were not on established trade routes (see Zewde, 2002, p23) or part of the Red Sea slave trade

(see Shell, 2018, p66) or areas suitable for forest coffee cultivation.<sup>6</sup> The military camps were connected to Addis Ababa via other military camps over a narrow 3,200 meters high saddle between Mount Kaka and Mount Enkolo. Despite this, the route did not have any significant impact on economic or political integration in the past or today. Satellite night light image in Figure A.2 and data from Michalopoulos and Papaioannou (2013) confirm this, as less than 3 percent of the Arsi homeland is lit.

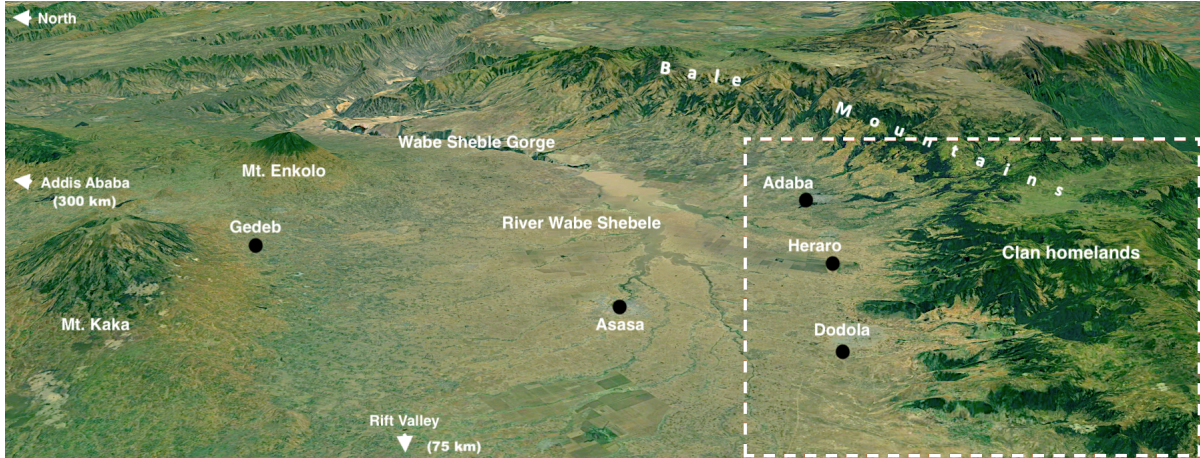


Figure 2: The Location of Camps in the Study Area

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

In summary, markets emerged accidentally from military camps without introducing new forms of state administration or leaving behind memory and trace of military origins. Moreover, the locations of these markets were determined by geographical suitability to defense and were unrelated to potential for trade. Thus, the primary legacy of military camp locations appears to be their role in the coordination of market places (see for example, Bleakley and Lin, 2012).

### III. Field Setting and Data

In this section, I first provide an overview of the field setting, followed by data on market distance, civic values, and rules.

#### III.A. Field Setting

The Arsi homeland is divided into two parts by the river Wabe Shebele. Those descending patrilineally from the ancestor named *Siko* live to the north of the river on a plateau,

<sup>6</sup>The region's altitude is unsuitable for forest coffee, which thrives at 1,500 meters. Forest coffee grows naturally to the south of the Bale mountains in Harena forest, but this lies on a steep escarpment with a fall of 1000 meters.

whereas those descending 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 Mando Arsi.

**Clans and Groups.**— The Arsi society is divided into clans (*gosa*) and people live in clan-based settlements whose boundaries can be traced back to historical times (Gnamo, 2014, p63). Since land suitable for building homesteads in the mountains is scarce, the clan-based settlements were fragmented into small hamlets. To reap benefits from economies of scale, adjacent hamlets from different clans coalesced to form a “group”. There are 56 groups in the study area. Figure 3 shows their location on a topographical map, where each white polygon is a group. The inset image to the left shows a group with three hamlets, one each from three different clans.

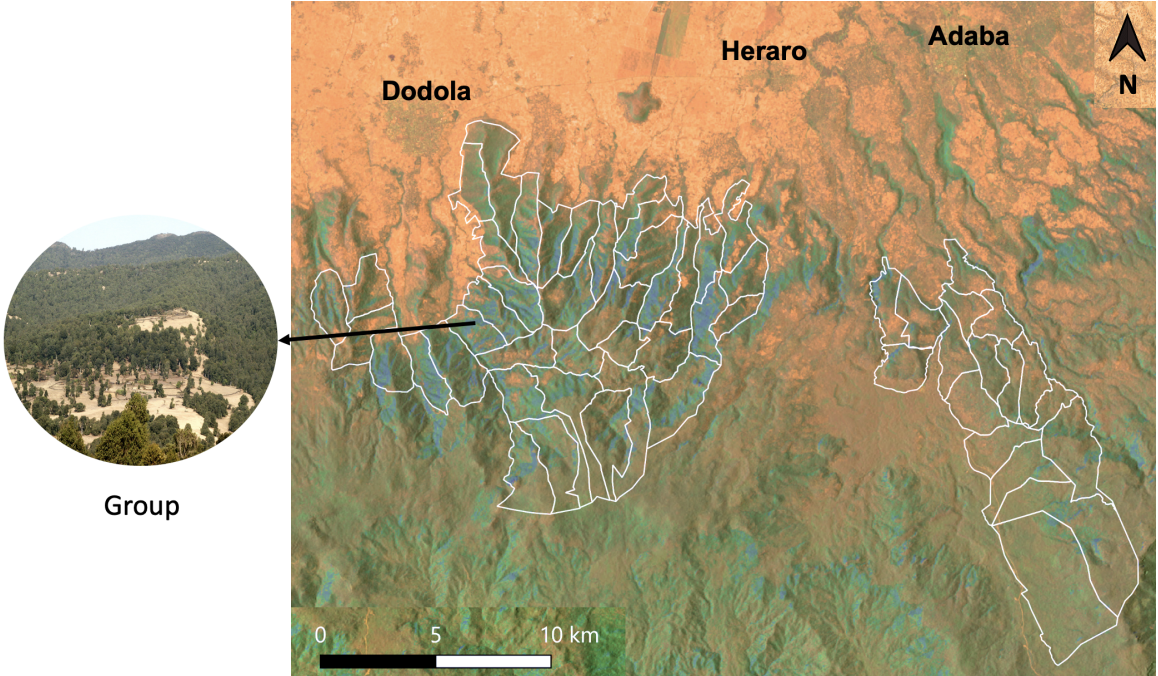


Figure 3: Location of Groups and Markets

*Notes:* The figure shows the location of 56 groups in the study area. Each group appears as a polygon whereby white lines depict group boundaries. The inset figure on the left illustrates the organization of a typical group. This group has three hamlets from three different clans. The hamlet on the top has households from the *Abiyu* clan, the hamlet on the left has households from the *Doyo* clan, and the hamlet on the right has households from the *Waji* clan. All groups are located in the Bale Mountains. There are three markets in the study areas: Dodola, Heraro, and Adaba.

The groups are homogeneous in terms of aspect, occupation, religion, organizational structure, and socio-economic development. All groups are located on the north facing slope of the Bale Mountains at an altitude which ranges from 2600m–3500m. The primary occupation is small-scale herding (buying and selling livestock), followed by forest gathering, and subsistence agriculture (growing barley). The main religion is Sunni Islam

mixed with Oromo beliefs. The groups have on average 26 households who are from different hamlets and clans. The average Herfindahl index of settlement fragmentation and clan fractionalization in a group are 0.49 and 0.41. This means that the likelihood two randomly selected households from a group will be from different hamlets and clans is 49 and 41 percent, respectively. The level of socio-economic development in the groups is poor: there are no roads, electricity, mobile towers, tap water, dispensary, irrigation, and veterinary care.

All groups are engaged in a forest commons management program that was launched by the Oromia government with support from the German Development Cooperation. Under the program, each group was given property rights to manage the forest within its boundary as a common property. The program allows households from each group to use the forest within its boundaries to graze livestock, harvest timber and non-timber forest products, and retain farms and homesteads. In return for these benefits, the groups are required to maintain their forest cover and prevent outsiders from using their forest. The organizational structure to administer forest management is also similar: each group elects a leader and a four-member executive committee.

**Livestock Markets.**— There are three periodic livestock markets in the study area: Dodola, Heraro, and Adaba (see Figure 3). The markets are on average 12 km apart. Each market resembles a livestock fair and is attended by thousands of individuals. The markets are held for limited duration (11:00 am - 3:00 pm) on two consecutive days in a week called ‘market days’ on a large ground(s), which can be over 12 times the size of a standard football (soccer) field. The first day of the market is reserved mainly for livestock trade, which includes cattle, horses, donkeys, goats, and sheep. The second day is reserved for trade in other goods like rubber boots, polyester textiles, candles, metal utensils, butter, honey, fuel wood, charcoal, and bamboo. The medium of exchange in the market is money (Ethiopian Birr).

The markets attract a large fluctuating population of buyers and sellers who are not personally known to each other. These could be Oromo people from different clans or non-Oromo people from different ethnic groups (Amhara, Gurage, Somali, Sidamo, and Tigray). For example, the market in Dodola attracts around 5,000 people. Individuals tend to attend only one market because the distance between markets is large and markets operate for limited duration. The sellers do not have a fixed location from which they operate; instead, locations are taken on the basis of arrival. There is no shade, food, and water for livestock in the markets.

Livestock exchange takes place against instant payment in cash after a lengthy negotiation process. There are no intermediaries, advance orders, or credit access. The size of exchange is typically small, 1-2 animals. People buy and sell livestock to maintain a continuous flow of cash for consumption smoothing, for the purposes of breeding, to

maintain a continuous supply of milk from cows, for herd management, and draft work in agriculture. Livestock, in particular cattle, also play an important role in rituals, and are bought and sold on occasions such as marriage to pay bride price and for slaughter on funerals and child naming ceremonies.

On average, households have 25 adult animals (half are cattle) of which they sell over 15 per year. Every visit to a market does not result in exchange, as some visits are for scouting, resulting in a transaction rate of 30-40 percent. These characteristics are not peculiar to the study area but are also observed in a dataset of 21 countries in sub-Saharan Africa collected by International Livestock Research Institute (ILRI) (Wijk et al., 2019). In this dataset, households own on average 24 animals (excluding chickens and rabbits) of which 11 are sold per year, and the transaction rate is also below 50 percent.

Livestock exchange is prone to asymmetric information, as it is difficult to verify the quality of livestock at the time of purchase (see for instance, Chymis et al., 2007; Hennessy and Wolf, 2018). Weak state capacity in Ethiopia means there are hardly any impartial third parties to remedy this problem. The nature of market interactions are such that the scope of repeated interactions is uncommon. I discuss these points in detail in Section VI, as these are crucial to understand the mechanisms underlying the effect of market exposure on civic values and rules.

### III.B. Data

The sample comprises household from 52 out of 56 groups in the study area. Three groups in which a pilot study was conducted were excluded and data for the fourth group were not available. I describe below the data on market distance, followed by civic values and rules.

**Market Distance.**— There are no paved roads or transport that connects people to markets. Instead, people walk along cattle trails to reach the nearest market. I use data from community surveys conducted with 5-10 households from each group to capture market exposure as market distance: *one-way* travel time in hours on foot from a group to the closest market. The groups vary in their distance to markets, which ranges from 1 – 5 hours; the mean is 2.85 hours (s.d. 1.11) and the median is 3 hours. Figure A.3 plots a histogram of market distance.

I verify that market distance indeed captures market exposure using data on frequency of market visits in a week collected using a community survey. Table A.1 shows a strong negative effect of market distance on market visits which is statistically significant at the 1-percent level. One standard deviation increase in market distance leads to a large drop in market visits by 0.28 trips per week. This result is of crucial importance and I will revert to it when discussing plausible channels underlying the effect of market distance.

**Civic Values and Rules.**— One of the most important civic value is reciprocity or conditional cooperation, which is defined as the 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). I complement this 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. I describe each of these measures in detail below.

*Propensity for Conditional Cooperation.*— Measuring conditional cooperation is challenging due to confounding motives operating at the same time. Many studies rely on pro-social behavior as a proxy for conditional cooperation. However, individuals may behave pro-socially not because of civic values but because of benefits from repeated interaction and reputation formation. In addition, individuals might have similar levels of conditional cooperation and yet display very different pro-social behavior because of differences in beliefs about other people’s behavior. For instance, conditional cooperators with optimistic beliefs will cooperate, but those with pessimistic beliefs will defect. These concerns imply that observed pro-social behavior might be a poor proxy for conditional cooperation. I overcome these concerns using a one-shot anonymous public goods game in the strategy method (Selten, 1967; Fischbacher, Gächter and Fehr, 2001; Fischbacher and Gächter, 2010; Rustagi, 2024). This allows me to rule out repeated interaction, reputation formation, and beliefs from playing a role (see Appendix B for instructions and procedures).

During the game, two players were randomly paired in a one-shot, anonymous interaction. The players did not know the exact identity of the other player: this could be an individual from a different clan, or a different hamlet in a group, or of different type (conditional cooperator or free rider). Henrich et al. (2010) argue that such designs are well suited to studying civic values that support cooperation in impersonal and ephemeral interactions because they limit the scope of expectations associated with repeated interaction and reputation formation, thereby “forcing players to default to local norms”.

Each player received an endowment of six bills of one Ethiopian Birr and had to decide on 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:

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

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 the players to contribute nothing. However, 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 decided simultaneously on their contribution to the public good and beliefs about other players' contribution played a role. In the conditional decision, I used the strategy method to shut down beliefs. The players decided on their contribution in response to all seven possible contribution decisions of the other player. To ensure incentive compatibility, both decisions were made payoff relevant. A lottery was used to determine for which player the unconditional decision is taken; this was matched with the other players' corresponding conditional decision to calculate payoffs.

I took many steps to ensure that individuals understood the game since this was the first time that they took part in an experiment (see Appendix B). On average, each player earned 7.5 Birr, which was slightly over one day's wage in Dodola, the largest town. Altogether, 720 individuals from different households (on average 14 households per group) took part in the experiment, implying a response rate of over 50 percent of the households in the study area. Given the importance of clan in the organization of the Arsi society, I test for sample representation using clan frequencies. The top panel of Figure A.4 in Appendix A shows that the proportion of ten major clans in the sample is the same as in the population ( $p$ -value = 0.45).

The conditional decision offers a revealed measure of conditional cooperation: (i) *free riders* are expected to contribute zero regardless of the other players' contribution; (ii) *altruists* are expected to 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 measure propensity for conditional cooperation as the Spearman rank correlation between self-and other players' contribution in the conditional decision. The higher the Spearman  $\rho$  the higher is the propensity to cooperate conditionally, whereas zero implies a tendency for free riding.<sup>7</sup> The average propensity for conditional cooperation in the sample is 0.499 (s.d. 0.518).

While conducting robustness checks, I use three other measures: (a) An indicator for conditional cooperator – individuals for whom the Spearman  $\rho$  is positive and statistically significant at  $p$ -value < 0.05 (mean 0.47, s.d. 0.499); (b) Contribution in the unconditional decision (mean 2.05, s.d. 1.51); and (c) Beliefs about other players' contribution in the unconditional decision (mean 2.67, s.d. 1.71).

*Time Spent Monitoring.* – Maintaining forest commons requires groups to actively monitor the use of their forest. There is no tax-funded third party like the police, so individuals take turns to patrol the forest in teams of 2-3 persons from each household. This kind

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<sup>7</sup>The Spearman  $\rho$  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.

of decentralized monitoring is prone to a second-order free rider problem: patrolling is individually costly but benefits the group. Individuals are better off if others spend time monitoring the forest while they can do something else in the meantime. Moreover, there is no monitoring of monitors and so on so forth. Therefore, time spent monitoring the group-managed forest can be considered as a proxy for civic values. In a household survey conducted privately with 508 households, I collected data on the time individuals spent monitoring their forest in a month. The average turns out to be 27.80 hours (s.d. 15.48) per month. As before, I test for sample representation in the middle panel of Figure A.4, which shows that the frequency of different clans in the sample is similar to that in the population ( $p$ -value = 0.38).

*Grazing Rules.*— Rules regulating grazing 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 grazing and 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. 509 households took part in this survey, of which 44 percent reported having a rule regulating grazing for 1.47 months in a year (s.d. 1.761). Data from books maintained by the groups yields similar results. As before, this sample is also representative, as the proportion of different clans in the sample is the same as in the population ( $p$ -value = 0.44) (see bottom panel of Figure A.4).

## IV. Empirical Specification

I study the effect of market exposure on civic values and rules using the following 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 ban) reported by individual  $i$  from the group  $g$ , clan  $c$ , and attending market  $m$ .  $MD$  is one-way market distance measured in hours.  $\mathbf{X}$  and  $\mathbf{H}$  are vectors of household and group specific characteristics that are expected to affect civic values and rules. Following previous studies on the determinants of civic values and commons management, I control for latitude, altitude, group size, group fragmentation, female share, Gini of cattle ownership, Gini of land ownership, and education (see Ostrom, 1990; Balland and Platteau, 1996; Bardhan, 2000; Kosfeld and Rustagi, 2015). Panel B of Table 1 provides the definition and summary statistics of these variables.

Table 1: Balance Check

	Sample size	Mean (s.d.)	Coefficient on		Difference (3) - (4)
			Market Distance	Alternative Market Distance	
	(1)	(2)	(3)	(4)	(5)
Panel A: Civic Values and Rules					
Conditional Cooperation ( <i>rho</i> )	720	0.499 (0.518)	-0.159 (0.037)	0.001 (0.041)	-0.158 (0.052)
Monitoring (hours)	508	27.804 (15.480)	-5.147 (1.769)	1.809 (1.267)	-6.957 (2.339)
Grazing Rules (months)	511	1.472 (1.761)	-0.721 (0.210)	0.429 (0.216)	-1.150 (0.293)
Conditional Cooperator (binary)	720	0.468 (0.499)	-0.111 (0.036)	0.025 (0.035)	-0.135 (0.048)
Unconditional Contribution (Birr)	704	2.054 (1.511)	-0.527 (0.217)	-0.050 (0.102)	-0.477 (0.225)
Beliefs (Birr)	704	2.675 (1.710)	-0.594 (0.171)	0.175 (0.116)	-0.769 (0.227)
Grazing Rules (binary)	511	0.446 (0.498)	-0.174 (0.056)	0.058 (0.062)	-0.232 (0.078)
Panel B: Main Covariates					
Altitude	52	0.231 (0.425)	-0.078 (0.061)	-0.062 (0.065)	-0.015 (0.068)
Group Size	52	26.308 (4.625)	0.327 (0.986)	0.708 (0.938)	-0.382 (0.966)
Group Fragmentation	52	0.489 (0.246)	0.029 (0.048)	-0.023 (0.049)	0.052 (0.056)
Female Share	52	0.202 (0.113)	0.002 (0.028)	0.007 (0.019)	-0.006 (0.024)
Gini - Cattle Ownership	52	0.338 (0.077)	-0.001 (0.018)	-0.015 (0.021)	0.014 (0.023)
Gini - Land Ownership	52	0.294 (0.076)	0.003 (0.015)	-0.016 (0.018)	0.019 (0.020)
Education	720	3.369 (3.063)	0.073 (0.185)	0.114 (0.178)	-0.040 (0.259)

*Notes:* Market distance and alternative market distance are one-way walking distance (hours on foot) to livestock market and alternative markets, respectively. 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 the number of months grazing is forbidden inside the forest in a year. Conditional cooperator is an indicator which equals 1 if an individual behaves as a conditional cooperator in the conditional decision of the public goods game, otherwise 0. Unconditional contribution is contribution in the simultaneous decision of the public goods game. Belief is about other player's unconditional contribution in the public goods game. High altitude is an indicator for groups over 3000 meters. 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 be from different hamlets. Female share is the share of female headed households in a group. Gini of cattle and land are the Gini indices. Education is years of schooling. Column 3 reports the coefficient from a regression of each variable separately on market distance after controlling for clan and market fixed effects, latitude, and altitude (except when altitude is the dependent variable). Column 4 does the same using alternative market distance. Column 5 reports the difference between the two coefficients. Obs. stands for the number of observations.

$\theta_c$  is a fixed effect for the clan. I consider indicators for seven clans that account for over 70 percent of the households in the study area.  $\eta_m$  is a fixed effect for the market

that the individual attends. I cluster standard errors at the group level. The results hold when I use alternative form of clustering when conducting robustness checks. The coefficient of interest is  $\beta$ , which captures the effect of market distance on civic values and rules. Market distance is plausibly exogenous because of the natural experiment outlined in Section II. I discuss my strategy to mitigate two potential concerns below.

#### IV.A. Market and Clan Fixed Effects

There could be a concern that market distance is incidentally correlated with pre-existing differences in culture and institutions. To mitigate such concerns, I control for market and clan fixed effects. This allows me to absorb time-invariant unobserved differences in market locations and clan characteristics. Within the clan, individuals are expected to have similar culture from common patrilineal descent and similar exposure to common religious (*Qallu*) and political (*Gada*) institutions (Hassen, 1994). Crucially, this strategy allows me to compare individuals who attend the same market and belong to the same clan but vary in their distance to that market because of the natural experiment.

#### IV.B. Sorting by Market Distance

The concern over sorting by market distance could arise in two ways: shifting of clan boundaries and movement of people across groups. I use a combination of historical narratives and data to argue that while it may not be possible to fully rule out this concern, its scope was likely limited.

**Shifting of Clan Boundaries.**— The Mando Arsi established settlements based on clan affiliations before the markets emerged. The locations of clan homelands were determined through a custom called *Baala Buusa* (leaf laying ceremony) through which historical clan leaders (*Abba baala*) declared communal property rights to land on a first-come-first-served basis by placing a leaf. This custom forbade clans from conquering the territories of other clans, ensuring 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 ensured stability of these rights through bonding over regional identity (*Arsooma*), friendships (*firooma*), marriage ties (*soddomaa*), and matrimonial alliances (*dhalooma*). As a result, clan boundaries have remained stable and were unaffected by Menelik’s conquest (Gnamo, 2014, p63).

**Movement of People Between Groups.**— Migration between groups, especially within the clan homeland is a possibility. I argue that the scope of such movements was likely limited for two reasons. First, the Arsi prefer to reside on their ancestral land where their

forefathers lived, died, and were buried. 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 is due to the belief that ancestral spirits (*Ayyana Abba*) influences their lives. For this reason, the Arsi regularly visit the tombs of their ancestors (Gnamo, 2014, p64). Second, the Arsi rely on forest in the vicinity of their homesteads for livestock grazing, timber, and non-timber forest products. So, they were reluctant to share the forest with potential migrants, as this would have put more pressure on their existing resource.

I also conducted a survey with the elderly in each group to gauge the extent of migration in and out of each group in the last six decades. I find that aside from women relocating due to marriage, the movement of households from one group to the other is indeed rare. While household heads did move, the fraction was small (only 1.37 percent of the household heads) and they moved to large cities like Shashemene and Addis Ababa. Figure A.7 shows that there no association between the share of household heads from a group who migrated and market distance ( $r = -0.045$ ,  $p$ -value = 0.75).

#### IV.C. Balance Check

I carry out a balance check in which I regress one covariate at a time on market distance after controlling for market and clan fixed effects, as well as geography (latitude and altitude). This also allows me to study whether groups near and far from markets are comparable in their characteristics, especially those that could have been influenced by sorting. If there was a large scale sorting then groups *near* markets are likely to have higher population density. If the migrants did not settle in the existing hamlets but formed new hamlets then groups near markets are expected to have higher settlement fragmentation. If the new migrants deforested land to build homesteads then groups near markets are also likely to have lower forest cover. Column 3 in Panel B of Table 1 reports the results using main covariates. The coefficient on market distance is very small in magnitude and is always statistically insignificant. Table A.2 shows that similar results are obtained when I consider other covariates, such as measures related to population and past forest cover.

#### IV.D. Descriptive Results

Column 3 in Panel A of Table 1 reports results from a regression of different proxies of civic values and rules on market distance, after controlling for clan and market fixed effects, as well as geography (latitude and altitude). I find that there is strong negative association between market distance and the different proxies of civic values and rules, which is always statistically significant at the 1-percent level. Note that these results stand in contrast to those reported in Panel B, which show that there is no association

between market distance and covariates. The bin-scatter plots in Figure 4 visualize this negative association using the three main measures of civic values and rules: propensity for conditional cooperation, time spent monitoring, and grazing rule. Figure A.5 shows similar results using the remaining four measures: indicator for conditional cooperation, unconditional contribution, beliefs about other players contribution, and an indicator for grazing rules. Figure A.6 shows this negative association using a 3-D plot in which I superimpose a scatter plot between market distance and civic values and rules on a map. These results clearly suggest that civic values and rules decline with market distance.

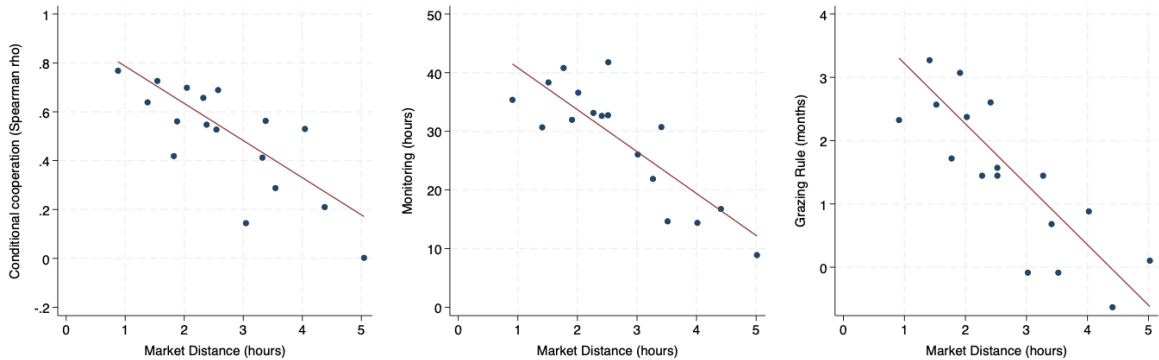


Figure 4: Civic Values and Rules by Market Distance

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

## V. Results

### V.A. Main Results

Table 2 presents results from the estimation of Equation 2 using OLS. The dependent variable is conditional cooperation in Panel A, time spent monitoring in panel B, and number of months grazing is forbidden in Panel C. Column 1 is without any control variables and shows that market distance has a large negative coefficient, which is statistically significant at the 1 percent level in all the panels. It explains large variation in civic values and rules, which ranges from 12 percent in Panel A and 39 percent in Panel B to 49 percent in Panel C. When I introduce clan and market fixed effects in column 2, the coefficient on market distance remains stable in magnitude in Panel A, but declines in magnitude by 10-20 percent in Panels B-C. Nevertheless, it remains statistically significant throughout. The fixed effects are jointly statistically significant and their introduction leads to rise in the  $R$ -squared by 2-8 percent. In column 3, when I introduce the remaining control variables, the magnitude of the coefficient on market distance increases slightly in Panel A, but declines in Panels B-C. Nonetheless, it remains sizable in magnitude and retains statistical significance at the 1 percent level across all panels. The controls variables are

jointly statistically significant in all panels and their inclusion leads to a jump in the  $R$ -squared by 2-7 percent.<sup>8</sup> According to the estimates in column 3, one standard deviation increase in market distance (1.11 hours) leads to a decline in conditional cooperation by 0.18 points, time spent monitoring by 6 hours per month, and grazing ban by 0.8 months per year. These effects are large in relation to the mean of the dependent variables. A randomization inference test with 5000 repetitions confirms these results.

Table 2: Market Distance, Civic Values, and Rules

	No Controls (1)	Fixed Effects (2)	All Controls (3)
Panel A: Conditional Cooperation ( <i>rho</i> )			
Market Distance	-0.158 (0.022)	-0.156 (0.026)	-0.167 (0.037)
$R^2$	0.12	0.14	0.16
Observations	720	720	720
Panel B: Time Spent Monitoring (hours)			
Market Distance	-8.527 (1.091)	-7.329 (1.029)	-5.568 (1.656)
$R^2$	0.39	0.47	0.54
Observations	508	508	508
Panel C: Grazing Rules (months)			
Market Distance	-1.098 (0.120)	-0.889 (0.127)	-0.716 (0.198)
$R^2$	0.49	0.57	0.61
Observations	509	509	509
Fixed Effects	No	Yes	Yes
Control Variables	No	No	Yes

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

## V.B. Robustness Checks

**Alternative forms of clustering.**— I show in Table A.5 that the main results are robust to clustering of standard errors at different levels, including group and market, group and clan, group and local administration, and accounting for spatial correlation of errors.

**Other measures of civic values and rules.**— Table 3 shows that the results hold when I consider an indicator for conditional cooperator (column 1), unconditional contribution (column 2), beliefs about other players' contribution (column 3), and an indicator for grazing rule (column 4). According to the estimates, one standard deviation increase

<sup>8</sup>Table A.4 reports the coefficients on covariates.

in market distance leads to a decline in the share of conditional cooperators by close to 13 percentage points, unconditional contribution by 0.6 Birr, beliefs about other players' contribution by 0.65 Birr, and the likelihood of having a grazing rule by 19 percentage points. These magnitudes are large in relation to the mean of the dependent variables and are also statistically significant at the 1-percent level.

Table 3: Market Distance and Other Measures of Civic Values and Rules

	Dependent Variable is:			
	Conditional cooperator (indicator) (1)	Unconditional contribution (Birr) (2)	Beliefs - others' contribution (Birr) (3)	Grazing ban (indicator) (4)
Market Distance	-0.114 (0.037)	-0.537 (0.205)	-0.588 (0.161)	-0.172 (0.057)
$R^2$	0.11	0.18	0.11	0.65
Mean	0.468	2.054	2.675	0.446
Observations	720	704	704	511
Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

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

**Other measures of market distance.**— The effect of market distance may not be linear. I consider in Panel A of Table A.6 an indicator for above the median market distance. It enters with a large negative and statistically significant coefficient. Although the histogram in Figure A.3 shows that market distance is not prone to outliers, I show in Panel B that the results also hold when I use log of market distance.

**Additional control variables.**— In Table A.7, I introduce two sets of additional controls. In column 1, I include population density and past forest cove, which are likely to be affected by sorting. In column 2, I include the duration a group has been under the commons management program and the number of bordering groups, as this could affect time spent monitoring and grazing rules. Table A.3 reports the definition and summary statistics on these variables. The additional controls are jointly statistically significant, but their inclusion 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.

**Distance from administration, school, and mosque.**— Market distance might be confounded with access to administration, schooling, and religious places. The field setting allows me to disentangle these effects. The administrative offices relevant to households are not located in market towns. In addition, individuals rarely have more than 3 years

of education, so access to primary school is of utmost importance. Individuals send their children to primary schools situated near groups and not in market towns. Similarly, individuals attend local mosques near their homes rather than those located near market places. I perform a robustness check by including distance to local administration offices, local primary schools, and local mosques as additional control variables. Table A.3 reports the definition and summary statistics on these variables. Column 1 of Table A.8 shows that these variables have a jointly statistically significant effect in all panels ( $p$ -value  $< 0.01$ ). Despite this, the coefficient on market distance retains its magnitude and significance. In column 2, I directly control for the trust in local government, acknowledging that it is endogenous. This does not lead to any major changes in the coefficient on market distance, which remains statistically significant at the 1-percent level.

## VI. Plausible Channels

What explains the observed decline in civic values and rules with market distance? I examine the scope of two plausible channels: Asymmetric Information Hypothesis and Modernization Hypothesis. Subsequently, I construct falsification tests to separate the effect of these two channels. Finally, I assess indirectly the role of cultural transmission.

### VI.A. Asymmetric Information Hypothesis

Livestock exchange is prone to asymmetric information as it is difficult to verify quality at the time of exchange. Weak state capacity in Ethiopia means there are no impartial third parties to remedy this situation. This locks buyers and sellers in a cooperation problem. Depending on market distance, societies develop different kinds of solutions to counteract the effects of asymmetric information, which shapes civic values and rules. Table 4 summarizes this hypothesis.

- In areas far from markets, people seldom visit markets to trade livestock because of high opportunity costs (see Table A.1). It is plausible that in these areas people retain the community exchange system, where trade in livestock occurs in personalized and repeated interactions. In these situations, individual reputation is enough to counteract the effect of asymmetric information and there is no need for civic values and rules. This could explain why civic values and rules are weaker in areas far from markets.
- In areas near markets, people visit markets to trade livestock because the opportunity cost of visiting markets is low (see Table A.1). It is plausible that in market-based interactions people trade impersonally, so asymmetric information creates a demand for civic values as lubricants to allow for trading parties to benefit from

gains of trade. 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.” This could explain why civic values and rules are stronger in areas near markets.

Table 4: Asymmetric Information Hypothesis

	Distance from Markets		Implication
	Near	Far	As market distance increases
Opportunity Cost	Low	High	Market visits ↓
Exchange Type	Impersonal	Repeated	Impersonal exchange ↓
Mechanism	Civic Values	Reputation	Demand for civic values ↓

*Notes:* The table summarizes the the three key elements of the Asymmetric Information Hypothesis. The opportunity cost refers to that of visiting markets. Exchange type refers to whether livestock is traded in impersonal or repeated interactions. Mechanism refers to the kind of solution societies use to counteract the effect of asymmetric information.

Thus, for asymmetric information hypothesis to serve as a plausible channel, I expect the scope of impersonal exchange and demand for civic values to decline with market distance. I provide this evidence using rich data collected via vignette studies. In the vignettes, I presented two different scenarios imitating local livestock exchange to 8-10 individuals in each group, followed by open-ended questions. The vignettes were very popular and led to stimulating discussions that lasted up to 2 hours per group. I list the vignettes below together with key questions that were used to guide the discussions.

### Vignette Study A

*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 bought Ibsaa’s 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? 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.
2. Do you think that people who buy livestock mostly buy from the same seller/s? Why (not)?
3. What should Barentu do? Will other people buy cows from Ibsaa?
4. How easy is it for people to sell sick cows? Do you have a mechanism to detect people who cheat? How does it work?

5. Does market exchange teach you not to cheat? Do you implement the social norm of 'do not cheat' in other areas of your life in dealing with people from your own group or people from other groups? How?

### Vignette Study B

*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. What happens if Ibsaa says he did not know that his cows were sick? Will you agree with him? Why / Why not?
2. In your opinion, what Ibsaa did to Barentu was right or wrong? Are you pleased or displeased with Ibsaa? Will you say anything to Ibsaa if you met him? What will you say?
3. 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 or the leader of the group to which Barentu belongs
4. Will you buy cows from Ibsaa? Why / Why not? Do you think other people will buy cows from Ibsaa? Will you buy other products from Ibsaa? If Barentu told everyone that Ibsaa cheated, will Ibsaa take revenge?

**Asymmetric Information and Weak State Capacity.**— I begin by confirming that livestock exchange is indeed prone to asymmetric information. In response to Question 1 in both Vignette A and B, the respondents noted that sellers have more information about livestock quality than buyers. While buyers can use some cues to assess quality, these are imperfect. For instance, sellers know their cow's milk yield or symptoms of diseases that are not easily detectable at the time of purchase and may become apparent only later. This problem is further aggravated by the absence of veterinarian checks before purchase. Furthermore, livestock exchange is based on informal agreements that cannot be verified by impartial third parties. Responses to Question 2a of Vignette B reveal that state capacity is indeed weak, as people do not consider going to the police. As such, the small size of the transactions makes formal action inefficient. Informal enforcement agencies, such as mafias or private armies, are also absent.

**Scope of Impersonal Exchange.**— Next, I show that the exchange pattern changes from impersonal interactions to personalized community exchange as we move away from markets. In response to Question 2 in Vignette A and B, people who live *far from* markets reported exchanging livestock repeatedly with known individuals. In these cases, people mentioned that they interact in domains beyond livestock exchange, such as attending marriages, funerals, and naming ceremonies of children.

In contrast, people who live *near* markets reported exchanging livestock mostly in markets, which takes the form of impersonal interactions. In these cases, traders have little information on past transaction history at the time of exchange and there is no expectation that the exchange will be repeated. Some respondents even laughed when they heard Question 2 in Vignette B “will you say anything to Ibsaa if you met him” and Question 3a “find Ibsaa and get his money back”. Respondents noted that this was because of high search costs. It is difficult and time consuming to find the same buyer or seller among thousands of traders and markets operate for limited duration. It is challenging to coordinate in advance market visits on the same day, or roles as buyer or seller, or livestock type to be exchanged. As an example, the respondents noted that a buyer may not attend the market the same day as the seller, or may struggle to find the same seller in crowded markets, or wants to buy cattle but the seller has only donkeys for sale, or the seller is also intending to buy livestock that day. There is no electricity and cell phone reception towers to facilitate coordination along these lines.

These responses suggest that individuals near markets are much more likely to trade livestock in impersonal exchange. I test this using an indicator variable which equals 1 if people reported trading livestock mostly in impersonal interactions, otherwise 0. Table 5 shows that without or with controls the coefficient on market distance is negative and statistically significant at the 1-percent level. According to the estimate with the full set of controls (column 3), one standard deviation increase in market distance is associated with a fall in the likelihood of impersonal exchange by close to 40 percentage points.

Why do we observe this kind of exchange pattern? The answer lies in the opportunity cost of visiting markets. People who live far away have a high opportunity cost, as they need an entire day to visit a market. However, markets offer no shade or food or water for livestock. Moreover, markets operate for limited duration (3-4 hours) and the likelihood of a successful transaction is 30-40 percent. This puts immense pressure on people traveling from distant groups to sell livestock in the limited time they have. Failure to sell livestock means sunk costs in the form of time and effort spent visiting the market, as well as walking back through the forest in the dark with unsold livestock. In view of these handicaps, people worry that potential buyers might sense this weaknesses, take advantage of it to bargain hard, and make them take an unfavorable decisions of selling livestock in the market at a price which is lower than what they could have fetched in their neighborhood. For these reasons, people from groups far from markets prefer to

sell livestock in their neighborhood than in markets.

Table 5: Market Distance and Impersonal Exchange

	Dependent Variable: Impersonal Exchange ( 1 = Yes)		
	No Controls	Fixed Effects	Control Variables
	(1)	(2)	(3)
Market Distance	-0.357 (0.031)	-0.420 (0.042)	-0.359 (0.053)
$R^2$	0.65	0.75	0.80
Observations	52	52	52
Fixed Effects	No	Yes	Yes
Control Variables	No	No	Yes

*Notes:* OLS estimates with robust standard errors in parentheses. Column 1 is without any controls, column 2 includes fixed effects for clan and market, and column 3 additionally includes control variables. These include latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. Data are at the group level.

**Civic Values.**— Next, I show that these differences in exchange patterns have implications for the mechanisms people use to counteract the effects of asymmetric information. Specifically, I find that in groups near markets people rely on civic values to foster exchange, but those far from markets rely on individual reputation.

*Civic Values.*— I investigate the scope of civic values using response to Question 5 in Vignette A, where I asked: “Does market exchange teach you not to cheat?...How?”. Note that ‘not to cheat’ is a form of civic value which in my context is the same as conditional cooperation. This is because cheating implies selling a sick animal to the buyer for the price of a good quality animal. In contrast, not to cheat implies reciprocating, that is, selling a healthy animal for the price of a good quality animal. The response to Question 5 varied widely by market distance. Individuals from groups near markets emphasized the importance of *amanuu* (trust). They noted that if buyers *trust* that sellers will offer a healthy animal for the price of good quality then for market exchange to be successful the sellers must resist the temptation to cheat and instead *reciprocate* by selling animal quality commensurate with price. Overall, there was a broad consensus among people from groups near markets on the importance of trust and reciprocity in fostering livestock exchange in markets. However, in groups further away from markets the reactions of the respondents were muted and most stated that they rarely sold livestock in markets.

Based on these responses, I construct an indicator variable that equals 1 if people responded ‘yes’ to Question 5, otherwise 0. Table 6 reports the results. The coefficient on market distance is always negative and statistically significant at the 1-percent level.

The estimate with the full set of controls in column 3 implies that one standard deviation increase in market distance leads to a drop in the stated role of markets teaching civic values by 40 percentage points. This result mirrors those obtained from the public goods game, where both beliefs about player’s contribution (trust) and conditional cooperation (reciprocity) decline with market distance.

Table 6: Market Distance and Learning Civic Values

	Dependent Variable:		
	Markets Teach Civic Values (1 = Yes)		
	No Controls	Fixed Effects	Control Variables
	(1)	(2)	(3)
Market Distance	-0.368	-0.393	-0.361
	(0.028)	(0.040)	(0.043)
$R^2$	0.67	0.77	0.81
Observations	52	52	52
Fixed Effects	No	Yes	Yes
Control Variables	No	No	Yes

*Notes:* OLS estimates with robust standard errors in parentheses. Column 1 is without any controls, column 2 includes fixed effects for clan and market, and column 3 additionally includes control variables. These include latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. Data are at the group level.

*Sanctioning at Community Level.*— Responses to Questions 3–4 in Vignette A and Questions 2-4 in Vignette B further revealed that people use different kinds of sanctioning mechanisms to discipline defectors. In groups far from markets, people reported that if a seller cheats by selling a poor quality cow for the price of a good quality and the cow becomes sick or dies soon after purchase, this news spreads within the community. In response to Question 3 of Vignette A and especially Question 3d of Vignette B, people reported that Ibsaa should tell everyone of the incident so that people become aware. Responses to Question 2-4 of Vignette B further revealed that the dishonest seller acquires a bad reputation and faces punishment not only in the form of exclusion from future livestock exchange, but also from social occasions including marriages and funerals. Thus, in groups far from markets, people rely on individual reputation to deter dishonest behavior and foster exchange.

In contrast, in groups near markets, people reported that they rarely have information on the exact identity of sellers to target punishment at the individual level. Instead, they use a sanctioning mechanism to target the clan (if Oromo) or the ethnic group (if non-Oromo) of the individual who cheats by excluding them from future exchange. As an example, in response to Question 3 of Vignette A and 3d of Vignette B, people reported that Ibsaa should tell everyone of the incident and the clan of the seller. I was fascinated

to observe that in the study area, a unique custom makes it straightforward to identify a person’s clan. The Arsi Oromo always announce their clan while meeting other Oromo people. Gnamo (2014, p63) writes “...when two [Arsi] 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 [Clan].” During my eight-month stay in the study area, I noted that when the respondents (Arsi) met my local assistants (also Arsi) responsible for logistics, they always asked “gosti meka” or “what is your clan?”. If they happened to be from the same clan, then common kinship was celebrated in the form of hugging/kissing on the cheek (same gender) or by stretching one’s hand forward to kiss the wrist (across gender).<sup>9</sup> For traders from different ethnic groups, the Arsi rely on ethnic markers like appearance, language, and accents. These results provide first empirical evidence in support of theoretical insights by Deb (2020), who shows that announcing one’s name before transactions can help in fostering exchange through sanctioning at the community level. Since the responses on the importance of sanctioning at the community level align with those on civic values in Table 6, I do not report these separately.

Importantly, I also found that people are wary of moral hazard problems. In response to Question 4 of Vignette A they stated that people intervene only when livestock becomes sick or dies shortly after exchange, but not otherwise. Together, these results provide evidence on the scope of asymmetric information hypothesis as a plausible channel underlying the negative effect of market distance on civic values and rules.

## VI.B. Modernization Hypothesis

Another possibility is that market distance affects prosperity and exposure to outsiders, resulting in a broader societal change that includes civic values and rules (Inglehart and Baker, 2000; Allport, 1954). Therefore, a decline in prosperity and outside exposure by market distance could explain the negative effect of market distance on civic values and rules. We already saw in Table A.1 that market visits decline by market distance. I test the scope of prosperity and other proxies of outside exposure below.

**Prosperity.**— Measuring prosperity in resource-poor environments is difficult. Instead of relying on several proxies whose choice may be arbitrary, I extract the first principal component of several proxies as a summary measure of prosperity (PC\_Prosperty). These include self-reported financial rating, number of livestock units, and land holding. Self-reported financial rating was elicited through a survey in which individuals were asked to rate themselves as being poor or average or rich. Livestock units use weights based on market price: horse (1), cattle (0.8), donkey (0.7), sheep (0.2), and goat (0.2). Land

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<sup>9</sup>Individuals may have an incentive to lie about their clan, especially if they want to cheat, but this is clearly not happening. If lying were common, the custom of announcing one’s clan would lose value and likely cease, which is clearly not the case.

holding is based on units of land.<sup>10</sup> The left-hand side of Figure 5 presents the results from a regression of PC\_Proprosperity on market distance. Regardless of the specification I choose, the coefficient on market distance is negative and robust in magnitude, but it loses statistical significance once control variables are included. These results suggest that prosperity declines with market distance, although it is noisily estimated.

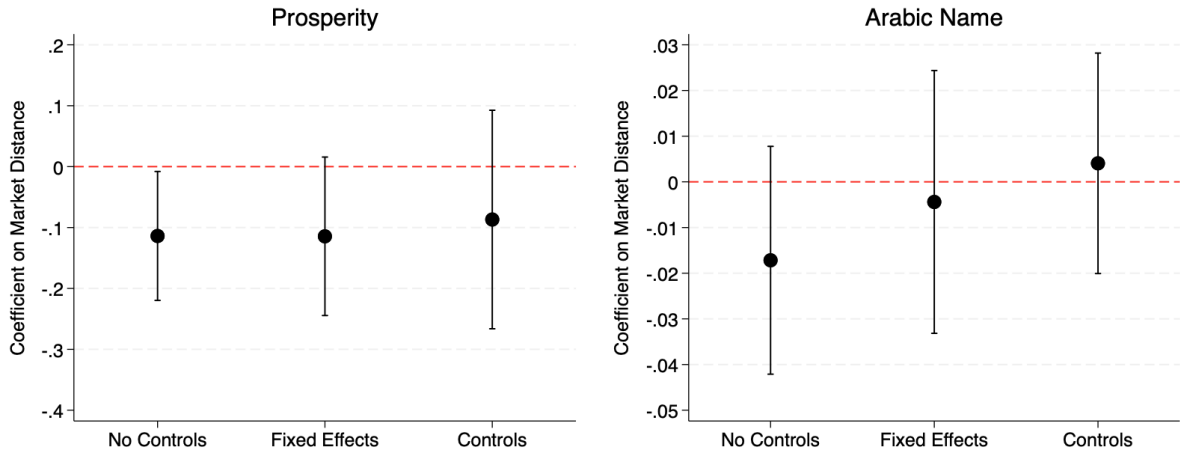


Figure 5: Prosperity, Naming Pattern, and Market Distance

*Notes:* The figure plots the coefficient on market distance from a regression of the first principal component of prosperity (left-hand side) and an indicator for Arabic name (right-hand side). The principal component uses data on self-reported financial rating, number of livestock units, and land holding. Arabic name is an indicator for having an Arabic element in the name. No controls is without any controls. Fixed Effects are for clan and markets. Controls include latitude, altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, and education. The capped bars indicate 95 percent confidence bands.

**Outside exposure.**— Following Algan et al. (2022) and Cantoni, Mohr and Weigand (2024), I use naming patterns as a proxy for exposure to outside world and cultural change. In the study area, individuals have either an Oromo (Cushitic) or an Arabic (Semitic) name. I examine whether the proportion of individuals with an Arabic name varies by market distance. I use a novel administrative dataset on the names of all 1596 households potentially eligible as “main” or “dependent” under the commons management program. 97 percent of the names in the register consist of 3-4 elements, whereas the remaining 3 percent have two elements. I classify a name as ‘Arabic’ if at least one element in the name is of Arabic origin. 43 percent of the names meet this criterion. In Panel B of Figure 5, I examine whether the likelihood of having an Arabic element in a name varies by market distance. In the model without controls, though the coefficient on market distance is negative, it is very small in magnitude and is also statistically insignificant. When I introduce fixed effects and controls variables, the coefficient becomes positive but remains economically and statistically insignificant. The results do not change when I

<sup>10</sup>I do not consider housing quality as a proxy for prosperity because there is no variation: all households live in a house with a mud floor and thatched roof.

consider the number of elements of Arabic origin.

Together, these results suggest that though market visits and prosperity decline with market distance, there is no change in naming pattern as a proxy for outside exposure.

## VI.C. Separating the Channels

I construct two falsification tests to separate the role of asymmetric information and modernization hypotheses. These tests allow me to study the effect of market distance on civic values and rules in an environment which limits the scope of asymmetric information without affecting the scope of modernization. In other words, I ask: what happens to the effect of market distance on civic values and rules when asymmetric information is withdrawn but modernization is allowed to play a role?

**Alternative Market Distance.**— The first falsification test exploits distance from three alternative markets in the study area that are held once a week. The average walking distance to these markets is 2.20 hours (s.d. 1.042). The test is based on the premise that:

- Alternative markets are less prone to asymmetric information than livestock markets. This is because trade in alternative markets occurs primarily in metal utensils, polyester textiles, rubber boots, etc. In comparison to livestock, the quality of these goods is more easily verifiable through visual inspection: dents or finish of metal pots, texture and weave of polyester textiles, and cracks in rubber boots.<sup>11</sup>
- Alternative markets are similar to livestock markets with respect to exposure to modernization and outside world. Figure A.8 shows that prosperity and visits to alternative market decline significantly with alternative market distance, but the naming pattern remains unaffected. The coefficient on alternative market distance is similar in magnitude to the coefficient on market distance. Since the coefficients are standardized, they are directly comparable.

If market exposure operates through asymmetric information, then the effect of alternative market distance is expected to be weaker than the effect of market distance. However, if market exposure operates through modernization then the effect of alternative market distance should be comparable to that of market distance.

Once concern with this falsification test is that while market distance is determined through a natural experiment, we know little about how alternative market distance was determined. As a result, there could be a concern over omitted variables bias. I implement

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<sup>11</sup>Also, sellers do not produce these goods, so it is highly unlikely that they can alter quality. As such, other goods are already made of cheaper materials, making substitution with even cheaper materials unlikely.

a number of checks to mitigate this concern. First, in addition to the market fixed effects, I control for alternative market fixed effects. This allows me to absorb time invariant differences across alternative markets. Second, I show that the locations of alternative markets were unlikely to be influenced by the locations of livestock markets. When I regress alternative market distance on market distance after controlling for geography and fixed effects in Table A.9, the coefficient on market distance turns out to be economically and statistically insignificant. Third, I carry out a balance check on covariates in column 4, Panel B of Table 1. The coefficient on alternative market distance is always small in magnitude and is also statistically insignificant. Fourth, in the balance test on covariates, the coefficients on market distance and alternative market distance are comparable to each other, such that the difference between the two coefficients, reported in column 5, is small and statistically insignificant.

Panel A of Table 1 shows that there is no association between alternative market distance and civic values and rules (column 4). This result stands in contrast to the large negative coefficient on market distance (column 3), which is significantly different from the coefficient on alternative market distance (column 5). I test for the robustness of these findings in Table 7 by introducing controls sequentially. Column 1 control for market distance, column 2 for clan and all market fixed effects, and column 3 for control variables. In all panels, the coefficient on alternative market distance enters with a counterintuitive positive sign, which is even statistically significant at the 5-percent level in Panel B-C. In contrast, the coefficient on market distance enters as expected with a large negative sign and is always statistically significant at the 1-percent level. I get similar results in Panel D when I use responses from the vignettes on the importance of markets teaching civic values. While there is no association between the stated role of markets and alternative market distance, the association with market distance is negative and statistically significant. Table A.10 shows that similar results are obtained when I use the remaining four proxies of civic values and rules. These results suggest that when the scope of asymmetric information is weaker, the negative effect of market distance disappears.

Table 7: Falsification Test I: Distance to Alternative Markets and Livestock Markets

	Market Distance (1)	Fixed Effects (2)	All Controls (3)
Panel A: Conditional Cooperation ( <i>rho</i> )			
Alternative Market Distance	0.032 (0.035)	0.022 (0.047)	0.042 (0.050)
Market Distance	-0.153 (0.024)	-0.161 (0.028)	-0.177 (0.036)
$R^2$	0.12	0.14	0.17
Observations	720	720	720
Panel B: Monitoring (hours)			
Alternative Market Distance	3.263 (1.161)	3.005 (1.588)	3.353 (1.487)
Market Distance	-7.945 (1.100)	-6.523 (0.911)	-5.640 (1.396)
$R^2$	0.43	0.52	0.58
Observations	508	508	508
Panel C: Grazing Rules (months)			
Alternative Market Distance	0.338 (0.122)	0.405 (0.194)	0.367 (0.181)
Market Distance	-1.039 (0.121)	-0.758 (0.170)	-0.687 (0.213)
$R^2$	0.53	0.60	0.64
Observations	509	509	509
Panel D: Market Teaches Civic Values (indicator)			
Alternative Market Distance	0.008 (0.044)	0.049 (0.061)	0.012 (0.064)
Market Distance	-0.367 (0.029)	-0.428 (0.044)	-0.384 (0.043)
$R^2$	0.67	0.79	0.85
Observations	52	52	52
Fixed Effects	No	Yes	Yes
Control Variables	No	No	Yes

*Notes:* OLS estimates with robust standard errors in parentheses in Panel D and clustered on the group in Panels A-C. Column 1 controls only for market distance, Column 2 includes fixed effects for clan, livestock markets, and alternative markets, and Column 3 additionally includes control variables like latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. Alternative market distance is the walking distance to markets that trade mostly in products that are less prone to asymmetric information problem. Market distance is the walking distance to markets that trade mostly in livestock which is prone to asymmetric information problem. In Panels A-C, civic values and rules are at the individual level, and in Panel D at the group level.

**Market Visits on First and Second Day.**— The second falsification test exploits the variation in visits to the same market but on different days. Recall that livestock markets are held twice a week. Trade occurs in livestock on the first day, but in other goods on the second day (metal utensils, polyester textiles, rubber boots, etc). I argue that if asymmetric information is the main channel then market visit on the first day should have a stronger effect on civic values and rules than market visit on the second day.

This falsification test is based on the premise that:

- Individuals are much more likely to be exposed to asymmetric information on the first day than on the second day. As mentioned before, this is because livestock quality is much more prone to asymmetric information, but the quality of other goods can be assessed through visual inspection.
- Market visits on first and second days decline significantly with market distance (see Table A.11). However, while the decline in visits on the first day implies weaker exposure to asymmetric information and modernization, the decline in visits on the second day implies weaker exposure only to modernization.

Thus, if asymmetric information is the main channel then I should observe a large positive effect of first day market visit on civic values and rules, but weaker effect of second day market visit. However, if modernization is the key channel then second day market visit should also have a large positive effect on civic values and rules.

Table 8 reports the results, where I use standardized measures of market visits on first and second day to allow for a direct comparison of these two coefficients. I find that without or with controls, the coefficient on Market Visit - First Day has a large positive magnitude which is always statistically significant at the 1-percent level. In contrast, Market Visit- Second Day enters with a very small and statistically insignificant coefficient in Panel A. In Panel B, it has a counterintuitive negative sign. However, in Panel C, it enters with a large positive and statistically significant coefficient which is as large as the coefficient on Market Visit - First Day. I extend this analysis to responses in the vignette on the importance of markets in teaching civic values. While Market Visit - First Day has a large positive and statistically significant coefficient, Market Visit - Second Day has a small positive and statistically insignificant coefficient. In each panel, I report  $P$ -value from a test of equality of the coefficients on first and second market visits. The two coefficients are significantly different from each other in Panel A-B and D, but not in Panel C. Table A.12 shows that similar results are obtained when I use the remaining four proxies of civic values and rules. In summary, while market visits on both days decline with market distance, yet barring the grazing ban, we observe a consistent positive effect of only the first day visits on civic values and rules.

In summary, while neither of the two falsification tests are bullet proof, they both point towards the same result, which renders confidence on the role of asymmetric information as an important channel underlying the negative effect of market distance.

Table 8: Falsification Test II: Market Visits on First and Second Day

	No Controls (1)	Fixed Effects (2)	Control Variables (3)
Panel A: Conditional Cooperation (Spearman $\rho$ )			
Market Visit - First Day	0.173 (0.038)	0.185 (0.041)	0.165 (0.046)
Market Visit - Second Day	0.007 (0.033)	0.022 (0.038)	0.024 (0.037)
$P$ -value	0.014	0.021	0.037
$R^2$	0.12	0.13	0.16
Observations	720	720	720
Panel B: Time Spent Monitoring (hours)			
Market Visit - First Day	11.626 (1.328)	11.024 (1.422)	8.984 (1.611)
Market Visit - Second Day	-0.840 (0.956)	-1.275 (1.125)	-1.729 (0.987)
$P$ -value	0.000	0.000	0.000
$R^2$	0.51	0.55	0.60
Observations	508	508	508
Panel C: Grazing Rules (indicator)			
Market Visit - First Day	0.269 (0.052)	0.236 (0.046)	0.191 (0.057)
Market Visit - Second Day	0.155 (0.049)	0.194 (0.048)	0.143 (0.045)
$P$ -value	0.259	0.627	0.580
$R^2$	0.59	0.68	0.73
Observations	511	511	511
Panel D: Markets Teach Civic Values (indicator)			
Market Visit - First Day	0.335 (0.047)	0.401 (0.068)	0.286 (0.089)
Market Visit - Second Day	0.021 (0.049)	0.056 (0.054)	0.013 (0.049)
$P$ -value	0.000	0.002	0.018
$R^2$	0.48	0.65	0.72
Observations	52	52	52
Fixed Effects	No	Yes	Yes
Control Variables	No	No	Yes

*Notes:* OLS estimates with robust standard errors in parentheses in Panel D and clustered on the group in Panels A-C. Column 1 is without any controls. Column 2 controls only for clan and market fixed effects. Columns 3 additionally includes control variables like latitude, altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, and education. Market Visit - First Day is weekly visits to market on the first day when trade occurs in livestock. Market Visit - Second Day is weekly visits to market on the second day when trade occurs in other goods.  $P$ -value is from a test of the equality of the two coefficients. In Panels A-C, civic values and rules are at the individual level, and in Panel D at the group level.

## VI.D. Cultural Transmission

Since Menelik's era, no infrastructure has been developed to lower the opportunity cost of market attendance for individuals in groups farther from markets. This means that the effect of market distance could be capturing both the direct effect of current market exposure, as well as the indirect effect of ancestral exposure through cultural transmission (Bisin and Verdier, 2001). I do not have data to offer a clean evidence on cultural transmission. So, I gauge this indirectly under the assumption that because of cultural transmission the effect of market distance will be fairly similar across the different age-groups despite the length of exposure to markets being higher among older age-groups. Columns 1-4 of Table A.13 shows that the effect of market distance on civic values and rules is similar in magnitude across the different age-groups and is also always statistically significant. I also investigate if the effect holds across men and women. This is interesting because even though women attend markets, they move after marriage. Since the age of marriage is low, cultural transmission could have caused men and women to have similar values. Columns 5-6 report that this is indeed the case. These results offer indirect evidence in support of cultural transmission.

## 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 establishing causality and identifying the underlying mechanisms. I resolve these challenges using the context of livestock markets from rural Ethiopia, which offer a natural experiment on the emergence and location of markets.

Using market distance as a proxy for market exposure, I show a strong negative effect of market distance on seven different 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.

To uncover the channels, I investigate the role of asymmetric information and modernization hypotheses. I find strong evidence suggesting that asymmetric information creates a demand for civic values and rules. To this end, I use data from vignette studies to show that livestock markets are prone to market failure from asymmetric information, as it is difficult to verify livestock quality. Depending on their distance from markets, groups develop different kinds of mechanisms to mitigate this problem. In groups far from markets people rely on repeated interaction and reputation, whereas in groups near

markets people rely on civic values and community sanctioning. Falsification tests based on market distance and market visits on different days show that when asymmetric information is weaker, the negative effect of market distance on civic values and rules mostly disappears.

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) in part 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 Background

Figure A.1 shows that a typical camp was made of perishable materials.



Figure A.1: A Military Camp of Menelik

*Notes:* Source: Borelli (1890).

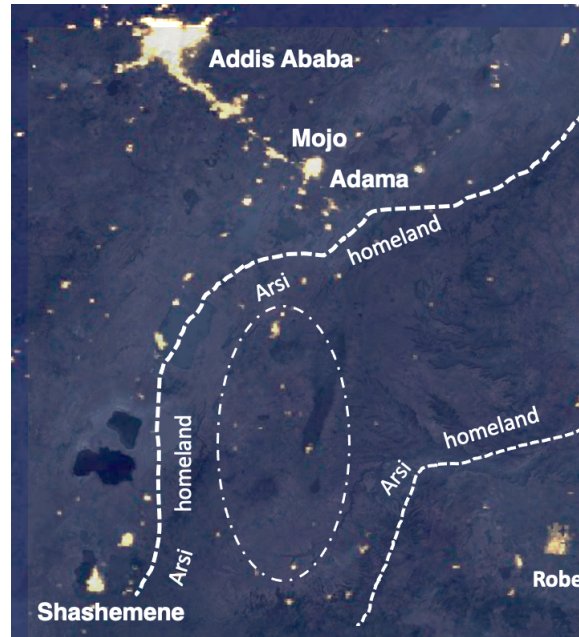


Figure A.2: Night Time Light Density in the Arsi Homeland

*Notes:* The figure shows night time light density from NASA (March 2025). The dotted line shows the boundary of the Arsi homeland. The dotted oval shape shows the area through which the military camps were connected to Addis Ababa. Places like Shashemene, Adama, Mojo, and Robe are outside the Arsi Homeland.

## II. Data

**Market Distance.**— Figure A.3 shows the histogram of market distance.

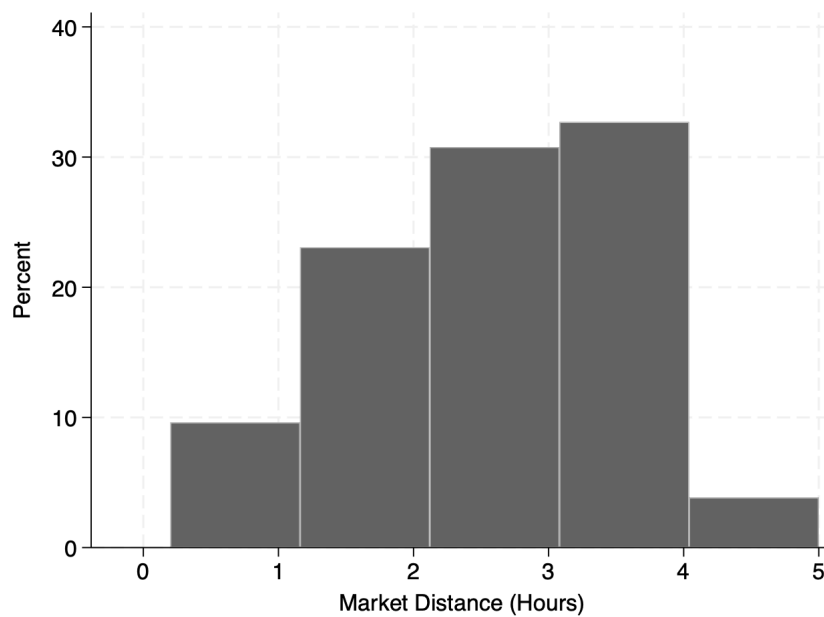


Figure A.3: Histogram of Market Distance

**Market Distance and Market Visits.**— Table A.1 shows that market visits decline significantly with market distance.

Table A.1: Market Distance and Market Visits

	Dependent Variable: Market Visits per Week
Market Distance	-0.246 (0.058)
$R^2$	0.81
Observations	52
Fixed Effects	Yes
Control Variables	Yes

*Notes:* OLS estimates with robust standard error in parentheses. Column 1 is without any controls, column 2 includes fixed effects for clan and market, and column 3 additionally includes control variables like latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. Data are at the group level.

**Sample Representation.**— 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.

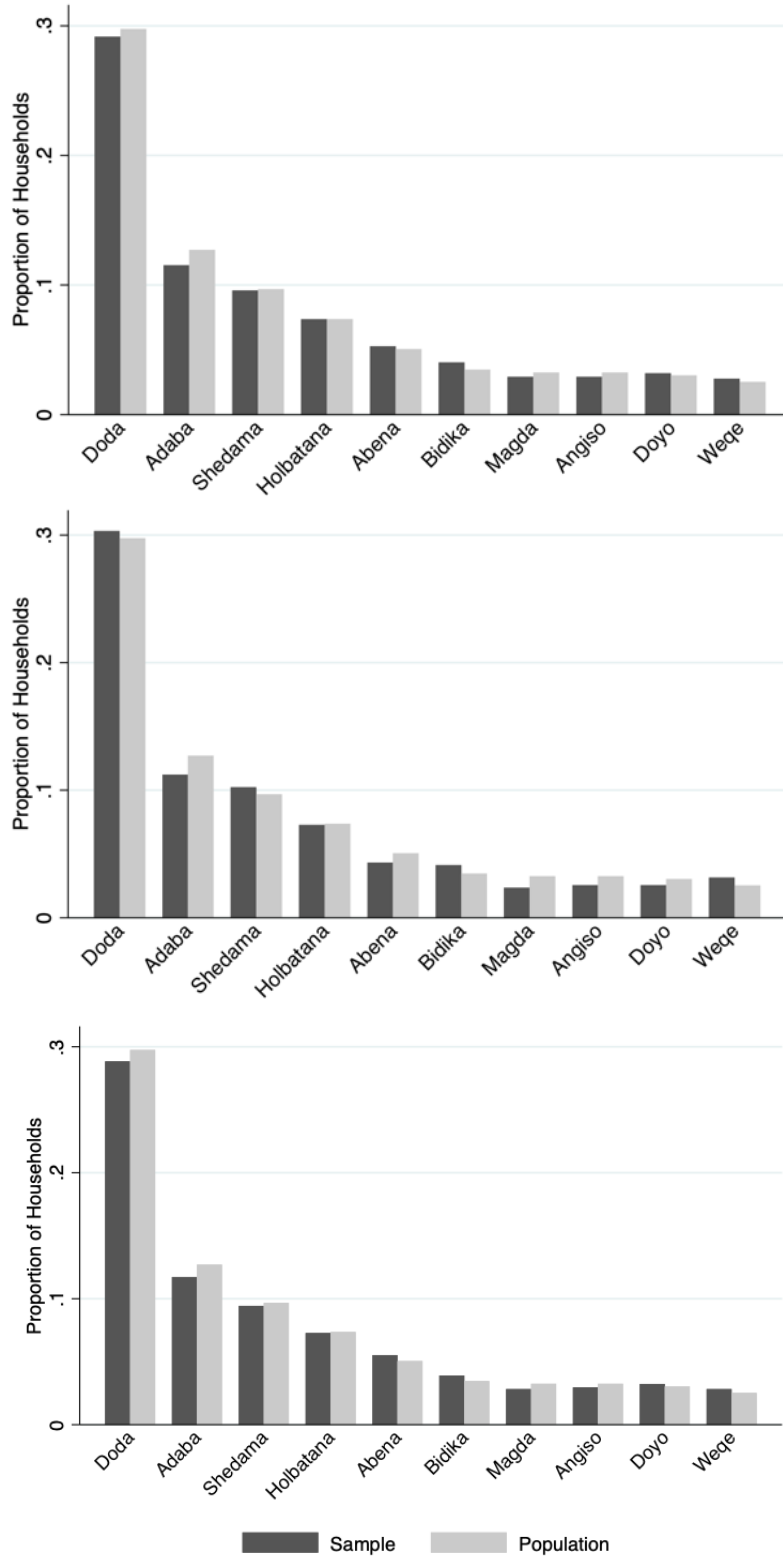


Figure A.4: Proportion of Households from Main Clans in the Population and Experimental Sample (top), Monitoring Survey Sample (middle) and Grazing Rules Survey Sample (bottom)

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

**Descriptive Results.**— Figure A.5 uses bin-scatter plots to show a negative association of market distance with other proxies of civic values and rules, such as an indicator for conditional cooperators, unconditional contribution, beliefs about other players' contribution, and the likelihood of forming grazing rules.

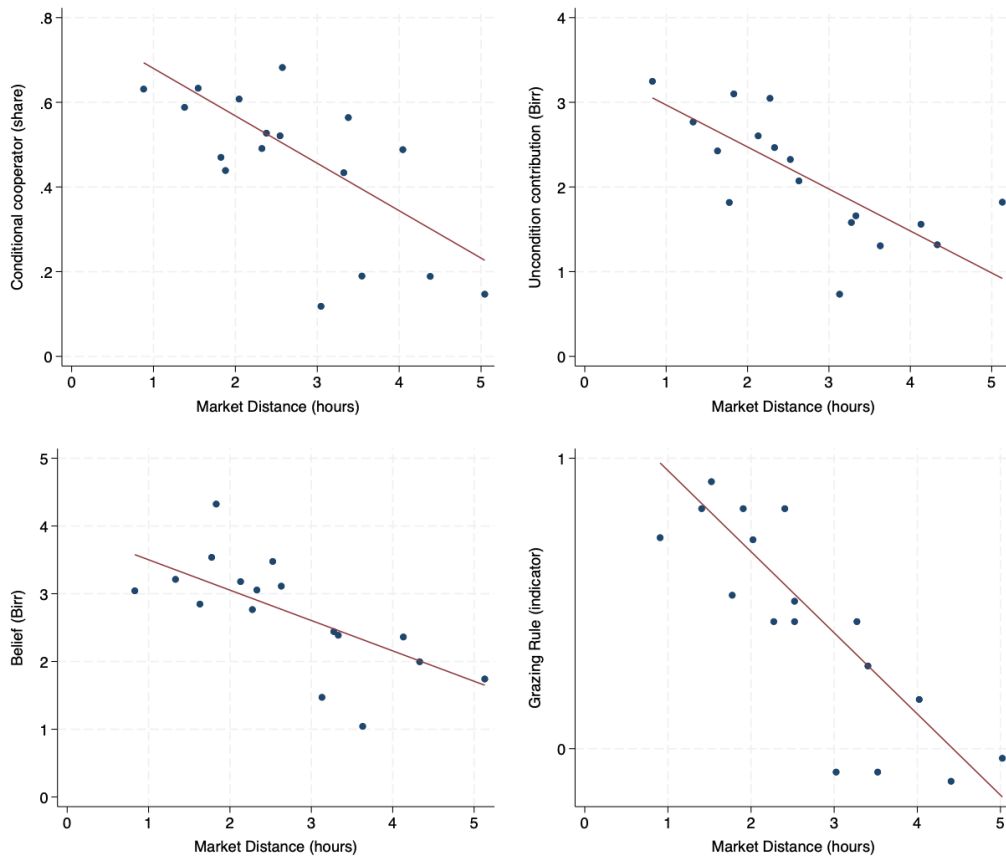


Figure A.5: Bin-Scatter between Market Distance and Civic Values and Rules

Figure A.6 presents a 3-D plot which shows: a) map of spatial variation in market distance (shades of blue); b) scatter plot between market distance and civic values and rules, whereby each circle represents the quartile of civic value and rules (shades of gray); and c) the size of the circle is weighted by sample size. As an example, the first plot shows the association between market distance and conditional cooperation ( $\rho$ ). In places near markets (lighter shades of blue) most individuals have higher propensity for conditional cooperation (4th quartile of Spearman  $\rho$ , solid black circle). In contrast, in places far from markets (darker shades of blue) most individuals have lower propensity for conditional cooperation (1st-2nd quartile of Spearman  $\rho$ , solid white or light gray circle). Similar patterns are observed when using time spent monitoring, formation of grazing rules (share of individuals reporting yes), an indicator for conditional cooperators, unconditional contribution, beliefs about others' contribution in the unconditional decision, and number of months grazing is forbidden inside the forest.

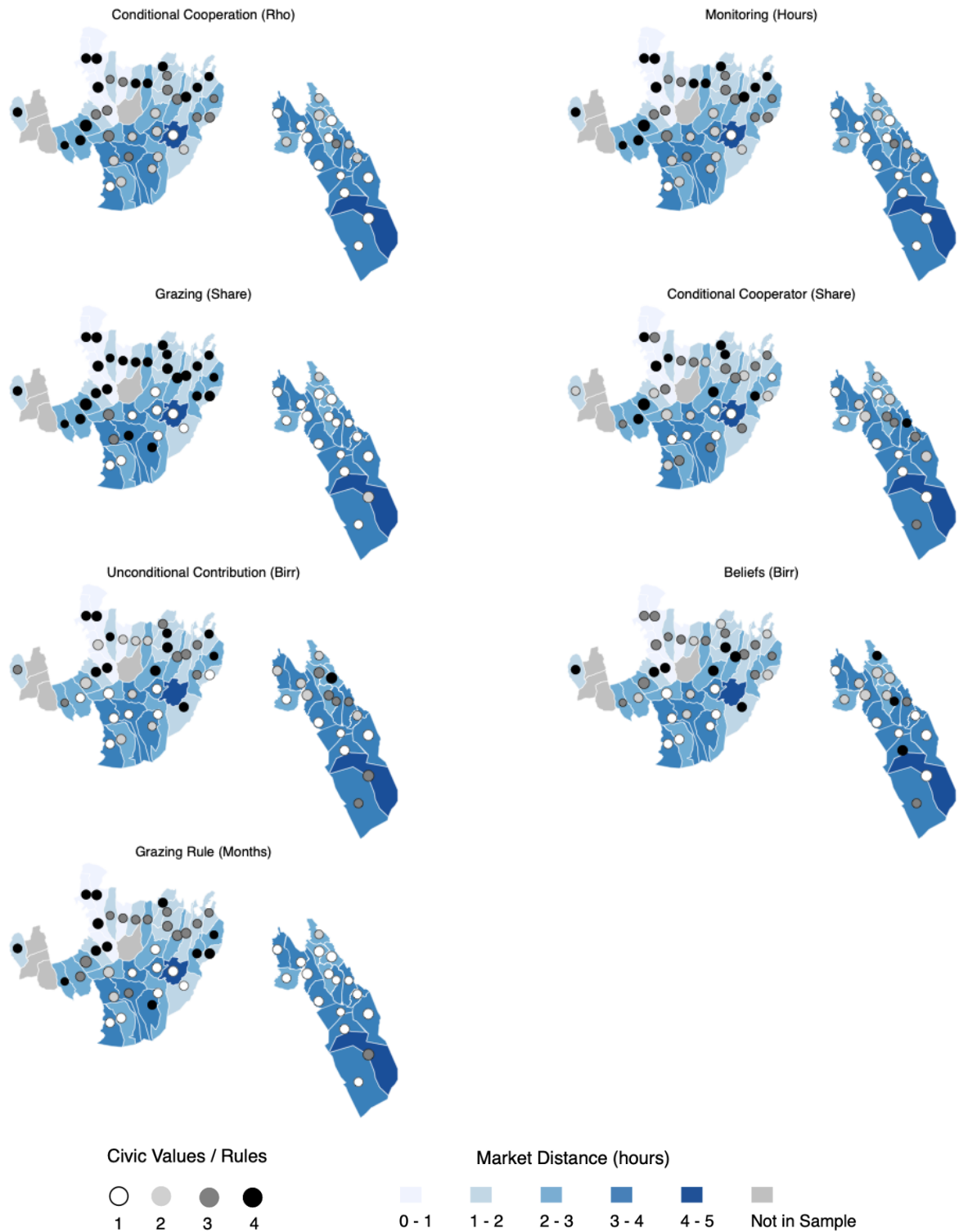


Figure A.6: Civic Values and Rules by Market Distance: Scatter Plot on a Map

*Notes:* The figure shows a 3-D plot of the association between market distance and civic values and rules. Each polygon is a group and displays the variation in market distance in shades of blue, where lighter shades mean closer to markets and darker shades mean further away from markets. Each circle represents the quartile of civic values and rules, where lighter shades mean lower quartile and darker shades mean higher quartile. The size of the circle is weighted by the number of observations. See text for the interpretation of the figure.

### III. Empirical Strategy

**Sorting by Market Distance.**— Table A.2 shows that the association of market distance with various indicators of sorting is weak and statistically insignificant.

Table A.2: Scope of Sorting by Market Distance

	Total Population (1)	Population Density (2)	Past Forest Cover (3)
Market Distance	6.155 (12.735)	-0.104 (1.170)	-2.618 (2.215)
Geography	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Sample Mean	181.19	13.99	39.70
Observations	52	52	52

*Notes:* OLS estimates with robust standard errors in parentheses. Total population is the number of individuals residing in a group. Population density is the number of people per km<sup>2</sup> in a group. Forest type is the ratio of area under plantation forest to area under natural forest in a group. Past forest cover is the median percentage of the group area that was under forest in 1990-2000 computed using data from (Hansen et al., 2013). Sample mean refers to mean of the dependent variable listed in the column. All regressions control for geography (altitude, latitude) and fixed effects for clan and market. Data are at the group level.

Figure A.7 shows via a bin-scatter plot that there is no association between share of household heads who migrated and market distance.

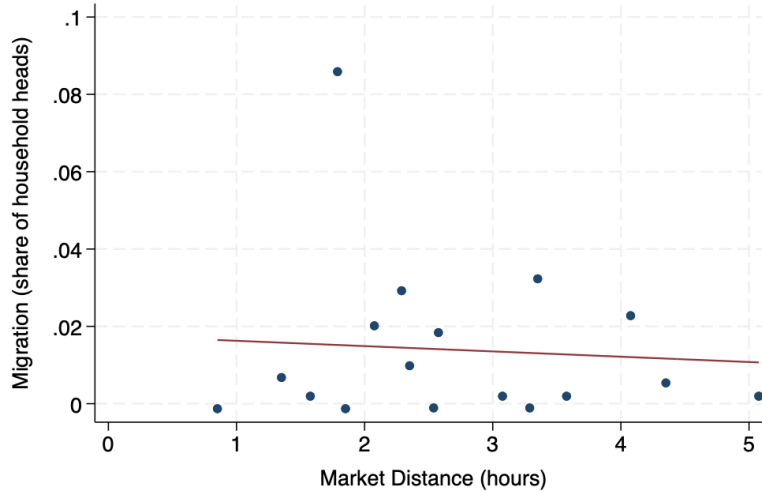


Figure A.7: Migration by Household Heads and Market Distance

*Notes:* The bin-scatter plot shows the association of share of household heads who have migrated and market distance after accounting for market fixed effect.

**Control Variables for Robustness Checks.**— Table A.3 reports the summary statistics of the control variables used in testing the robustness of the main results.

Table A.3: Summary Statistics: Additional Control Variables

	Measurement unit	Obs.	Mean	Standard deviation
<i>Other Controls</i>				
Population Density	group	52	13.988	8.738
Past Forest Cover (%)	group	52	39.701	9.788
Number of Neighbors	group	52	3.788	1.661
Program Duration	group	52	47.000	19.630
Trust in Government	individual	503	0.406	0.491
<i>Distance Related Controls</i>				
Administration	group	52	1.649	1.105
School	group	52	0.822	0.378
Mosque	group	52	0.702	0.462

*Notes:* *Other Controls:* Population density is the number of people per km<sup>2</sup> in a group. Number of neighboring groups is the number of other groups surrounding a group. Past forest cover is the median percentage of the group area under forest in 1990-2000 computed using data from (Hansen et al., 2013). Program duration is the number of months a groups has been under the commons management program. 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. Main Results

**Coefficient on Control Variables.**— Table A.4 shows the coefficients on the control variables in the main results presented in Table 2.

Table A.4: Market Distance, Civic Values, and Rules:  
Coefficients on Control Variables

	Dependent variable:		
	Conditional cooperation	Monitoring	Grazing rule
	(1)	(2)	(3)
Market Distance	-0.167 (0.037)	-5.568 (1.656)	-0.716 (0.198)
Latitude	-1.785 (2.046)	50.946 (66.242)	5.570 (6.946)
High Altitude	-0.171 (0.107)	-4.384 (3.811)	-0.085 (0.525)
Group Size	-0.003 (0.006)	-0.545 (0.237)	0.015 (0.023)
Group Fragmentation	0.233 (0.105)	3.941 (3.667)	-0.408 (0.401)
Female Share	-0.189 (0.229)	4.749 (10.876)	-1.643 (0.782)
Gini - Cattle Ownership	0.530 (0.336)	4.449 (13.368)	-1.670 (2.500)
Gini - Land Ownership	0.265 (0.378)	4.914 (13.910)	3.054 (1.583)
Education	-0.005 (0.007)	0.325 (0.165)	0.032 (0.023)
Fixed Effects	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
Observations	720	508	509

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

**Clustering Standard Errors.**— Table A.5 shows that the coefficient on the market distance is robust to different types of clustering of standard errors: on group and market in column 1, on group and administration in column 2, on group and clan in column 3, and on geographical space in column 4.

Table A.5: Market Distance, Civic Values, and Rules:  
Clustered Standard Errors by Market, Administration, Clan, and Space

	Standard errors are clustered by			
	Group & Market (1)	Group & Clan (2)	Group & Admin (3)	Space (4)
	Panel A: Conditional Cooperation (Spearman Rho)			
Market Distance	-0.167 (0.047)	-0.167 (0.038)	-0.167 (0.046)	-0.167 (0.049)
	Panel B: Monitoring (Hours)			
Market Distance	-5.568 (2.159)	-5.568 (1.632)	-5.568 (2.901)	-5.568 (2.337)
	Panel C: Grazing Rules (Months)			
Market Distance	-0.716 (0.236)	-0.716 (0.215)	-0.716 (0.207)	-0.716 (0.208)
Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

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

**Other Measures of Market Distance.**— Table A.6 shows that the results are robust to using median (Panel A) and log of market distance (Panel B).

Table A.6: Market Distance, Civic Values, and Rules:  
Results using Median and Log Market Distance

	Dependent variable:		
	Conditional cooperation (1)	Monitoring (2)	Grazing rule (3)
	Panel A: Median Market Distance		
Above Median Market Distance	-0.261 (0.100)	-10.114 (4.198)	-1.412 (0.520)
$R^2$	0.14	0.52	0.59
Observations	720	508	509
	Panel B: Log Market Distance		
Log of Market Distance	-0.343 (0.091)	-8.888 (3.892)	-1.533 (0.529)
$R^2$	0.16	0.51	0.60
Observations	720	508	509
Controls	No	No	Yes
Fixed Effects	No	Yes	Yes

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

**Additional Controls.**— Table A.7 shows that the results are robust to controlling for additional controls like population density, past forest cover, number of neighboring groups, and program duration.

**Distance from Administration, School, and Mosque.**— Table A.8 shows that the results are robust to controlling for distance from the administration, school, and mosque, as well as trust in local government.

Table A.7: Market Distance, Civic Values, and Rules:  
Robustness Checks using Additional Controls

	(1)	(2)
Panel A: Conditional Cooperation		
Market Distance	-0.175 (0.040)	-0.184 (0.038)
Population Density	-0.010 (0.004)	-0.015 (0.005)
Past Forest Cover	-0.003 (0.004)	-0.003 (0.004)
Neighboring Groups		-0.021 (0.020)
Program Duration		-0.005 (0.004)
Panel B: Monitoring		
Market Distance	-5.696 (1.678)	-4.957 (1.514)
Population Density	0.005 (0.172)	0.307 (0.168)
Past Forest Cover	-0.075 (0.138)	-0.073 (0.118)
Neighboring Groups		0.321 (0.686)
Program Duration		0.346 (0.100)
Panel C: Grazing Rules		
Market Distance	-0.747 (0.174)	-0.613 (0.169)
Population Density	-0.001 (0.022)	0.057 (0.024)
Past Forest Cover	-0.018 (0.020)	-0.018 (0.017)
Neighboring Groups		0.091 (0.117)
Program Duration		0.065 (0.020)

*Notes:* OLS estimates with robust standard errors clustered on the group in parentheses. All columns include clan and market fixed effects, and control for latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. Population density is the number of people per km<sup>2</sup> in a group. Number of neighboring groups is the number of other groups surrounding a group. Past forest cover is the median percentage of the group area under forest in 1990-2000. Program duration is the number of months a groups has been under the commons management program. Trust in government is a binary variable, where 1 implies the government is cooperative and 0 otherwise. The number of observations is 720 in Panel A, 508 in Panel B, and 509 in Panel C.

Table A.8: Market Distance, Civic Values, and Rules:  
Distance from Administration, Schools, and Mosque

	(1)	(2)
	Panel A: Conditional Cooperation	
Market Distance	-0.171 (0.037)	-0.160 (0.040)
Administration Distance	0.024 (0.042)	0.024 (0.050)
School Distance	-0.011 (0.086)	-0.066 (0.090)
Mosque Distance	-0.320 (0.057)	-0.330 (0.070)
Trust in Government		-0.054 (0.046)
	Panel B: Monitoring	
Market Distance	-9.759 (1.911)	-9.636 (1.788)
Administration Distance	4.186 (1.603)	4.852 (1.519)
School Distance	9.282 (3.670)	7.713 (3.594)
Mosque Distance	1.336 (2.578)	1.265 (2.493)
Trust in Government		4.981 (1.473)
	Panel C: Grazing Rules	
Market Distance	-0.707 (0.218)	-0.704 (0.211)
Administration Distance	-0.410 (0.201)	-0.370 (0.202)
School Distance	1.090 (0.448)	1.003 (0.443)
Mosque Distance	-0.049 (0.316)	-0.037 (0.308)
Trust in Government		0.335 (0.153)

*Notes:* OLS estimates with robust standard errors clustered on the group in parentheses. All columns include fixed effects for clan and market, as well as controls like latitude, altitude, group size, group fragmentation, female share, Gini of livestock ownership, Gini of land holding, education, and program duration. The number of observations is 720 in Panel A, 508 in Panel B, and 509 in Panel C.

## V. Plausible Channels

**First Falsification Test.**— Figure A.8 shows that the effect of alternative market distance on prosperity, market visits, and naming patterns is comparable to the effect of market distance.

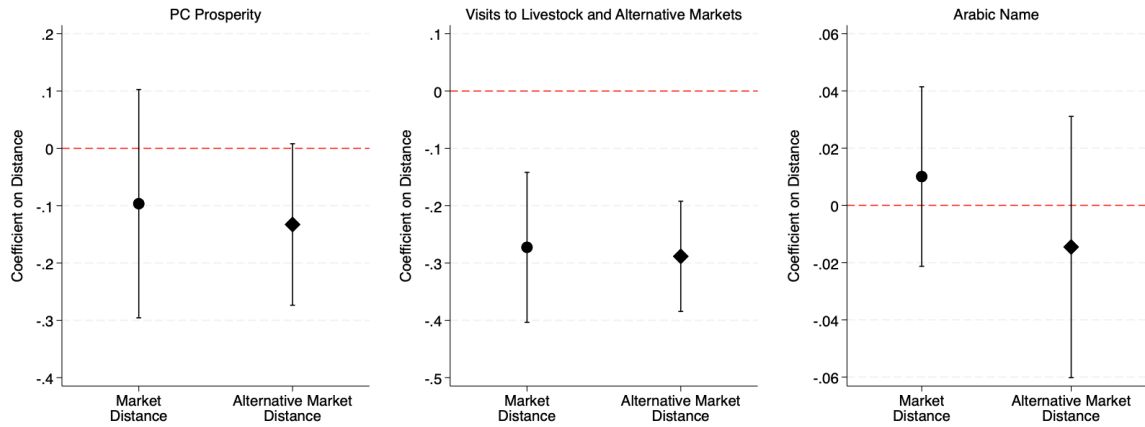


Figure A.8: Market Visits, Prosperity, Naming Pattern by Distance to Livestock and Alternative Markets

*Notes:* The figure plots the coefficient on alternative market distance. It is based on a regression with the full set of fixed effects and controls. The dependent variable is principal component of prosperity in the left figure, visits to alternative markets in the center figure, and an indicator for Arabic name in the right figure. Fixed Effects are for clan and alternative markets. Control variables include latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. To allow for easy comparison, I also plot the coefficient on market distance obtained in a similar manner, except that in the center figure now the dependent variable is visits to livestock markets. Both alternative market distance and market distance are standardized to allow for easy comparison. The capped bars indicate 95 percent confidence bands.

Table A.9 shows that there is no association between market distance and alternative market distance. Table A.10 and Table A.12 show that the results of falsifications tests 1 and 2 hold when I use alternative measures of civic values and rules. Table A.11 show that both first and second visit to market decline with market distance.

Table A.9: Market Distance and Alternative Market Distance

Dependent Variable:	
Alternative Market Distance	
Market Distance	0.084 (0.149)
$R^2$	0.72
Observations	52
Fixed Effects	Yes
Geography	Yes

*Notes:* OLS estimates with robust standard errors in parentheses. Fixed effects are for clan and livestock market. Geographical controls include latitude and altitude.

Table A.10: Falsification Test I: Distance to Alternative Markets and Livestock Markets  
Robustness Check

	Dependent Variable is:			
	Conditional cooperator (indicator) (1)	Unconditional contribution (Birr) (2)	Beliefs - others' contribution (Birr) (3)	Grazing ban (indicator) (4)
Alternative Market Distance	0.066 (0.051)	0.061 (0.099)	0.212 (0.101)	0.058 (0.052)
Market Distance	-0.126 (0.035)	-0.548 (0.187)	-0.528 (0.164)	-0.165 (0.062)
$R^2$	0.12	0.19	0.12	0.66
Mean	0.468	2.054	2.675	0.446
Observations	720	704	704	511
Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

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

Table A.11: Market Distance and Market Visits:  
First and Second Day

	Dependent Variable: Market Visits per Week	
	First Day (1)	Second Day (2)
Market Distance	-0.109 (0.038)	-0.136 (0.051)
$R^2$	0.66	0.70
Observations	52	52
Fixed Effects	Yes	Yes
Control Variables	Yes	Yes
Mean (s.d.)	0.74 (0.26)	0.22 (0.39)

*Notes:* OLS estimates with robust standard errors in parentheses. In column 1, the dependent variable is average visits to market in a week on the first market day. In column 2, the dependent variable is average visits to market in a week on the second market day. Fixed effects are for clan and market. Control variables include latitude, altitude, group size, group fragmentation, share of female members, Gini of livestock ownership, Gini of land holding, and education. Data are at the group level.

Table A.12: Falsification Test II: Market Visits on First and Second Day  
Robustness Check

	Dependent Variable is:			
	Conditional cooperator (indicator) (1)	Unconditional contribution (Birr) (2)	Beliefs - others' contribution (Birr) (3)	Grazing ban (indicator) (4)
Market Visit - First Day	0.162 (0.041)	0.345 (0.184)	0.638 (0.164)	0.192 (0.058)
Market Visit - Second Day	0.021 (0.040)	0.181 (0.112)	0.182 (0.114)	0.152 (0.044)
<i>P</i> -value	0.037	0.527	0.049	0.649
<i>R</i> <sup>2</sup>	0.13	0.17	0.12	0.71
Mean	0.468	2.054	2.675	0.446
Observations	720	704	704	511
Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

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

**Results by Age-Groups and Gender.**— Table A.13 shows the effect of market distance is similar by age-groups and gender. I do not introduce controls variables because the sample size is small.

Table A.13: Market Distance, Civic Values, and Rules:  
Effects by Age Quartile and Gender

	Age-Quartiles				Gender	
	First (1)	Second (2)	Third (3)	Fourth 4	Men 5	Women 6
Panel A: Conditional Cooperation (Spearman <i>rho</i> )						
Market Distance	-0.175 (0.040)	-0.156 (0.053)	-0.083 (0.038)	-0.168 (0.050)	-0.145 (0.023)	-0.146 (0.070)
Obs.	177	188	170	153	620	68
Panel B: Monitoring (hours)						
Market Distance	-6.798 (0.799)	-7.828 (1.363)	-7.992 (1.084)	-6.859 (1.889)	-7.141 (1.002)	-7.447 (1.301)
Obs.	131	137	130	108	453	52
Panel C: Grazing Rules (indicator)						
Market Distance	-0.804 (0.240)	-0.782 (0.167)	-0.938 (0.100)	-1.052 (0.155)	-0.870 (0.138)	-0.955 (0.180)
Obs.	132	138	130	107	454	52
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

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

# 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  $12 \text{ Birr} + 12 \text{ Shillings} = 18 \text{ Birr}$ . 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  $6 \text{ Birr} + 6 \text{ Shillings} = 9 \text{ Birr}$ . 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.)

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