WHITHER FORMAL CONTRACTS?

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To measure the benefits of formal contract enforcement for society, I create a market with merchants and buyers, in which buyers can choose whether to buy, and whether to pay. A set of multiple "state-favored" ethnic groups control the state. I experimentally vary whether formal contracts are required and the composition of buyer-merchant pairs. The design separately identifies the effect of the contracts on the buyers' incentive to pay and on their incentive to buy. I document two ways in which society limits the benefits of contracts. First, contracts reduce buyer cheating, thus increasing merchants' profits, if, and only if, the merchant is state-favored. Buyers' beliefs suggest that the merchants can enforce the contracts if, and only if, the merchant is state-favored. Second, holding constant whether the pair is state-favored, contracts only influence buyer choices when the buyer and the merchant belong to two, different, state-favored ethnic groups. Buyers' choices and beliefs confirm that, in that case, the contracts are expected to be enforceable, but they have no effect on buyers' choices because reputation already governs the incentives to cheat within groups. The findings temper the view of the state as independent from society, offer a rationale for why contracts are not adopted, and nuance the notion of state weakness.

KEYWORDS: Contracts, property rights, ethnicity, state.

1. INTRODUCTION

WITHOUT THE ABILITY to exchange, the potential for growth is rather limited. Historically, European state contracting institutions solved commitment problems inherent in exchange, supporting economic growth. Economists view enforcement through those as more efficient than informal enforcement. Yet, across the world, state enforcement of contracts is infrequent. Why do societies fail to adopt it, even as it is presumably more efficient?

One possible explanation is that the state does not always have the power to enforce contracts. Informed by the European state formation experience, some scholars have argued that strong states are a precursor to strong markets and have studied how strong

states arise (Besley and Persson (2010)). In this view, the state is exogenous to society, and so-called "developing" societies tend to have high informality because their state may be weak.

Another possibility is that, in those societies, even if the state is capable of enforcing contracts, society has strong pre-existing groups. If some groups govern whether contracts are enforced, the benefits of enforcement may not materialize for large segments of society.

This paper shows that, in a purportedly weak state, just as some groups capture policy (Besley and Coate (1997)), they also capture contracting institutions and contract enforcement. Furthermore, groups govern reputations hence, with groups, contracts are not needed.

I conducted a study in the town of Bukavu, Democratic Republic of the Congo (D.R.C.). The economy in Bukavu is informal. A set of multiple ethnic groups control the state and discriminate against the rest. I refer to this set as the "state-favored," and to the rest as "disfavored." Informal institutions in each state-favored ethnic group have existed for millennia.

Trade and contract adoption are endogenous, and thus, self-selected. To address this challenge, I created a market. Specifically, I hired state-favored and disfavored merchants to make door-to-door offers to potential buyers in which the buyers can defect. I randomized the matching of merchants to buyers, and the requirement to sign a contract.

This design allows to estimate the effect of the contract on the buyers' choice whether to buy and pay (exchange), buy and defect (cheat), or reject. I use a simple framework to infer, from these choices, whether contracts make cheating costlier. Consider a buyer's payoffs of exchange, cheat, and reject. The buyer has utility from consuming the good and from income, but cheating can be costly. A higher cost of cheating tends to discourage *cheat* and encourage *exchange* or, when utility from consumption is low, encourage *reject*.

Yet, in reality, the contract requirement may affect buyers' choices through channels other than the cost of cheating; for example, it may convey information about the merchant's type, or buyers may simply have a taste for contracts. In that case, the decision to reject the offer by buyers with a contract may capture these channels, creating a selected sample, and influencing the choice to exchange other than through the effect of the contract on the cost of cheating.

The research design tackles this issue. I can analyze the choice of buyers, all of whom accepted to buy with a contract required, but some of whom were forced to go through with it and others not—ensuring that buyers who trade with or without a contract are comparable.

Specifically, in the *sale on credit*, the buyer had to commit to pay within three days and sign a contract. After the buyer said whether they accepted the offer (*initially accept*), I randomized whether the contract requirement was lifted or maintained, and allowed all buyers to reconsider. I first analyze whether they rejected, exchanged, or cheated after learning whether it was lifted (*ultimate choice*). I also analyze whether those who initially accepted paid. Since no buyers in that set had reasons to ultimately reject, those who sign are comparable to those who do not.

Yet, buyers may have a preference to trade with merchants of their own ethnic group (*taste for co-ethnic exchange*). In that case, buyers who accept with a given merchant group may be a selected sample. This sample selection would bias the comparison of the effect of the contract across groups. The research design also tackles this issue. In the *placebo* sale, implemented with all buyers, there was no room for buyer fraud. This allows to separately identify the effect of the trade environment on the buyers' preference for exchange.

I begin by examining the buyers' ultimate choices in the sale on credit. First, buyers with requirement maintained were less likely to cheat, more likely to exchange and to reject—but only if the merchant was state-favored. But among those assigned to lifted, assignment to state-favored versus disfavored merchant made no difference. This suggests that buyers anticipated state bias in contract enforcement. Second, among state-favored buyer-merchant pairs, the requirement had no effect in co-ethnic pairs. Yet, buyers in co-ethnic pairs with lifted requirement were *already* less likely to cheat than those in non-co-ethnic pairs. This suggests that contracts are in vain in co-ethnic trade because ethnic groups already govern trade.

To support this interpretation, I analyze the buyers' beliefs about what would happen if they cheated in this sale. Buyers with maintained requirement worried about prosecution, if, and only if, the merchant was state-favored—both in co-ethnic and non-co-ethnic pairs. Those with lifted requirement worried about reputation, if, and only if, the pair was co-ethnic. This confirms that reputation governs co-ethnic informal trade, and that only state-favored merchants can enforce contracts, both in co-ethnic and non-co-ethnic trade.

I then examine whether buyer selection into trade with maintained requirement confounds this analysis. I link buyer's choice in the placebo to that in the sale on credit. I find that buyers with maintained requirement in the sale on credit who ultimately accepted and those who ultimately rejected were as likely to have rejected the placebo. I repeat the analysis of ultimate choices controlling for placebo choices. I find no evidence of confounding.

The analysis then fully shuts down buyer self-selection into contracts. Specifically, I condition the analysis on the selected sample of buyers who initially accepted. All of these buyers ultimately accepted. Thus, comparing those who sign the contract and those who do not isolates the effect of the contract on the incentive to pay. Their choices confirm state bias, co-ethnic governance, and the effect of co-ethnic governance on state enforcement.

The realism of this market comes at the cost of giving discretion to the merchants. I thus analyze robustness to their discretion. I find that the results are robust to bargained price, buyer non-compliance to sign, and to the merchants' mis-recording of choices.

I then consider potential threats to inference. I first analyze whether lifting the requirement has an independent effect on buyers' beliefs. I then examine possible mis-attribution of ethnic group, and taste for co-ethnic exchange. I find no evidence of confounding.

Having ruled out threats to inference, I then provide evidence to support the state bias mechanism. I find that state-favored buyers, especially those that are state officials, discriminate against disfavored merchants in the placebo sale. This elicits that state-favored have anti-disfavored bias. This result is consistent with the buyers anticipating state bias by state-favored-controlled contracting institutions, when choosing whether to cheat.

Finally, I quantify the gains for state-favored merchants in non-co-ethnic trade. Contracts increase daily profits by 276% of daily p.c. income. This is significant, for a state considered weak.

This study nuances the notion of "state weakness" and rationalizes why contracts are not adopted. Economists generally assume that low state capacity can explain why informality persists. The state is typically portrayed as a deus ex machina, and informality as inefficient (Munshi and Rosenzweig (2016), Greif (1993), Dixit (2003)). I conduct this study in a society with high levels of informality and a state portrayed as weak. In this context, it is natural to assume that state weakness causes informality. The findings show that the state is capable of enforcing contracts. Even then, some groups cannot trust the state; others do not need it.

The paper also responds to a puzzle in economic history (Greif (1993)). European contracting institutions dominated long-distance trade in the 14th century over societies that used informal enforcement. If informal enforcement was inefficient, why did those societies not adopt formal contracts? While there are likely various explanations, I propose that of the dominant groups' wishes. I show that some segments of society capture them and use them to express their preferences, thus the rest cannot trust them to enforce contracts. Just as with foreign contracting institutions (Greif, Milgrom, and Weingast (1994)), disciplining contracting institutions intra-nationally is challenging when a segment of society has captured the state.

This study also answers a question central to the evolution of societies: how does the state interact with society? This question has recently received theoretical attention (Jackson and Yiqing (2020)). Empirically, some scholars have analyzed society's informal organization (Anderson (2011), Greif (1994)), and how that may interact with the state (Chandrasekhar, Kinnan, and Larreguy (2018)). Due to obvious empirical challenges, in this literature, studies are often in settings in which matches are endogenous, or they are in the lab. This paper complements this literature, inducing naturally occurring trade and randomly forming trading pairs with and without contracts. I separate incentives from selection randomizing in two steps, a method I import from the study of credit markets (Karlan and Zinman (2009)).

This study also complements the literature on ethnicity. Even as identity is known to be socially constructed (Brubaker and Cooper (2000), Nix and Qian (2015), Cassan (2015)), scholars often lump divisions into "ethnic diversity" (Habyarimana, Humphreys, Posner, and Weinstein (2007), Miguel and Gugerty (2005), Lowes, Nunn, Robinson, and Weigel (2017)). The findings presented in this paper show that the very term ethnicity can be misleading. In this context, one division is about state bias, the other about reputation.

2. CONTEXT

This section presents the people and institutions in Bukavu, Sud Kivu province's capital.

The state-favored set. There is a set of sixteen ethnic groups who have co-existed in the region for millennia and control the provincial state institutions. They self-identify as "natives."[1] I refer to this set as the set of state-favored, to two individuals in the same ethnic group in this set as co-ethnics, and to individuals from two different ethnic groups in this set as non-co-ethnics. Between groups in this set, group membership is easily recognizable.[2]

Co-ethnic governance. Social networks are segregated by the different ethnic groups of this set (Nest, Grignon, and Kisangani (2011)). Their ethnic homelands are in rural areas. Intra-ethnic disputes are resolved in the ethnic group "Barzas," a conflict resolution institution specific to each ethnic group. Barzas are based on community meetings led by the ethnic groups' leaders in their respective rural homeland. Barzas frequently ostracize cheaters, thieves, or witches from the ethnic community. Small conflicts are solved involving the family, the community, or the church using social networks that are often along ethnic lines. Thieves can also risk public beatings, which cut across ethnic groups.[3]

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[1]So-called "autochthonous." They descend from the Bantu expansion 2,000–3,000 years ago from West Africa. They are composed mostly of the Bashi and Bahavu. The other groups in this set are: Babembe, Batembo, Bafuliru, Bavira, Bangubangu, Babembe, Babuyu, Joba, Kabwari, Banu, Balega, Banyiundu, Songoora, Bazimba.

[2]They have different facial features, culture, languages, and accents. Source: see Table I, Panel 3 and p. 15.

[3]Source: own interviews and Clark (2008). Homeland reputation matters because of land and ancestors' respect.

The disfavored. Beyond the state-favored set, there are the Tutsi (henceforth, "disfavored"). They form a marginalized group that originated a century ago from Rwanda, and have a notably distinct physiognomy. They are excluded from politics and from access to the state, which opposed recognizing them (Vlassenroot (2002)). The state-favored designate them as foreigners and are hostile against them. As a result, the state-favored groups use the provincial administration as a vehicle for expressing anti-disfavored bias against them.[4]

State bias. The provincial administration is controlled by the groups in the state-favored set. At the time of the study, the governor of the Province, his Executive Secretary, Executive Vice-Secretary, Director of Cabinet, and Vice-Director of Cabinet belonged to the state-favored groups. So did nine out of ten provincial Ministers. Ministries are also staffed along ethnic lines. As a result, among 78 non-Ministerial positions in the provincial Ministries of Sud Kivu at the time of the study, only five were filled by Tutsi. Of these, four occupied positions in the Ministry of Transport, an agency with little strategic value, and one was a driver. Expressing state bias, a provincial Minister told the study team: *"You have to put someone you trust in here. The Governor does not trust Tutsi."* [5]

State contracting institutions are controlled by the state-favored groups. This influences the expectations of obtaining a favorable outcome for anyone victim of defection. The procedure for enforcing contracts is as follows. Any person victim of defection with a legally valid contract can file a complaint to the neighborhood's territorial police, located at a five-minute walk from the average household. There, the Judicial Police Officer (OPJ) can detain the accused for up to 72 hours based on the complaint alone. The accuser typically covers the transport costs and salary of the police to detain the accused. Typically, disputes are solved at this step. But if the accused does not pay, the OPJ can transfer the complaint to the provincial prosecution office, located at a 30-minute walk from the study's neighborhoods. Prosecution can take long and can require bribing.

If the accuser enjoys the prosecutor's support, unlikely for accusers from the disfavored group, prosecution can be completed in a day. Both the OPJ and the prosecutors are known to belong to the state-favored groups and to discriminate against the accusers of the disfavored group, thus accusations by disfavored are known to get little attention.

The section that follows analyzes the effect of this social structure on trade governance.

3. THE HYPOTHESIS

This section discusses how trade governance affects the choices made by buyers.[6]

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[4]The Tutsi of Sud Kivu are a semi-nomad pastoralist people, locally denominated Banyamulenge, composing between 4% and 13% of the population of Sud Kivu (Prunier (2008) and this study). The largest migration wave from Rwanda is believed to be in the 19th century (Jackson (2006)). Due to historical contact with Bantu groups in Rwanda, Banyamulenge have Bantu genetic heritage but, unlike the other Bantu, they have Nilo-Saharan genetic heritage. While their homeland is in Uvira, Fizi, and Mwenga, they do not have a recognized political unit. Riots against Tutsi are frequent. Vlassenroot (2002) noted: *"The attempts to prevent the Banyamulenge from gaining access to the political realm [...] explain their marginalised position"* and *"[the community of Tutsi pastoralists] was never treated as [a distinct community], either by the colonial administration nor by the independent state."*

[5]So-called "La Roche" agreements require the Governor to consult customary chiefs ("Bami") to allocate Ministries. Two provincial bureaucrats said: *"One has to be Bashi [in this Ministry] to obtain the good positions. The others receive no consideration."* and *"I am [Ba]Havu, it is an opportunity for me to place two brothers [...] We are one."* Source: Sud Kivu provincial administration census I conducted in 2017.

[6]In a webpage appendix (Sánchez de la Sierra (2021)), I developed a microfounded theory that shares the predictions in this section.

Consider a merchant supplying a unit of an indivisible good at price $p > 0$ to a buyer. The buyer's payoff from consuming the good is $v \in \mathbb{R}$. She chooses $b \in \{C, E, R\}$, respectively denoting cheating (buy and defect), exchange (buy and pay), and reject, and yielding payoffs $u^C = v - \phi$, $u^E = v - p$, $u^R = 0$, where $\phi \geq 0$ is cost of cheating. She is a utility maximizer. If the cost of cheating is low, that is, if $\phi \leq p$, she cheats. If the merchant anticipates this and can choose whether to offer or not to maximize his own profit, he does not offer, that is, incentives to cheat destroy trade. I thus only analyze the buyer's incentives to cheat.

Trade governance is anything that exogenously increases ϕ. Consider the buyer's incentive if she received an offer. Suppose that $v \geq p$, thus, $u^R \leq u^E$, the buyer never rejects. If, and only if, the cost of cheating is sufficiently low, that is, when $\phi \leq p$, we have $u^E \leq u^C$, hence the buyer cheats. And if, and only if, it is high, $\phi > p$, we have $u^C < u^E$, hence the buyer exchanges. In sum, if $v \geq p$, trade governance dis-incentivizes cheating, and incentivizes exchange. Suppose that $v < p$, thus $u^E < u^R$, the buyer never exchanges. If, and only if, $\phi \leq p$, $u^R \leq u^C$, hence the buyer cheats. But if, and only if, $\phi > p$, $u^C < u^R$, hence she rejects. In sum, if $v < p$, trade governance discourages cheating and incentivizes reject. Thus, trade governance discourages cheating and encourages rejection and exchange.

Applied to the setting in Section 2, this generates predictions for the effect of formal contracts. When trade is with a contract recognized by the state contracting institutions, that is, a formal contract, I call that formal trade. I call trade otherwise informal trade. Whether trade is formal or informal is exogenous. Suppose that $\phi = \phi^I + \phi^F$. ϕ^I captures the costs society inflicts on cheating even when trade is informal.[7] ϕ^F is the penalty exogenously imposed by contracting institutions on the buyer upon cheating. That is, in informal trade $\phi^F = 0$. A stylized application to the setting is: (a) if the merchant is state-favored, $\phi^F > p$, otherwise, $\phi^F = 0$, that is, the buyer anticipates the bias of contracting institutions against disfavored merchant; (b) in trade within groups, $\phi^I > p$, but across groups, $\phi^I = 0$. This stylized structure yields the following two predictions.

1. *State bias.* If the merchant is state-favored, $\phi^F > p$, thus cheating in formal trade tends to be costlier than in informal trade, yielding less cheating, and either more rejecting and/or more exchange. If the merchant is disfavored, $\phi^F = 0$, whether trade is formal or informal has no influence on the buyer's choice.

2. *State enforcement with co-ethnic governance.* Suppose that the merchant is state-favored and that merchant and the buyer are non-co-ethnics. Without formal contracts, since $0 = \phi^I < p$, the buyer cheats. Formal trade makes cheating costlier since $\phi^F > p$, incentivizing exchange and/or reject. Suppose now that the merchant and the buyer are co-ethnics. Since $\phi^I > p$, the buyer does not cheat. Like in trade with a disfavored merchant, formal contracts do not influence buyer choices. But there is one difference: in co-ethnic informal trade, cheating is *already* dis-incentivized, and the buyer chooses reject or exchange.[8] I refer to the latter as co-ethnic governance.

In general, this framework has two implications for the empirical strategy.

First, I seek to test whether formal contracts decrease cheating, and increase exchange and rejection, in different samples: disfavored versus state-favored merchants, co-ethnic versus non-co-ethnic pairs. To characterize the role of ethnic groups, and distinguish state

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[7]In a webpage appendix (Sánchez de la Sierra (2021)), I show this can be endogenously generated from repeated interaction.

[8]I assume the costs of cheating are additive. The sections that follow show that the data support this assumption.

bias from state enforcement with co-ethnic governance, I also seek to test whether co-ethnic matching in informal trade decreases cheating, increasing exchange and rejection.

Second, one mechanism for state bias is that the state-favored people have anti-disfavored bias, and also control the state. But if state-favored do have anti-disfavored bias, among state-favored buyers, v may be lower whenever the merchant is disfavored—that is, there would be taste-based discrimination. Then, if state-favored/disfavored pairs are pooled with non-co-ethnic pairs, discrimination could confound co-ethnic governance. To identify state enforcement with co-ethnic governance and state bias, when introducing formal contracts, I seek two distinct sources of variation. Varying whether the merchant is state-favored isolates state bias. *Holding constant* whether the pair is state-favored and varying whether it is co-ethnic identifies the effect of co-ethnic governance on state enforcement. Furthermore, in informal trade, varying whether the pair is co-ethnic isolates co-ethnic governance. This is precisely the variation inherent in the context: a disfavored group, and state-favored set of various ethnic groups.

The next section presents the empirical strategy informed by this framework.

4. EMPIRICAL STRATEGY

The experiment identifies the benefit of formal contracts through trade governance.

The previous section showed that formal contracts influence the incentives of buyers to reject trade. Thus, whether buyers defect in naturally occurring trade could reflect selection of buyers into trade. If contracts increase the cost of cheating, buyers who do not value trade may cheat in informal trade but reject formal trade. These buyers would be out of sample in naturally occurring formal trade. That is, formal trade is a self-selected sample.

To overcome this challenge, I create a market. This allows to control the actions of merchants, and to observe the actions of all buyers in different trade environments—including the action of opting-out, which would otherwise be unobserved and create selection in naturally occurring trade. I experimentally vary formal versus informal offers, disfavored versus state-favored merchants, and, in that case, co-ethnic versus non-co-ethnic buyer-merchant pairs.

However, even with random assignment to contracts, a selection problem remains. Indeed, the requirement to sign a contract may affect the buyer's payoffs beyond its effect on the anticipated cost of cheating. Some buyers may interpret the need to sign the contract as a signal of the merchant's preference for punishing or, of merchant's trust. Or they may interpret it as a cue of prevalent social norms. Or they may have a taste for or against signing the contract. That is, even with random assignment of contract requirements, buyers may voluntarily self-select into formal trade for other reasons than the cost of cheating.

If these mechanisms exist, buyers who buy in formal trade could differ from those who buy in informal trade on unobserved characteristics that are relevant to the decision to pay. This would confound the comparison of formal and informal trade as trade governance.

Anticipating this selection problem, the experiment was designed so that the requirement to sign a contract, thus any signaling effect of contract, is held constant. I analyze the effect of accidentally lifting such requirement on the choice to reject, cheat, and exchange, and provide evidence against the contract inducing buyer self-selection. Then, I shut down all channels through which the contract leads buyers to buy. Specifically, I separately identify the effect of the contracts on the incentive to pay for the subset of

buyers who would buy in formal trade. This section presents the experiment design and the econometric strategy.

4.1. *Experiment Design*

I create a part-time door-to-door merchant job in Bukavu. The job is the outcome of a market study I conducted to determine market opportunities for selling household goods. I recruit young males of Bukavu for this job. Half of them are state-favored. Merchants keep the revenue from each sale and pay a marginal cost I impose. Households are located in three ethnically diverse neighborhoods of Bukavu. These are sub-divided into 20 avenues.[9]

First, I partition the set of merchants into teams of two merchants. Each team of merchants is composed exactly of one state-favored merchant, and one disfavored merchant.

Second, I partition the set of avenues, and randomly allocate each avenue to a team.

Third, in each avenue assigned to a team, I randomly allocate the households between the two merchants of that team. If a household is unavailable, it is replaced at random.[10]

Fourth, in each household allocated to a merchant, the merchant randomly samples one available adult, whom I refer to as the buyer, using a list of pre-determined random numbers.

Fifth, the merchant sequentially makes the following *two* independent offers to the buyer.

4.1.1. *The Placebo Sale*

Presentation of the placebo offer. The merchant offers 3 USD worth of known mobile airtime phonecards. The buyer can inspect the phonecards, hence there is no room for merchant fraud. Price is negotiable, but I encouraged merchants to sell for 2 USD. Merchants largely complied to this requirement, hence average sale price is 1.95 USD.[11] To receive the mobile airtime now, the buyer must pay now, leaving no room for buyer fraud.

Buyer's choice in the placebo. The buyer can only accept or reject this offer. If she rejects, denoted $b^0 = R$, the sale is over. If she accepts, denoted $b^0 = E$, for exchange, she simultaneously gets the cards and pays, with no room for buyer or merchant cheating.

The placebo sale ends here. Independently on the choice made by the buyer in the placebo sale, the merchant then makes a second offer to *all* buyers, which I describe next.

4.1.2. *The Sale on Credit Offer*

Presentation of the sale on credit offer. The merchant offers again 3 USD worth of airtime, for the price of 2 USD. As in the placebo, merchants have limited discretion to bargain, hence the mean price was 1.92 USD. The presentation of this offer is identical

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[9]The experiment took place in November 2013 in Nguba I, Nguba II, and Irambo neighborhoods, composed of 7, 8, and 5 avenues. For security, merchants are males. I refer to the merchant as "he" and the buyer as "she."

[10]Due to their larger size, six of the 20 avenues were allocated to two teams of merchants, rather than one. In each avenue, I sampled an equal proportion of households by geographic random selection.

[11]Daily per capita income in D.R.C. in 2013 was 1.12 USD (World Bank (2020)). Merchants sell 300 units of Airtel. These are typically sold by 100 units for 1 USD each, thus, market value of a sale is 3 USD. Encouraged price was 2 USD because I found that a price higher than 2 USD would have led to a large number of rejections. The price has remained constant for at least the last ten years. This rules out that beliefs about future prices affecting buyer choices. For each sale of the 300 units of Airtel phone credit, I charged the merchant 1 USD for realism.

to the placebo's presentation, with one exception. Here, the merchant informs the buyer that, if she accepts it, she can pay within three days using a cell phone system, but must sign a formal contract. When presenting this offer, the merchant shows a contract template.[12]

Figure 1 depicts the timing of the sale on credit that follows the presentation of the sale.

The buyer's initial choice. After this presentation, the merchant asks the buyer to say whether she accepts this offer. Based on this presentation, the buyer tells the merchant whether she accepts, denoted $A = 1$, or rejects, denoted $A = 0$ (henceforth, initial choice).

Contract randomization. Independently of the buyer's initial choice, I randomize whether the requirement to sign the contract is maintained (denoted $\tilde{F} = 1$). If it is lifted (denoted $\tilde{F} = 0$), the merchant explains that he does not have other contracts than the template and that, in that case, his policy requires the merchant to proceed without the contract.[13] Both if it is lifted and if it is maintained, the merchant then implements the following step.

The buyer's ultimate choice. For buyers who initially accept, and for those who initially reject, the merchants offer the buyers to reconsider their initial choice.[14] The buyer's answer constitutes the "ultimate choice." If she ultimately rejects, denoted $b = R$, the sale ends. If, and only if, she ultimately accepts, denoted $b \neq R$, the following sequence of actions follows: a. the merchant supplies the airtime; b. if, and only if, the requirement was assigned to be maintained, the merchant asks the buyer to sign and return the contract immediately; c. the buyer has three days to pay (where she may choose to not pay). If she pays, I denote her ultimate choice $b = E$, indicating that she chose to exchange, and if she does not pay, I denote it $b = C$, indicating that she cheated. If her ultimate choice was $b = R$, reject, she has no room to pay nor to defect.

4.2. *Data Sources*

The merchant records the buyer's initial and ultimate choices. The supervisor records the payments. At the end of the sale, the merchant administers a short exit survey to the buyer.

After the study ended, I conducted: (1) a survey of merchants; (2) a survey to an additional random sample of 500 households in these avenues. In that survey, surveyors show to the respondent the individual photos of each merchant and verbally describe the sale on credit. I gather the respondents' beliefs about each merchant shown and about the sale on credit (henceforth, "perceptions survey"); (3) debriefing calls with merchants and buyers.

I first describe the characteristics of the merchants whom I hired. Merchants tend to live in a household of 2–3 individuals, be around 30 years old, and have around 13 years

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[12]The buyer could instead meet the merchant in town within three days, pay in cash, and get a receipt. The supervisor recorded the payments. A lawyer drafted the contract, which the province's Interior Ministry stamped.

[13]Randomization is implemented as follows. The merchant privately examines a pre-determined randomization roster. Randomization is within avenue and merchant. The lifting of the contract requirement is presented as an omission on the side of the merchant, and the merchant says that it is corporate policy to continue without it.

[14]If the buyer is assigned to maintained, the merchant simply asks whether she would like to reconsider her choice. Only buyers who initially rejected, and who were assigned to lifted, reconsidered their choice.
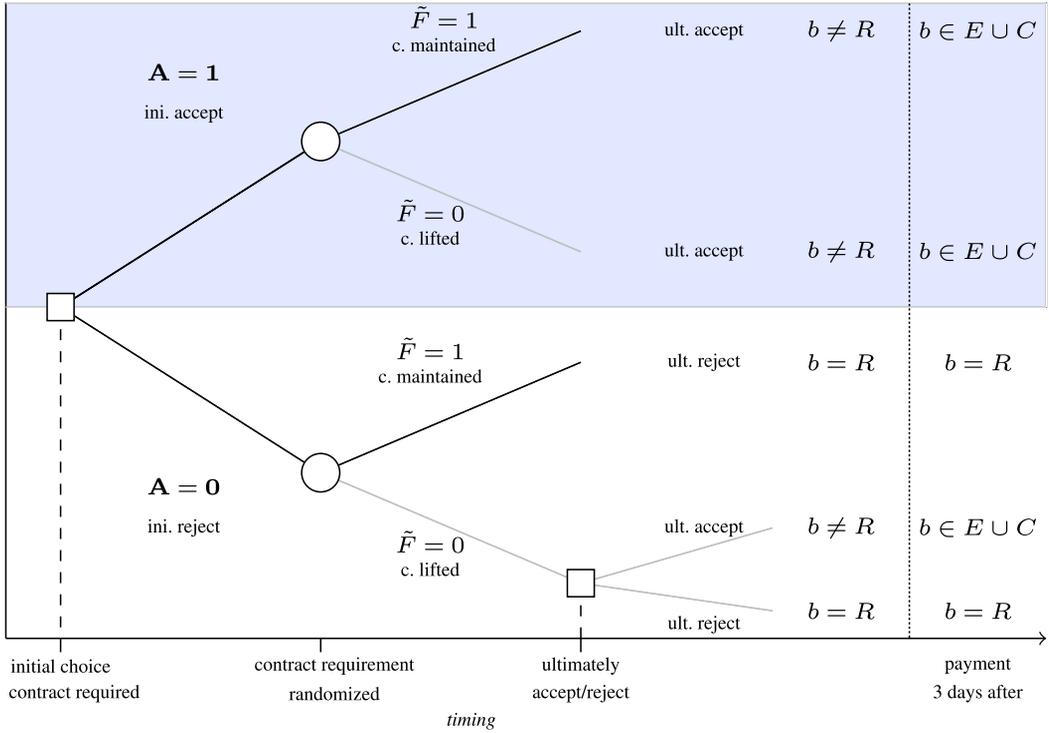
FIGURE 1.—The sale on credit—timing of buyer choices and contract randomization. *Notes*: The merchant informs the buyer that she can pay within three days but must sign a formal contract. The buyer says whether she accepts the offer (initial choice, denoted $A = 1$ and $A = 0$). Independently of the buyer's initial choice, I randomize whether the requirement to sign the contract is maintained ($\tilde{F} = 1$) or lifted ($\tilde{F} = 0$) ("contract requirement randomized"). If it is lifted, the merchant informs the buyer that he does not have enough copies and the sale will proceed without a contract. Finally, the merchant allows the buyer to reconsider her initial choice ("ultimately accept/reject"). Buyers for whom the requirement is maintained, or who initially accepted and the contract was lifted, stuck to their initial choice. Thus, the figure omits these decision nodes. $b \in \{C, E, R\}$ is the representation of the buyers' ultimate choices. Only buyers who chose $b \neq R$ can choose E or C.

of education. They worked part time: 5% had some public office, 45% were students, 6% had a private sector occupation, and 33% worked for an NGO in some capacity. Buyers correctly guessed the merchants' main occupation in the exit survey. By descent, 40% were state-favored Bashi, 15% were state-favored other groups, and the remainder were disfavored, that is, Tutsi (source: survey of merchants, see Table A.I, Panels A and B in the Supplemental Material (Sánchez de la Sierra (2021))).[15]

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[15]The ethnic groups of the rest of state-favored merchants were distributed as follows: 20% were Bahavu and 10% were Babembe, similar to the proportions in the respondent sample in the perceptions survey. I code the merchant's ethnic group based on social descent. Since buyers' perceptions about the merchant's identity may vary, using the buyers' self-reported perception about the merchant's identity would introduce omitted variable problems. Thus, I assigned identity of each merchant at the merchant level. For all merchants, social identity, how they are recognized in society, was identical to the identity by descent. There was one exception. One merchant, who was disfavored by descent, had stark Bashi traits and was accepted as a Bashi. His social identity was state-favored. Reflecting this, while 20% of buyers indicated in the exit survey that they believed he was disfavored, 80% believed he belonged to the state-favored ethnic groups. Of these, 77%, including

TABLE I

DESCRIPTIVE STATISTICS OF BUYERS AND EXPERIMENTAL BALANCE[a]

	All	$\tilde{F}=1$	$\tilde{F}=0$	S	D	$S\{N\}$	$S\{C\}$	pval.	pval.	pval.
Observations	958	472	486	552	406	248	171			
1. Pre-Characteristics										
Household size	3.50	3.53	3.47	3.47	3.53	3.50	3.40	0.58	0.59	0.58
Age	31.09	30.80	31.37	31.63	30.37	31.95	31.58	0.34	0.03	0.67
Female	0.40	0.41	0.39	0.40	0.41	0.49	0.36	0.48	0.71	0.01
Years of education	11.77	11.86	11.67	11.61	11.97	11.15	12.10	0.45	0.17	0.02
Occupation: public	0.11	0.10	0.11	0.14	0.05	0.16	0.11	0.66	0.00	0.15
State-favored: *Bashi*	0.47	0.46	0.48	0.48	0.46	0.33	0.93	0.65	0.54	0.00
State-favored: *others*	0.38	0.38	0.38	0.42	0.34	0.67	0.07	0.91	0.02	0.00
Disfavored	0.15	0.16	0.14	0.11	0.20	0.00	0.00	0.43	0.00	—
Replacement household	0.25	0.25	0.25	0.30	0.17	0.30	0.25	0.94	0.00	0.30
2. Experiment design validity										
Sees sale as strange	0.04	0.05	0.03	0.04	0.04	0.04	0.04	0.16	0.71	0.64
Contract is suspicious	0.00	0.01	0.00	0.00	0.01	0.00	0.00	0.09	0.02	—
Contract signals state connection	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.71	0.89	0.21
Contract signals seriousness	0.10	0.11	0.08	0.11	0.07	0.07	0.16	0.21	0.09	0.01
Contract signals low trust	0.06	0.08	0.03	0.08	0.02	0.08	0.03	0.00	0.00	0.02
Objective is research on trustworthiness	0.04	0.04	0.04	0.04	0.05	0.04	0.02	0.92	0.28	0.18
Objective is research on ethnic groups	0.02	0.02	0.02	0.02	0.02	0.04	0.00	0.52	0.69	0.01
Objective is scoping a new market	0.07	0.06	0.08	0.09	0.05	0.08	0.09	0.48	0.02	0.69
Has previously heard of this sale	0.07	0.05	0.08	0.05	0.09	0.06	0.02	0.16	0.02	0.02
3. Beliefs about merchant's identity										
Seen as state-favored	0.66	0.67	0.65	0.97	0.24	0.98	0.99	0.37	0.00	0.71
Seen as co-ethnic	0.31	0.32	0.31	0.34	0.13	0.20	0.66	0.82	0.00	0.00
Buyer's subjective well-being	0.00	0.02	−0.01	0.12	−0.18	0.14	0.11	0.70	0.00	0.74
4. Ultimate choices										
Rejects the placebo offer, $I_R(b_i^0)$	0.59	0.61	0.58	0.58	0.62	0.59	0.54	0.39	0.30	0.34
Rejects the sale on credit, $I_R(b_i)$	0.28	0.32	0.24	0.27	0.30	0.26	0.33	0.01	0.25	0.12
Buys and defects in sale on credit, $I_C(b_i)$	0.55	0.51	0.59	0.56	0.53	0.58	0.44	0.02	0.34	0.01
Buys and pays in sale on credit, $I_E(b_i)$	0.17	0.17	0.17	0.17	0.17	0.16	0.22	0.89	0.91	0.12
5. Beliefs about sanctions for cheating										
Reputation loss	0.10	0.10	0.09	0.10	0.10	0.05	0.16	0.51	0.87	0.00
Psych. cost (shame)	0.13	0.13	0.12	0.15	0.09	0.21	0.10	0.54	0.01	0.00
Violence (public beating)	0.03	0.04	0.03	0.04	0.03	0.03	0.03	0.24	0.75	0.86
Legal prosecution	0.18	0.23	0.13	0.24	0.10	0.22	0.23	0.00	0.00	0.70
Can contract lead to prosecution? (all)	0.51	0.61	0.43	0.55	0.46	0.67	0.42	0.00	0.01	0.00

[a]Columns ($\tilde{F}=1$)–($S\{C\}$) report, respectively, the means of buyers assigned to contract lifted ($\tilde{F}=0$), contract maintained ($\tilde{F}=1$), those matched to a state-favored (S) or a disfavored merchant (D), and state-favored buyers-merchant non-co-ethnic pairs ($S\{N\}$) and state-favored buyer-merchant co-ethnic pairs ($S\{C\}$). Columns with headers *pval.* report, respectively from left to right, the p-value of the test of equality of means between columns $\tilde{F}=1$ and $\tilde{F}=0$, S and D, $S\{N\}$ and $S\{C\}$. Sources: exit survey and buyers' choices.

Table I, Panel 1, shows the pre-characteristics of the subjects, the buyers. Their household size is 3–4 individuals on average, they are 31 years of age on average, 40% of them are female, have 11.8 years of education, and 11% are state officials. Ethnic composition of buyers is as follows: 47% belong to the state-favored Bashi ethnic group, 28% to other

Bashi buyers, believed that he was a Bashi. Thus, in the remainder of the analysis, this merchant is coded as a state-favored merchant.

state-favored ethnic groups, and 15% are disfavored.[16] Twenty-five percent are a random replacement.

4.3. *Experimental Balance*

Columns ($\tilde{F} = 1$)–($S\{C\}$) report, respectively, the means of buyers assigned to lifted ($\tilde{F} = 0$) or maintained ($\tilde{F} = 1$), to a state-favored (S) or a disfavored merchant (D), and state-favored buyers-merchant pairs that are non-co-ethnic ($S\{N\}$) or co-ethnic ($S\{C\}$).

It is reassuring that all buyer pre-characteristics are balanced across $\tilde{F} = 1$ versus $\tilde{F} = 0$.

Pre-characteristics are also fairly balanced between buyers in S and those in D, with two telling exceptions.[17] First, buyers with a state-favored merchant are 9 pp. more often a state official. Second, buyers with a state-favored merchant are 9 pp. less often disfavored.

These two imbalances reflect precisely the source of state bias: the state-favored discriminate against the disfavored. Disfavored merchants feared talking to state officials, and disfavored households feared talking to state-favored merchants. The latter implied that households are 13 pp. more often a replacement when the merchant is disfavored.[18] Since replacements are random, the sample of state-favored civilian buyers is a random sample of state-favored civilians. Thus, in the analysis that follows, I also show the results including only this sample as robustness. Third, buyers matched to a state-favored merchant are one year older. This difference vanishes when the analysis is restricted to state-favored households.

Pre-characteristics of buyers are also fairly balanced between buyers in a non-co-ethnic ($S\{N\}$) or a co-ethnic ($S\{C\}$) match. Among the 419 state-favored/state-favored matches, 171 (41%) are co-ethnic and 248 (59%) non-co-ethnic.[19] There are no significant differences in household size, age, public occupation, or replacement status. Buyers in a co-

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[16]The ethnic groups of buyers with recorded ethnic group are: 47% Bashi, 13% Banyamulenge, 8% Balega, 5.7% Bahavu, 4.8% Bafuliiru, 3.3% Babembe, 2.5% Bangubangu, 1.3% Batembo. The proportions in the random sample of perceptions survey respondents are comparable: 57 % Bashi, 8.5% Bahavu, 4% Babembe.

[17]This sample is obtained as follows. There are 958 transactions recorded. Of these, 59 cannot be linked to their exit survey because of identifiers. Of the 899 that can, 25 have no data on buyer's mother tongue. That is, a total of 84 transactions have missing data on the buyer' tongue. To palliate this, local consultants examined the name of the buyers among these 84 and were able to identify 9 of them as being unambiguously disfavored, two of them had names that could belong to both state-favored and disfavored, and the rest were unambiguously state-favored. This recovered the mother tongue of 4 out of the 25 with exit survey data but missing language data. For Panel 1, the means are derived from the 899 transactions with exit survey data. For the specific ethnic group variable, the analysis uses the sample of 878 buyers with known ethnic group (874), or recovered (4).

[18]Reflecting that disfavored merchants were fearful to talk to state-favored officers, 87% and 60% of buyers who are state officials are state-favored respectively for state-favored and disfavored merchants.

[19]The co-ethnic versus non-co-ethnic sample is obtained as follows. First, of the 958 buyers, I exclude the 122 for whom the variable "mother tongue" indicates that they are disfavored. I do so for the analysis of co-ethnicity as announced in Sections 2 and 3 to separate the bias against the disfavored from co-ethnic mechanisms among the state-favored. Second, of the remaining 836, I exclude 59 that have no exit survey data, and no ethnic group variable; 5 of those were identified in secondary name check as being disfavored and thus necessarily excluded—the remaining 54 are state-favored, but their ethnic group cannot be recovered. Third, of the 777 buyers in this sample, 470 are matched to a state-favored merchant. Fourth, of the 470 state-favored merchant matches, 22 had no information on their mother tongue (4 of which were identified in secondary name check as disfavored), and 29 had mother tongue Swahili. While Swahili distinguishes the state-favored groups, it does not identify the specific ethnic group. The resulting 419 are those among the 470 for whom the ethnic group can be identified.

ethnic pair are 13 pp. less often a female and have 0.95 more years of education. Representative of Bukavu, Bashi buyers are 93% of co-ethnic and 33% of non-co-ethnic pair.[20]

In sum, the pre-characteristics of buyers are fairly balanced across treatment arms. In the analysis that follows, I control for those that are imbalanced in each treatment respectively.

4.4. *External Validity of Experiment Design*

In this section, I show that buyers perceive the sale as naturally occurring trade.

Panel 2 in Table I presents the mean of buyers' beliefs about the sale. The variables in this panel are obtained from the exit survey, administered at the end of the transaction.

One concern is that buyers may perceive the sale unnatural, or even strange. In the exit survey, buyers who ultimately rejected were asked why they rejected. Variable *"Sees sale as strange"* shows that only 4% of buyers rejected and reported the sale to be strange.

Another concern is that, since the economy is largely informal, the requirement could signal other attributes of the sale. Variables *"Contract is suspicious"*–*"Contract signals low trust"* show the buyers' answers to the question: *"how did the requirement to sign this contract change your image about me?"*; 0% (i.e., 3 buyers) reported that they found the requirement suspicious, and 1%, 10%, 6% indicated that it signaled a connection with the state, seriousness, and low merchant trust, respectively. These are small fractions. They provide reassurance that signaling channels cannot be a concern for external validity.

Another concern is that the buyers could perceive the sale to be part of a study about trustworthiness or ethnicity. Variables *"Believes objective is..."* use the buyers' answers to the open-ended question in the exit survey: *"what do you think is our objective?"* I coded the answers. Only 4% indicated that the sale was related to studying trust, 2% mentioned ethnic groups; 7% believed it was related to scoping a new market by cell phone companies.

A remaining concern is that buyers could have talked to their neighbors about the sale, creating information spillovers. The first variable is whether buyers had previously heard of the sale (*"Has previously heard of this sale"*). Only 7% had. Of those, one mentioned they heard of a contract, six a survey, and 94% only mentioned sale made on credit.

The perceptions survey answers, shown in Table A.II, Panel A, also support that the sale is natural: 60% of the respondents found the sale on credit to be normal. On average, perceptions survey respondents received door-to-door sales 142 days per year.

In sum, this analysis indicates that the buyers perceived the transaction as being natural.

4.5. *Econometric Strategy*

Let i index buyers. For each ultimate choice $b \in \{C, E, R\}$ in the sale on credit, let $I_b(b_i) \in \{0, 1\}$ be an indicator variable taking value 1 if buyer i ultimately chose b and value 0 otherwise. Let $l_i \in \{SC, SN, SD, D\}$ be the ethnic composition of buyer i's buyer-merchant pair, where SC, SN, SD, D denote respectively a pair in which the merchant and the buyer are state-favored and from the same ethnic group, one in which

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[20]This proportion corresponds to random matching in this population. Consider the probability that a match is Bashi–Bashi, 55%. Let $p_M^B = 73\%$ be the probability that a state-favored merchant is Bashi. Let $p_H^B = 73\%$ be the probability that a state-favored buyer is Bashi. By random assignment, the probability that a randomly selected match is Bashi–Bashi is: $p^{BB} = p_M^B \times p_H^B = 0.53$. Similarly, the probability that a match is any other state-favored co-ethnic is $0.27 \times 0.27 = 0.073$. Thus, the probability that a co-ethnic pair is Bashi–Bashi is $0.53/(0.53 + 0.073) = 0.88$.

the merchant and the buyer are state-favored but from different ethnic groups, one in which the merchant is state-favored and the buyer is disfavored, and one in which the merchant is disfavored. I use S to denote whether the merchant is state-favored, thus $S = SC \cup SN \cup SD$. Let $\tilde{F}_i \in \{0, 1\}$ be an indicator for whether buyer i was assigned to lifted or maintained.

4.5.1. *Analysis Including Selection*

I first analyze the buyers' ultimate choices in the sale on credit, $b \in \{C, E, R\}$. To estimate the effect of the contract, I analyze ultimate choices in the sale on credit, $b \in \{C, E, R\}$, for buyers assigned to lifted and compare them to those of buyers assigned to maintained. Specifically, I estimate the following equation with OLS:

$$I_b(b_i) = c_b^{\tilde{F}} + a_b^{\tilde{F}} \tilde{F}_i + \mathbf{X}_i' \Gamma^{\tilde{F}} + u_{b,i}^{\tilde{F}}. \tag{1}$$

Motivated by state bias in the setting, I estimate equation (1) for each of the following four samples $l_i \in \{S, D, SN, SC\}$, first for $l_i \in \{S, D\}$, and then for $l_i \in \{SN, SC\}$:

(i) the sample of buyers assigned to a state-favored merchant, $l_i \in S$, and the sample of buyers assigned to a disfavored merchant, $l_i = D$. In the sample $l_i \in S$, the coefficient $a_b^{\tilde{F}}$ estimates the effect of assignment to maintained on the probability that the buyer chooses b in the sample $l_i \in S$. If buyers anticipate that state-favored merchants can enforce formal contracts, state bias implies that signing the contract in $l_i \in S$ decreases cheating, and increases exchange and rejection. I thus seek to test whether, in this sample ($l_i \in S$), $a_C^{\tilde{F}} < 0$, $a_E^{\tilde{F}} > 0$, $a_R^{\tilde{F}} > 0$. If buyers anticipate that disfavored merchants *cannot* enforce the formal contract, state bias implies that, in $l_i = D$, $\tilde{F}_i$ has no effect on cheating, exchange, nor rejection. I thus seek to test whether, in sample $l_i = D$, $a_C^{\tilde{F}} = 0$, $a_E^{\tilde{F}} = 0$, $a_R^{\tilde{F}} = 0$. In both cases, the vector of buyer-level covariates, $\mathbf{X}_i'$, includes age, an indicator for whether the buyer is disfavored, an indicator for whether the buyer is a replacement, an indicator for whether the buyer is a public servant, and randomization blocks for assignment to maintained versus lifted (an indicator for each merchant, and an indicator for each avenue), as shown in Section 4.

(ii) the sample of buyers in co-ethnic pairs, $l_i = SC$, and that of buyers in non-co-ethnic pairs, $l_i = SN$. If co-ethnic governance already governs cheating, the effect of signing the contract in $l_i \in SC \cup SN$ will arise from $l_i = SN$. I thus seek whether $a_C^{\tilde{F}} < 0$, $a_E^{\tilde{F}} > 0$, $a_R^{\tilde{F}} > 0$ if $l_i = SN$, and $a_C^{\tilde{F}} = 0$, $a_E^{\tilde{F}} = 0$, $a_R^{\tilde{F}} = 0$ if $l_i = SC$. The vector of buyer-level controls is that in (i) and, in addition, indicators for gender and for Bashi buyer, and years of education.

If $a_b^{\tilde{F}}$ cannot be distinguished from zero in a sample defined by l_i, this could mean that contracts do not increase the cost of cheating. But it could also mean that trade is already informally governed in l_i. To isolate informal governance in l_i, I analyze ultimate choices with $\tilde{F}_i = 0$. If trade in l_i is already informally governed, trading in l_i will exhibit less cheating, and more exchange and rejection than in $l_i' \neq l_i$. I estimate equation (2) with OLS:

$$I_b(b_i) = c_b^I + a_b^I I_l(l_i) + \mathbf{X}_i' \Gamma^I + u_{b,i}^I, \tag{2}$$

where $\mathbf{X}_i$ is a vector of buyer-level covariates and I indicates Informal trade regression. To identify co-ethnic and state-favored informal governance separately, I estimate (2) in:

(i) the sample of state-favored pairs $l_i \in SC \cup SN$. In that case, the explanatory variable is $I_{SC}(l_i)$, taking value 1 if buyer i is in co-ethnic pair, $l_i = SC$, 0 otherwise, $l_i = SN$. In

that case, the coefficient a_b^I estimates the effect of co-ethnic matching on the probability that the buyer chooses b. Co-ethnic governance implies that co-ethnic trade decreases cheating, and increases exchange and rejection, thus I seek to test $a_C^I < 0$, $a_R^I > 0$, $a_E^I > 0$. In that case, the vector of buyer-level covariates, $\mathbf{X}_i'$, includes an indicator for gender, one for Bashi buyer, and years of education. These are the imbalances in SC, as shown in Section 4.

(ii) the sample of *all* buyers and *all* merchants. In that case, the explanatory variable is $\boldsymbol{I}_S(l_i)$, taking value 1 if buyer i is matched with a state-favored merchant, $l_i \in S$, and 0 otherwise, $l_i = D$. In that case, the coefficient a_b^I estimates the effect of matching to a state-favored merchant on the probability that the buyer chooses b. If there is no state-favored informal governance, matching with a state-favored merchant should have no effect on buyer choices, thus I seek to test, in this sample, $a_C^I = 0$, $a_R^I = 0$, $a_E^I = 0$. In that case, the vector of buyer-level covariates, $\mathbf{X}_i'$, includes age, an indicator for whether the buyer is disfavored, an indicator for whether the buyer is a replacement, an indicator for whether the buyer is a public servant, and randomization blocks for assignment to a state-favored merchant (indicators for each merchant team, and for each avenue), as shown in Section 4.

4.5.2. *Analysis Excluding Selection*

To shut down the effect of the contract on buyer selection, I then condition the analysis of $b_i \in \{E, C\}$ *on buyers who initially accept*. The dependent variable is thus an indicator variable, $\boldsymbol{I}_E^A(b_i)$ for buyers who initially accept, taking value 1 if the buyer pays.

$(\hat{a}_E^{\tilde{F}})$ In each sample $l_i \in \{S, D, SN, SC\}$, $\hat{a}_E^{\tilde{F}}$ estimates the effect of maintained contract on whether buyers pay, conditional on the subset who would accept in formal trade with l_i.

$(\hat{a}_E^I)$ In the sample of buyers assigned to $\tilde{F}_i = 0$, when the explanatory variable is $\boldsymbol{I}_S(l_i)$, or $\boldsymbol{I}_{SC}(l_i)$, $\hat{a}_E^I$ estimates of the effect of, respectively, matching to a state-favored, or co-ethnic matching, on whether buyers pay in contract lifted, conditional, in the subset of buyers who would accept in formal trade with ethnic composition l_i.

I now discuss key identifying assumptions.

4.5.3. *Internal Validity: Identifying Assumptions*

1. Endogenous selection of buyers. The requirement to sign may affect the buyers' choice to ultimately accept through many channels, including taste for contracts. Since buyers are heterogeneous, if those who accept when it is lifted tend to have lower payoff from exchange, buyers assigned to maintained would exchange and reject more, thus cheat less often. If this channel is present only with state-favored merchants, this would confound the analysis of state bias. I tackle this channel in Section 5.1.1 (Figure 2). The analysis excluding selection holds selection constant, thus selection of buyers does not affect internal validity in that analysis.

2. Independent effect of lifting the requirement. Lifting the requirement could affect buyers' choices through other channels than switching to informal trade. For instance, *lifting* the requirement could affect the buyers' beliefs about the sale. Such beliefs could independently influence choices. Panel 2, in Table I, analyzes the buyers' beliefs about the sale.

First, variable "*Sees sale as strange*" shows that the fraction of buyers who reject and report that the sale is strange is indistinguishable between $\tilde{F}_i = 1$ and $\tilde{F}_i = 0$. Second, variables "*Contract is suspicious*"–"*Contract signals low trust*" show that the fraction of

buyers who view the contract as suspicious, as a signal of state connection, or as a signal of seriousness is indistinguishable for buyers assigned to lifted or maintained. The fraction of buyers who report that the requirement signals low trust is 5 pp. smaller for those assigned to lifted. Even if this belief could affect these buyers' choices, the fraction of affected buyers is small (5%). Third, variables "*Objective is research on trust*"–"*Objective is marketing*" shows that lifting the requirement does not change the buyers' perceived objective of the sale. Finally, buyers assigned to lifted are just as likely to have heard of the sale.

In sum, Panel 2 suggests that lifting is unlikely to create bias. Section 5.4.1 includes these variables as controls for robustness (see Figure 4).

3. Mis-attribution of ethnic composition. If buyers randomly mis-attribute l_i, $\hat{a}_b^I$ and $\hat{a}_b^{\bar{F}}$ will have attenuation bias. Imperfectly observing l_i also creates room for a second type of bias: the buyer can have incentives to mis-represent her ethnic group in order to avoid intra-ethnic sanctions, that is, "passing" (Nix and Qian (2015), Cassan (2015)). Since l_i uses the buyers' self-identified ethnic group, this can bias $\hat{a}_b^I$. I now analyze beliefs about ethnic group. Table I, Panel 3, suggests that l_i is observed.

First, while only 24% of buyers matched to a disfavored believe that the merchant is state-favored (D), 97% of buyers matched to a state-favored do (S). Similarly, 19% of state-favored buyers matched to a non-co-ethnic state-favored merchant believe that their merchant is a co-ethnic ($S\{N\}$). But 65% of the buyers matched to a co-ethnic do ($S\{C\}$).

Second, given anti-disfavored taste, if buyers guess the merchant's identity, state-favored buyers should feel more unease after interacting with a disfavored merchant. The third variable in Panel 3 is the standardized satisfaction with life scale, measured in the exit survey. It is known to negatively correlate with anxiety (Pavot and Diener (2013)). It is 0.3 sd. smaller in state-favored buyers matched to a disfavored. The difference is statistically significant. The difference is driven by buyers who guess whether the merchant is state-favored (unreported). It is zero between buyers in non-co-ethnic and co-ethnic pairs.[21]

Third, Table A.II, Panel B, shows the perceptions survey respondents' guesses about the merchants' ethnic groups. Respondents detected state-favored merchants for 88% of merchant photo × respondent interactions in which the merchant was state-favored. They detected disfavored merchants in 67% of interactions in which the merchant was disfavored.

Section 5.4.2 replicates the analysis using the buyers' belief of the merchant's group (see Figure 5), and refers to a bounding exercise in Section C that tackles strategic passing.

4. Taste for co-ethnic exchange. Ethnic composition l_i could affect the buyers' preferences for E, independently of its effect on the cost of cheating. Specifically, suppose that buyers have a taste for co-ethnic exchange. In that case, just as co-ethnic governance would predict, co-ethnic matching would increase exchange. But unlike co-ethnic governance would predict, it would decrease rejection. While this discussion allows to distinguish taste for co-ethnic exchange from co-ethnic governance, Section 5.4.3 analyzes the buyers' placebo choices to rule out taste for co-ethnic exchange (see Figure 5).

5. ANALYSIS OF THE EXPERIMENT DATA

I first describe informative patterns in the means of buyers' choices and beliefs.

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[21]Merchants told the research team that they prefer to use the ethnic tongue when possible. One indicated: *"Ethnicity matters a lot. Just starting the interview with a salutation in their own language changes everything."*

Table I, Panel 4, column *All*, shows that, while 59% of buyers rejected the placebo ($I_R(b_i^0)$), only 28% rejected the sale on credit ($I_R(b_i)$).[22] This divergence could reflect that buyers anticipate that they may defect in the sale on credit. Indeed, 55% of the buyers ultimately buy and defect ($I_C(b_i)$). Only 17% ultimately buy and pay ($I_E(b_i)$). Thus, the sale on credit choices are likely influenced by the buyers' anticipation of the cost of cheating.

What governs the cost of cheating? The means in Panel 4 show that, for state-favored buyer-merchant pairs, buyers in co-ethnic pairs ($S\{C\}$) are significantly less likely to cheat in the sale on credit than those in non-co-ethnic pairs ($S\{N\}$). Furthermore, buyers assigned to requirement maintained ($\tilde{F} = 1$) are more likely to reject and less likely to cheat than those assigned to requirement lifted ($\tilde{F} = 0$). This suggests that the cost of cheating is governed by co-ethnic matching and by state-enforced contracts.

The raw means of the buyers' self-reported beliefs support this interpretation. In Panel 5, variables "*Reputation loss*"–"*Legal prosecution*" are the answers to the open-ended question, asked to all buyers in the exit survey: "*if you (had accepted the sale on credit and) reneged, what consequences do you think you will (would) face?*" Variable "*Can contract lead to prosecution?*" is an indicator for whether the buyer answered "yes" to: "*Do you think that this contract (if you had signed it) could lead to a prosecution between us?*"[23] Co-ethnics are worried about reputation, those assigned to maintained about prosecution.

I now turn to formally analyzing the experiment data.

5.1. *Analysis Including Selection: Ultimate Choices and Buyer Beliefs*

This section formally tests the paper's hypothesis analyzing the buyers' ultimate choices. Recognizing that the choices exchange, cheat, and reject are interdependent, I analyze the buyers' ultimate choices, and present the results separately for each $b \in \{C, E, R\}$ side-by-side. I estimate equations (1) and (2), sequentially analyzing the samples laid out in Section 4.5 (respectively, (i) and (ii)). To complement this analysis, I also present, side-by-side, the analysis of buyers' beliefs about the sanctions that would ensue if they chose to cheat.

Table II estimates equation (1) in the sample of buyers matched to a state-favored merchant, $l_i \in S$ (Panel A), and in that of buyers matched to a disfavored merchant, $l_i = D$ (Panel B). It implements point (i) in Section 4.5 for equation (1) estimation. As before, $\tilde{F}_i \in \{0, 1\}$ is an indicator for whether buyer i is assigned to requirement maintained. In Panel A, the sample are the 511 buyers assigned to a state-favored merchant, $l_i \in S$. In Panel B, they are the 380 buyers assigned to a disfavored merchant, $l_i = D$.

Panel A shows that formal contracts are effective if the merchant is state-favored. The coefficients in columns (1)–(3) are $-0.16, 0.05, 0.11$ respectively, and their standard errors

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[22]The placebo sale was cancelled in the last 4 days due to budget limitations. The sample size is therefore 740.

[23]This variable differs from "formal prosecution likely" in two ways. First, it asks whether it is possible, not likely. Second, it only accommodates a yes or no answer. This makes prosecution more salient. Its mean is thus naturally higher. As this variable was added later than the rest, the interpretation of the means for this variable requires care. Analysis of the variable and debrief conversations suggest that 12 merchants coded "not applicable" for buyers who rejected the sale. Of these, seven merchants coded "not applicable" for buyers assigned to maintained. The rest used "not applicable" for "no" as instructed. In this table, I present the raw means of this variable without correcting for surveyors' measurement error. In the regression analysis that follows, when this variable is analyzed, I include controls for merchant dummies interacted with whether the buyer accepted and with assignment to maintained. Since it was asked to all buyers, I show only regression estimates of equation (2).

TABLE II

FORMAL TRADE—STATE BIAS (EQUATION (1)). C: CHEAT (BUY AND DEFECT); E: EXCHANGE (BUY AND PAY); R: REJECT[a]

| | Dependent: | Buyers' Choices | | | Buyers' Beliefs | | | |
| | | $I_C(b_i)$ | $I_E(b_i)$ | $I_R(b_i)$ | Rep. | Shame. | Viol. | Pros. |
Explanatory		(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Buyer-merchant pairs with state-favored merchants*: lifted vs. maintained*								
Requirement maintained, $\tilde{F}_i$		−0.16	0.05	0.11	0.03	−0.01	0.02	0.13
		(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.01)	(0.03)
Obs.		511	511	511	511	511	511	511
R^2		0.25	0.23	0.23	0.44	0.27	0.26	0.25
Mean dep.		0.64	0.16	0.20	0.08	0.15	0.03	0.17
Multinomial coeff.			0.73	0.91				
Multinomial SE.			0.29	0.25				
B. Buyer-merchant pairs with disfavored merchants*: lifted vs. maintained*								
Requirement maintained, $\tilde{F}_i$		0.01	−0.03	0.02	−0.01	0.04	0.01	0.03
		(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)
Obs.		380	380	380	380	380	380	380
R^2		0.17	0.09	0.24	0.37	0.29	0.14	0.31
Mean dep.		0.52	0.16	0.29	0.10	0.07	0.03	0.08
Multinomial coeff.			−0.18	0.06				
Multinomial SE.			0.30	0.27				

[a]0. Estimates from equation (1), estimated in samples described in (i) in Section 4.5 ($l_i \in S$ vs. $l_i = D$).
1. Dependent variables are in panel headers, coefficients in body of table, and standard errors are in parentheses.
2. Dependent variables are indicator variables taking value 1 for the following ultimate choices of the buyer in the sale of credit and zero otherwise, from left to right: buys and defects ($I_C(b_i)$), buys and pays ($I_E(b_i)$), rejects ($I_R(b_i)$), believes that the following sanctions would likely ensue if she defected: reputation loss (rep.), psychological costs (shame), violence (viol.), formal prosecution (pros.). Explanatory variable "Requirement maintained" is an indicator for whether the requirement to sign the contract is maintained for buyer i, $\tilde{F}_i \in \{0, 1\}$.
3. In Panel A, the sample is all buyers matched to a state-favored merchant ($l_i \in S$).
4. In Panel B, the sample is all buyers matched to a disfavored merchant ($l_i = D$).
5. Mean dep. reports the mean of the dependent variable in the comparison group of the corresponding sample.
6. Multinomial coeff. and Multinomial SE. report the logit coefficients and their respective standard errors in a multinomial logistic regression of buyer choice on the corresponding independent variable, base choice $b = C$.

0.04, 0.03, and 0.04. The means of the dependent variable are 0.64, 0.16, 0.20, showing that, if merchants are state-favored, assignment to maintained decreases cheating in 25%, and leads to increase exchange by 31% and rejection by 55%, all statistically significant effects at conventional levels. Table notes "Multinomial coeff." and "Multinomial SE." show that the multinomial coefficients are large and statistically significant, confirming the OLS result.

To support the mechanism for this effect, the next columns analyze the buyers' beliefs. Columns (4)–(6) show that formal contracts do not influence anticipation of informal sanctions. Specifically, assignment to maintained does not have an effect on whether the buyer expects to damage her reputation if she cheats (column 4), feeling shame (column 5), or being subject to violence (column 6). In contrast, column (7) shows that, while 17% of buyers assigned to a state-favored merchant mention prosecution if they are assigned to lifted requirement, this fraction increases to 30% if they are assigned to maintained requirement (the difference is significant at the 1% level). Thus, signing a formal contract, when the merchant is state-favored, leads buyers to expect that formal prosecution is more likely if they cheat.

TABLE III

FORMAL TRADE—STATE ENFORCEMENT WITH CO-ETHNIC GOVERNANCE (EQUATION (1)). C: CHEAT (BUY AND DEFECT); E: EXCHANGE (BUY AND PAY); R: REJECT[a]

| | | Buyers' Choices | | | Buyers' Beliefs | | | |
| | Dependent: | $I_C(b_i)$ | $I_E(b_i)$ | $I_R(b_i)$ | Rep. | Shame. | Viol. | Pros. |
Explanatory		(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. State-favored non-co-ethnic *buyer-merchant pairs: lifted vs. maintained*								
Requirement maintained, $\tilde{F}_i$		−0.24	0.07	0.17	0.05	−0.01	0.04	0.08
		(0.06)	(0.04)	(0.05)	(0.03)	(0.05)	(0.02)	(0.05)
Obs.		248	248	248	248	248	248	248
R^2		0.34	0.30	0.31	0.32	0.31	0.22	0.26
Mean dep.		0.69	0.13	0.18	0.03	0.20	0.02	0.18
Multinomial coeff.			1.21	1.42				
Multinomial SE.			0.45	0.38				
B. State-favored co-ethnic *buyer-merchant pairs: lifted vs. maintained*								
Requirement maintained, $\tilde{F}_i$		−0.10	0.06	0.04	0.08	0.01	0.00	0.23
		(0.08)	(0.06)	(0.08)	(0.05)	(0.04)	(0.02)	(0.06)
Obs.		171	171	171	171	171	171	171
R^2		0.26	0.40	0.23	0.50	0.42	0.37	0.36
Mean dep.		0.49	0.21	0.30	0.13	0.11	0.04	0.12
Multinomial coeff.			0.75	0.46				
Multinomial SE.			0.57	0.57				

[a]0. Estimates from equation (1), estimated in samples described in (ii) in Section 4.5 ($l_i = SN$ vs. $l_i = SC$).
1. Dependent variables are in panel headers, coefficients in body of table, and standard errors are in parentheses.
2. Dependent variables are indicator variables taking value 1 for the following ultimate choices of the buyer in the sale of credit and zero otherwise, from left to right: buys and defects ($I_C(b_i)$), buys and pays ($I_E(b_i)$), rejects ($I_R(b_i)$), believes that the following sanctions would likely ensue if she defected: reputation loss (rep.), psychological costs (shame), violence (viol.), formal prosecution (pros.). Explanatory variable "Requirement maintained" is an indicator for whether the requirement to sign the contract is maintained for buyer i, $\tilde{F}_i \in \{0, 1\}$.
3. In Panel A, the sample is all buyers in state-favored non-co-ethnic buyer-merchant pairs ($l_i = SN$).
4. In Panel B, the sample is all buyers in state-favored co-ethnic buyer-merchant pairs ($l_i = SC$).
5. Mean dep. reports the mean of the dependent variable in the comparison group of the corresponding sample.
6. Multinomial coeff. and Multinomial SE. report the logit coefficients and their respective standard errors in a multinomial logistic regression of buyer choice on the corresponding independent variable, base choice $b = C$.

Panel B estimates equation (1) in the sample of buyers matched to a disfavored merchant, $l_i = D$. In contrast to that in sample $l_i \in S$, the coefficient on maintained has no statistical or economic significant effect on buyer choices nor on their beliefs about prosecution.[24]

To summarize, the buyers' choices $b \in \{C, E, R\}$ confirm the state bias hypothesis. I now turn to the analysis of the second hypothesis: state enforcement with co-ethnic governance.

Table III estimates equation (1) for non-co-ethnic pairs, $l_i = SN$ (Panel A) and, separately, co-ethnic pairs, $l_i = SC$ (Panel B). It implements point (ii) in Section 4.5, equation (1).

Panel A shows that assignment to maintained has a large effect in non-co-ethnic pairs. Columns (1)–(3) show that the effect of formal contracts on $b \in \{C, E, R\}$ is concentrated in non-co-ethnic pairs. Columns (4)–(7) show that, for non-co-ethnic pairs, assignment to contract has no effect on informal sanctions but leads buyers to expect prosecution.

[24]Table A.III shows that the results are robust to excluding disfavored buyers. Figure A.1 in the Supplemental Material confirms that this result is not driven by a particular group.

In contrast, Panel B, columns (1)–(3), show that formal contracts have no effect on buyers' choices in co-ethnic pairs. Column (7) shows that assignment to maintained increases significantly the fraction of co-ethnic buyers who think prosecution is likely.

This shows that contracts are ineffective in co-ethnic pairs, yet buyers in co-ethnic pairs see the merchant just as able to enforce the contracts. This contrasts with trade with disfavored merchants, in which buyers' beliefs confirmed that the merchant cannot enforce the contract. This result suggests that formal contracts are ineffective among co-ethnics because ethnic groups already govern trade informally. I now examine this possibility.[25]

I now analyze informal trade to elicit whether co-ethnic governance already governs informal trade. I also analyze informal trade with state-favored versus disfavored merchant to rule out that the same mechanism explains the state bias result. Table IV, Panel A, estimates equation (2) in the sample of state-favored buyer-merchant pairs, $l_i \in \{SC, SN\}$ (point (i) in Section 4.5's description of equation (2)). The explanatory variable is an indicator for whether the pair is co-ethnic, $I_{SC}(l_i)$. Panel B estimates equation (2) in the sample of all buyers; the explanatory variable is $I_S(l_i)$ (point (ii) in same description).

Panel A suggests reputation governs informal trade within state-favored ethnic groups.

Columns (1)–(3) estimate equation (2) for each indicator for the buyer ultimate choice of C ($I_C(b_i)$), E ($I_E(b_i)$), R ($I_R(b_i)$) in sales assigned to lifted. The sample in those columns is the 207 state-favored buyers assigned to lifted matched to a state-favored merchant. The coefficients in columns (1)–(3) are -0.24, 0.10, 0.14 respectively, and their standard errors 0.09, 0.07, 0.08. The mean dependent variables are 0.69, 0.13, and 0.18. Thus, in the set of state-favored buyer-merchant matches, buyers in co-ethnic trade are 35% less likely to cheat, 77% more likely to buy and pay, and 78% more likely to reject, all statistically significant. Table rows denoted "Multinomial coeff." and "Multinomial SE." present the coefficients and standard errors from a multinomial logit. The conclusions from the multinomial model are identical.

In columns (4)–(8), I analyze buyers' beliefs to examine the mechanisms for this result.

Specifically, in columns (4)–(7), for each sanction, the dependent variable is an indicator taking value 1 if the buyer believes that cheating would trigger this sanction. Column (4) shows that co-ethnic trade triggers concerns for reputation. While only 3% of those in non-co-ethnic pairs mention concern for reputation were they to defect, 13% of those in co-ethnic pairs do, a 333% increase significant at the 5% level (p-value of 0.04 not reported). In contrast, columns (5) and (6) show that buyers in co-ethnic pairs are as likely to expect feeling shame, or fearing violence, as those in non-co-ethnic pairs.[26] Column (7) indicates that, while 18% of buyers in non-co-ethnic sales assigned to lifted mention legal prosecution, the proportion is 3 pp. lower and indistinguishable for buyers in co-ethnic pairs.

Would co-ethnics prosecute if a contract was available? The dependent variable in column (8) is an indicator for whether the buyer answered yes to: "*Do you think that this contract (if you signed it) could lead to prosecution?*" While 89% of buyers in a non-co-ethnic pair answer yes, only 59% of those in a co-ethnic pair do. This suggests that buyers in co-ethnic pairs believe that informal sanctions substitute for prosecution if they cheated.[27]

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[25]Figure A.2 shows that this result is not driven by any group.

[26]In addition, buyers in a co-ethnic pair are 23% more likely to mention "other," and this effect is statistically significant. While "other" did not allow for open explanations, debrief conversations indicate that "other" includes informal extortion meetings with the cheater and ethnic witchcraft rituals to identify and cast a spell on the cheater. The effect on reputation and "other" consequences combined is 0.20 and the coefficient on co-ethnic is significant at the 1% level. This suggests that these "other" threats are particularly strong among co-ethnics.

[27]Table A.IV shows that expecting intra-ethnic or formal prosecution curbs cheating, in a 2SLS estimation.

TABLE IV

INFORMAL TRADE—CO-ETHNIC GOVERNANCE (EQUATION (2)). C: CHEAT (BUY AND DEFECT); E: EXCHANGE (BUY AND PAY); R: REJECT[a]

| | | Buyers' Choices | | | Buyers' Beliefs | | | | |
Explanatory	*Dependent*:	$I_C(b_i)$ (1)	$I_E(b_i)$ (2)	$I_R(b_i)$ (3)	*Rep.* (4)	*Shame.* (5)	*Viol.* (6)	*Pros.* (7)	*Pros.** (8)
A. State-favored buyer-merchant pairs: co-ethnic vs. non-co-ethnic (requirement lifted)									
Coethnic pair, $I_{SC}(l_i)$		−0.24	0.10	0.14	0.10	−0.02	0.02	−0.03	−0.30
		(0.09)	(0.07)	(0.08)	(0.05)	(0.07)	(0.03)	(0.07)	(0.06)
Obs.		207	207	207	207	207	207	207	402
R^2		0.13	0.11	0.04	0.07	0.05	0.03	0.04	0.24
Mean dep.		0.69	0.13	0.18	0.03	0.20	0.02	0.18	0.89
Multinomial coeff.			1.23	1.05					
Multinomial SE.			0.59	0.48					
B. Buyer-merchant pairs with state-favored vs. disfavored merchant (requirement lifted)									
State-favored m., $I_S(l_i)$		0.07	−0.00	−0.07	−0.04	0.10	0.00	0.07	0.10
		(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Obs.		449	449	449	449	449	449	449	868
R^2		0.21	0.14	0.23	0.21	0.21	0.11	0.18	0.35
Mean dep.		0.52	0.19	0.38	0.10	0.07	0.03	0.08	0.63
Multinomial coeff.			−0.12	−0.53					
Multinomial SE.			0.32	0.29					

[a]0. Estimates from equation (2), in samples (i) ($l_i \in SC \cup SN$) and (ii) (all buyers), specified in Section 4.5.
1. Dependent variables are in panel headers, coefficients in body of table, and standard errors are in parentheses. 2. Dependent variables are indicator variables taking value 1 for the following ultimate choices of the buyer in the sale of credit and zero otherwise, from left to right: buys and defects ($I_C(b_i)$), buys and pays ($I_E(b_i)$), rejects ($I_R(b_i)$), believes that the following sanctions would likely ensue if she defected: reputation loss (rep.), psychological costs (shame), violence (viol.), formal prosecution (pros.), and whether the buyer believes that, were she to sign the contract, prosecution is a possible outcome (pros.*).
3. In Panel A, the sample is all buyers in state-favored buyer-merchant pairs ($l_i \in SC \cup SN$) and assigned to requirement lifted ($\tilde{F}_i = 0$). Explanatory variable *Co-ethnic pair* is an indicator for whether the pair is co-ethnic ($l_i = SC$), and zero if the pair is non-co-ethnic ($l_i = SN$), that is, the explanatory variable is $I_{SC}(l_i)$.
4. In Panel B, the sample is all buyers assigned to requirement lifted ($\tilde{F}_i = 0$). Explanatory variable *State-favored m.* is an indicator for whether buyer i is assigned to a state-favored *m*erchant, $I_S(l_i)$.
5. Mean dep. reports the mean of the dependent variable in the comparison group of the corresponding sample.
6. Multinomial coeff. and Multinomial SE. report the logit coefficients and their respective standard errors in a multinomial logistic regression of buyer choice on the corresponding independent variable, base choice $b = C$.

Panel B serves as a benchmark. The sample includes merchants and buyers from both the state-favored and disfavored groups, and the explanatory variable is $I_S(l_i)$. Columns (1)–(6) confirm that the state-favored set as a whole does not govern trade.[28] Columns (7) and (8) show that buyers believe prosecution by state-favored merchants would be conceivable if they signed a formal contract.

In sum, the analysis of buyers' ultimate choices and beliefs show that state bias and co-ethnic governance diminish the benefits for formal contracts. However, so far, I have assumed that the effect of the contracts on whether the buyer ultimately accepts the sale is due to their effect on anticipation of formal prosecution. If the requirement to sign the contract leads buyers to self-select on unobserved attributes that correlate with the deci-

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[28]In sales assigned to lifted, the choices and beliefs of buyers matched to a state-favored merchant are indistinguishable to those matched to a disfavored merchant. There is one exception. Consistent with anti-disfavored discrimination, column (5) shows that buyers matched to a state-favored merchant are more worried to experience shame if they defect, consistent with anti-disfavored bias. Yet, those buyers are not less likely to cheat.

sion to exchange, this could confound the previous analysis. In the section that follows, I directly account for selection of buyers based on their preferences for exchange.

5.1.1. *Potential Confounder: Requirement to Sign the Contract and Buyer Self-Selection*

Figure 2 analyzes whether the requirement to sign a contract selects buyers with a different preference for exchange. I focus on sales in which the merchant is state-favored.

In Panel A, the dependent variable is the choice in the placebo, $I_R(b_i^0)$, and the explanatory variable is an indicator for whether the buyer ultimately rejects the sale on credit, $I_R(b_i)$. I separately estimate this equation for assignment to maintained or lifted, $\tilde{F}_i$. The coefficient on $I_R(b_i)$ measures the selection effect of the requirement to sign. The placebo choices of buyers who ultimately reject the sale on credit, both when the requirement is lifted or maintained, cannot be distinguished from those that accept. This rules out that the requirement leads to a selected sample with significantly higher preference for exchange.[29]

In Panel B, I examine whether the state bias result is robust to controlling for this form of selection. Specifically, I estimate equation (1) in the sample of buyers matched to a state-favored merchant, for each buyer action $b \in \{C, E, R\}$, and I include as control an indicator for whether the buyer had rejected the placebo, $I_R(b_i^0)$. The effect of the contract remains large and significant. This reaffirms that buyer selection is unlikely to be a confound.[30]

This section has shown that the state bias and co-ethnic governance conclusions cannot be explained by selection of buyers with different preferences.

However, the requirement to sign the contract could affect the decision of the buyers to accept the sale for other reasons. In the next section, the design allows me to fully shut down all channels through which contracts may affect the selection of buyers.

5.2. *Analysis Excluding Selection: Do Buyers Who Initially Accept Pay?*

This section analyzes whether buyers who initially accept pay, or instead defect ($I_E^A(b_i)$). Figure 3 presents the OLS estimates from equations (1) and (2), in the sample of buyers who *initially accept*. I henceforth omit mention of initially accept.

Panel A presents the estimates of equation (1). It includes buyers assigned to $\tilde{F}_i = 1$ and those to $\tilde{F}_i = 0$.

In the left panel, I separately estimate equation (1) for buyers matched to a state-favored merchant (the two columns on the left) and for buyers matched to a disfavored merchant (the two columns on the right). Among buyers matched to a state-favored merchant and who initially accept, 20% of those assigned to lifted pay, against 31% of those

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[29]The theoretical framework suggests this will be the case. If the contract increases the cost of cheating, the requirement to sign the contract discourages buying for buyers with low utility from consumption who would otherwise cheat without a contract. As a result, the mass of buyers with willingness to pay higher than the price is will be larger when a contract is requested. The data support this prediction: the fraction who accept the placebo is 10 pp. higher among buyers who ultimately accept when a contract is requested than when it is not.

[30]Sánchez de la Sierra (2021) presented a companion study, implemented in rural areas of Sud Kivu, in which formal and informal placebo sales were implemented. Formal placebo sale requires the buyer to sign the exact same contract, specifying that it acts as a proof of completed exchange. The fraction who reject was indistinguishable in formal and informal trade. This suggests that it is unlikely that buyers in this experiment have a taste for contracts.
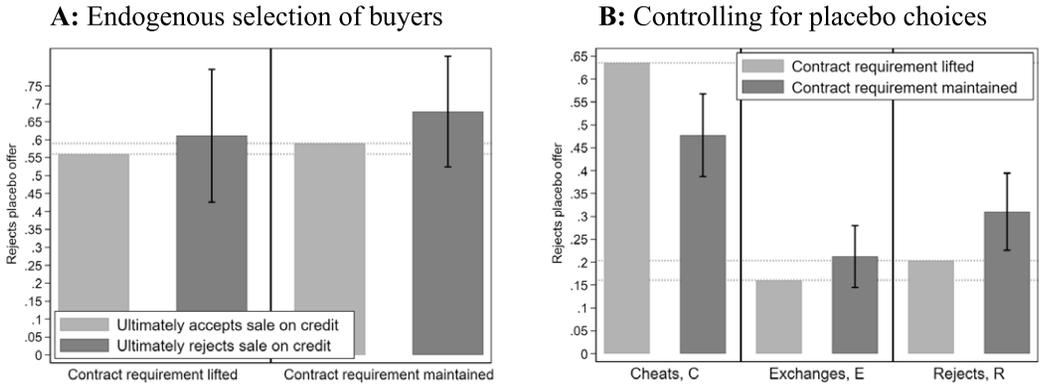
FIGURE 2.—Contract requirement leads buyers to self-select on preference for exchange? *Notes*: Both panels analyze the choices of buyers matched to a state-favored merchant, $l_i \in S$. Panel A uses as dependent variable the buyers' choices in the placebo, $I_R(b_i^0)$. I regress $I_R(b_i^0)$ on whether the buyer ultimately rejected the sale on credit, $I_R(b_i)$, separately for two samples: buyers assigned to lifted, $\tilde{F}_i = 0$, and buyers assigned to maintained, $\tilde{F}_i = 1$. Given $\tilde{F}_i$, the coefficient on $I_R(b_i)$ measures the difference in the mean rejection of the placebo sale between buyers who ultimately accept ($b_i \neq R$) and buyers who ultimately reject ($b_i = R$) the sale on credit. Black brackets indicate 95% confidence intervals on the level-adjusted coefficient for the indicator for whether the buyer ultimately rejected the sale on credit, $I_R(b_i)$. Panel B estimates equation (1), with dependent variable $I_b(b_i)$ for each buyer action $b \in \{C, E, R\}$ and the explanatory variable is assignment to lifted, $\tilde{F}_i = 0$, versus maintained, $\tilde{F}_i = 1$. I include as control an indicator for whether the buyer had rejected the placebo, $I_R(b_i^0)$. Black brackets indicate 95% confidence intervals on the level-adjusted coefficient on the indicator for contract maintained, $\tilde{F}_i$.

assigned to maintained. That is, assignment to maintained requirement to sign the contract increases the fraction of buyers who pay by 55% (11 pp.), and the increase is statistically significant at the 1% level. For buyers matched to a disfavored and who initially accept, assignment to lifted or maintained has no distinguishable effect. This confirms that there is state bias in the enforcement of contracts.

The right panel decomposes this effect of $\tilde{F}_i$ for state-favored buyer-merchant pairs, between co-ethnic, $l_i = SC$, and non-co-ethnic pairs, $l_i = SN$. In non-co-ethnic sales, only 16% of buyers assigned to lifted pay and 31% of buyers assigned to maintained do, a 15 pp. (94%) increase significant at the 1% level. In co-ethnic sales assigned to lifted, 30% of buyers pay. Since this is indistinguishable from co-ethnic and non-co-ethnic sales assigned to maintained, this suggests that state enforcement does not out-compete co-ethnic trade.

Panel B presents the estimates of equation (2) to confirm co-ethnic governance. The left panel first assures that the state-favored set as a whole does not govern informal trade. The sample is all the buyers assigned to lifted, $\tilde{F}_i = 0$, and the explanatory variable is $I_D(l_i) \in \{0, 1\}$. The right panel shows co-ethnic governance. The sample is all state-favored buyer-merchant pairs, $l_i = SC \cup SN$, assigned to lifted, $\tilde{F}_i = 0$, and the explanatory variable is $I_{SC}(l_i)$. In non-co-ethnic pairs, 16% of buyers pay. In co-ethnic pairs, 37% do (21 pp. higher), a 131% increase, significant at the 5% level. Ethnic groups govern within group trade informally.

5.3. *Robustness to Deviations Arising From Merchant Discretion*

I then examine whether the analysis excluding selection is robust to deviations in implementation arising from the discretion which, for realism, was given to the merchants.

A: *Formal vs. informal trade – State bias, and state enforcement with co-ethnic governance*
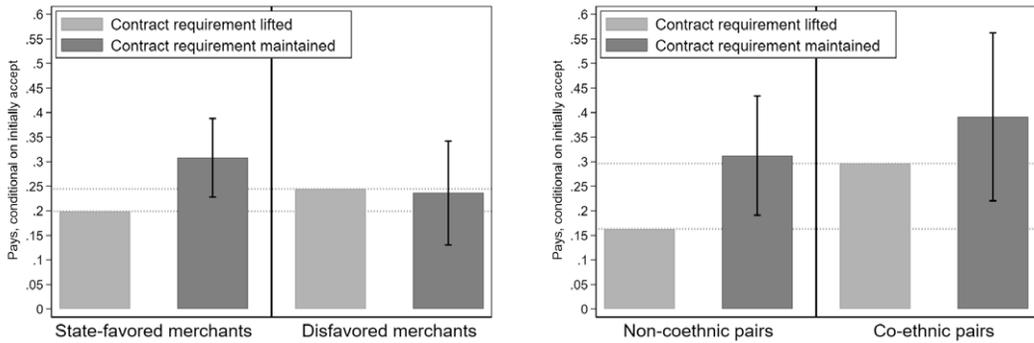
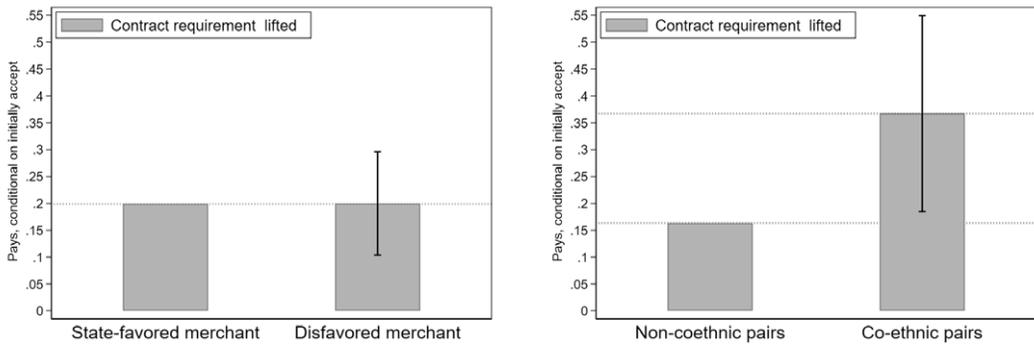**B:** *Informal trade – Co-ethnic governance*

FIGURE 3.—Analysis excluding selection—Main result. *Notes*: 0. The sample in both panels excludes buyers who initially reject and the dependent variable is whether buyer i pays or defects, $I_E^A(b_i)$. Left quadrants compare buyers matched to a state-favored/disfavored merchant ($l_i \in S$ vs. $l_i = D$), right quadrants compare buyers in non-co-ethnic pairs ($l_i = SN$) to those in co-ethnic pairs ($l_i = SC$). 1. Panel A reports the estimates of equation (1). The explanatory variable is an indicator for whether the buyer is assigned to maintained, $\tilde{F}_i$. In the left panel, the sample is (comparison (i) in description of (equation 1)): left quadrant includes all buyers matched to a state-favored merchant ($l_i \in S$), and right quadrant includes all that are matched to a disfavored ($l_i = D$). In the right panel, the sample includes only buyers in state-favored buyer-merchant pairs and is as follows (comparison (ii) in description of equation (1)): left quadrant is buyers in non-co-ethnic pairs ($l_i = SN$), and right quadrant is buyers in co-ethnic pairs ($l_i = SC$). 2. Panel B reports the estimates of equation (2) in the sample of buyers assigned to lifted, $\tilde{F}_i = 0$. In the left panel, the sample is all those buyers ((ii) in description of equation (2)), the explanatory variable is $I_D(l_i) \in \{0, 1\}$. In the right panel, the sample is buyers in state-favored buyer-merchant pairs ((i) in description of equation (2)), the explanatory variable is $I_{SC}(l_i) \in \{0, 1\}$. 3. Significance. Black brackets are 95% confidence intervals on $a^{\tilde{F}}$ (Panel A) and on a^I (Panel B).

First, despite the encouragement to stick to a fixed price sale, merchants had discretion to bargain. Merchants recorded the agreed price, and were intentionally not sanctioned for deviating from the recommended price. This ensured that the recording of prices was incentive compatible. I control for bargained price. Second, merchants could have incentives to skip the step in which they obtain "initially accept" in order to save time, and record it as ultimately accept (i.e., shirking). I thus estimate $a^{\tilde{F}}$ and a^I after correcting for bias that would arise if merchants skipped this step. For robustness, I make the extreme assumption that all buyers who ultimately accept only if the contract is lifted cheat. For

TABLE V

ANALYSIS EXCLUDING SELECTION—ROBUSTNESS TO MERCHANTS' DISCRETION[a]

	(1) $I_E^A(b_i)$	(2) $I_E^A(b_i)$	(3) $I_E^A(b_i)$	(4) $I_E^A(b_i)$	(5) $I_E^A(b_i)$	(6) $I_E^A(b_i)$	(7) $I_E^A(b_i)$	(8) $I_E^A(b_i)$	(9) $I_E^A(b_i)$
Requirement maintained, $\tilde{F}_i$	0.11	−0.01		0.15	−0.01		0.17	0.09	
	(0.04)	(0.05)		(0.05)	(0.07)		(0.06)	(0.08)	
Ethnic comp., $I_l(l_i)$			−0.02			-0.04			0.18
			(0.05)			(0.06)			(0.09)
Obs.	353	259	321	261	189	242	178	109	152
R^2	0.30	0.12	0.21	0.29	0.15	0.22	0.38	0.49	0.17
Mean dep.	0.20	0.24	0.24	0.22	0.25	0.25	0.16	0.30	0.16
Explanatory variable	$\tilde{F}_i$	$\tilde{F}_i$	$I_S(l_i)$	$\tilde{F}_i$	$\tilde{F}_i$	$I_S(l_i)$	$\tilde{F}_i$	$\tilde{F}_i$	$I_{SC}(l_i)$
Sample	S	D	$S \cup D$	$S \setminus SD$	D	$S \setminus SD \cup D$	SN	SC	$SN \cup SC$
Price coeff.	0.11	−0.02	−0.02	0.14	−0.03	−0.04	0.15	0.07	0.18
Price p	0.01	0.67	0.70	0.01	0.70	0.51	0.01	0.40	0.04
IV coeff.	0.12	−0.01		0.15	0.00		0.22	0.07	
IV p	0.00	0.80		0.00	0.96		0.00	0.35	
Adj. coeff.	0.08	−0.05	0.01	0.10	−0.05	−0.01	0.13	0.07	0.15
Adj. p	0.06	0.38	0.85	0.05	0.44	0.80	0.02	0.43	0.07
Diff. est		−0.13			−0.15			−0.09	
Diff. p		0.05			0.04			0.32	

[a]1. This table presents the estimates of equations (1) and (2), estimated in the sample of buyers who initially accept. The dependent variable is an indicator variable for whether the buyer pays, $I_E^A(b_i) \in \{0, 1\}$. Coefficients in body of table and standard errors are in parentheses. Columns (1), (4), (7) estimate $a^{\tilde{F}}$, columns (3), (6), (9) a^I. The explanatory variable is $\tilde{F}_i$ in columns (1), (2), (4), (5), (7), (8), $I_S(l_i)$ in (3) and (6), and $I_{SC}(l_i)$ in (9).
2. Sample. (1)–(3): all buyers; (4)–(6): state-favored civilian buyers; (7)–(9): state-favored buyers and merchants.
3. Mean dep. reports the mean of the dependent variable in the comparison group of the corresponding sample.
4. Price coeff. and Price p report coefficients and p-value adding controls for negotiated price.
5. Adj. coeff. reports the coefficient adjustment, computed with the extreme assumption that merchants systematically mis-code ultimate choice for initial choice. Adj. pval. reports the p-value of the bias-corrected coefficient.
6. IV coeff. and IV p are the second-stage coefficient and p-value in 2SLS. The endogenous regressor is whether the buyer actually signs the contract. The instrumental variable is an indicator for assignment to maintained.
7. Diff. est and Diff. p report the difference of the coefficient reported in that column (3, 6, or 9) and the preceding column (respectively, 2, 5, or 8), and the p-value of a Wald test for whether that difference is different from zero.

$a^{\tilde{F}}$, this provides a lower bound.[31] Third, merchants had discretion to relax the contract requirement. I thus instrument whether the buyer actually signs with assignment to $\tilde{F}_i$ in 2SLS.[32] I present this analysis in Table V, respectively showed in table notes "Price coeff," "Adj," and "IV coeff." Replicating this analysis in separate samples, the table also

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[31]With little algebra, the coefficient on $\tilde{F}_i$, $\hat{a}^{\tilde{F}}$, can be shown to be an unbiased estimator of $a^{\tilde{F}} + P_E(0, l_i) \frac{S(0,l_i)-S(1,l_i)}{S(0,l_i)S(1,l_i)}$, where $S(\tilde{F}, l_i) = P_C(\tilde{F}, l_i) + P_E(\tilde{F}, l_i)$ is the selection of buyers who ultimately accept the offer with $(\tilde{F}, l_i)$, $P_b(\tilde{F}, l_i)$, $b \in \{C, E, R\}$, is the probability that a randomly selected buyer chooses b in trade environment $(\tilde{F}, l_i)$, and thus $P_E(0, l_i) \frac{S(0,l_i)-S(1,l_i)}{S(0,l_i)S(1,l_i)}$ is the selection bias arising from mis-coding initially accept. Clearly, if $S(0, l_i) = S(1, l_i)$, there is no selection and the OLS estimate is an unbiased estimator of $a^{\tilde{F}}$. If there is selection, $S(0, l_i) > S(1, l_i)$. The biased-adjusted estimate uses empirical estimates of $P_E(0, l_i) \frac{S(0,l_i)-S(1,l_i)}{S(0,l_i)S(1,l_i)}$ to correct the OLS coefficient, under the assumption that all merchants systematically mis-code initially accept. The biased-adjusted estimate can be shown to be a lower bound of $a^{\tilde{F}}$.

[32]Section 4 showed that 94% of buyers assigned to $\tilde{F}_i = 1$ and who ultimately accepted actually signed the contract. For buyers in $l_i \in S$ and $l_i = D$, the first-stage coefficient, its p-value, and the effective F-statistic are, respectively, 0.97 and 0.94, 0.00 and 0.00, 8322 and 1731.

accounts for the fact, shown in Section 4, that state-favored and disfavored merchants were less likely to recruit, respectively, disfavored buyers and state officials.

Columns (1)–(6) confirm state bias. Column (1) shows that the effect of $\tilde{F}_i$ remains strong and significant for $l_i \in S$ even after accounting for each of the robustness checks. Column (2) shows that this effect stays insignificant for disfavored merchants. The table notes in column (2) compare, in addition, the coefficients in columns (1) and (2). The difference is -0.13 (Diff. est) and the p-value of the difference (Diff. p) is 0.05. Columns (4)–(6) exclude disfavored buyers and those that are state officials. These results reinforce state bias.

Columns (7)–(9) confirm co-ethnic governance. Columns (7) and (8) confirm that state enforcement, even when available, cannot improve upon co-ethnic governance, and that it is concentrated in non-co-ethnic trade and indistinguishable from zero in a co-ethnic pair. In column (9), the explanatory variable is $\boldsymbol{I}_{SC}(l_i)$. It confirms co-ethnic governance.[33]

The cost of creating a realistic market is that merchants had to be given some discretion. However, this section has shown that the analysis excluding selection is robust to deviations arising from merchants' discretion, including their discretion to negotiate prices, to forgive the contract requirement, to skip initially accept (shirking), and their ability to recruit households. This provides reassurance that implementation issues did not confound the result. However, a number of threats to validity, unrelated to merchant discretion, remain. I analyze these threats to inference sequentially in the section that follows.

5.4. *Examining Remaining Mechanisms*

5.4.1. *Independent Effect of Lifting the Requirement*

The first identifying assumption is that lifting the contract only affects buyers' choices through its effect on formal versus informal trade. As I have shown in Table I, Panel 2, lifting the contract requirement had no effect on buyers' beliefs about the sale. Panel 3 provides additional evidence that lifting the requirement had no independent effect. Indeed, I find that buyers assigned to maintained or lifted are equally likely to believe their merchant is state-favored, co-ethnic, and their subjective well-being is indistinguishable.

In this section, I show that controlling for these variables leaves the estimates unaffected. Figure 4 shows the estimates from equations (1) and (2), $\hat{a}_E^{\tilde{F}}$ and $\hat{a}_E^I$, where I also have included, as controls, all the variables in Panel 2 of Table I. It replicates Figure 3 with the inclusion of these controls. State bias and co-ethnic governance are unaffected.

In sum, lifting the contract requirement almost surely creates no confounding effect.

5.4.2. *Mis-Attribution of Ethnic Composition*

Table I showed that mis-attribution of l_i is unlikely to be possible in this context. Figure 5 estimates (1) and (2), $\hat{a}_E^I$ and $\hat{a}_E^{\tilde{F}}$, using the buyers' self-reported belief about the merchant's ethnic group. It replicates Figure 3 using pair ethnic composition using buyer i's report, $\hat{l}_i$.

Panel A estimates $a_E^{\tilde{F}}$ in equation (1). To separate between the samples S, D, SC, and SN, I use the buyers' self-report to construct $\hat{l}_i$. In Panel B, to estimate a_E^I, I use a 2SLS approach. Since assignment to $l_i \in S$ versus $l_i = D$ is random, in the left panel, I instrument

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[33]If merchants always mis-code initially accept, the coefficient on *Co-ethnic*, $\hat{a}^I$, is an unbiased estimator of $a^I + \pi_E(0, l = SC)\frac{S(1,SC)-S(0,SC)}{S(0,SC)} - \pi_E(0, l = SN)\frac{S(1,SN)-S(0,SN)}{S(0,SN)}$, where $S(\tilde{F}, l_i) = P_E(\tilde{F}, l_i) + P_C(\tilde{F}, l_i)$ is the fraction of buyers who ultimately accept and $\pi_E(0, l = SC) = \frac{P_E(\tilde{F},l_i)}{P_E(\tilde{F},l_i)+P_C(\tilde{F},l_i)}$.

A: *Formal vs. informal trade – State bias and state enforcement with co-ethnic governance*
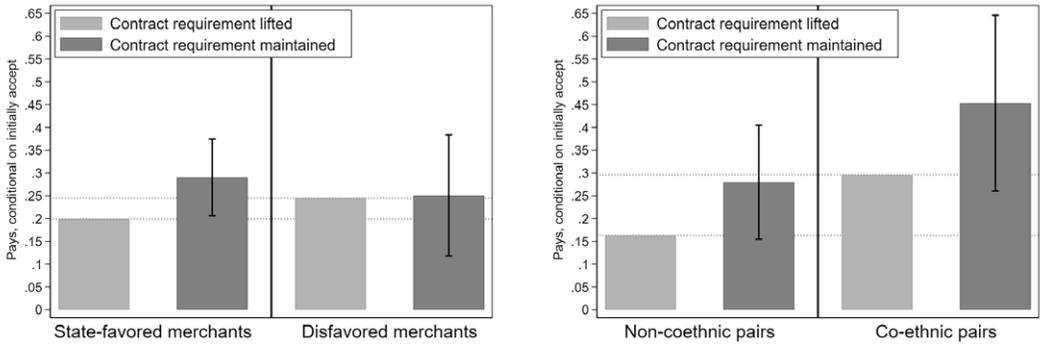
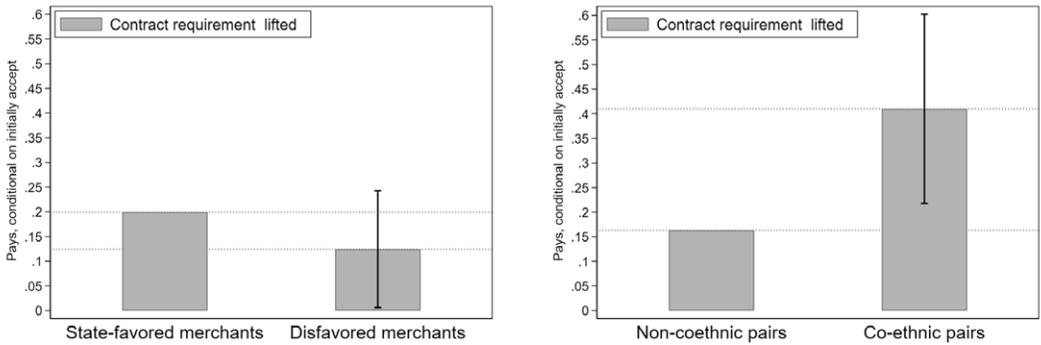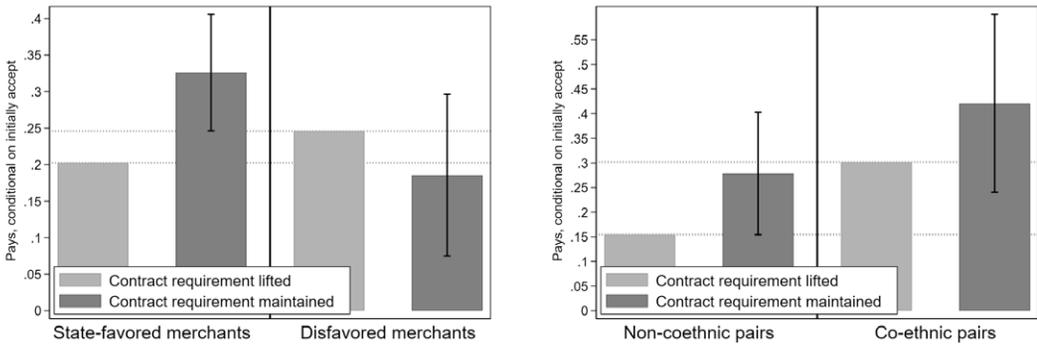**B:** *Informal trade – Co-ethnic governance*

FIGURE 4.—Independent effect of lifting the requirement? *Notes* as in Figure 3. The estimates presented in all quadrants are from estimating equations (1) and (2) where I include, in addition, as control variables, indicators for whether the buyer had heard of the sale, and for whether the buyer believes: that the sale is strange, that the objective is research on trustworthiness, that the objective is research on ethnic groups, that the contract is suspicious, that the contract signals a state connection, that the contract signals seriousness, that the contract signals low trust.

the buyer self-report $I_D^A(\hat{l}_i)$ with the true assignment $I_D^A(l_i)$ and report the second-stage coefficient: this measures the effect of believing the merchant is state-favored, induced by random assignment to merchants. In the right panel, to determine whether the buyer reports to believe to be in a co-ethnic or a non-co-ethnic pair, I use the buyer report about her own ethnic group and about the merchant. Then, I instrument that indicator for perceived co-ethnic pair, $I_{SC}^A(\hat{l}_i)$, with the indicator for actual co-ethnic pair, $I_{SC}^A(l_i)$, in the sample $\hat{l}_i \in S$, constructed using the true merchant's ethnic group.

The estimates of $a_E^{\bar{F}}$ and a_E^I are preserved. $a_E^{\bar{F}}$ is reinforced in $l_i \in S$ and weakened in $l_i = D$. This shows that mis-attribution of ethnic composition cannot be a confound.

Section C derives bounds for coefficient a_E^I that account for the possibility that a fraction of buyers who initially accepted and who are in a truly co-ethnic pair strategically mis-report their ethnic group to appear non-co-ethnics. I pin down the maximal fraction of "strategic passers" that, given the OLS estimate $\hat{a}_E^I$, would be compatible with the existence of co-ethnic governance. I find that co-ethnic governance is robust to up to 42% of buyers passing as non-co-ethnic. Given that ethnic group is easy to observe, this shows that strategic passing is also unlikely to be a concern.

A: *Formal vs. informal trade – State bias and state enforcement with co-ethnic governance*

B: *Informal trade – Co-ethnic governance*

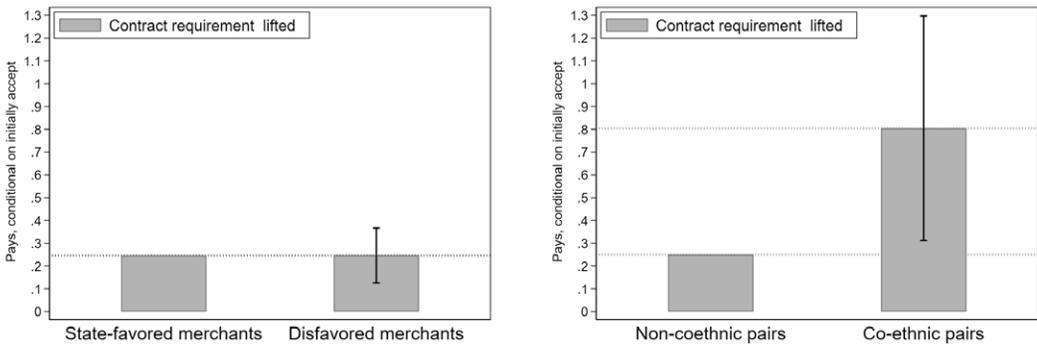
FIGURE 5.—Mis-attribution of ethnic composition? *Notes* as in Figure 3. This figure estimates equations (1) and (2), $\hat{a}_E^I$ and $\hat{a}_E^{\bar{F}}$, using the buyers' self-reported belief about the merchant's ethnic group. Specifically, in Panel A, the samples are defined using the buyers' self-reported belief about her ethnic group, and about the merchant's ethnic group, that is, I use $\hat{l}_i$ instead of the measure of ethnic composition of the pair based on the merchant's true ethnic group and the buyer's report about her own. In Panel B, I instrument the ethnic composition constructed from the buyer's reported ethnic group of her and of the merchant, that is, $\hat{l}_i$, which is endogenous, with the ethnic composition I compute using the true ethnic group of the merchant, which is randomly assigned to the buyer, and the buyer's reported ethnic group, that is, with $\hat{l}_i$. Panel B reports the IV coefficient, constant, and confidence intervals. The effective F-statistic of the first stage (Olea and Pflueger (2013)) is 566.72 and 18.89 for the left and right panels, respectively.

In sum, imperfectly observing ethnic group is unlikely to explain the main result.

5.4.3. *Taste for co-Ethnic Exchange*

To examine whether taste for co-ethnic exchange may create a problem of self-selection, I now analyze the buyers' choices in the placebo.

In Figure 6, I report estimates from equation (2), in the sample of state-favored pairs, $l_i \in SC \cup SN$. The dependent variable is an indicator for whether the buyer rejects the placebo, $\boldsymbol{I}_R(b_i^0)$. The independent variable is an indicator for co-ethnic, $\boldsymbol{I}_{SC}(l_i) \in \{0, 1\}$. The estimate $\hat{a}_R^I$ is insignificant. This rules out taste for co-ethnic exchange.

In sum, confounds arising from violations of the identifying assumptions in Section 4.5 are very unlikely. The estimates support the hypothesis that formal contracts are inef-
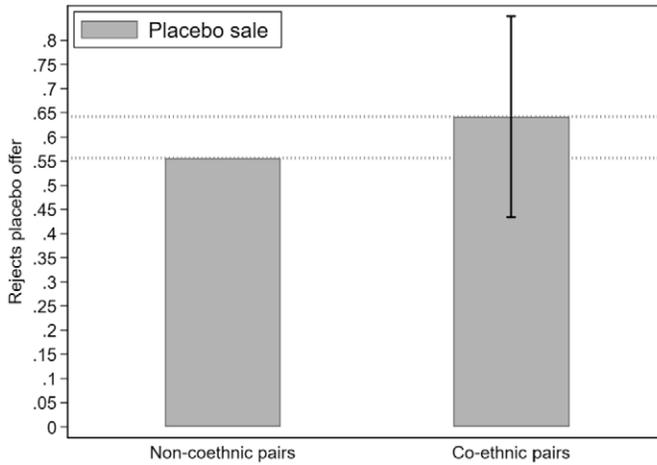
FIGURE 6.—Taste for co-ethnic exchange? *Notes*: This figure reports the estimates from equation (2) in the sample of state-favored buyer-merchant pairs, $l_i \in SC \cup SN$, where the dependent variable is an indicator for whether the buyer rejects the placebo, $\boldsymbol{I}_R(b_i^0)$. The independent variable is an indicator variable for whether the buyer-merchant pair is co-ethnic, $\boldsymbol{I}_{SC}(l_i) \in \{0, 1\}$. Black brackets indicate 95% confidence intervals on the coefficient on $\boldsymbol{I}_{SC}(l_i)$.

fective when the merchant is disfavored, and that they cannot improve upon co-ethnic governance.

5.5. *Mechanism of State Bias: Anti-Disfavored Discrimination by State-Favored Bureaucrats*

I now provide evidence that buyers anticipate that judiciary bureaucrats, who are state-favored themselves, discriminate against disfavored merchants. To elicit anti-disfavored taste by the state-favored, I analyze the choices of state-favored buyers in the placebo.

In Figure 7, I report the estimates from equation (2) in the sample of state-favored buyers. The dependent variable is an indicator for whether the buyer rejects the placebo, $\boldsymbol{I}_R(b_i^0)$. The independent variable is an indicator for whether the merchant is disfavored $\boldsymbol{I}_D(l_i) \in \{0, 1\}$. The left quadrant shows the results for all state-favored buyers. The fraction who reject rises from 58% to 68% when the merchant is disfavored; the difference is statistically significant. The right quadrant narrows this sample to the state-favored buyers who are also state officials by occupation, as per the exit survey. The sample is small, thus the analysis is tentative. For those, the fraction who reject rises from 58% to approximately 100% if the merchant is disfavored. Thus, state-favored individuals, and especially the state officials, have anti-disfavored taste.[34]

This section has established state bias and co-ethnic governance. I have ruled out that the results can be explained by any form of buyer selection based on the contract requirement, various forms of merchant discretion, the effect of lifting the requirement, ethnic group mis-reporting, and taste for co-ethnic exchange. I have shown that buyers' beliefs support these conclusions, and ruled out that the effect of the contract is through informal sanctions. I then showed that state-favored people discriminate against the disfavored,

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[34]Figure A.3 in the Supplemental Material replicates Figure 3 with, as control, the choice in the placebo. The results are unchanged, reassuring that anti-disfavored taste does not bias the estimates.
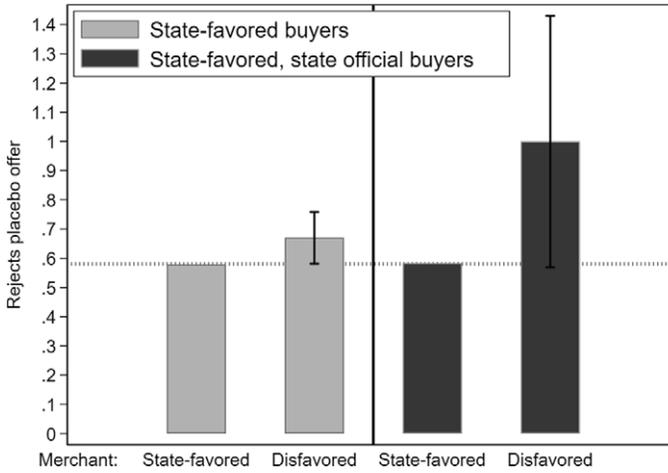
FIGURE 7.—State-favored buyers discriminate against disfavored merchants. *Notes*: This figure reports the estimates from equation (2). The dependent variable is an indicator for whether the buyer rejects the placebo, $I_R(b_i^0)$. The independent variable is an indicator for whether the merchant is disfavored, $I_D(l_i) \in \{0, 1\}$. The left quadrant shows the results for all state-favored buyers. The right quadrant narrows this sample to those that are state officials. Black brackets indicate 95% confidence intervals on the coefficient on $I_D(l_i)$. Sample size: 581 and 58 for left and right quadrant, respectively. Right only suggestive.

which supports that state bias is an expression of anti-disfavored taste, by state-favored contracting institutions.

In the next section, I gather results implicit in this descriptive analysis to draw general conclusions about the benefits of formal contracts, given this social structure.

6. WELFARE IMPLICATIONS

The previous section provides descriptive evidence for the effect of formal and informal trade on buyers' choices. I present some conclusions pertinent to welfare in this section.

My first exercise uses the descriptive results of buyers' choices to analyze the effect of trade governance on merchants' profits. This allows me to recover the merchants' willingness to pay for having a formal contract at the current price. Since the formal contract is ineffective when the merchant is disfavored, I focus on state-favored merchants' profit.

Recognizing that the experiment may induce merchants to extend offers that merchants may not naturally extend, I first compute the implied profits if merchants would perfectly observe the buyers' ability to commit to pay. In that case, the expectation of merchant profit per attempted sale is $\mathbb{E}(\Pi(\tilde{F}_i, l_i)) = P_E(p - c)$. Using $p = 2$ and $c = 1$, the gain of introducing formal contract in non-co-ethnic trade, or from turning to co-ethnic trade is, respectively, per attempted sale, $\Delta^F = P_E(1, l = SN) - P_E(0, l = SN)$ and $\Delta^C = P_E(0, l = SC) - P_E(0, l = SN)$. The coefficient on *Contract required* in Table III, column (2), 0.07, and that on *Co-ethnic pair* in Table IV, column (2), 0.10, are respectively unbiased estimators of Δ^F and Δ^C. They imply that, in non-co-ethnic trade, $\mathbb{E}(\Pi(0, l = SN)) = 0.13$ USD without a contract, but with a contract, $\mathbb{E}(\Pi(1, l = SN)) = 0.20$ USD and in co-ethnic trade, $\mathbb{E}(\Pi(0, l = SC)) = 0.23$ USD. That is, formal contracts increase profits by 54%, co-ethnic governance by 77%. At 10 attempted sales per day, a state-favored merchant's willingness to pay to access a formal contract is, *each* day, 0.7 USD, and 1 USD each day for being matched instead with co-ethnic partners—63% and 89% of daily p.c. income.

I then consider the other extreme assumption. Suppose that merchants, in reality, cannot observe whether a buyer will pay. The merchants' expected profit per attempted sale, given $(\tilde{F}_i, l_i)$, $c = 1$, and $p = 2$, is $\mathbb{E}(\Pi_{AI}(\tilde{F}_i, l_i)) = pP_E(\tilde{F}_i, l_i) - c(P_E(\tilde{F}_i, l_i) + P_C(\tilde{F}_i, l_i)) = P_E(\tilde{F}_i, l_i) - P_C(\tilde{F}_i, l_i)$. Then, the profit gains of introducing contracts and of turning to co-ethnic informal trade are respectively $\Delta_{AI}^F = \mathbb{E}(\Pi_{AI}(1, l_i)) - \mathbb{E}(\Pi_{AI}(0, l_i))$, $\Delta_{AI}^C = \mathbb{E}(\Pi_{AI}(0, l = SC)) - \mathbb{E}(\Pi_{AI}(0, l = SN))$. Using columns (1) and (2) in Panel A of Tables IV, III to calculate Δ_{AI}^F and Δ_{AI}^C, the expected profit per attempted sale is -0.56 USD in informal non-co-ethnic trade by a state-favored merchant, -0.25 USD if a formal contract is introduced (a 0.31 USD increase, 31% of the absolute value of the profit of a successful sale), and -0.22 in informal co-ethnic trade (a 0.34 USD increase). Thus, contracts *or* co-ethnic matching would yield additional 3.1 USD and 3.4 USD daily in profits—276% and 303% of daily p.c. income.

My second exercise derives implications for societal welfare. The taxonomy presented in Definition 1 classifies buyers based on their choice in formal versus informal trade.

DEFINITION 1—Taxonomy: Given l_i, p, the buyer can be divided into five categories, of mass $\alpha_k(p)$, $k = 1, \ldots, 5$. 1. *Peaches*: always accept the offer and pay ($\alpha_1(p)$). 2. *Never-buyers*: never buy ($\alpha_2(p)$). 3. *Informal bad apples, formal peaches*: buy, but only pay if trade is formal ($\alpha_3(p)$). 4. *Informal bad apples, formal non-buyers*: buy and defect in informal trade, and reject in formal trade ($\alpha_4(p)$). 5. *Bad apples*: always buy and defect ($\alpha_5(p)$).

I use these estimates to discuss the effect of formal contracts on exchange. I estimate α_k, $k = 1, \ldots, 5$ from the OLS following regressions:

$$I_E^A(b_i) = \alpha^p + \beta_S^p S_i + \beta_C^p SC_i + \beta_F^p \tilde{F}_i + \beta_{FS}^p \tilde{F}_i \times S_i + \beta_{FC}^p SC_i \times \tilde{F}_i + e_i^p,$$

$$I_{E \cup C}(b_i) = \alpha^b + \beta_C^b SC_i + \beta_F^b \tilde{F}_i + \beta_{FS}^b \tilde{F}_i \times S_i + \beta_{FC}^b SC_i \times \tilde{F}_i + e_i^b.$$

Panels A and B of Table A.V report their calculation, estimates, p values, and tests. The estimates of α_k, $k = 1, \ldots, 5$ have the following implications on successful exchange.

First, state enforcement increases exchange with state-favored merchants. Specifically, considering non-co-ethnic trade with state-favored merchants, the mass of informal bad apples-formal peaches, $\alpha_3^{SN}(p)$ (column S{N}), is 0.09, statistically significantly different from zero. In contrast, for co-ethnic trade with state-favored merchants, $\alpha_3^{SC}(p)$ is smaller (0.05) and not statistically significantly different from zero (column S{C}).

Second, formal state enforcement does not produce any exchange gain when merchants are disfavored. Indeed, the mass of informal bad apples-formal peaches when the merchant is disfavored, $\alpha_3^D(p)$ (column D), is indistinguishable from zero.

Third, formal contracts do not outperform co-ethnic trade. The mass of peaches in informal co-ethnic trade and non-co-ethnic formal trade is respectively $\alpha_1^{SC}(p) = 0.19$ and $\alpha_1^{SN}(p) + \alpha_3^{SN}(p) = 0.20$, which are statistically indistinguishable.

Fourth, introducing co-ethnic governance increases exchange. Comparing the mass of peaches, $\alpha_1(p)$, in columns S{C} and S{N}, I have that $\alpha_1^{SC}(p) = 0.19 > \alpha_1^{SN}(p) = 0.11$, co-ethnic trade induces a 72% increase in quantity exchanged.

7. CONCLUDING REMARKS

The findings of this paper temper the view of the state as a technocratic deus ex machina for trade enforcement. They suggest that a captured state and functioning informal institutions may justify, and perpetuate, economic informality. The paper also opens two puzzles.

One puzzle is that, if formal contracts bring trade closer to its efficient level when informal governance is weak and merchants can trust the state (in my context, non-co-ethnic state-favored trade), why do these segments of the economy remain informal? Can a culture of informal relations acquired through millennia preceding the Congolese state, created in the last century, explain it? Did the history of inter-ethnic group relations segregate the economic structure, leading to low opportunities of inter-ethnic trade? Perhaps the cost of violating a formal contract remains too low to compensate for the cost of accessing the state. In ongoing work, Chen et al. (2021) use companion study data to estimate structural parameters for "state capacity" in non-co-ethnic state-favored trade to quantify the actual cost of violating a formal contract, and apply it to measure whether state capacity decays with travel costs to state offices.

There is another remaining puzzle. If the state cannot commit not to expropriate merchants that are discriminated against, why doesn't the dominated group coordinate the actions of its merchants to discipline the contracting institutions?

Historically, contracting institutions providing little protection against defection and expropriation of foreign merchants are prevalent. This tends to discourage foreign merchants from trading in their jurisdiction. Greif, Milgrom, and Weingast (1994) documented how, in response to this commitment problem, merchants' guilds emerged to discipline foreign contracting institutions. The absence of such a mechanism implies lost fiscal or bribe revenues for the state authorities and state officials. Why is it absent?

One possibility is that distaste against disfavored people may be so strong among the state-favored that it can simply not be compensated through the proceeds of foregone trade. In that case, there is simply no inefficiency, nor associated Coasean bargain that would recover surplus presumed lost. While the implications for welfare would then be complicated by biased social preferences, the absence of a mechanism that addresses this form of trade destruction has nonetheless important distributional implications.

Another possibility is that state bias reflects the choices made by administrators, and that administrators do not internalize the benefits to the state of taxing the trade that is currently otherwise destroyed. This would indicate that agency problems in the state organization are the foundation for foregone economic opportunities and state revenue arising from voluntary failure to enforce formal contracts, for some groups.

Answers to these questions would bring valuable insights to our current understanding of the social and institutional foundations of economic growth.

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