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From personalized exchange towards anonymous trade: A field experiment on the workings of the invisible hand

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ABSTRACT

The experimental literature has shown the tendency for experimental trading markets to converge to neoclassical predictions. Yet, the extent to which theory explains the equilibrating forces in markets remains under-researched, especially in the developing world. We set up a laboratory in 94 villages in rural Sierra Leone to mimic a real market. We implement several treatments, varying trading partners and the anonymity of trading. We find that when trading with co-villagers average efficiency is somewhat lower than predicted by theory (and observed in different contexts), and markets do not fully converge to theoretical predictions across rounds of trading. When participants trade with strangers efficiency is reduced more. Anonymizing trade within the village does not affect efficiency. This points to the importance of behavioral norms for trade. Intra-village social relationships or hierarchies, instead, appear less important as determinants of trading outcomes. This is confirmed by analysis of the trader-level data, showing that individual earnings in the experiment do not vary with one's status or position in local networks.

1. Introduction

A central tenet of neoclassical economics is that in equilibrium there are no unexploited gains to trade. The workhorse model within economics implies that in a perfectly competitive market, the first function of the equilibrium price is to efficiently allocate scarce resources to market participants. This principle, embodied in Adam Smith's invisible hand metaphor, represents the backbone of the measurement of the gains to trade, provides guidance into optimal tax policy, and embodies why market-based interventions are often proposed as a key element of policy reform agendas for developing countries. The theoretical consistency of the efficient outcome of market allocations has been substantiated through experimental studies conducted in a developed world social context (Smith, 1962; Roth, 1995; Holt, 1995; List, 2002, 2004).

We explore the limits of the applicability of this theoretical prediction for rural inhabitants in developing countries, and test whether outcomes in market-trading games remain efficient when participants originate from communities with little

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exposure to markets. In addition, participants in our sample are more socially connected than most market experimental studies which typically include college students. Specifically, we report on a double-sided decentralized oral auction that was run as a lab-in-the-field experiment in 94 villages in Eastern Sierra Leone. Our subjects live in areas that are between 2 and 6 h walk away from market towns. Market trading in these areas occurs at low volumes and over a small range of products. On average, only 43 percent of our subjects reported to buy or sell something more than once a week.,[1] Most are subsistence farmers with, most of the time, hardly any cash to spend at a market. Indeed, 12 percent of our subjects reports that they never go to markets.

Our approach enables us to consider whether exchange patterns are driven by forces other than profit motives. Rural life in Africa is to an important extent governed by social norms (linked to social status, kinship norms, social ties etc.) and institutions that are distinct from those (implicitly) assumed in neoclassical economics. This might matter for the efficiency of markets. Granovetter (2005), for example, argues that the impact of social relations on trading prices may vary with the nature of the relationship, the cost of shifting to other partners, and the market situation: "*The theoretical issue is often not one of economic and sociological arguments conflicting, but rather of the weakness of both in understanding how actors with simultaneous economic and non-economic motives will act.*" (2005: 38)

The objectives of this paper are twofold. First, we extend the work of Smith etc. into the field and explore the efficiency of market behavior by conducting intra- and inter village trading experiments with subjects from one of the poorest regions in the world. Second, we intend to make a methodological contribution and probe whether social dimensions are a potential impediment to trade, interfering with the workings of the invisible hand.[2] We try to distinguish between behavioral norms associated with exchange behavior within the village, and person-specific social relations – one's position in local networks or hierarchies. For example, we ask whether status and social relationships (patron-client networks, kinship relations, or trust-based relations) interact with market structures to cause inefficient trading behavior. While most experimental papers on status and efficiency are based on status randomly induced *within* the experiment (e.g., Ball et al., 2001; Moxnes and van der Heijden, 2003; Frey and Meier, 2004; Kumru and Vesterlund, 2010)[3] we use subjects who are socially connected in real life and take advantage of the existing, endogenously formed status hierarchies in their community. In our set up, we experimentally vary the social distance between buyers and sellers, as well as the trading technology and the anonymity of partners trading.

We report three key insights. First, earlier experimental findings reported in Smith (1962) and List (2002, 2004) do not fully extend to our environment. Specifically, when using a conventional double auction setting, overall efficiency levels are lower than previously observed, and aggregate behavior *across* trading rounds in experimental markets does not seem to fully converge towards theoretical predictions of efficiency.

Second, we find that there exists a social dimension to trade, and speculate that norms about intra-village behavior affect economic efficiency. While one's own position in local hierarchies does not explain profits from trade, we observe that trading efficiency is higher in samples drawn from the same social network than in samples where trading partners are strangers. Eliminating face-to-face interaction from the within-village treatment, or making within-village trade anonymous, does not matter for efficiency, but affects the number of trades and the distribution of the surplus.

Third, based on analyses of trader behavior and realized trades in the experiment, we find that some observable personal characteristics affect market outcomes. For instance, literate people are more likely to trade. We also find that men and younger agents earn higher profits in the experiment than women or older participants. Status and one's position in local networks or hierarchies, instead, does not matter for experimental earnings.

This paper is organized as follows. Section 2 provides theoretical background, introduces conceptual foundations and discusses the academic [or scientific] contributions of the study. In Section 3 we explain the experimental design and develop a series of exploratory hypotheses. In Section 4 we analyze the aggregate data, focusing on efficiency levels and differences across experimental treatments. In Section 5 we consider how individual characteristics drive results. Conclusions and discussion of our results ensue in Section 6.

2. Conceptual foundations

Our experiment speaks to various literatures. First, given our sample of subsistence farmers with very little trading experience, the results speak to the literature on the transition from personalized exchange to anonymous trade (see also Fafchamps, 2011; Kimbrough et al., 2008). This issue is not merely a theoretical nicety. For example, the dominant agricultural development paradigm in current policy circles is to enhance the efficient operation of markets and to link producers (and consumers) to regional or international markets and value chains (Byerlee et al., 2009). However, according to some theories, there are complementarities in exchange modalities, which imply that such a transition might not easily occur. If most

[1] Typically, food crops (cassava, rice, beans), cash crops (palm oil, cocoa, coffee), animals (fish, bush meat) are brought to the markets and imported essentials, such as salt, sugar, and soap, are brought back to the villages.

[2] Previous studies in Western societies indicate that social distance and the degree of anonymity affects play in dictator and ultimatum games (e.g., Hoffman et al., 1996; Bohnet and Frey, 1999; Charness and Gneezy, 2008). Baldassari and Grossman (2013) focus on the effects of group attachment and social position on prosocial behavior (measured via dictator games) in Uganda.

[3] See Chandrasekhar et al. (2013) for an example of the use of real-life social networks in a field experiment.

villagers opt for one exchange modality, network externalities cause others to follow regardless of whether this modality is "globally efficient." In communities where all potential trading partners adopt one trading technology (say, trading based on reputation and trust), then it is in the interest of newcomers to also invest in this technology, even if net welfare gains would occur if all villagers could somehow coordinate on the simultaneous switch to another more superior technology (say, based on external enforcement of contracts via arbitrators and courts). Hence, there are theoretical reasons to suspect that communities may end up caught in an institutionally-induced poverty trap (Kranton, 1996; Kumar and Matsusaka, 2009).

Our experiment also extends the body of experimental literature that spawned from the seminal work of Smith (1962) and that has provided robust laboratory evidence that market outcomes tend to approach neoclassical expectations. List (2002, 2004) moved the analysis from the laboratory to the field by organizing a field experiment wherein real-world market participants engaged in face-to-face bilateral bargaining (market transactions) in a more natural setting. A key result was the strong tendency for exchange prices to approach the neoclassical competitive model predictions, especially in symmetric markets. But does this result extend to a context characterized by personalized exchange and non-economic (i.e. social) trade motives? Analysts of tribal economies and peasant markets argue that trading is typically characterized by "clientalization" and restricted to "designated others in foreign groups" (e.g., Sahlin, 1972) resulting in prices that deviate from competitive levels (Davis, 1973). Our analysis allows us to probe whether social factors matter for trading efficiency.

Finally, while it is useful to establish that social dimensions might interfere with market efficiency, it is clearly more useful to know which social dimensions matter (most). Villages in rural Sierra Leone are socially stratified, based on ranked-based (family) lineages. They typically contain elites, landowning families, the descendants of slaves, and "strangers" (often descendants of people who moved into the village generations ago). The latter two categories depend on the former social groups for access to key resources, such as land, and "reciprocates" by supplying labor (Richards, 1996). Mokuwa et al. (2011) provide an example of how social stratification in eastern Sierra Leone manifests itself in abusive local judicial systems.

In rural Sierra Leone, exchange often takes the form of repeated, personalized interaction, embodied in kinship ties and patron-client relations. The location that villagers occupy in social networks (center versus periphery, or high-status versus low-status) may therefore affect bargaining positions, trading behavior, and ultimately trading outcomes. For example, Leach (1994, p.186) mentions that "*a wide range of financial expectations is now associated with social relations of various kinds. These money transfers are not merely moral obligations; they also structure the relations of both power and security.*" However, it is not a-priori clear how social relations will be manifested in the context of Sierra Leone's rank-based lineage systems characterized by strong patron-client relations. Patrons are expected to provide clients with economic and political support, in return for clients' labor, political allegiance and other services (Leach, 1994). While relatively wealthy and powerful principals are able to grab most of the surplus when interacting with their clients, they might prefer not to do this (or rather; not *always* to do this). This experiment may function as an opportunity for principals to signal generosity and public-spiritedness, and their respect for local norms of generosity – thus cementing their position of authority. Trading generously could be an investment in the patron-client relationship that allows high-status participants to demand favors from low-status participants in the future (ties of 'indebtedness' which the giver can recall, for example, in the form of 'voluntary' labor provision by the client).

In addition to specific relations and positions in local hierarchies, the social dimension also includes broader considerations related to (local) culture. For example, there may be moral imperatives to share, not to antagonize trading partners by appearing greedy, not to bargain hard for private gains, or not to deliberately strive for the accumulation of private wealth because this threatens existing systems of mutual dependency (e.g., Platteau, 2001). Social norms prescribing acceptable or appropriate behavior apply to all villagers, and transcend personalized relations. Possibly such norms extend beyond the village, although parochial sentiments may of course imply that co-villagers are (or "should be") treated differently than others. In what follows we try to examine the role of both individual social relations, and the position of individuals in social networks, as well as general behavioral norms on trading behavior, and whether they extend beyond the village, or not.

3. Experimental design and hypotheses

Our test of competitive market theory is based on the experimental design of List (2004), designed to "give neoclassical theory its best chance to succeed." Like List, we used double-sided oral auctions (bilateral bargaining between buyers and sellers) and multiple rounds (details below). Unlike List, we used subjects from rural African villages with very little access to markets or experience in trading. We recruited 1504 subjects from 94 villages in eastern Sierra Leone, close to the border of the Gola Rainforest National Park. Our design differs slightly from List – we used eight buyers and eight sellers (rather than twelve) and used ten rounds of trading (rather than five).[4]

This study commenced with a series of pilot experiments in 16 villages in Sierra Leone, in the winter of 2011. We assigned villagers to various treatments of a double auction market experiment. In the benchmark treatment, we invited villagers to trade with their fellow villagers, and observed that levels of efficiency (measured as the share of potential rents captured by the trading partners) lagged behind levels of efficiency measured in Western contexts. However, we also observed that efficiency levels appear to respond to variation in the level of anonymity between players, suggesting local relationships matter for the functioning of markets. These findings encouraged us to scale up the experiment, so we returned to the field

[4] We wanted more than 5 rounds to give our experimental market a good chance to succeed in "converging" towards theoretical predictions. But logistical constrains also pose a limit on the number of rounds per session, and 10 rounds appeared as a reasonable compromise.

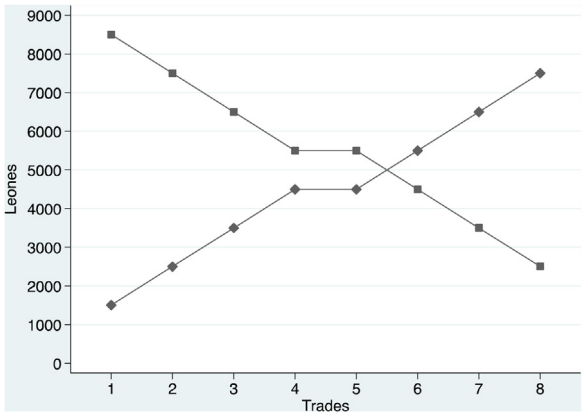

Fig. 1. Experimental Market. *Note:* Figure plots reservation values (in Leone) of the experimental market.

in the spring of 2013. The analysis below is based on these new data. We do not pool data from the two data waves because we adjusted the sampling procedure.[5]

Our sample strategy and experimental design are as follows (detailed protocols and instruments are included in the Appendix B). We invited participants to 24 central locations (typically a school) in ten of the 149 Chiefdoms in Sierra Leone. Participants were selected during a pre-experiment visit to the communities. We stratified the selection of participants by status in order to obtain the same number of high and low status individuals in each session. Specifically, we selected the nine highest status participants from each village (called "*Taa Gbakoi*" in Mende the local language, which typically includes the village chief, town speaker, village imam, women's and youth leader). From the pool of "low-status individuals" in the village (locally referred to as "*Nu Gbamei*", or "person of nothing"), we randomly selected nine to participate in the experiment.[6] Ninety-seven percent of the people we invited were willing and even eager to participate.

Experimental sessions lasted about two hours and began with an extensive introduction and a few practice rounds. We took great care to ensure participants understood the experiment by demonstrating example trades, asking them to explain the game back to us, and going through a few test runs. We created a market for a simple block of wood that had no inherent value. Participants were randomly assigned a role as either a buyer or a seller, and these roles were randomly re-allocated at the beginning of each trading round. Each trader privately received a randomly selected reservation price, or induced value, at the beginning of each round. Buyers were given a maximum willingness to pay and sellers were given a minimum willingness to accept for the wooden block. Buyers (sellers) were not allowed to trade above (below) their own reservation values. Profits earned in the trade (the difference between reservation value and agreed trading price) were paid to participants at the end of the experiment. Each trading round lasted five minutes. Traders were instructed that as soon as a trade was consummated, the buyer and seller had to approach a "trade master" (one of our research assistants) who recorded the trade and publicly announced the agreed price to all participants. Our main dependent variables are aggregate profits, market efficiency (profits captured in the trades divided by total potential profits), the number of trades, and profit earned on individual trades.

Fig. 1 summarizes the reservation prices experimentally induced in our experimental market. Market equilibrium occurs at a price between 4500 and 5500 Leones (or USD 1.05–1.28) and a quantity of five units traded. Importantly, in each round some buyers and sellers receive induced values that should place them out of the market. The induced value for three sellers is too high to profitably sell at the equilibrium price. Similarly, three buyers have reservation values that are too low to profitably purchase at the equilibrium price. Since reservation values are randomly assigned at the beginning of each round, the identity of subjects who are "in" or "out" of the market varies from one round to the next, and anchoring effects should be minimized.

Our experiment involves four treatments, randomly assigned to each experimental group (hence each respondent participated in only one treatment). We used a blocked randomization design to spread treatments across trading locations.

[5] Specifically: we randomly invited villagers to participate for the pilot study, and deliberately "oversampled" members of the elite for the scaled-up version of the experiment (see main text, below). We have also analysed the pilot data, and for this (much smaller) sample we obtain results that are qualitatively different from the ones presented below, see Appendix. Specifically; in the pilot we observed that social relations and hierarchies are significant determinants of trade outcomes (high status participants are more likely to trade and conditional on trading make less profits). In contrast to the second round results, the middleman treatment (+3%) and inter village treatments (+5%) have higher profits than the intra village treatment. These differences are however not significant. This was likely due to the low power of the pilot study, motivating us to collect a new round of data. Since we significantly changed the participant selection protocol we could not pool the data but present results of the pilot in the Appendix.

[6] In each village we selected nine individuals of the high- and low-status category even if we needed only eight to participate in the experiment. If all nine individuals showed up, we (randomly) send one home immediately after paying them the show-up fee. We told the village what day we would run the experiment. On that day, we arranged transport for preselected villagers, taking them to a central town where we had secured permission to use school classrooms.

Table 1
Descriptive Statistics Trading Sessions.

	Sessions	Number of Participants	Total Observations	Total Trades Executed
A: Intra-village	23	368	3680	1295
B: Inter-village	24	384	3840	1331
C: Middlemen revealed	23	368	3680	1134
D: Middlemen anonymous	24	384	3840	1119
Total across treatments	94	1504	15040	4879

Notes: Each experimental session contains subjects from a different village (94 in total), except for Treatment B where half the subjects are from a different village. Each session contains data for 16 participants (8 buyers and 8 sellers) and 10 rounds.

Table 1 summarizes the number of sessions conducted. In Treatment A, or the intra-village treatment, buyers and sellers come from the same village. Since the villages included in this study are small (less than 100 households), all subjects know each other. We ask buyers and sellers to engage in bilateral bargaining and haggling, interacting in a central market place. If our subjects bring their experiences from daily life into the lab, the trading outcomes in Treatment A may be governed both by generalized norms about behavior as well as by the position of individuals in the local hierarchy – deviations from the competitive market equilibrium might occur for two reasons. Treatment A allows us to address research question 1:

RQ 1: *If we run a continuous double auction market in a setting with participants who know and interact with each other outside the game, to what extent will trading behavior approach the level of market efficiency predicted by theory? To what extent do personal relations and general behavioral norms invite deviations from efficient trading?*

Based on the findings in the pilot experiment, we expected efficiency levels in Treatment A to be lower than those obtained in experimental settings elsewhere. To probe whether efficiency is thwarted by norms or relations (networks) we introduce a second treatment. In Treatment B, or the inter-village treatment, all potential trading partners come from a *different* village. We made sure to invite subjects from villages relatively far apart so that the great majority of buyers and sellers would not know each other (this was confirmed by our exit survey, on average participants are far less likely to say they are related by blood, farm together, maintain regular contact or frequently exchange goods with participants from the opposite trading group). Hence, key features associated with one's position in local networks (and personal relationships) are plausibly eliminated, as are norms about appropriate (trading) behavior within the village. Of course general norms about appropriate trading behavior remain. By comparing behavior in treatments A and B we are able to address research question 2:

RQ 2: *Does the elimination of pre-existing social relations, hierarchies and within-village behavioral norms affect market efficiency?*

To further probe these issues we developed Treatments C and D, in which we eliminate face-to-face contact between the trading partners. We used middlemen from other villages as agents for the buyers, unknown to all buyers and sellers before the experiment (but buyers and sellers are still from the same village). Buyers and sellers now remained in separate rooms. Each buyer was assigned one middleman, and allowed to give that middleman any instructions he desired on how to negotiate during the trading period with sellers in the other room. There were still eight buyers and sellers, and the bargaining still took place in face-to-face interactions, only now those interactions were between middlemen and sellers. The middleman was paid a fixed wage for his efforts whether or not he made a trade and regardless of the profit he secured for the buyer, so there were no private (monetary) incentives to exert effort in searching or bargaining. To avoid envy and resentment we paid middlemen approximately what the average trader earned for the day.

We hypothesize that market structure may attenuate market distortions introduced by norms or social relations. We try to purge the cultural imperative to engage in gift giving or relaxed bargaining. Because buyers and sellers are not interacting face-to-face there is less social pressure to make trading decisions based on social considerations. Specifically, the middleman is not bargaining for his own profit, and can therefore bargain harder without appearing greedy or attempting to accumulate private wealth. Similarly, the seller is not engaging with the buyer directly, and does not know the broker personally, so he also can adopt a more business-oriented role. The middleman treatments therefore represent an alternative means to probe the relevance of behavioral norms vis-à-vis social relations. We ran two versions of the middlemen game. In Treatment C the middleman should reveal the identity of the buyer to the seller (and vice versa when returning to the buyer for additional instructions during the bargaining stage). In contrast, in Treatment D middlemen were instructed *not* to reveal the identity of buyers or sellers to the other transacting party (compliance with this rule was overseen by research assistants). Hence, while Treatment C may attenuate specific norms regarding appropriate trading behavior (but not necessarily eliminate social relationships between buyers and sellers), we argue that treatment D ensures full anonymity and attenuates the role of relationships and networks as well as specific norms.[7] However, trading still occurs between co-villagers, and it is unclear a-priori to what extent behavioral norms are fully eliminated in Treatments C and D.

We speculate that if within-village behavioral norms are the most important determinant of trading behavior, and if villagers have "internalized" such norms, the outcomes in Treatments C and D will resemble outcomes in Treatment A. If norms

[7] Observe that comparisons across treatments may be confounded by extra transaction costs associated with trading via middlemen. However, lack of time was never a binding factor in any of the trading rounds, so we believe this factor to be relatively unimportant.

are fully internalized such that they are relevant even in the context of anonymous trade, then outcomes in Treatment D will resemble outcomes in Treatment A. Instead, if social networks and specific relationships are the most important determinants of trading behavior, then outcomes in Treatment D should resemble outcomes in Treatment B (as such relationships are unimportant in both treatments).

RQ 3: *Does the introduction of a trading technology that makes trade more impersonal (i.e. middlemen) lead to greater efficiency?*

RQ 4: *If so, is efficiency fostered by the technology of trading through an agent or by the anonymity of buyers and sellers? Is trading efficiency impeded by pre-existing social relationships or by behavioral norms?*

In short: if (generalized) norms of face-to-face contact are the dominant force governing behavior, then trading outcomes in Treatments A should resemble those in Treatment B (and be different from outcomes in C and D). If within-village norms of appropriate behavior govern trade, and have been internalized such that they also apply when interacting with an anonymous co-villager, then outcomes in A, C and D should be comparable. In contrast, when local hierarchies and networks determine trading outcomes, then we would expect A and C to be the same, and to be different from B and D.

Empirically, we examine these issues in a regression format allowing us to control for relevant experimental design features (such as the randomization blocks and clustering). We include dummy variables for the separate treatments (using the intra-village treatment A as the omitted category). We first look at aggregate results by round and estimate:

$$Efficiency_{jr} = \alpha + \beta TB_j + \gamma TC_j + \delta TD_j + \mu S_k + \varepsilon_{jr}, \quad (1)$$

where $Efficiency_{jr}$ is the efficiency of group j (with $j = 1, \ldots, 94$) in round r (with $r = 1, \ldots, 10$), S_k are the randomization block dummies (with $k = 1, \ldots, 24$) and TB, TC and TD are the treatment dummies. We cluster standard errors at the level of the experimental group. We estimate model (1) both for the full set of 10 rounds, and a restricted set of the final five rounds to take out learning dynamics during the first rounds of trading. Results are rather comparable, and we will only report results for the full set of data (results for the final five rounds are available on request). We also assess the number of trades completed by round, and the number of trades where participants made no profits.

Next, we wish to move beyond an aggregate measure of trading efficiency, and consider who gains from trade. We conduct an analysis at the level of the individual trader, and explain both participation in trade and gains from trade with a range of experimental and household (or respondent) variables. Specifically, for each respondent i from trading group j we estimate:

$$Trade_{irj} = \alpha + \beta TB_j + \gamma TC_j + \delta TD_j + \theta CV_{ij} + \mu S_k + \gamma R_r + \rho X_{ij} + \varepsilon_{irj}, \quad (2)$$

$$Profit_{irj} = \alpha + \beta TB_j + \gamma TC_j + \delta TD_j + \theta CV_{ij} + \mu S_k + \gamma R_r + \rho X_{ij} + \varepsilon_{irj}, \quad (3)$$

where $Trade_{\mathrm{irj}}$ indicates whether respondent i participated in a transaction in group j and round r, $Profit_{irj}$ captures the associated profits, CV is a vector of card values, R is a vector of round dummies, and X is a vector of trader characteristics (including a buyer dummy). We conjectured age, gender, income, and literacy to affect success in the game. In addition, we explore the effect of social status on trading propensity and profitability. As explained earlier, if participants use their game play to invest in patron-client networks or if they adhere to social norms of sharing with those in power in their local hierarchies, status would have a significant effect on trading behavior. Insights from the individual trade level data thus complement the aggregate analysis discussed above.

We use two specifications when estimating model (3). We consider the case where the dependent variable is profits conditional on trading (as captured in Eq. (2)), as well as the case where all non-traders are coded as zero profit traders. The latter specification may be the more appropriate model in light of the fact that the decision to trade is to some extent endogenous (even if randomly assigned card values obviously matter as well).

RQ 5: *Do individual characteristics, including demographic information, social status, and social connectedness, explain trading behavior and profits in a systematic way?*

Based on our results from the pilot (see Appendix Table C4), we expected that men would earn more than women, and that high-status buyers would earn less than low-status buyers on average. Social network effects are particularly difficult to theoretically pin down (see Jackson, 2014 for a review). For example, reciprocity can be negative or positive, depending on unobserved behavior outside the game. Also, what is locally inefficient in the game may be globally efficient when analyzed in the context of real-world strategic interactions between experimental subjects. Because experiments have rarely used non-anonymous, personally connected subjects, when embedding games in real-life networks we had little information on which to base a prior belief. We view this study as an exploratory approach to informing theory on the workings of markets in a developing world context.

After the trading sessions we implemented a short exit survey recording individual characteristics such as age, gender, literacy, farm size (number of acres) and prior trading experience. We also asked subjects about their motives for engaging in trade, in the experiment. As mentioned, we stratified sampling on status, or formal and informal positions of authority within the community (village chief, youth leader, women leader, religious leader, etc.). We create a binary variable to indicate whether a respondent holds such a position. During the exit interview we developed an alternative status proxy. We asked participants to line up in order of social status in their experimental group after the trading, which provided us with a respondent-specific ordinal measure of his or her position in the local hierarchy. Higher scores indicate a place at the back of the line and relatively low status. Our respondents had no difficulty agreeing on the proper line-up.

Table 2
Descriptive Statistics and Balance.

		Treatments					
		A	B	C	D	*F* test (p-value)	N
Age	Respondent age in years	42.167	42.630	42.641	42.073	0.950	1502
		(0.981)	(0.908)	(0.788)	(0.929)		
Literate	Dummy, 1 if respondent can read and write	0.251	0.261	0.189	0.226	0.200	1423
		(0.028)	(0.027)	(0.025)	(0.034)		
Male	Dummy, 1 if respondent is male	0.662	0.701	0.649	0.646	0.387	1500
		(0.029)	(0.026)	(0.018)	(0.027)		
Farm size	Respondent farm size (acres of upland rice planted previous year)	2.608	2.504	2.304	2.311	0.229	1491
		(0.129)	(0.107)	(0.127)	(0.126)		
Trader	Dummy, 1 if respondent is trader	0.415	0.398	0.398	0.531	0.041	1501
		(0.037)	(0.038)	(0.035)	(0.039)		
Social status order	Respondent place in line up in order of who is the most influential, second most influential, etc. in village	8.591	8.500	8.495	8.500	0.187	1424
		(0.103)	(0.017)	(0.005)	(0.004)		
High status	Dummy, 1 if subject is village chief, religious leader, youth leader, woman's leader or other male leader	0.399	0.452	0.375	0.467	0.043	1497
		(0.022)	(0.022)	(0.022)	(0.040)		

Note: Data is at the individual respondent level, standard errors in brackets clustered at experimental session level.

Table 2 provides descriptive statistics for these variables and includes a balance test on the equality of means across all treatments. Most variables are balanced across treatment arms, except whether the respondent is a trader (higher in Treatment D) and of high status (higher in Treatments B and D). When we control for these variables in the regression models below, results are qualitatively identical. Nevertheless we acknowledge that some trader characteristics (perhaps unobserved) appear to vary across treatments, and this could be a confounding factor affecting overall trading outcomes. Of course there are differences in the incidence of social relationships for the intra-village treatments and the inter-village treatment.[8] These differences, however, are intentional and indeed form an important part of the treatment.

4. Experimental results: aggregate data

Our first set of results is summarized in Fig. 2 and Table 3. Panel A of Fig. 2 reports the average number of trades per round for each treatment, and Panel B reports average efficiency levels. When analyzing the aggregate data, we define efficiency of a particular round as the percentage of the potential surplus that is captured by the traders in that round. The maximum sum of producer and consumer surplus in our experimental market, as summarized in Fig. 1, is 17,000 Leones. If the sum of producer and consumer surplus in a round equals, say, 14,000 Leones, the level of efficiency is simply 82%.

A natural grouping of treatments is suggested in Fig. 2. In terms of the number of trades, the intra- and inter-village treatments appear similar (A and B) as do the two middleman treatments (C and D), but these two sets appear distinct from each other. This provides tentative support for the hypothesis that norms about appropriate exchange behavior when interacting face-to-face are important, and that trading outcomes can be changed by manipulating the trading technology. This pattern is not readily evident from the efficiency data (panel B). However, as panels A and B suggest, there is some learning during early stages of the experiment (especially in the middlemen treatments). To analyze these differences across treatment arms more formally below, we use a regression framework.

Turning to Table 3, and focusing on intra-village Treatment A first, we observe that the number of trades is above theoretical predictions (5.6 on average rather than 5), and associated efficiency levels are below theoretical predictions. This is consistent with a cultural imperative not to disappoint a potential trading partner (even if the surplus for trading is very small, and it may be better to search for an alternative trading partner). Overall, participants capture up to 84.4 percent of profits. The degree to which this is regarded as inefficient is, of course, subjective, but we do observe this figure is significantly

[8] Comparing Treatment A to Treatment B, the percentage blood related drops from 29% to 4%, co-farming drops from 11% to 2%, regular contact goes from 56% to 6% and exchange partners from 20% to 3%.

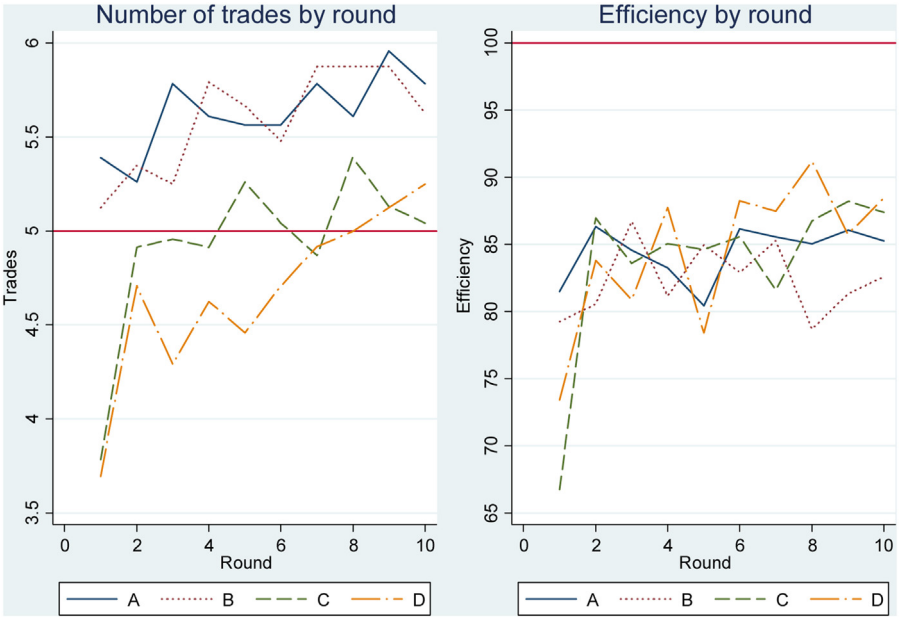

Fig. 2. Number of Trades and Efficiency by Treatment A–D. *Notes:* Efficient outcome is 5 units traded and 100% of profits captured

Table 3
Trading behavior across treatments.

Market Period	1	2	3	4	5	6	7	8	9	10
Treatment A (intra village)										
Average price (Leone)	4940,37	4826,81	4853,00	4846,01	4919,20	4790,37	4970,81	4757,51	4817,91	4859,42
SD	1404,84	1269,90	1349,01	1367,32	1440,43	1229,05	1258,64	1261,03	1237,31	1260,51
Trade	5,39	5,26	5,78	5,61	5,57	5,57	5,78	5,61	5,96	5,78
Trade in core	2,22	2,61	2,52	2,13	2,61	2,61	2,83	2,87	2,78	2,83
Trade without profits	0,48	0,48	0,91	0,52	0,96	0,78	0,70	0,74	0,78	0,91
Efficiency	0,82	0,86	0,85	0,83	0,80	0,86	0,86	0,85	0,86	0,85
Buyer efficiency share	0,44	0,48	0,48	0,46	0,43	0,50	0,46	0,48	0,48	0,47
Seller efficiency share	0,38	0,38	0,37	0,37	0,37	0,36	0,40	0,37	0,38	0,38
Treatment B (inter village)										
Average price (Leone)	4740,53	4597,21	4734,31	4821,18	4851,84	4713,61	4772,40	4720,14	4887,00	4776.3
SD	1436,11	1322,41	1192,38	1397,17	1349,67	1291,07	1176,26	1396,82	1383,39	1309.1
Trade	5,13	5,35	5,25	5,79	5,67	5,48	5,88	5,88	5,88	5.6
Trade in core	2,38	2,48	2,71	2,58	2,88	2,87	2,96	2,71	2,67	2.9
Trade without profits	0,42	0,61	0,50	0,83	0,71	0,61	0,88	0,83	0,58	0,6
Efficiency	0,79	0,81	0,87	0,81	0,85	0,83	0,85	0,79	0,81	82.6
Buyer efficiency share	0,47	0,51	0,53	0,47	0,50	0,50	0,50	0,48	0,45	57.4
Seller efficiency share	0,32	0,30	0,34	0,34	0,34	0,33	0,35	0,30	0,36	42.6
Treatment C (Middlemen revealed)										
Average price (Leone)	4632,97	4688,41	4709,73	4650,72	4567,75	4730,43	4822,26	4749,07	4817,03	4797.1
SD	1015,64	1017,38	1103,08	1044,25	1085,07	1015,22	1056,57	956,07	971,34	860.0
Trade	3,78	4,91	4,96	4,91	5,26	5,04	4,87	5,39	5,13	5.0
Trade in core	1,74	2,43	2,30	2,52	2,35	2,70	2,70	3,04	2,70	2.7
Trade without profits	0,35	0,26	0,43	0,22	0,39	0,13	0,30	0,52	0,17	0,4
Efficiency	0,67	0,87	0,84	0,85	0,85	0,86	0,82	0,87	0,88	87.4
Buyer efficiency share	0,38	0,49	0,50	0,49	0,51	0,48	0,45	0,48	0,48	52.8
Seller efficiency share	0,29	0,38	0,34	0,36	0,34	0,38	0,36	0,39	0,41	47.2
Treatment D (Middlemen anonymous)										
Average price (Leone)	4743,48	4682,99	4598,26	4687,15	4600,35	4639,24	4767,43	4723,61	4803,47	4790.3
SD	1066,49	1061,59	1014,90	922,63	921,55	967,10	930,34	898,57	992,87	1052.7
Trade	3,70	4,71	4,29	4,63	4,46	4,71	4,92	5,00	5,13	5.3
Trade in core	1,39	2,42	2,21	2,75	2,67	2,71	2,71	3,04	2,88	2.8
Trade without profits	0,04	0,33	0,38	0,17	0,13	0,21	0,38	0,13	0,29	0,3
Efficiency	0,73	0,84	0,81	0,88	0,78	0,88	0,88	0,91	0,86	88.5
Buyer efficiency share	0,44	0,50	0,47	0,51	0,44	0,52	0,49	0,52	0,46	56.1
Seller efficiency share	0,30	0,34	0,34	0,37	0,35	0,37	0,39	0,39	0,40	43.9

Note: Tables reports round averages.

Table 4
Analysis of aggregate trading behavior.

	(1) Overall efficiency	(2) Efficiency buyer	(3) Efficiency seller	(4) # Trades	(5) # trades with no profit
B	−0.0275*	0.0208	−0.0484***	0.0573	0.0495
	(0.0152)	(0.0155)	(0.0167)	(0.116)	(0.189)
C	−0.0117	0.00394	−0.0157	−0.641***	−0.454***
	(0.0149)	(0.0144)	(0.0165)	(0.105)	(0.148)
D	−0.000740	0.0158	−0.0165	−0.894***	−0.435***
	(0.0131)	(0.0132)	(0.0140)	(0.0865)	(0.128)
Constant	0.855***	0.486***	0.368***	5.834***	1.147***
	(0.0182)	(0.0194)	(0.0140)	(0.144)	(0.273)
Observations	917	917	917	917	917
Number of Clusters	92	92	92	92	92
pvalue B = C	0.20	0.25	0.034	0.000	0.002
pvalue B = D	0.021	0.69	0.021	4.6e-15	0.0011
pvalue C = D	0.36	0.35	0.95	0.0092	0.87

Estimated using OLS. Regression includes blocking (experimental location) fixed effect.
** p < 0.05.
* p < 0.10.
*** p < 0.01.

lower (in relative terms) than efficiency levels reported in similar studies conducted in the lab and/or in the developed world (e.g. List, 2004).

Moreover, there is only limited evidence of convergence towards market equilibrium: while aggregate efficiency levels tend to increase between rounds 1–5 and rounds 6–10, the effect is small (difference is 2.4%), and not significant at conventional significance levels (*p value* = 0.11). A regression of efficiency on round ID gives a slope of 0.3 with a *p*-value of 0.27 (standard errors clustered at experimental session). As a comparison, List's field study, focusing on sports card trading, yielded 89 percent efficiency in the first round and 97 percent efficiency in the fourth and fifth rounds. There is no such trend for Treatment A. This suggests other motivations, perhaps related to social structures and pre-existing relationships, may interfere with market behavior and compromise efficiency. This leads to the following result:

Result 1: *Within our subject pool the intra-village treatment yields trade behavior that is somewhat less efficient than would be predicted by economic theory. There is no convergence across rounds of trading.*

Also observe from Table 3, we are studying a market where buyers are able to secure most of the profits from trade. This may reflect a cultural imperative, or may be an artefact of our design (for example: while sellers were asked to stay in the same location, buyers were encouraged to move around and learn about alternative offers).

Result 2: *There is an asymmetry in our experimental market: buyers on average make greater profits than sellers.*

We next examine whether face-to-face interaction and local hierarchies matter for economic behavior, or whether norms about appropriate behavior affect profits from trade. To this end we compare profits across the four treatments. This can be done by casually inspecting trading outcomes in Table 3, or by using a regression framework. We formally test the comparisons in Table 4. Efficiency increases by 2.75% when participants are trading with fellow villagers, compared to a situation where they have to trade with strangers. To some extent this is not surprising – it is well-known to economists that social relationships may lower transaction costs and that markets can only function properly in the context of trust (Fafchamps, 2011). But anthropologists have pointed to the role of patron-client and kinship networks as factors impeding efficiency. While such concerns may indeed mediate the allocation of goods within tightly knit communities, we do not find that trading outcomes are less efficient. The reverse is true in our experiment.

In contrast, we observe that the introduction of a trading technology eliminating face-to-face interaction between the buyer and seller ("middlemen") does not affect efficiency. The dummies associates with treatments C and D are not significantly different from zero. Hence eliminating face-to-face interaction with co-villagers, or even making trade with co-villagers fully anonymous, does not affect efficiency compared to regular intra-village trade. It appears as if specific social relations are relatively unimportant for overall efficiency, but that within-village norms of appropriate behavior do matter and persist across trading technologies (i.e., face-to-face interaction or not, anonymous or not).

Interestingly, while personal relationships (A) and impersonal trading (C, D) have the same effect on overall efficiency, they arrive at this outcome via different channels. Specifically, and as evident from columns (4–5) of Table 4, the middlemen treatments improve efficiency by lowering the number of trades, and weeding out transactions that should not have occurred (because they involve partners that should have been "out of the market" given their induced values) or that generate zero profit for at least one of the trading partners.

Another observation is noteworthy. Trading with people from a different village lowers efficiency because sellers earn less than in the intra-village treatments (column 3). It appears as if buyers are more generous towards sellers from their own village than towards villagers from other villagers. Representation by a middleman does not seem to matter, which

Table 5A
Trading outcomes as explained by experimental variables.

	(1) Trade YN	(2) Trade profit	(3) Profit, 0 = notrade
B	0.00717	−65.20**	−43.04***
	(0.0150)	(30.07)	(15.59)
C	−0.0871***	104.9***	−14.76
	(0.0148)	(30.84)	(15.83)
D	−0.111***	167.1***	9.368
	(0.0107)	(21.57)	(12.76)
Observations	14672	9554	14672
Number of Clusters	92	92	92
pvalue B = C	1.0e-08	0.000000080	0.031
pvalue B = D	4.3e-14	8.4e-15	0.000045
pvalue C = D	0.085	0.024	0.091

Estimated using OLS. Dependent variable is decision to trade and profits. Regression includes blocking (experimental location) fixed effect, induced values, group dummy if card value correction, buyer dummy and round dummies.
* p < 0.10.
** p < 0.05.
*** p < 0.01.

speaks to an emerging literature suggesting the involvement of delegates may invite more selfish behavior (e.g., Hamman et al., 2010; Coffman, 2011).

Result 3: *Face-to-face trading with an unknown partner from another village reduces efficiency, and especially sellers earn less when engaging with somebody from another village. When trading with a co-villager, respondents achieve higher overall efficiency regardless of the trading technology or the level of anonymity of the trading partner.*

Alternative explanations exist for why trading via middlemen produces more efficient outcomes than the inter-village treatment. Middlemen have no incentive to make a trade, and can more credibly threaten to walk away from any potential deal. Another compelling interpretation is that the act of giving instructions to the middleman helped the buyer to make more rational trading choices, and the fact that the buyer could not readjust his or her strategy on the fly in reaction to social pressures meant that the rational trading strategy was more likely to be followed. That is, perhaps the middlemen treatment fosters commitment to a specific trading strategy. However, observe that these explanations appear inconsistent with the finding that the extra value that is created is not shifted to the buyer that is represented by the middleman, and with the simple observation that efficiency is not different than in Treatment A. Alternatively, in case the broker has to consult regularly with the buyer, it may be true that the "cost of trading" varies across treatments. This could be another factor explaining differences in trading behavior and outcomes – if the process of trading through a middleman is more time consuming, the low number of trades may have been caused mechanically by less time available to negotiate. However, we view this as unlikely because the time limit was not a binding constraint in the trading sessions.

5. Experimental results: trader-level data

We next explore the efficiency of trades in more detail at the level of individual traders. In Table 5A we estimate a series of parsimonious OLS models, only including as controls the dummies for randomization blocks, and a vector of experimental variables: treatment dummies, induced (card) values, a buyer dummy, and trading round dummies.[9]

Echoing earlier results, column (1) reveals that the propensity to engage in trade is reduced when middlemen are active. Individuals are 9–11% less likely to exchange when trading via a middleman. In the other columns we explain variation in individual profits (controlling for the same covariates). In column (2) we focus on the subsample of agents who actually made a trade, and in column (3) we also consider participants who did not engage in trade (for example because of unfavorable induced values) and include them as zero-profit traders. Not surprisingly, we again find that trading outcomes are "worse" in the inter-village treatment. But another insight emerges from these columns. While the middleman trading technology is equally efficient as Treatment A, it produces high profits for a smaller subsample of respondents. The "gap" in profits under the middlemen scenarios and the intra-village scenario diminishes when zero-profit traders are included in the analysis. That is; the middlemen treatments raise profits, and cluster these profits in the hands of a relatively small number of villagers. Profits in Treatment A are significantly lower than in Treatments C and D when zooming in on the subset of traders, but this gap evaporates when also taking the non-traders into account.

Result 4: *Intra-village Treatment A spreads the profits from trade more equally across participants than the treatments involving middlemen.*

[9] Similar results are obtained when using a logistic or probit model to explain variation in the decision to engage in trade—details available on request.

Table 5B
Trading outcomes as explained by experimental variables and observables.

	(1) Trade YN	(2) Trade profit	(3) Profit, 0 = notrade
B	−0.00199	−50.57*	−35.75**
	(0.0142)	(30.39)	(17.50)
C	−0.0875***	115.5***	−5.549
	(0.0149)	(33.81)	(18.18)
D	−0.112***	183.8***	22.38
	(0.0103)	(23.02)	(14.62)
Age	−0.0002	−2.991***	−2.292***
	(0.0002)	(0.953)	(0.630)
Literate	0.0299***	10.17	35.51*
	(0.009)	(29.16)	(20.26)
Gender	−0.0039	82.28***	50.66***
	(0.008)	(23.77)	(18.08)
Farm size	0.002	−4.638	−0.221
	(0.0018)	(6.198)	(4.348)
Trader	0.004	25.96	19.48
	(0.005)	(21.82)	(15.54)
Social Line-up	−0.0006	−1.830	−1.619
Order	(0.0007)	(3.125)	(2.162)
High Status	−0.004	1.785	9.168
	(0.007)	(28.06)	(19.77)
Observations	12996	8423	12996
Number of Clusters	90	90	90
pvalue B = C	0.000	0.000	0.053
pvalue B = D	0.000	0.000	0.000
pvalue C = D	0.090	0.023	0.098

Estimated using OLS. Dependent variable is decision to trade. Regression includes blocking (experimental location) fixed effect, induced values, group dummy if card value correction, buyer dummy and round dummies.

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Not surprisingly, card values matter as well (see Appendix Table A1). For buyers, higher card values are also associated with a greater probability of trading, and greater profits. The reverse is true for sellers, which is of course what we would expect if players understood the game. Finally, the coefficients of the round dummies do not vary much (for trade and profit models), confirming the lack of convergence at the aggregate level in our experimental market towards theoretical predictions.

We find similar results when controlling for individual (and experimental) characteristics. These characteristics are obviously not randomly assigned, so these associations should be interpreted with more care. Results are provided in the top 3 rows of Table 5B. Some individual characteristics are significantly associated with the propensity to trade or the profits from trade. Consistent with other writings, we find that older participants and women tend to earn less from trade. While the effects for age is relatively small, the gender effect seems to be not only statistically but also economically significant. There is little literature on how individual characteristics may matter for personal exchange. The literature mainly focuses on the role of market experience (see List, 2003). Insofar as age, gender and literacy proxy market experience, the results are consistent with a perspective that younger and literate males are more likely to trade and make higher profits. But observe that the trader dummy does not enter significantly, so it appears as if market experience is less important in our setting than in other contexts that have been studied. Average profits from trading (pooling across treatments and rounds) amount to Le 1350, so on average trading men earn about 7 percent more than women. This effect is similar in size as being some 27 years older, according to the regression coefficients. In contrast, farm size (our proxy for wealth) is not correlated with profits in the experiment.[10]

Contrary to our expectations we find across our models that our status variables do not enter as significant determinants of profit. This is true for the social line up variable as well as the authority dummy. Hence, while social relationships and knowledge of the identity of trading partners helps to overcome trading inefficiencies, they do not systematically favor

[10] In other words: while there is some heterogeneity in literacy, wealth or experience in our sample, this heterogeneity does not explain variation in profits in the experiment. We therefore have no reason to attribute the relatively low levels of trading efficiency (compared to previous trading experiments) in our experiment to low average levels of literacy, wealth and experience.

certain social groups over others – high status individuals do not exploit low status individuals, but they also do not appear to be particularly generous towards them.

Finally, the results in Table 5B may be used to consider one plausible explanation for low trading efficiency in our experiment (our Result 1 above). Perhaps low efficiency levels are caused by the simple fact that we are dealing with a sample of inexperienced and relatively poorly educated subjects, learning more slowly than their counterparts in Western societies. To probe this issue one may ask whether trading behavior (and profits) vary with "years in school" or literacy status. Table 5B reports mixed support for this hypothesis. There is no significant association for years of education, but literate subjects are more likely to engage in trade. They do not consistently earn higher profits (depending on the treatment of no-traders). This suggests that, in addition to behavioral norms and local hierarchies, low education levels may also be partly responsible for the relatively low level of efficiency we observe in our experiment.

Result 5: *Several individual characteristics (age, gender and literacy) are correlated with trading behavior in the market experiment. Status and social connectedness do not seem to be major determinants of trading performance.*

6. Discussion and conclusion

We use a field experiment in rural Sierra Leone to study how trading behavior conforms to neoclassical market theory. We extend the testing of competitive market theory into the field, and hope that our explorations will be useful to sharpen theoretical predictions. Unlike earlier field experiments, our results are based on a subject pool of subsistence farmers from a remote region of Sierra Leone, Africa. While our respondents occasionally exchange goods and services in market settings, their normal exchange patterns are based on repeated and personalized interaction, and rarely involve cash prices. Our study is also markedly different than others in the literature because our participants are socially connected in daily life. They are from the same social networks. Based on previous literature and a pilot study we developed an experimental design with treatments that allow us to explore the dynamics of real-life interactions in rural Sierra Leone, including friendship, animosity, patron-client relationships, and reciprocation of all types. This allows us to study the effects of (and interaction between) behavioral norms and pre-existing social relations on trading efficiency in a unique geographical setting. Our results speak to the transformation from personalized exchange to anonymous exchange that has historically taken place as economies develop.

With the paucity of data on behavior in competitive markets in developing countries, our (explorative) study offers novel perspectives and opens new directions for additional research. It offers mixed support for the hypothesis that the introduction of competitive markets produces efficient outcomes in the setting we study. Inter-village trade is relatively inefficient for our sample of African subsistence farmers, and we find no evidence of market convergence across rounds. Efficiency decreases in the inter-village treatment, and in contrast to the literature emphasizing the adverse impacts of kinship networks or local hierarchies on economic efficiency, we find that socially-connected traders do better than socially-unconnected ones – not worse. This finding may also speak to the literature on the role of parochialism in trade. For example, studying trade across ethnic groups (rather than across villages), Bowles and Gintis (2004) find that foregoing trading opportunities with "outsiders" may facilitate trade with co-ethnics, so that exchange within narrow parochial networks may co-exist alongside anonymous market trade. While we do not observe that villagers decline to trade with outsiders, the terms of trade appear different and overall efficiency gains are reduced. Future research could explore to what extent these tendencies are caused by a preference for practices geared towards excluding people from other villages, or by the evolution of within-village information and enforcement structures that do the same.

We also find that the efficiency of intra-village trade is not thwarted when we make trade "more anonymous." This is consistent with norms of appropriate behavior towards fellow villagers that are to some extent internalized by our respondents. Nevertheless, there are differences across the intra-village treatments, depending on whether or not there is face-to-face interaction between agents and the level of anonymity. Specifically, too many trades take place when trading takes place on a face-to-face basis, so it appears as if our subjects (especially our sellers) are averse to disappointing potential trading partners. They engage in exchange even if the surplus is small and one of the trader parties could do better by walking away and search for another partner with a more favorable reservation price.[11] This seems easier when interacting with somebody who has no personal stake in the trade. Future research should examine this further.

Several caveats remain for the interpretation of our aggregate experimental results. For example, not all subject characteristics were perfectly balanced across treatment arms, and this could introduce a possible confound. The share of traders in the sample was some 20% larger in one of the middlemen treatments and in the inter-village treatment high-status subjects were over-represented. It is unclear whether these trader characteristics are a relevant confound or not, but we take comfort in the observation that status and trader experience are unimportant when explaining variation in individual trading behavior. Of course trader characteristics may also be different along different, unobserved dimensions. It is also unclear whether differences in trading behavior and outcomes are partly caused by differences in transaction costs across treatments – especially when comparing treatments with and without middlemen.

[11] We do not claim that our findings spill over to other domains of human interaction than market behavior. For example, other studies have shown that social relations and hierarchies matter greatly for, say, pro-social behavior (e.g., Baldassari and Grossman, 2013).

Individual-level profit results allow us to probe the determinants of trade a little deeper. Confirming the patterns in the aggregate data, we again find that social relationships are relatively unimportant as determinants of trading behavior. While variables such as gender and age affect the profits from engaging in trade, other important variables such as education, farm size (wealth proxy) and trading experience do not robustly affect individual profits. We also do not find that status or the position within local hierarchies affects trading outcomes. High status individuals do not exploit low status individuals, but also do not seek them out for favors. Nevertheless we acknowledge that low literacy levels among our pool of subjects can also contribute to explaining the observed low levels of efficiency.

The generality of our results come with some qualifications. On the one hand, this region of Sierra Leone is representative of only select areas of Africa. It is an isolated and forested region, with very low incomes, and low levels of market integration.[12] The finding that neoclassical theory does not fare so badly even in this extreme context is perhaps remarkable. Our finding that behavioral norms may be more important than social relationships may reflect local Mende culture, which is distinct from other (African) cultures (e.g., Leach, 1994; Richards, 1996).[13] Further research should explore to what extent our results extend to other regions in (West) Africa and the world. An alternative interpretation of external validity concerns generalizability beyond the (Sierra Leonean) lab. For example, the suggestion that cultural norms adversely affect efficiency might partly explain why so much of interregional trade in Sierra Leone used to be organized through agents and subagents – "trading on commission" (Riddell, 1974). The standard explanation for the widespread occurrence of this phenomenon is based on low trading volumes and high transaction costs (rendering trading on commission efficient), but we conjecture that behavioral imperatives might also play a role (of course we recognize the two need not be independent).

Finally, earlier theoretical studies of the transition from personalized to anonymous exchange assume traders are of a specific type – flourishing in either personalized or anonymous exchange. For example, Kranton (1996) assumes agents choose to become a reciprocal trader or an anonymous trader, but not both. Similarly, Kumar and Matsusaka (2009) assume agents can use only one of two types of human capital: local capital (facilitating personalized exchange) or market capital (facilitating anonymous exchange). Such specialization in specific trade modalities combined with complementarities in trade (the gain from being a personalized trader increases as the share of personalized traders in the population goes up) implies scope for so-called "lock-in effects" where inefficient equilibria persist. Our results cast new light on this issue, and attenuate concerns about lock-in phenomena. While changing cultural imperatives about appropriate (trading) behavior may be a difficult and slow process, our sample of inexperienced traders is able to quickly adapt its behavior, seizing potential gains from trade, if we alter the market institution. This implies that if structural features of rural markets can be adjusted, for example by introducing agents of change (brokers) linking rural producers to regional markets, trading efficiency could improve rapidly.

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Appendix A. Additional tables

Appendix B. Experimental protocol and instruments

See separate Appendix documents: the experimental protocol (B1) and instruments (B2) in Supplementary materials.

[12] Other data suggest that our sample is not necessarily representative for Sierra Leone as a whole (certainly not for urban areas). For example, Muslims (97%) and Mende-ethnics (92%) are over-represented in our sample, compared to the national average (60% and 31%, respectively, see CIA, 2013).

[13] The Mende society we study is part of a larger group of slave-based ranked lineage societies, characterized by a chieftaincy institution and a large labouring underclass. These are agrarian communities occupying the margins of the Upper Guinean Forest. For a treatment of these societies, and a comparison with more egalitarian communities at the forest core, refer to Chauveau and Richards (2008).

Table A1
Trading Outcomes as Explained by Experimental Variables.

	(1) Trade YN Round 1–10	(2) Trade YN Round 6–10	(3) Trade profit Round 1–10	(4) Trade profit Round 6–10	(5) Profit, 0 = notrade Round 1–10	(6) Profit, 0 = notrade Round 6–10
B	0.00717	0.0310*	−65.20**	−126.2***	−43.04***	−71.17***
	(0.0150)	(0.0160)	(30.07)	(33.91)	(15.59)	(21.31)
C	−0.0871***	−0.0635***	104.9***	69.35**	−14.76	−15.71
	(0.0148)	(0.0149)	(30.84)	(33.01)	(15.83)	(20.91)
D	−0.111***	−0.0821***	167.1***	141.7***	9.368	31.94*
	(0.0107)	(0.0107)	(21.57)	(25.86)	(12.76)	(16.98)
cvb==2500	−0.129***	−0.149***	−289.4***	0	−121.6***	−118.8***
	(0.0179)	(0.0251)	(46.16)	(0)	(14.03)	(18.72)
cvb==3500	0	0	0	190.6***	0	0
	(0)	(0)	(0)	(60.75)	(0)	(0)
cvb==4500	0.254***	0.277***	−26.47	189.1**	167.7***	182.0***
	(0.0211)	(0.0291)	(46.13)	(73.96)	(21.75)	(34.89)
cvb==5500	0.552***	0.571***	272.4***	484.0***	614.7***	631.7***
	(0.0190)	(0.0250)	(45.32)	(57.55)	(23.27)	(30.83)
cvb==6500	0.648***	0.646***	699.3***	883.5***	1091.8***	1065.6***
	(0.0208)	(0.0282)	(47.63)	(74.12)	(36.90)	(54.83)
cvb==7500	0.698***	0.703***	1392.0***	1655.3***	1812.1***	1884.5***
	(0.0182)	(0.0228)	(59.94)	(83.18)	(49.93)	(61.99)
cvb==8500	0.717***	0.709***	2233.2***	2406.4***	2655.7***	2625.5***
	(0.0170)	(0.0231)	(62.87)	(94.96)	(55.72)	(77.56)
cvs==1500	0.785***	0.0558***	1712.4***	1826.3***	2096.1***	1375.2***
	(0.0188)	(0.0138)	(69.69)	(90.18)	(66.00)	(69.38)
cvs==2500	0.769***	0.0449***	1083.4***	1044.2***	1452.6***	580.5***
	(0.0200)	(0.0130)	(51.97)	(70.16)	(47.76)	(57.90)
cvs==3500	0.714***	0	516.2***	498.1***	851.9***	0
	(0.0232)	(0)	(40.52)	(55.50)	(32.00)	(0)
cvs==4500	0.608***	−0.0739***	164.7***	150.4***	447.4***	−384.4***
	(0.0207)	(0.0176)	(31.38)	(47.64)	(19.89)	(38.48)
cvs==5500	0.277***	−0.454***	13.30	−23.08	147.2***	−731.5***
	(0.0237)	(0.0295)	(33.12)	(47.74)	(14.95)	(38.23)
cvs==6500	0	−0.744***	0	0	0	−870.5***
	(0)	(0.0258)	(0)	(0)	(0)	(37.96)
cvs==7500	−0.123***	−0.859***	−124.9**	−42.35	−66.32***	−930.4***
	(0.0188)	(0.0194)	(49.73)	(53.08)	(10.20)	(35.47)
Dummy, 1 if S/D correction	0.0112	−0.0219	53.91*	110.5***	54.83***	68.20***
	(0.0146)	(0.0144)	(28.81)	(32.60)	(14.17)	(20.80)
round_id==2	0.0680***	0	4.368	0	90.61***	0
	(0.0174)	(0)	(25.76)	(0)	(21.95)	(0)
round_id==3	0.0674***	0	4.286	0	88.11***	0
	(0.0175)	(0)	(26.15)	(0)	(21.86)	(0)
round_id==4	0.0892***	0	−25.51	0	92.91***	0
	(0.0155)	(0)	(25.17)	(0)	(21.91)	(0)
round_id==5	0.0889***	0	−43.73	0	70.24***	0
	(0.0171)	(0)	(26.39)	(0)	(20.15)	(0)
round_id==6	0.0849***	0	−3.219	0	108.3***	0
	(0.0177)	(0)	(26.86)	(0)	(22.30)	(0)
round_id==7	0.105***	0.0208	−29.40	−25.83	99.66***	−8.166
	(0.0167)	(0.0134)	(24.83)	(27.50)	(20.04)	(16.88)
round_id==8	0.120***	0.0357**	−48.01	−45.14	104.4***	−3.411
	(0.0180)	(0.0139)	(29.22)	(28.60)	(19.97)	(16.96)
round_id==9	0.125***	0.0408***	−54.12*	−51.69*	102.6***	−5.199
	(0.0160)	(0.0124)	(28.02)	(27.92)	(21.10)	(17.30)

Table A1 (*Continued*)

	(1) Trade YN Round 1–10	(2) Trade YN Round 6–10	(3) Trade profit Round 1–10	(4) Trade profit Round 6–10	(5) Profit, 0 = notrade Round 1–10	(6) Profit, 0 = notrade Round 6–10
round_id==10	0.114***	0.0297**	−34.51	−32.37	107.8***	−0.0693
	(0.0170)	(0.0138)	(28.00)	(26.63)	(21.15)	(15.67)
Buyer	0.0430**	−0.680***	199.1***	−48.56	71.33***	−798.8***
	(0.0188)	(0.0243)	(43.36)	(61.45)	(14.15)	(43.72)
Constant	0.189***	0.968***	451.3***	476.0***	19.89	954.6***
	(0.0276)	(0.0276)	(49.22)	(54.80)	(26.97)	(43.83)
Observations	14672	7344	9554	4954	14672	7344
Number of Clusters	92	92	92	92	92	92
B = C	1.0e-08	5.3e-09	0.000000080	2.3e-10	0.031	0.0029
B = D	4.3e-14	7.2e-12	8.4e-15	7.6e-15	0.000045	0.000000026
C = D	0.085	0.19	0.024	0.0097	0.091	0.0095

Estimated using OLS. Dependent variable is decision to trade. Even columns restrict the sample to the last 5 rounds. Regression includes blocking (experimental location) fixed effect, induced values, buyer dummy and round dummies.

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Appendix C. Experimental results round 1

See Table C1

Table C1
Descriptive Statistics Trading Sessions.

	Sessions	Number of Participants	Total Observations (Participant-Rounds)	Total Trades Executed (Trade-Rounds)
Control	7	112	480	314
Middlemen	3	48	224	154
All strangers	5	80	360	246

See Table C2

Table C2
Trading behavior across the three treatments.

Market period	1	2	3	4	5	6	7	8	9	10
Panel A: Base treatment: trade with co-villagers										
Average price	4432	5062	4844	4809	4937	4802	5006	4877	4765	4822
SD	721	442	515	521	230	463	182	360	491	400
Buyer Profit	9000	6429	8357	7286	7643	7800	7300	7900	7200	8500
Seller Profit	5286	6000	6357	5429	7071	6000	6500	6900	6200	6500
Trades (N)	4.1	4.7	5.0	5.0	5.9	5.8	6.0	5.2	5.8	5.4
Trades in core	2.0	1.9	2.6	2.3	3.4	3.0	2.8	3.0	2.6	3.0
Efficiency	84%	73%	87%	75%	87%	81%	81%	87%	79%	88%
Panel B: Middlemen treatment: trade through middlemen										
Average price	5278	5050	4800	4889	4983	4883	4472	4833	4936	4642
SD	385	136	346	96	275	375	413	382	192	625
Buyer Profit	6000	7500	7833	7833	8333	8167	9333	7333	7750	8500
Seller Profit	6333	6500	6833	7167	7667	7500	5667	6333	5750	6500
Trades (N)	4.0	5.7	5.7	5.7	5.3	5.7	5.3	6.3	6.0	5.5
Trades in core	1.3	2.7	3.3	2.7	4.0	3.3	3.3	3.0	2.0	3.5
Efficiency	73%	82%	86%	88%	94%	92%	88%	80%	79%	88%
Panel C: Inter-village treatment: trade with individuals from other villages										
Average price	4730	5016	4848	4823	4880	4992	5013	4608	4671	4788
SD	179	341	509	284	295	232	401	79	513	25
Buyer Profit	8400	7700	6900	8400	7600	7750	7500	8875	9750	8500
Seller Profit	6800	6700	6700	6400	6600	7500	7250	5875	6500	6750
Trades (N)	5.2	5.8	5.2	5.8	5.4	5.3	5.5	5.8	5.5	5.3
Trades in core	2.8	3.0	2.6	2.8	2.8	3.8	3.0	3.0	3.0	3.5
Efficiency	89%	85%	80%	87%	84%	90%	87%	87%	96%	90%

See Table C3

Table C3
Efficiency Round 1 data.

	(1)efficiency	(2)# trades
Inter village	0.0510	0.233
	(0.0459)	(0.261)
Middlemen	0.0333	0.267
	(0.0495)	(0.316)
Constant	0.820***	5.233***
	(0.0430)	(0.204)
Observations	133	133
Number of Clusters	15	15
Intra = Middleman	0.56	0.91

Estimated using OLS. Standard errors clustered at experimental group level.
*p < 0.10.
**p < 0.05.

Table C4
Trade outcomes Round 1 Data.

	(1) Trade YN	(2) Trade YN	(3) Trade profit	(4) Trade profit	(5) Profit, 0 = notrade	(6) Profit, 0 = notrade
Intra village	−0.0291	−0.0116	−31.43	−52.53	−56.23	−59.26
	(0.0304)	(0.0275)	(68.31)	(70.98)	(49.60)	(50.00)
Middlemen	0.00377	0.0193	−22.69	−32.44	−17.45	−12.45
	(0.0364)	(0.0352)	(47.61)	(43.82)	(28.77)	(23.51)
cvb==2500	−0.137***	−0.127***	−203.8**	−174.7**	−94.83***	−93.69***
	(0.0304)	(0.0318)	(81.59)	(75.48)	(16.36)	(22.21)
cvb==3500	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)
cvb==4500	0.232***	0.231***	76.06	50.36	154.0***	141.9***
	(0.0543)	(0.0588)	(70.36)	(55.74)	(31.66)	(29.91)
cvb==5500	0.544***	0.547***	363.9***	337.9***	561.5***	542.8***
	(0.0521)	(0.0589)	(60.90)	(74.83)	(45.06)	(56.16)
cvb==6500	0.645***	0.650***	889.4***	892.8***	1130.7***	1139.7***
	(0.0620)	(0.0632)	(58.90)	(71.64)	(81.90)	(87.79)
cvb==7500	0.697***	0.704***	1681.5***	1696.1***	1964.7***	1980.5***
	(0.0444)	(0.0488)	(135.5)	(141.4)	(127.5)	(130.5)
cvb==8500	0.712***	0.716***	2252.5***	2248.7***	2555.5***	2558.0***
	(0.0377)	(0.0414)	(137.0)	(148.6)	(139.3)	(143.6)
cvs==1500	0.871***	0.867***	2211.6***	2041.2***	2413.5***	2417.4***
	(0.0497)	(0.0505)	(233.7)	(231.4)	(197.9)	(200.1)
cvs==2500	0.871***	0.873***	1185.6***	1015.5***	1417.2***	1425.7***
	(0.0504)	(0.0536)	(116.4)	(193.0)	(115.1)	(114.2)
cvs==3500	0.812***	0.811***	849.7***	654.5***	1022.2***	1011.2***
	(0.0566)	(0.0544)	(160.1)	(117.0)	(96.54)	(92.42)
cvs==4500	0.767***	0.769***	380.7***	196.3	566.8***	565.4***
	(0.0577)	(0.0569)	(96.72)	(115.9)	(45.20)	(43.83)
cvs==5500	0.368***	0.358***	164.8	−43.52	183.8***	165.9***
	(0.0617)	(0.0627)	(128.8)	(97.52)	(35.41)	(28.59)
cvs==6500	0.112***	0.111***	156.4	0	67.60***	53.39**
	(0.0328)	(0.0317)	(132.4)	(0)	(21.42)	(24.53)
cvs==7500	0	0	0	−143.5	0	0
	(0)	(0)	(0)	(154.3)	(0)	(0)
round_id==2	0.1000**	0.106**	−202.0**	−191.1**	−50.00	−37.56
	(0.0392)	(0.0368)	(78.69)	(88.22)	(78.12)	(81.33)

Table C4 (*Continued*)

	(1) Trade YN	(2) Trade YN	(3) Trade profit	(4) Trade profit	(5) Profit, 0 = notrade	(6) Profit, 0 = notrade
round_id==3	0.0917**	0.0978**	−132.6*	−120.0	8.333	23.23
	(0.0357)	(0.0336)	(67.49)	(75.72)	(59.43)	(59.33)
round_id==4	0.117*	0.111*	−183.9**	−187.8**	−20.83	−24.50
	(0.0555)	(0.0565)	(79.81)	(84.88)	(55.09)	(51.26)
round_id==5	0.139**	0.134**	−151.0*	−110.3	34.21	60.30
	(0.0489)	(0.0509)	(83.56)	(91.96)	(56.72)	(54.76)
round_id==6	0.130**	0.130**	−161.5	−138.1	16.88	27.78
	(0.0570)	(0.0555)	(112.0)	(113.8)	(69.89)	(65.01)
round_id==7	0.148***	0.143***	−196.0**	−164.1*	11.60	32.09
	(0.0382)	(0.0376)	(80.81)	(89.17)	(55.34)	(54.32)
round_id==8	0.148***	0.137**	−179.4*	−132.3	16.81	44.17
	(0.0483)	(0.0461)	(86.99)	(86.65)	(57.96)	(50.10)
round_id==9	0.153***	0.149***	−202.0**	−187.2*	10.42	16.47
	(0.0373)	(0.0346)	(94.15)	(97.79)	(71.87)	(63.03)
round_id==10	0.112**	0.101**	−101.4	−71.35	54.68	64.11
	(0.0447)	(0.0418)	(88.81)	(83.11)	(46.54)	(40.98)
Buyer	0.167***	0.157***	190.2**	22.29	102.3***	99.96***
	(0.0330)	(0.0320)	(76.48)	(110.8)	(16.04)	(21.11)
Age		−0.00103		−1.065		−1.672
		(0.000619)		(1.434)		(1.176)
School years		0.00256		15.93		12.45
		(0.00197)		(12.66)		(8.726)
Female		0.0320		−121.4**		−63.04*
		(0.0239)		(46.29)		(32.15)
Farm size		−0.0104*		−4.612		−13.47*
		(0.00546)		(9.488)		(7.330)
Trader		0.0126		25.26		44.04
		(0.0167)		(46.38)		(34.42)
High Status		0.0433**		−112.2*		−48.45
		(0.0173)		(60.14)		(40.34)
Constant	0.00000385	−0.0115	472.2***	891.7***	49.15	220.8**
	(0.0502)	(0.0536)	(114.1)	(175.1)	(73.11)	(99.77)
Observations	2128	2042	1428	1370	2128	2042
Number of Clusters	15	15	15	15	15	15
Inter = Middleman	0.42	0.41	0.92	0.79	0.47	0.34

Estimated using OLS. Even columns add trader controls.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Appendix D. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jebo.2016.10.014.

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