Market Design for Land Trade: Evidence from Uganda and Kenya*

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November 18, 2022

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Abstract

Agriculture in low-income countries is characterised by fragmented farms and misallocation of land across farmers. We explore the role of market design in remedying these problems. We use a survey in Uganda to show that agricultural land markets are thin, prone to exposure risk, and suffer from coordination frictions; characteristics predicted to hamper decentralized trade. Evidence from a lab-in-the-field experiment in Uganda confirms that decentralized trade is inefficient in a setting with these characteristics. Market design may improve outcomes by thickening markets, finding chains, and enforcing conditional contracts, but designs that are well-tailored to the land trade setting may be unfamiliar and hard to understand. We show that a very simple market design intervention, a centralization treatment that brings farmers together at a set time, significantly increases efficiency in the same setting in Uganda. We then test whether designs better tailored to the land trade problem, but harder to understand, can further improve outcomes. In a second lab-in-the-field experiment in Kenya we show that a computerized package exchange, which allows traders to specify a sequence of conditional trades as a single transaction, significantly improves outcomes relative to a less-tailored computerized continuous double auction. Our results suggest that farmers in the developing world can understand and benefit from tailored market designs that address fundamental frictions in the land market.

*We thank Abhijeet Anand for his help in programming the experimental interfaces. We thank Jacob K. Goeree and Luke Lindsay for useful comments and thank Charles R. Plott for early discussions of continuous time package auctions related to the Electronic BushBroker Exchange. We thank The Busara Center for Behavioral Economics, especially Nikhil Ravichandar, Ruth Wambua, Mercy Musya, Kenneth Okumu, and Pauline Wanjeri for fieldwork in Kenya, and Metajua Limited, especially Charles Angebault, Apollo Tumusiime, and Sunday Dismus for fieldwork in Uganda. We thank Luis Schmidt for outstanding research assistance. We also thank numerous seminar and conference audiences. We gratefully acknowledge the financial support of the Centre for Market Design at the University of Melbourne Faculty, the ARC Future Fellowship Research Grant FT190100630, STICERD, and Handelsbanken's Research Foundations, grant nos B2014-0460:1, BF17-0003, and P2017-0243:1. The Uganda experiment and pre-analysis plan are registered on the AEA trial registry at https://doi.org/10.1257/rct.4581.

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1 Introduction

Increasing agricultural productivity is key to reducing cross country income differences: relative to rich countries, agriculture in low-income countries is both less productive and allocated more workers (Caselli 2005). Inefficient land allocation is a source of low productivity: labor productivity increases with farm size (Foster and Rosenzweig 2022), but poor countries have smaller, more fragmented farms (Adamopoulos and Restuccia, 2014; Ali, Deininger and Ronchi, 2015; Deininger et al., 2016); and despite substantial heterogeneity in farmer productivity, there is almost no correlation between farmer productivity and land holding (e.g., Chen, Restuccia and Santaeulalia-Llopis 2022*b*). These findings suggest large unrealized gains from land trade, a claim borne out by quantitative and experimental analyses.[1] We use survey data from Uganda, and lab in the field experiments in Uganda and Kenya, to argue that commitment and information constraints mean decentralized land markets will be inefficient, even with otherwise conducive institutions, and that careful market design can improve outcomes.

We build our argument in three steps. First, survey evidence reveals an environment with significant trade frictions. Section 2 reports results from a survey of 1404 Ugandan small holder farmers. We document five *facts*: 1. farmers recognize that there are increasing returns at the *plot level* that are currently unrealized; 2. farmer ability and land quality are heterogeneous and complementary; 3. farmers predict limits to how much land they can productively cultivate, implying decreasing returns at the *farm level*; 4. cultural constraints imply that not all plots would be tradable, even in a well-functioning market; and 5. farmers have private information about their own land values, but do not believe there is a lemons problem.

To better understand the implications of these survey findings, Section 3 introduces a land trading game that incorporates the facts in a stylized way. Figure 1 gives an example. Gains from trade arise from *defragmentation*, when a farmer consolidates multiple non-contiguous plots to a single farm (fact 1), and from *sorting*, when higher-ability farmers are matched to better-quality land (fact 2). But trade could also lead to losses. Decreasing returns at the farm level (fact 3) implies that surplus can be lost if trade leads some farmers to hold too much land (we call these *exposure* losses: they arise due to exposure risk, as we explain below).

We use the game to illustrate how the five facts jointly imply three *frictions* predicted to impede decentralized trade: I) thin markets; II) exposure risk; and III) coordination frictions. Thin markets exacerbate two-sided information problems á la Myerson and Satterthwaite (1983). Exposure risk is a generalization of hold-up that occurs when a sequence of trades is needed to realize a surplus, but later trades cannot be guaranteed (Goeree and Lindsay, 2019). Coordination frictions arise when efficiency requires coordinating the actions of many traders (Milgrom, 2017). Friction I is mostly due to plot level IRS (fact 1), which implies likely buyers are a small set of farmers with adjacent land. Frictions II and III arise because farmers often wish to condition current trades on future trades with other parties, leading to chains with

[1] Acampora, Casaburi and Willis (2022) provide experimental rental subsidies, and find positive returns that exceed the payments, consistent with the presence of unrealized gains from trade. Several studies estimate gains from land reallocation using quantitative models. Estimated returns vary widely (from about 20% to over 300%), but are typically positive (e.g. Chari et al. 2020; Chen, Restuccia and Santaeulalia-Llopis 2022*a*,*b*; Bolhuis, Rachapalli and Restuccia 2021; Adamopoulos et al. 2022; Britos et al. 2022). See Gollin and Udry (2021) and Aragón, Restuccia and Rud (2022) for a discussion of some of the empirical challenges that exist in this literature.

many participants. These frictions mean farms likely remain *fragmented*, and poorly *sorted*.

Second, Section 5 introduces a lab in the field experiment that demonstrates that, as predicted, decentralized trade is inefficient in a setting characterized by our five facts. In the experiment, our sample of Ugandan farmers tried to defragment and sort land in the land trade game introduced above. They did this by freely trading hypothetical land allocations in trading periods that lasted one week.[2] Despite strong financial incentives, participants realize only 23% of the potential gains from trade. Furthermore, there is striking heterogeneity across sources: they realize around 60% of potential defragmentation gains, but only 25% of sorting gains. The fact that efficient sorting is particularly hard is important, because this is the source of gains emphasized in the quantitative literature. Participants also frequently end up with exposure losses, indicating failure to complete a planned sequence of trades.[3]

The inefficiency of decentralized trade opens the possibility that careful market design could improve outcomes. Design can address the three frictions by increasing thickness (friction I); enforcing conditional contracts (friction II); and helping farmers find chains (friction III). But the designer faces a trade-off between participant understanding, and the value of tailoring a design to the specific setting. Tailored designs are predicted to improve efficiency for rational agents, but they can often be complex and unfamiliar. For example, the antique market is likely thin, so antique fairs, which centralize trading at a specific time and place, likely increase efficiency and are easily understood. Centralization could also thicken the housing market (e.g., through online real estate aggregators), but houses suffer from an acute exposure problem: I only want to buy your house if I can sell mine. A more tailored market design allowing conditional contracts could further increase efficiency, but may in fact reduce efficiency by making transactions harder for inexperienced traders to understand. The trade-off between tailoring and familiarity is critical for our target population of low-numeracy smallholder farmers, and it is an empirical question which effect will dominate.

The final step in our argument again uses lab in the field experiments to show that better market design can improve efficiency, and that our most-tailored designs perform particularly well. We begin by studying a simple market centralization intervention. After a week of decentralized trade, all participants in the above described experiment were given a surprise opportunity to trade together in a single location for about one hour. As noted, this should thicken the market and may also help find chains and enforce conditionality, because everyone in the market is present, and agreements to trade will be more observable to others in the social network. We find substantial increases in efficiency, with gains coming from additional defragmentation (reaching 70% of the optimum) and near-complete unwinding of exposure losses.[4] But we see zero additional sorting, and overall efficiency remains below 50%.

We then study the impact of further tailoring of designs. Section 6 introduces a second experiment, conducted in Kenya.[5] We created a centralized, computerized land exchange on

[2] We follow the engineering philosophy advocated by Roth (2002), that suggests using lab experiments to build an understanding of the environment and iterate on design.

[3] The results do not reflect a general inability to trade: participants reached over 90% efficiency in a pair of training games similar to those in Chamberlin (1948).

[4] These improvements are not simply driven by additional time – we show below that the impacts of the centralization intervention dwarf those of exogenous variation in time to trade.

[5] This experiment was conducted chronologically prior to experiment 1; see Section 4 for more background.

which farmers played a smaller-scale version of the land trading game with six players and two plots per player. We created three designs, increasing in how well they are tailored to the problem. First, we created a version of the classic continuous double auction (CDA), in which bidders could bid to buy or sell a single plot at a time, which is a general purpose untailored design.[6] This exchange enforces one condition: e.g., "I will sell this plot conditional on receiving at least X shillings" and we refer to it as "Package-1." Our most tailored design, "Package-4," is also based on the CDA, but is a package exchange in which farmers could make offers with up to four conditions: e.g., "I will sell these two plots, conditional on receiving those two plots and paying no more than Y."[7]

Package-4 is highly tailored to our environment: reaching efficiency is possible with just one transaction per participant, potentially eliminating exposure risk, and the platform is responsible for finding chains, setting prices and enforcing all conditions. The market is also thickened because conditioning purchases on sales removes the importance of the initial endowment, leading to more potential purchasers for each plot. By facilitating wholesale reallocation with a single bid, Package-4 may be particularly effective at facilitating efficient sorting. In contrast, Package-1 is poorly tailored: it requires many trades to reach efficiency leaving open the exposure problem, and does little to help find chains or enforce conditions. We also examined an intermediate "Package-2" treatment that permitted swapping one plot for another; we conjectured that this would help with defragmentation, but less with sorting.

Efficiency in our benchmark Package-1 condition is quite high, around 70%, indicating that farmers could understand the potentially complex computer based trading environment.[8] The most-tailored treatment, Package-4, increases efficiency by 7 percentage points. Moreover, Package-4 is particularly effective at unlocking sorting gains that were not realized by any of our other interventions. Collecting findings, we find that increased tailoring always increases efficiency, a result that initially seemed unlikely in our low numeracy setting.

More tailored designs improve efficiency, but it is also important to understand distributional effects. We investigate this using an Atkinson index to measure (in)equality of final outcomes. We find that our more tailored designs *reduced* inequality. The centralization intervention in our Uganda experiment has a first-order effect, mostly by eliminating exposure losses. Package-4 also reduces inequality. This was unexpected: we were concerned that this design could enable sophisticated traders, who understood the mechanism well, to profit at

[6]Continuous double auctions have a long tradition in experimental research, dating back to the classic work of Smith (1962, 1964). Empirically, the double auction has good efficiency properties even when the number of buyers and sellers is small (e.g., Smith and Williams (1981); Smith (1990)). We chose the double auction as our starting point since it can be implemented both synchronously and asynchronously and does not require an auctioneer to start and stop bid rounds and hence could be used in real settings at the village level with low variable costs.

[7]Our algorithm is based on the one developed in Goeree and Lindsay (2019). Unlike their design, we imposed XOR bidding which ensured that only one bid from each player was triggered in a given transaction, we always allowed communication, used a different visualization protocol, and larger packages, tailored to our land trade setting. While there has been considerable recent work on package auctions, package exchanges have attracted much less attention. Combinatorial exchanges have been explored in the context of airport take-off and landing slots (Rassenti, Smith and Bulfin 1982; Grether, Isaac and Plott 1989; Balakrishnan 2007), native vegetation offset permits (Nemes, Plott and Stoneham 2008), pollution permits (Fine et al. 2017), housing (Goeree and Lindsay 2019) and the reallocation of spectrum (Milgrom and Segal, 2017). For a review of the literature see Milgrom (2007) and Loertscher, Marx and Wilkening (2015).

[8]It is tempting to interpret the improvement from the low efficiency seen in experiment 1 as a treatment effect of the exchange, but this is at best suggestive due to the other implementation differences between experiments.

others' expense.

We also provide analyses aimed at understanding some of the facts and frictions that restrict trade, these are presented in 7. We find that making some plots nontradable, inline with survey evidence on cultural constraints (fact 4) does not reduce efficiency in decentralized trade, but may exacerbate inequality. We show similar results for credit constraints that are predicted to exacerbate exposure risk. Credit constraints do not significantly affect efficiency, but lead to more unequal outcomes under Package-1 than our tailored Package-2 and Package-4 interventions.

Prior work emphasizes a number of barriers to efficient land trade. For example, incomplete property rights (de Soto, 2000; Deininger and Feder, 2001; Deininger and Jin, 2006; Goldstein and Udry, 2008; Field, 2007; Besley and Ghatak, 2010; Galiani and Schargrodsky, 2010, 2011; Fenske, 2011; de Janvry et al., 2015; Lawry et al., 2016; Agyei-Holmes et al., 2020), low quality cadastral maps (Libecap and Lueck, 2011; D'Arcy, Nistotskaya and Olsson, 2021), incomplete credit markets (Eswaran and Kotwal, 1986; Binswanger and Rosenzweig, 1986; Binswanger and Elgin, 1988; Carter and Mesbah, 1993), and culture (Platteau, 2015) have been argued to constrain reallocation. Our results show that addressing these concerns is not sufficient to ensure efficiency, it is also necessary to consider appropriately tailored market design. Our results also provide clear guidance on a set of designs that are likely to be useful, and that can be understood by small holder farmers.

Historical experience is consistent with our claim that efficient land trade is hard, even when institutions are strong and property rights secure. Bleakley and Ferrie (2014), Smith (2019), and Finley, Franck and Johnson (2021) find that arbitrary initial distributions of land in the 19th century U.S. and 18th century France persisted for decades. Governments in many countries, presumably recognizing this, have frequently opted for centrally-planned land consolidation programs (see e.g., Hartvigsen, 2014). These programs may be challenging in settings with low state capacity and low trust in government. Market design offers a promising alternative with the additional ability to better leverage local information.

Closely related to our problem but not our setting, our package exchange was inspired by Goeree and Lindsay (2019), who study house reallocation. They propose, and experimentally verify, that a package market can help overcome exposure risk. Also related to our question, but not our setting, Tanaka (2007) compares the performance of different mechanisms that might be used to consolidate land, in lab experiments with US college students. Our study differs in that we (i) study an environment that is strongly connected to how farmers perceive the land-trade problem (ii) allow for communication in all treatments, and (iii) study the behavior of actual farmers in a development context. Less closely related, the design literature related to land has concentrated on the land assembly problem where a single buyer wants to buy complementary plots from a number of small sellers, and in which a holdout problem occurs. Several papers explore solutions (e.g, Plassman and Tideman 2010, Kominers and Weyl 2012, Grossman et al. 2019, Sarkar 2017 and Sarkar 2022).

A small number of papers also discuss market design for other development challenges. Many of these expect government to be the main buyer, for example, incentives for vaccines (Kremer, 2001*b*,*a*), refugee allocation (Delacrétaz, Kominers and Teytelboym, 2020), and antiquity protection (Kremer and Wilkening, 2015). A notable exception, closer to our motivation, is Hussam, Rigol and Roth (2022), who explore how revelation mechanisms might be used to select quality borrowers.

2 Describing the land trade problem

We start with descriptive evidence from a survey of 1,404 farmers from 68 villages (LC1s) in Masaka district, Uganda. We had two aims in fielding the survey: to understand the environment and whether it has features that theory predicts will hinder trade; and to validate the design of our experimental games. We first discuss the characteristics of the sample, before documenting five key facts about the production technology and trading environment.

We begin with a sample of rural villages in Masaka district, selected to ensure that agriculture was an important part of the economy.[9] In each village we asked the village leader (LC1 chairperson) for a list of households that would likely be willing to play our trading games 3 times over 3 weeks.[10] From that list, we randomly selected 22 households subject to the household cultivating some land, and deriving at least 50% of household income from farming.[11] We invited household heads first and foremost, but households could send another family member if they were unavailable. Table B1 describes the resulting sample, and provides comparable national averages for farm households in the World Bank's Living Standards Measurement Study. Our sample is reasonably representative, but slightly more educated (7 years on average) and with higher farm incomes.

Fact 1: Consolidation Gains due to Increasing Returns at the Plot Level

Our survey evidence strongly suggests that farms are fragmented and that farmers believe that there are increasing returns at the plot level. Table 1 summarizes the data. Fragmentation is clearly present. Over 60% of our sample owns two or more fragmented plots, which are on average 24 to 41 minutes' walk from one another. Inherited plots are much closer to home than those that are purchased or received as a gift, which is consistent with a poorly-functioning land market in which it is hard to purchase new plots close to existing ones.

Farmers recognize that this fragmentation is costly. When directly asked, 91% of respondents stated that they would prefer a consolidated 2-acre plot to two 1-acre plots. Restricting to participants who own fragmented plots, about 88% of them believe their earnings would increase if all of their land was consolidated, and estimate an average earnings gain of 53%. We also asked all participants their beliefs about the returns to adding a third acre to an initially-

[9] Starting with all villages within Masaka district we excluded villages with very high or low population density and villages in coastal locations, to ensure that agriculture was an important part of the economy. We then selected a random sample of villages, stratified at the parish level. See Appendix A for more details on the selection of villages and participants.

[10] We briefly described the games to the chairperson and showed some of the materials, to give them enough context for their recommendation. We did not explain the mechanics of the experiment, in particular we did not mention the surprise centralization treatment.

[11] Participants rarely opted or were screened out: there were zero exclusions in 93% of villages. We also required participants to have access to a mobile money account (either their own, or that of a friend or family member) so that they could be paid. This did not lead to any exclusions.

consolidated 2-acre farm. If the third acre is contiguous with the others, 66% of participants believe in increasing returns to scale, while 33% believe returns would be constant. If the third acre is not contiguous, only 30% believe there would be increasing returns, 37% expect constant returns, and 33% predict decreasing returns.

Fact 2: Sorting Gains due to Farmer-Farm Complementarity

Our sample also believes there is heterogeneity in farmer ability and farm quality, and that these are complements, implying gains to assortative matching. Evidence is provided in Table 2. 99% of farmers believe there is ability heterogeneity in the village. On average, they believe the best farmer in the village produces more than three times as much per acre as the worst, a very substantial difference. Further, 100% of participants believe total output in the village would increase if the best farmers cultivated a larger share of the village's land indicating both gains from matching, and that the current allocation of land does not maximize production.

Fact 3: Limited Agglomeration Gains due to Decreasing Returns at the Farm Level

Our sample also strongly believes that farmers face limits to the amount of land they can cultivate. Table 3 shows that only 60% believe they could farm more land than they have now. In addition, 99% believe that there is heterogeneity in span of control – some people could manage larger farms. On average, (assuming no hired labor) they estimate that the best farmer could manage nearly 5 acres while the worst could manage less than 1 acre. Farmers do not typically believe that they are the best farmer. On average, they consider their own household's maximum capacity to be 2.5 acres, or about 1 acre per adult member.[12]

Fact 4: There Are Cultural Constraints to Trade, but Trade is Possible

Our survey results also confirm that a thin land market already exists. Table 4 summarizes farmers' views about the current state of the land market. The top panel shows that all farmers in the sample are aware of land trade occurring, and that at least 17% of farmers attempted to exchange land via sales or leases in the last 12 months. As expected, leases are more common, with 17% of farmers leasing in some land and only 8% buying land. There is also asymmetry, with fewer people claiming they sold or rented out than those claiming they bought or rented in. This either reflects missing larger landlords in our sample, or possible fragmentation of plots at the time of sale. The final panel shows that while there are only a small number of trades per year, these accumulate and 45% of land that is currently owned was purchased rather than inherited or gifted. We also see that farmers report making purposeful attempts to consolidate land through trade, with 24% reporting an attempted consolidation. Only 50% of these attempts are successful, suggesting constraints to consolidation. Overall, the table provides direct evidence that trade is possible, although markets appear to be quite thin.

[12] These findings suggest limited gains from agglomeration but there may still be gains from better farmers cultivating more land and in principle it might be efficient for some to leave farming altogether. As discussed below, our respondents are reluctant to exit farming and move to the city and there appears to be some stigma about farmers leaving the village. To avoid such sensitive issues, we did not explore this type of agglomeration in out experiments.

We also find that complementary institutions are at least minimally functional. Table 5, shows that land registries and credit markets appear to be well functioning in the region. Most farmers know how to register land sales and there appears to be a formal way to transfer property rights. Credit markets also appear to function and 50% of farmers believe that they could borrow to purchase land. Over 70% of participants have participated in some kind of auction, suggesting familiarity with the kinds of solutions we are interested in, but these types of markets do not seem to exist for land trade.

However, there are cultural and institutional constraints that limit trade. Table 6 summarizes the evidence. From above, we know that the first-best allocation likely involves assortatively matching the best farmers to the best land. However, the survey suggests that more than 60% of farmers think that this would be unfair or very unfair. This is a potential strong constraint to trade, and highlights the importance of appropriate compensatory transfers when implementing such an allocation. There is also strong agreement that families should not sell ancestral land (i.e. land that has been passed down in the family). However, participants are more equivocal when asked if a family should be free to trade land to improve their situation, with more than 50% suggesting this is okay. Thus there is not a strong general taboo against land trade.

Farming is an important part of our respondents' identity. They strongly agree that it is important to them to own land, they would like their children to remain farmers, and they are generally unwilling to migrate even if they could fetch a good price for their land. Such attitudes might be more permissive toward land reallocation between farmers than toward reallocation that moves some people off the land altogether.

Fact 5: Private Information but No (Perceived) Adverse Selection

The information structure of the trading environment is important for determining what frictions apply and what solutions may be appropriate. A particular concern is *asymmetric* information. If potential buyers believe that sellers have private information about land quality, the market may completely unravel as in Akerlof (1970)'s lemons problem. Strikingly, our respondents do not believe this is a concern. Table 7 shows that 99% of farmers believe they know how to identify the best land in the village. They believe that they can evaluate the quality of land by looking at the crops (82%), soil (79%), slope (14%), and access to water (12%). Farmers also do not hold strong beliefs about the quality of land that people sell or rent and do not believe that it is hard to assess the quality of other farmers' land. Thus our participants do not *believe* there is an adverse selection problem.

Bargaining-based frictions as in Myerson and Satterthwaite (1983) rely on two-sided private information, meaning that farmers do not know one another's potential gains from a given transaction. Is that a reasonable assumption in our setting? Table 2 shows that 98% of respondents believe that everyone agrees on who the best farmers are, suggesting that at least some information about gains is not private. But there is substantial heterogeneity in the predicted productivity increase from assortative matching, implying uncertainty of the exact gains from a given transaction. Moreover, it seems implausible that farmers know all of their trading counterpart's outside options, investment plans, or the value they might assign to a given plot for idiosyncratic cultural and non-pecuniary reasons.

3 An Experimental Model of the Problem

We now describe a simplified model of the land trade problem, which mirrors the facts set out in section 2. This model has two purposes, first, it can help us to gain a theoretical understanding of why land trade is difficult, and second it forms the basis for our experiments, which help us to understand empirically whether land trade is difficult, and how better market design can improve the situation. Further details about the environment can be found in Appendix A, which includes all implementation details for Experiment 1.[13]

Figure 1 describes a land trading game consisting of a grid of 72 plots. Land is divided into three regions corresponding to low, medium, and high *quality*, indicated by symbols (🌽🌽, 🌽🌽🌽, 🌽🌽🌽🌽). There are 18 farmers, each described by a different color and number, and each with an initial allocation of three plots. Colors indicate *farmer ability*, which is low (blue), medium (orange), or high (green). Participants in the experiment take on the role of a farmer, and begin the game with endowments of land and game currency. Through trade, they can earn money by improving the value of their endowments.

We model increasing returns at the plot level (Fact 1) through *adjacency bonuses*. Farmers earn an additional return when they own two or three plots in the same quality region that are adjacent to one another. We model farmer-farm complementarity (Fact 2) by setting the return to a given plot equal to the product of its quality and the owner's ability. Better farmers therefore earn higher returns from any given plot. Note that while we always refer to quality, we could equivalently think of the quality dimension as capturing different plot sizes, in which case better farmers produce more from a given *quantity* of land. We model decreasing returns at the farm level (Fact 3) by a span-of-control constraint: each farmer can cultivate at most three plots, and if they have more they only earn the return to their best three. The combined implication of these three features is that in an efficient allocation, all farmers should hold three consolidated plots, and farmer ability and land quality should be assortatively matched.

We capture cultural constraints on trade (Fact 4) by assuming 18 plots, represented by white space, are not available for trade at any price. These could represent e.g. ancestral land. We also created a version of the game without non-tradeable plots in order to better understand the impact of cultural constraints of this type (See Figure 2). We will refer to the case with non-tradeable plots as complex, and the case without non-tradeable plots as simple.

We assume that all farmers know their own value of any plot or combination of plots, i.e. there is no adverse selection (Fact 5). However, these values are private to them and in our experiments they are asked not to share this information with other participants.

Figure 1a shows an example initial allocation, featuring fragmentation and misallocation of land. Figure 1b shows an efficient allocation, with consolidation and assortative matching. In our experimental games we study how farmers fare relative to the (known) first best.

[13] There were some implementation differences between the experiments, in particular the number of players. In what follows we describe the game that was implemented in Experiment 1. When we introduce Experiment 2 in section 6 we will explain how its design differed.

3.1 Why is Land Trade Hard?

We now discuss why Facts 1–5 imply three *frictions* that could impede efficient land trade.

Friction I: Thin Markets and Private Information

A thin market is one with only a small number of buyers and sellers willing to trade a particular good. In our setting, decentralized markets are likely to be thin due to increasing returns at the plot level (Fact 1) and farmer-farm complementarity (Fact 2). In our maps, the complimentary between farmer and land type implies that there are only ever 6 efficient buyers for each quality of plot. In a real world market heterogeneity is likely to be more continuous, but the matching requirement created by complementarity will still reduce thickness relative to a homogeneous goods market. Increasing returns at the plot level exacerbates the problem because a given plot is of relatively low value to those who do not already have nearby plots. For instance, if farmer 16 wishes to sell her plot of low-quality land, only farmers 3 and 18 would benefit from the increasing returns, meaning the market may only contain two interested buyers. Market thinness means that competitive prices may not emerge and transactions must instead be bargained over (Rustichini, Satterthwaite and Williams, 1994).

Thin markets combine with two-sided private information (Fact 5) to inhibit ex-post efficient trade. For a single buyer and a single seller, Myerson and Satterthwaite (1983) show that when valuations and costs are private and drawn from continuous distributions that overlap, it is impossible to construct an incentive compatible and individually rational mechanism that generates ex-post efficient trade, without generating a deficit. This result has been shown to extend to all problems where trade is one-to-one, and to a wide array of many-to-many allocation problems (Vickrey, 1961; Gresik and Satterthwaite, 1989; McAfee, 1992; Segal and Whinston, 2016; Delacrétaz et al., 2019). While it is difficult to directly tackle the private information problem through market design, a key implication of this discussion is that farmers would do better if it were possible to thicken markets.

The problem may be worsened by the presence of non-tradeable plots (due to cultural constraints, Fact 4). A benchmark for identifying the extent to which private information is likely to prevent trade is to calculate the expected deficit that would be generated when using the Vickrey-Clarke-Groves (VCG, Vickrey (1961); Clarke (1971); Groves (1973)) mechanism to reallocate units. The VCG mechanism is useful since it is the cost minimizing way to induce truthful reports in a large class of problems (Williams, 1999). In our setting, the deficit that results from the VCG mechanism being run on the complex map (with non-tradeable plots, Figure 2a) is always larger than on the simple map (no non-tradeable plots, Figure 2b).[14]

This literature gives us an additional prediction: it is easier to realize gains from consolidation than from sorting. The consolidation problem has important similarities to the the case

[14]The intuition is as follows. In the VCG mechanism, each individual's compensation is related to the difference in surplus that other players receive when this individual participates in the mechanism, versus when the player opts out and retains his original land. In the simple map, the adjacency bonuses of others are almost never impacted by one individual being excluded from the mechanism, because they can always trade with someone else. This is not the case in the complex map because an individual's plot may act as a bridge between two components of the network and may be necessary to assign players to contiguous sets of land. Hence, in the complex maps many individuals must receive a high compensation, so we expect a large deficit.

of partners who own an asset in common and wish to dissolve the partnership. This is an important exception to the general inefficiency of trade with two sided private information, and Cramton, Gibbons and Klemperer (1987) show it is possible to reach ex-post efficiency. Consider a situation where farmer 16 and farmer 6 have each sold one piece of land and are negotiating over who will hold the two plots in the north-east corner of the high-quality region. We can think of this as a partnership with a single asset (the consolidated plot) and an outside option to keep the original allocation. Intuitively, the adjacency bonus means that it is common knowledge that there are gains to trade, and so efficiency is easier to achieve.[15]

In contrast, trades that aim to improve on assortative matching, for example farmer 16 purchasing the disjointed plot owned by farmer 7, are closer to the original Myerson and Satterthwaite (1983) setting. Because of this, we conjecture that consolidation will be easier than sorting. For all of our analysis we show results for overall efficiency, but also decompose them into gains from consolidation and sorting.

Friction II: Exposure Risk

Exposure risk arises when at least one party stands to make a loss if a chain of trades (a sequence of purchases and sales) is not completed. A leading example is hold-out, where a trader toward the end of a chain realizes they can capture most of the gains from the chain by holding out for higher price. Exposure can be thought of as a more general form of hold-out, including such strategic behavior but also any other reason the later trade may not take place, such as an exogenous financial shock to a buyer late in the chain.

Exposure risk is believed to reduce market efficiency (Goeree and Lindsay, 2019). Increasing returns at the plot level (Fact 1), farmer-farm complementarity (Fact 2), and decreasing returns at the farm level (Fact 3), combine to generate exposure risk.

Decreasing returns at the farm level generate chains in our setting. It implies that a farmer that buys land will also need to sell land, and vice versa (without decreasing returns, one farmer could just buy up all the land).

For the case of increasing returns at the plot level, note that sometimes a farmer may need to divide up a previously-consolidated farm in order to relocate elsewhere (e.g., Farmer 17 in Figure 1). The gain to their buyer may not be sufficient to cover the loss of an adjacency bonus, so one of them must make a loss on the first trade, to be recovered later in the chain. Alternatively, consider farmer 5 attempting to purchase the low-quality plot owned by farmer 10. If 5 fails to form a consolidated farm, that initial transaction might be unprofitable.

Turning to the role of farmer-farm complementarity, consider the case where farmer 16 wishes to first sell her low-quality plot and then buy the plot owned by farmer 6 in the north-east corner of the high-quality region. Farmer 3 is a natural initial trade partner for farmer 16 because they would receive an adjacency bonus when buying the land. Unfortunately, farmer 3 has much lower productivity than farmer 16 and cannot fully compensate 16 for the loss of value from trading away the plot. Thus, farmer 16 must take a loss from this first trade, in anticipation of a gain upon completion of the chain. If she cannot guarantee that she will buy

[15] Further, the initial ownership is exactly split, which Cramton, Gibbons and Klemperer (1987) show helps the case for efficiency as it is furthest from the single owner case of Myerson and Satterthwaite (1983).

the high-quality plot owned by 6, she risks being stuck with her initial loss.

A further implication of decreasing returns at the farm level is that farmers who start a chain by buying land are likely to make an initial loss, because they cannot cultivate the whole of their newly-expanded farm. As such, she risks having too much land if she cannot make the subsequent sale, and she is again placed in a holdup situation.

We conjecture that non-tradeable land, created by cultural constraints (Fact 4), could increases the potential for holdup because it reduces the number of adjacencies. Thus a farmer who initiates a chain has a smaller set of potential trading partners at the end of the chain, weakening their bargaining power.

Exposure risk could play out in two ways. First, it may lead farmers to avoid initiating chains, reducing the efficiency of decentralized markets. Alternatively, some farmers may decide to take on the risk by making a first trade, but end up holding inefficiently too much land which they cannot profitably cultivate due to decreasing returns. This second outcome is particularly important for us, both because it reduces efficiency, but also because a real world land market would involve potentially vulnerable individuals, and trade that amplifies inequality is ethically problematic. These observations motivate us to further separate efficiency gains in our games to show losses from exposure, generated by farmers left holding too much land.

A key implication of exposure risk is that farmers would like to make conditional offers, and these conditional offers can often grow into long chains involving many farmers. To illustrate, in the example above, 16 would like to sell her low quality plot to farmer 3 *conditional* on being able to purchase a high quality plot from farmer 6. This is a chain of three farmers that that will move toward an optimal allocation so long as farmer 16 can be convinced that the conditionality will be enforced. Hence, exposure risk creates demand for conditionality.

Friction III: Coordination Frictions

Increasing returns at the plot level, farmer-farm complementarity, and decreasing returns at the farm level (Facts 1–3) combine to imply that reaching efficiency likely requires long chains of trade involving many farmers.[16] In total, to get from the initial allocation in Figure 1a to the efficient allocation in Figure 1b requires 45 plots to change hands, and many of these will be conditional trades because of the exposure problem. As highlighted by Milgrom (2017), when many complex transactions are required to reach efficiency, even small transaction costs can make it difficult for decentralized markets to function well.

Because land is inherently immovable, initial allocations or trades can create "packing" problems where an individual who begins to assemble land in the wrong place can make it impossible for others to assemble land efficiently without additional trades occurring. These problems arise even in the simplest case but are exacerbated by the presence of non-tradeable plots due to cultural constraints (Fact 4) because this further reduces the number of potential efficient assignments.[17] We conjecture that this contributes to low efficiency, and may make it

[16]For example, 16 wishes to sell her plot in the south-west corner, but 3 only wants to buy it conditional on the sale of her unconsolidated plot. She would like to sell this unconsolidated plot, perhaps to 7, but again 7 only wants to do this if she can sell her plot in the north-eastern corner. 17 will buy this plot, but only conditional on selling her plot in the middle of the board, etc.

[17]For example, it is not possible to consolidate the high-quality region without trading with farmer 17. If farmer

harder for individuals to consolidate land once others have begun to do so.

Credit Constraints Worsen the Problem

Although our survey evidence suggests that farmers have some access to credit markets, poorly functioning credit markets are a common feature of low-income economies (e.g., Banerjee, 2003). They can play a particularly important role in land markets, where large cash payments are required upfront and the benefits accrue mostly in the future. If our farmer cannot borrow, the only way for her to raise funds for a land purchase may be to first sell some land. But then the problem simply passes along the chain: her buyer must also raise funds and may also be constrained, and so on. This naturally acts to exacerbate the need for chains, which are then subject to exposure risk and coordination frictions. We explore the issue of credit constraints in the second experiment where we vary participants' initial cash balances.

4 Experiments: Implementation and Analysis

We conducted two experiments based on the game described in Section 3. Experiment 1 was conducted in Masaka district, Uganda, in Fall 2019. It was designed to measure the efficiency of trade in a decentralized setting, the role of non-tradeable plots, and the effectiveness of a simple market centralization intervention. Experiment 2 was conducted in Kiambu country, Kenya, in Summer 2016. It studies the effectiveness of two increasingly tailored package exchanges, relative to a benchmark centralized land exchange. We also investigated the role of credit constraints. In our analysis we present experiment 1 first because although chronologically it was conducted later, it provides clear conceptual motivation for experiment 2.[18]

Our analysis of experiment 1 is based on a pre-analysis plan posted to the AEA trial registry. We follow the plan closely, and highlight deviations where they occur.[19] Experiment 2 did not have a pre-analysis plan but we implement the same regression specification, decomposition, and analysis strategy on that data as well, to ensure comparability. In both experiments we used blocked randomization of treatments and, within treatments, the different maps that formed the basis of the games. Our analysis always controls for the full set of fixed effects implied by the research design, and we specify these in the table notes to all regressions.

Our main specifications regress efficiency, measured as the fraction of gains from trade realized by participants, on treatment dummies. We also decompose efficiency into three components: consolidation, sorting, and (avoided) losses due to exposure. Our tables report the

10 buys farmer 4's neighboring medium-quality plot he blocks full consolidation of the medium-quality region, and so on.

[18]One of the key findings from experiment 2 was high efficiency in the benchmark treatment, which prompted our investigation of efficiency in decentralized trade in experiment 1. The reason for conducting the experiments in different locations was purely pragmatic and based on the capacity of our implementing partners to conduct the complex experimental designs we use.

[19]The plan can be found at `https://doi.org/10.1257/rct.4581`. One thing to note is that we pre-specified our analysis of simple versus complex maps (presented in Section 7) as primary hypotheses, and our analysis of the centralization intervention in Section 5 as secondary. We subsequently combined both experiments into this single paper, at which point it was narratively clearer to describe the intervention effects first. Appendix B contains additional exploratory analyses from the pre-plan.

decomposition measured in efficiency units (i.e., the three components add up to total efficiency), and also express consolidation and sorting as a percentage of the values they would take at an efficient allocation.

For the decomposed analyses we report q-values (Anderson, 2008) that adjust for multiple testing across the three components.[20]

5 Experiment 1: Decentralized and Centralized Trade

Our first experiment uses the game described above and depicted in Figure 1. Appendix A provides full implementation details. Here, we summarize the design, then turn to results.

The distinguishing feature of this experiment is that in each week of play we gave participants seven days to trade amongst themselves within the community, without oversight or input from the research team. We therefore interpret outcomes as reflecting what is achievable under *decentralized* trade. At the end of the second week, we surprised participants with a simple *centralization* treatment; giving them additional time to trade with everyone in the same room together. We will argue that this is informative about the potential benefits of simple market design interventions that centralize the market, without imposing additional structure in the form of trading rules.

In each of 68 participating villages we recruited a group of 18 farmers to play our experimental games over a two week period, plus four reserve farmers who would step in if a participant dropped out. Recruitment and sample characteristics are as described in Section 2. We conducted three meetings, with seven days in between each.

Meeting 1 (start of week 1): we introduced the study, and played two training games. We publicly explained the main games, privately distributed participants' materials, and dismissed them, giving them seven days to trade.

Meeting 2 (start of week 2): we collected final endowments from week 1, and calculated earnings. We conducted the survey from Section 2. We privately distributed materials for a new week of play, and dismissed participants for seven more days.

Meeting 3: we collected endowments from week 2, then surprised participants with the opportunity to continue trading for one more hour. We measured their final endowments, conducted an exit survey, and dismissed them.

We paid participants for every game they played, plus a show-up fee for each meeting. Each game involved beginning with an allocation of land and cash, plus an amount of debt.[21] Payments were always based on the value of net assets at the end of trade, meaning the value of their landholdings, plus their final cash balance, minus their debt. Debt served to sharpen incentives because it meant that a large share of participants' earnings were derived from gains from trade, rather than just their initial asset balance.

[20] Our pre-analysis plan stated we would adjust over efficiency and consolidation. Ex post we concluded it made more sense to treat the efficiency analysis as a standalone hypothesis test, and then adjust for multiple testing when we decompose into its three components.

[21] We set debt levels to equalize initial assets across participants.

5.1 Training games

We use two training games to introduce the idea of trading experimental titles, demonstrate how earnings would be calculated, and provide a benchmark against which to compare the outcomes of the main games. They were played by the 18 main participants and 4 reserves in each village. Earnings equalled final net assets (landholdings + cash balance − debt).

Training Game 1 followed the spirit of Chamberlin (1948) and introduced the concepts of heterogeneous ability and the potential value of trade. Participants were assigned a type corresponding to their payoff from owning a single land title; holding more than one land title yielded no additional payoff. Types were uniformly spaced and land titles were initially assigned to odd-numbered types. Everyone began with the same cash balance, and debt such that initial net assets were worth 4,000 UGX ($3.05 PPP) per player.

Trade was free-form but, unlike Chamberlin (1948), resale was allowed so players could buy and sell as many times as they liked. Play continued until nobody wished to trade any more. Efficiency was reached if all titles were owned by the highest-type players. Average earnings at an efficient allocation were worth 7,000 UGX ($5.34) per player, a 75% gain.

Training Game 2 introduced the concepts of heterogeneous land and multi-unit span-of-control constraints. Participants were assigned to one of three ability types, and there were three land quality types. The game featured a span of control constraint: players earned payoffs on their best three plots. Everyone began with one title, a cash balance, and debt, with initial net assets equal to 4,000 UGX ($3.05 PPP). Efficiency was reached if all titles were owned by high types, with at most three plots each. Mean net assets at an efficient allocation were worth 6,727 UGX ($5.13 PPP), a 68% gain.

5.2 Main games

The main games used the game introduced in Section 3. For each round of play we randomly assigned each village to a "map," which consisted of an initial allocation of three land titles for each player. We used a set of randomly-generated maps where the gains from trade were divided approximately 50–50 between consolidation and sorting.

In addition to their three titles, each player was given an (identical) cash balance in the form of printed paper bills, and debt such that their initial net assets were worth 14,000 UGX ($10.67 PPP).[22] Mean net assets at an efficient allocation were worth 21,940 UGX ($16.73 PPP), a 57% gain.

We explained the rules of the game in public, but told farmers their type privately, and instructed not to share this information with others. We gave them a card that showed them the value for each type of land and the adjacency bonus they would receive from farming two and three contiguous plots. Outside of understanding the span-of-control constraints, these were the only payoff parameters that farmers needed to keep track of for evaluating potential trades.

We gave each participant a map of all potential plots, marking their initial allocation as well as the (initial) owners of all other plots. Most players knew one another but we added

[22] In principle participants could exchange real money or non-game goods for land titles, which would affect our inequality measure but not efficiency. Nobody reported doing so.

measures to ensure they could find each other. Each participant had a player ID (from 1–18, uncorrelated with type), and was given a sheet on which they could record the other players' identities. The village chief (LC1 chairperson) was also given a sheet with all names and IDs, and participants could consult the chief if needed. Since most people were socially connected to the chief in some way, it was natural for them to play this role.

5.3 Outcome Measures

As noted in Section 3.1, the theoretical literature on two-sided trade suggests that consolidating land may be easier than assortative matching. Further, the exposure problem may influence both efficiency and inequality. Our outcome measures reflect both these theoretical insights. We compute these separately for each village and trading week. In the case of week 2 we compute outcomes both before and after the centralization treatment.[23]

Efficiency Efficiency is the fraction of possible gains from trade realized. Define *surplus* as the sum of the land values and adjacency bonuses of land owners, then:

$$\text{Efficiency} = \frac{\text{Final surplus} - \text{Initial surplus}}{\text{First-best surplus} - \text{Initial surplus}}.$$

Efficiency equals 0 if no trade occurs, and 1 if a first-best final allocation is reached. Negative realizations are possible if trade decreases total surplus. Note that the final allocation of game currency, while important for understanding how gains from trade are distributed among participants, is irrelevant for overall efficiency.

Efficiency has three components: Efficiency = Exposure + Consolidation + Sorting.

Exposure A consequence of exposure risk is that some participants may end the game stuck with too much or too little land As our production function assumes it is only possible to cultivate three plots, this will reduce overall efficiency.[24] We refer to such losses as *exposure*. To compute them, we identified all plots that were uncultivated in the final allocation, and reassigned these plots to individuals who owned less than three pieces of land, in such a way as to maximize total surplus. We define *exposure value* and *exposure bonuses* as the additional land value and adjacency bonuses generated by this reassignment. The loss due to exposure is calculated as:

$$\text{Exposure} = -\frac{\text{Exposure value} + \text{Exposure bonuses}}{\text{First-best surplus} - \text{Initial surplus}}.$$

Note that exposure is weakly negative and normalized by the same denominator as efficiency. Thus, it can be interpreted as the percentage of the overall potential surplus lost, due to partially completed sets of trades.

[23] We pre-specified the construction of these outcome variables. Our results are similar when we calculate consolidation and sorting gains without first adjusting for exposure, see Appendix B.

[24] This feature is included in the game for good reason: we wanted to capture decreasing returns without avoid overwhelming participants with parameters and difficult calculations. The downside is that this can amplify losses due to exposure, relative to a smoother decreasing-returns production function.

Consolidation Our measure of gains from consolidation is:

$$\text{Consolidation} = \frac{(\text{Final bonuses} + \text{Exposure bonuses}) - \text{Initial bonuses}}{\text{First-best surplus} - \text{Initial surplus}},$$

where *final bonuses* is the sum of the value of landowners' adjacency bonuses values in the final allocation and *exposure bonuses* is defined above. Exposure bonuses are added back so as to decompose gains from consolidation from gains from (avoided) exposure losses.

Sorting Our measure of the gains achieved through assortative matching is:

$$\text{Sorting} = \frac{(\text{Final land value} + \text{Exposure value}) - \text{Initial land value}}{\text{First-best surplus} - \text{Initial surplus}},$$

where *final land value* is the sum of landowners' plot values and *exposure value* is the additional values that would be attained if exposed plots were reassigned.

Inequality We use a version of the Atkinson index to measure to inequality, with the auxiliary assumption of log utility over income. Letting $\{y_1, y_2, \ldots, y_n\}$ represent the earnings of each player and μ representing the mean of these values, the Atkinson index is:

$$I^A(f) = 1 - \exp\left[\sum_i \frac{(\ln y_i - \ln \mu)}{n}\right]. \tag{1}$$

A nice feature of this specification is that the Atkinson index represents the proportion of lost social welfare that is due to inequality. For example, if $I^A = 0.3$, then it would be possible to reach the same social welfare with 70% of the income, but with equally distributed payoffs.

We compute the Atkinson index of players' final assets net of debt. As we set debt values to equalize initial net assets, the initial allocation features perfect equality. Ex post, around 7% of participants had negative net assets after trade, likely driven by exposure losses. A consequence is that $\ln y_i$ is not defined for these participants. Our analysis explores a range of adjustments to deal with this issue.

5.4 Treatment variations

Centralization intervention After the second week of the game was completed and we had recorded the players' post-trade holdings of land and cash, we surprised them with an additional hour to continue trading, this time in a centralized location.[25] We think of this as a very simple market centralization intervention. We conjectured that centralization would have two effects. First, the task of searching for chains of trade is much less cumbersome, reducing coordination frictions and potentially increasing market thickness. Second, the presence of everyone in the same room may make it easier for people to commit to conditional trades, as any reneging will be observed by a large part of the community. Being able to enforce conditionality will tend to reduce exposure risk, increasing efficiency and improving inequality.

[25] Since this treatment was applied in all villages at the end of week 2, an alternative explanation is that its effects come simply from more time to trade. We show below that this is unlikely to explain our findings.

Non-tradeable plots Our theoretical discussion suggests that cultural constraints on trade, leading to non-tradable plots on the map, will reduce trading efficiency and/or increase inequality of outcomes. As explained in Section 3 we constructed each map with a complex and simple form where simple maps had no non-tradeable plots but identical initial payoffs and potential gains from trade. Each village played a complex map in week 1 and a simple map in week 2, or vice versa. We use variation in map complexity to explore the importance of cultural constraints, which the theory above suggests will reduce trading efficiency, or create inequality. We further conjectured that the centralization intervention would be more effective in the complex treatment, because the problems of thin markets, exposure risk, and coordination frictions are more severe in the complex treatment.

In this section of the paper we focus on the effects of the centralization intervention, and discuss the effects of non-tradeable plots in section 7.

5.5 Results

We begin by benchmarking the efficiency in the main experiment against training games 1 and 2. This helps us to understand the relative difficulty of the land trade problem compared to similar environments that do not have a spatial dimension. We also decompose our analysis of efficiency in the main experiment into consolidation gains, sorting gains, and exposure losses. As discussed in section 3.1, we might expect the consolidation problem to be easier to solve than the sorting problem. Our first result establishes that the land trade problem is indeed hard relative to the training games, that sorting is harder than consolidation, and that exposure risk is a significant problem.

Result 1 *Relative to the training games, efficiency in the main experiment is low. Farmers are able to capture some of the gains from consolidation but capture very little of the potential gains from sorting. Further, many farmers end up with sub-optimal amounts of land leading to large exposure losses.*

Figure 3 shows that efficiency in the two training games is around 90%. The high level of efficiency suggests that farmers understand how to make simple trades and are able to achieve high efficiency in regular trading environments. In contrast, efficiency is below 40% in weeks 1 and 2 of the land trade game.[26] While we might have had anticipated learning effects, efficiency actually falls between week 1 and 2, driven primarily by larger exposure losses in week 2 (Table B2). We conjecture that, having become more comfortable with the game after week 1, players attempted more trade, moved into exposed allocations, and found themselves unable to unwind these positions. Experience by itself may not be sufficient to solve the problem.

In Panel A of Table 8, we decompose efficiency into consolidation, sorting, and exposure losses, averaging over weeks 1 and 2 of the land trade game. Farmers perform substantially better on the consolidation than the sorting dimension, realizing 61% and 25% of the potential gains from each, respectively. However, exposure losses are large, equalling roughly 20% of total potential gains from trade.

[26]This low efficiency is not due to inactivity. Around half of all plots changed hands in each week, and in our exit survey almost 95% of farmers reported trying to buy at least 1 plot over the course of the games, and 87% of farmers reported that they successfully traded at least one parcel.

Next, we study how our centralization intervention affects efficiency. Based on the theoretical discussion we would predict that centralized trading reduces exposure risk since trades can be conducted synchronously.

Result 2 *The centralization intervention delivers higher efficiency and a lower inequality. Efficiency gains are primarily due to the unwinding of exposure losses and improvements in consolidation. There are essentially no additional sorting gains.*

Panel B of Table 8 shows that efficiency improves markedly during the short period of centralized trade at the end of week 2. For villages playing a complex map, efficiency immediately prior to the centralized trade was around 11%. After around 1 hour of centralized trade it had increased to 45%.

These gains come predominantly from participants unwinding their exposure losses, which fall from 32% to just 4% over the course of centralized trade. They also do substantially more consolidation. From a baseline level equal to 57% of all gains from consolidation, they increase to 69%. In contrast, the effect on sorting is a precise zero.

Panel B of Table 9 shows that the centralization intervention also reduced inequality. The reduction in inequality is likely driven by the mitigation of exposure losses, which particularly hurt farmers left with too much land.

The main concern with our interpretation of the effects of centralization is that this intervention also increased the time available to trade. To rule out this explanation, we exploit plausibly exogenous variation in the morning/afternoon scheduling of village meetings that generated up to 8 hours' variation in the time each village had to trade prior to the centralization intervention. If the gains we see come from increased time we would predict that (i) villages that had more time during week 2 would do better prior to the centralization intervention, and (ii) the impact of the centralization intervention would be smaller for villages that had more time before the intervention. Figure B2 graphs efficiency against the number of hours available to trade during week 2. Neither prediction holds in the data. For (i) we find the relationship between efficiency and time to trade is essentially flat and comes nowhere close to the discrete jump up in efficiency during the period of centralized trade. For prediction (ii) we see that if anything the returns to the centralization intervention were larger for villages that had already had more time to trade.

Taken together, the results from our first experiment suggest that land trade is hard and that farmers often trade to inefficient allocations when trade is decentralized. Realizing gains from consolidation appears substantially easier than sorting. Bringing farmers together helps to unwind exposure losses and unlocks further consolidation gains, but has no effect on sorting. This suggest that there may be additional gains from more tailored market design interventions. We explore this possibility in our second experiment.

6 Experiment 2: Tailored Package Exchanges

Our first experiment demonstrated that the land trade problem is hard and that a simple centralization intervention can generate considerable efficiency gains on some, but not all dimensions. In particular, farmers find it hard to realize gains from sorting, and this is not improved by market centralization.

Our second experiment uses a simplified version of the same land trade game to study more tailored market design interventions. As a benchmark we set up a centralized computer-based land exchange in which farmers can trade individual plots via a Continuous Double Auction (CDA) mechanism. Our tailored interventions then add the possibility of package bids, whereby farmers can condition transactions on one another. For example they can bid to sell one or more plots simultaneously with buying one or more other plots at the same time. The exchange platform is responsible for finding feasible trades and setting prices. Furthermore, the platform facilitates submitting many such bids knowing that at most one will be accepted. Our theoretical discussion in Section 3.1 suggests that our tailored interventions will improve the efficiency of trade by thickening the market for any individual plot; removing exposure risk; and facilitating finding and bargaining over chains.

6.1 Design

We used a simplified version of the land trade game, with 6 farmers and 12 plots of land. Figure 4 gives an example, and Figure C1 shows all eight maps we used. Each farmer was initially allocated two plots and we imposed a span-of-control constraint such that they could only cultivate their two best plots. Otherwise the game structure was essentially the same as experiment 1.[27]

We set up two labs in a town in southern Kiambu County, Kenya, and recruited land-owning farmers from a census of the local area. Table D1 provides summary statistics and compares our sample characteristics to national averages.

We conducted 48 experimental sessions, each consisting of 6 farmers and 8 auction rounds. At the beginning of each session, farmers were randomly assigned a computer and an enumerator or "bid assistant" whose role was to train them on the game and then assist with the computer interface.[28] An additional enumerator was available in each session to pass messages between farmers. Enumerators were explicitly told not to suggest or actively organize trades. We allow for oral communication in this experiment since we are interested in developing exchanges that can be used in conjunction with current institutions. Given that communication is a feature in our target environment we consider it an important part of our design.

After the instructions and a 15 minute practice auction, we conducted eight 10-minute auction rounds, each using a different initial allocation, in blocked random order. Participants knew their types and that there were three ability types of farmers, but not the other players' payoffs (they were assigned to a new ability type after the 4th auction). As discussed in more

[27]The main differences were that (1) we did not target a 50-50 split of gains from consolidation and sorting (the realized split is 73/27), (2) we did not use debt, and (3) adjacency bonuses were calculated as 40% of the plot's value – meaning higher-quality plots earned larger bonuses – whereas in experiment 1 each participant had a single fixed adjacency bonus irrespective of land quality. See Appendix C for all details.

[28]Bid assistants are a common feature of real-life combinatorial auctions when the target population may have difficulty with the interface, and have been used, for instance, in the auction of slot machines and taxi medallions in Australia. In our experiment the assistants read the instructions in the participant's preferred language, answered questions, helped with calculations, and entered bids into the system. To reduce the influence that an individual bid assistant might have on the experiment, we randomized bid assistants across participants and treatments.

detail in the appendix, participants could see their current allocation and bids on their screen, and a centralized screen showed the map with labels for each current plot owner and icons indicating plots with active bids.

Participants were paid for all eight auctions. Mean initial assets were worth 47 KES per auction, ($1.20 PPP) while efficient play would result in average earnings of 55 KES ($1.40 PPP), a 17% gain. Proportional gains were smaller than in experiment 1 because we did not use initial debt, but there were still good incentives to exert effort. The average participant earned 418 KES ($10.61 PPP, around 1.5 days' wages).

Outcome Measures Our outcome measures of efficiency, sorting, consolidation, and inequality are identical to experiment 1, except that we have to slightly adjust how we compute sorting and consolidation because in this experiment the adjacency bonuses scale with land quality.[29]

6.2 Treatment variation

We implemented a three-by-two design where we varied the trading mechanism between sessions and varied cash constraints between auctions within a session. Our main analysis centers on the trading mechanisms, and we discuss the cash constraint treatment in section 7

Trading Mechanisms We consider three trading mechanisms based on the package market of Goeree and Lindsay (2019): the benchmark treatment, "Package-1," permits bids to buy or sell one plot at a time. The second treatment, "Package-2," adds the possibility of package bids in which one plot is bought conditional on selling a second one. The third treatment, "Package-4," allows packages consisting of up to two buy orders and up to two sell orders. In our game, two buys and two sells would be sufficient for every participant to move to an efficient allocation.

A package bid specifies which plots are to be traded (e.g., buy plot 1 and sell plot 4) and a maximum willingness to pay (which will be negative when the farmer must be paid to accept the trade). Bids are submitted sequentially and the computer searches for the existence of a set of bids where (i) supply equals or exceeds demand for all plots; (ii) only a single bid is used for each farmer; and (iii) there is a non-negative surplus of cash (i.e., total willingness to pay is weakly positive). If more than one set of bids satisfies the criteria, the computer triggers the set with the largest cash surplus. Plots that were offered but not purchased stay with their original owners, and all other plots are transferred according to the winning orders. Prices

[29] To do this, we compute consolidation gains as if the farmer's land quality stayed constant between their initial and final allocations, and then attribute the rest of their gains to sorting. Formally, let y_i denote the value associated with farmer i's two best plots (ignoring consolidation bonuses), and let $c_i \in \{0,1\}$ indicate whether these plots are fragmented ($c_i = 0$) or consolidated ($c_i = 1$). It follows that the total profit on farmer i's land is $s_i := (1 + 0.4c_i)y_i$. After some algebra, the change in surplus from a farmer's initial allocation to their final allocation can be rewritten as:

$$s_i^{final} - s_i^{initial} = \underbrace{0.4\left[c_i^{final} - c_i^{initial}\right] y_i^{initial}}_{\text{Consolidation}} + \underbrace{\left(1 + 0.4c_i^{final}\right)\left[y_i^{final} - y_i^{initial}\right]}_{Sorting}.$$

are set by dividing the surplus among the winning farmers as equally as possible subject to revealed-preference constraints generated by the bids of non-trading bidders.[30]

Bids are exclusive-OR (XOR), meaning that when a farmer has one of their bids accepted, their other bids become inactive. They then have the option to reactivate any inactive bid if they like. This ensures that farmers do not accidentally buy or sell too much land, or consolidate land and then break up land, in the same cycle of transactions or in quick succession.

We describe the algorithms used to trigger trades (the winner determination problem) and to allocate surplus formally in Appendix C. Here we note that our mechanism is a near real-time auction based on Goeree and Lindsay (2019), with some modification to impose XOR bidding. All bids were entered through a simple computer interface which is described in details in Appendix C.

Credit Constraints Section 3.1 discussed how credit constraints can exacerbate exposure risk: a chain must form when a buyer does not have enough liquid assets to compensate their seller. To model the effect of credit constraints of varying tightness, we varied participants' initial cash balances across auctions. In half of all auctions they began with enough money to induce any farmer to sell any single plot. In the other half, they were given only one third of this amount.[31]

6.3 Data Issues

Due to the complexity of our experimental design we encountered two implementation challenges. First, our lead enumerators raised concerns that the other enumerators did not initially fully understand the rules of the experiment (we gave them three days of training including practice sessions, in retrospect we should have had more). As the enumerators were responsible for translating the instructions and teaching farmers, it is likely that farmers also did not fully understand the mechanisms in the early sessions. We assigned sessions to treatment within blocks of six sessions. In the data we observe substantially lower efficiency in the first assignment block (for instance, 63% of auctions in which efficiency was negative occurred during this first block), plus a general tendency for efficiency to be higher in later blocks. We always control for block fixed effects since treatment was stratified at this level, and in addition our preferred specification drops the first block. Appendix Tables D2 and D3 report results for the full sample. We find qualitatively similar results, but the treatment effects on efficiency are somewhat weaker.

Second, we lost one session due to accidental reformatting of our server computers, one session where the wrong treatment was used, and two auctions where the wrong map configuration was used. In total our analyzed data consists of 40 sessions, 318 auctions, 240 farmers, and 1908 farmer-auction observations.

[30] These require that a non-trading party would not prefer to be part of the transaction given their expressed bids and the realized prices. See Kwasnica et al. (2005) for a broader discussion of revealed-preference constraints. We explain our surplus division rule to participants using the logic of a farmer who has buy offers from either one or two farmers. If there is only one buyer, we split the surplus evenly between the two farmers. If there are two buyers, the buying price must exceed the bid of the non buyer.

[31] Specifically everyone had enough to buy an unconsolidated low-quality plot from a high-ability farmer, a medium-quality plot from a medium-ability farmer, or a high-quality plot from a low-quality farmer.

6.4 Results

What should we expect to be the effects of our different trading mechanisms? First, our package mechanisms, especially Package-4, have the potential to thicken markets. Under Package-4, a bidder can easily offer to exchange any two plots for any other pair in a different location, independent of where they started. In principle this should make it easier for potential buyers to compete for different pairs of plots. Second, they reduce exposure risk by enforcing chains. The Package-2 treatment allows for the conditional purchase and sale of a single object and should facilitate moving from one piece of land to another. Package-4 further allows farmers who have already consolidated two pieces of land to exchange these two pieces jointly for another pair of plots, rather than having to break them up and then re-consolidate. Third, packages should reduce coordination frictions, because farmers only need to enter their bids and let the algorithm search for the necessary chain of transactions.

Thus, in principle we should expect higher efficiency in the more tailored package mechanisms. Moreover, we would predict that Package-2 reduces the complexity associated with consolidation, while Package-4, by facilitating wholesale relocations, can unlock sorting gains.

However there are good reasons to be concerned that these theoretical gains will not be realized in practice. It is very unlikely that any of our participants had ever participated in a computer-based auction before, and this unfamiliarity could lead to them making mistakes or not trading at all. The richer mechanisms may exacerbate these effects. We will particularly struggle to find chains if some participants focus on bidding on packages while others only bid on single plots. Finally, richer mechanisms may enable sophisticated participants to profit at the expense of the less-sophisticated, potentially exacerbating inequality.

Our first result paints a strongly optimistic picture for the potential of mechanisms ours to be effective in solving the land trade problem.

Result 3 *Average efficiency is 70 percent or higher in all three mechanisms. Package-4 achieves 7 percentage points higher efficiency than Package-1, and does not exacerbate inequality.*

Column (1) of Table 10 regresses overall efficiency on our package treatments. Average efficiency is high under all three mechanisms, so our concerns about the platform overwhelming our participants seems unfounded.

From a base of 70% efficiency in the Package-1 treatment, Package-4 increases efficiency, by 7 percentage points, or 10%. This difference is significant. Efficiency is 3 percentage points (5%) higher in Package-2 than Package-1, but the difference is not significant.

Table 11 shows that the efficiency gains from more tailored designs do not come at the cost of higher inequality. We split the analysis by the amount of cash available because our inequality measure is not invariant to the initial asset level. We find no evidence that inequality was increased by our package treatments, and some evidence that the package mechanisms reduce inequality when cash balances are low. One explanation may be that when buyers are credit constrained, they can only offer low prices so the distribution of surplus becomes more unequal. Packages allow buyers to compensate their sellers with land instead of cash, ameliorating this effect.

Farmers continue to find the sorting problem harder than the consolidation problem. In the baseline Package-1 treatment they realize 86% of the potential gains from consolidation. The high consolidation rate is in line with our findings from experiment 1, especially in the centralized market. This is consistent with our theoretical observation that consolidation is more like a partnership problem and so less subject to information problems.

In contrast, participants only achieve 44% of the potential gains from sorting in Package-1. However, as predicted, our Package-4 mechanism successfully unlocks significant additional sorting gains.

Result 4 *Relative to Package-1, the Package-4 treatment unlocks an additional 12 percent of the potential gains from sorting. In contrast, Package-2 unlocks just 3 percent of the potential gains, and this increase is not statistically significant.*

Columns (2) and (3) of Table 10 report the impact of our treatments on consolidation and sorting. We see some improvements in consolidation but these are not significant. The absolute gains on the sorting dimension are larger, and substantially larger when expressed as a percentage of total potential sorting gains.

Finally, there are low levels of exposure losses in this experiment when compared to experiment 1, and no significant difference across treatments. This is similar to what we saw in the centralized market in experiment 1, where farmers effectively eliminated exposure losses.

7 Additional Results

In this section we discuss a sequence of additional results that shed further light on the frictions that constrain optimal trade, and the strategies farmers use to work around them.

Cultural constraints and nontradable plots Experiment 1 was designed to test how nontradable plots, by complicating the problem of packing consolidated farms onto the map, would affect efficiency and the distribution of outcomes. This is important to understand because cultural considerations (Fact 4) suggest it is unlikely that all plots will be available to trade. As explained in Section 3.1, the presence of nontradable plots has the potential to exacerbate information and coordination problems and increase exposure risk.

Each village played the game twice, once with a "complex" map (as in Figure 2a and once with a "simple" map (Figure 2b) in random order. We exploit this within-village variation to estimate the effect of map simplicity.

Table 8 Panel C provides suggestive evidence that simpler maps yielded higher efficiency, by 4.4 percentage points, but this is not statistically significant.[32] This is made up of a (marginally significant) 2 percentage point improvement in consolidation, a similar decline in sorting, and a 4 percentage point decrease in exposure losses. None of these is significant after adjusting for multiple testing.

[32]Our pre-analysis plan stated we would conduct one-sided tests; the one-sided p-value is 0.08 which we take as very weak evidence.

The map complexity treatment seems to matter more for inequality. Table 9 Panel A shows that inequality of outcomes was around 15–30% lower on simple maps (depending on how we adjust for negative net asset positions), and the difference is significant in three out of our four specifications. This is likely to be driven by the reduction in exposure losses that we saw in Table 8.

Finally, we do not find that the centralization intervention helped more on the complex maps. Table 8 Panel D shows that if anything it caused larger efficiency gains on simple maps (though all point estimates are small, nonsignificant, and swamped by the overall gains from centralization). For inequality, our estimates in Table 9 Panel B point to larger gains from centralization on complex maps, but again the differences are relatively small and not significant.

Overall, the impact of our simple/complex treatment variation appears to be relatively modest. The presence of nontradable plots does not seem to contribute meaningfully to low overall efficiency, but may be relevant for inequality.

Other measures of map complexity In experiment 1, our complex maps varied in how many welfare-equivalent efficient allocations existed, averaging between 1.67–5.33 ways to assign six consolidated blocks in a given quality region (one assignment is a unique combination of L- and I-shaped consolidated three-plot units). In the simple maps there are 134 such assignments. Figure B1 graphs efficiency, and its decomposition, against the number of potential solutions in the complex treatment. Consistent with our finding that the simple/complex map treatment—which substantially affects how many solutions exist—did not have large effects, we do not see any clear relationship in these graphs.

In experiment 2, we used eight hand-generated maps which we ordered according to our own judgment of difficulty (see Appendix C and Figure C1). Figure D1 plots efficiency against this ordering. In general we find a decreasing but non-monotone relationship (with near-perfect efficiency on the easiest map, which could be solved by each player swapping one plot with their neighbor), suggesting that our judgment partly captured which problems were hardest to solve. The Package-4 treatment appears to have had its largest impacts on maps we judged to be more complex.

Credit constraints In experiment 2 we varied initial cash balances across auction rounds, to mimic the effects of missing credit markets, potentially worsening the exposure problem. Table 10 shows that low-cash auctions did not significantly lower efficiency or its subcomponents, and we see no significant interactions with the different package mechanisms. The main effects of our Package-4 mechanism remain very similar, but with less precision.[33]

However, Table 11 shows that low cash appears to have been important for inequality responds to the package mechanisms. It is hard to directly compare the levels of inequality across low and high cash because the Atkinson index is sensitive to total assets, but we see proportionally larger and statistically stronger reductions in inequality under Package-2 and

[33] Besides lack of statistical power, one possible explanation may be that our low cash treatment was not severe enough to really bind. The low cash treatment would likely prevent anyone from buying high quality land from a high quality farmer but since this is never efficient it may not have constrained many trades. It was still feasible to buy any plot from a low-quality farmer, and a low-quality plot from anyone.

Package-4 when cash balances are low. This might reflect, as discussed in section 3.1, that our package mechanisms relax the effects of low cash balances on exposure risk. In the Package-2 and Package-4 mechanisms, plots can be swapped. This can lead to transactions that involve only small monetary transfers that relate to the difference in valuation between plots. By contrast, in the Package-1 mechanism, farmers are constrained to buying and selling land for cash. When cash holdings are low, this may lead to cases where sellers cannot fully capture the value of their land from a cash-constrained buyer.

Coordination Frictions and Verbal bargaining Both experiments allowed participants to bargain verbally over trades, either directly or through a mediator, since this is likely to be a natural feature of any solution implementable in the field. But verbally coordinating on chains of trade may be very difficult, whereas package bids allow participants to express preferences to the system and let the algorithm search.

The transactions data in experiment 2 provide indirect evidence for this conjecture. We see many transactions with zero cash surplus, meaning that total willingness to pay and willingness to accept equalled one another (e.g., farmer 1 demands 300 and farmer 2 offers exactly 300). We use the proportion of trades with zero surplus as a measure of verbally agreed ("brokered") transactions. Figure D2 shows that 37% of transactions in the Package-1 treatment are brokered, but only 20% of in Package-2 and 16% in Package-4. All these differences are significant in an OLS regression with errors clustered at the session level (p-value < .01 for all comparisons).

Spontaneous centralization Besides addressing our three frictions, an additional benefit of market design may be to help coordinate on the *rules of the game*. In general, there are many potential land trading "rules" that farmers might need to agree on. How does the community feel about people who make conditional promises and then renege? Should trade be bilateral or organized in groups or by brokers? When should trade take place? Settling on such rules may add an additional level of complexity to decentralized exchange.

If centralization is so effective, why didn't communities in experiment 1 organize their own periods of centralized trade? They had good incentives to do so: the average participant's earnings from the experiment increased by 16% or 2,450 UGX during the period of centralized trade. In fact, participants did try to: 89% said that they gathered in groups during the weeks of trade. However, we see little relationship between the intensity of "endogenous centralization" and outcomes. Figure B3 divides the sample into "endogeneous centralization" villages where every participant said they gathered in groups (62% of villages), versus the remainder where not everybody said so. We see no differences in efficiency between these two groups. Endogenous centralization is associated with slight improvements in consolidation and sorting, but more exposure losses. We conjecture that even when everybody is trying to centralize the market, they were not able to all coordinate at the same time. As a result, there is additional value to an outsider "exogenously" defining where and when the market will be centralized.

8 Conclusion

Small farms and fragmented plots are hallmarks of the agricultural sector in less developed countries, and there is evidence of high potential returns to land consolidation and reallocation. To help understand how market design might be used to improve the land allocation, we conducted a survey and two lab-in-the-field experiments in Uganda and Kenya.

Our results suggest that the production technology and institutional environment has characteristics that theory predicts would restrict trade. Our lab in the field experiments, using a game inspired by the results of our survey, confirm this conjecture. We also find significant support for the hypothesis that more tailored market design can improve both efficiency and equality. We see our results as providing a first step toward better-functioning, more equitable land markets that leverage farmers' preferences and information to reshape the rural landscape.

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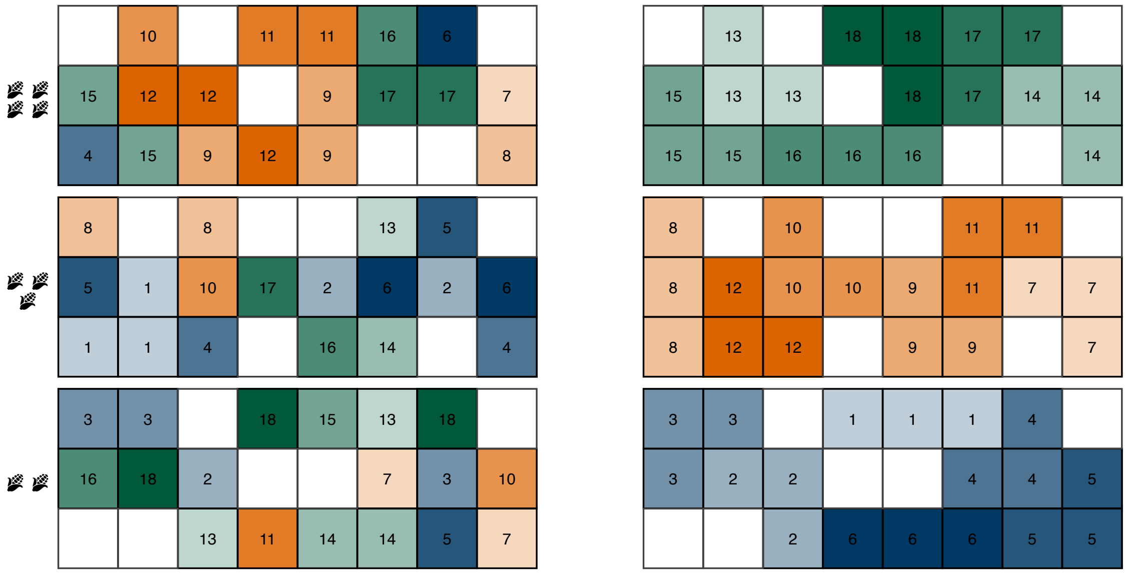
9 Figures

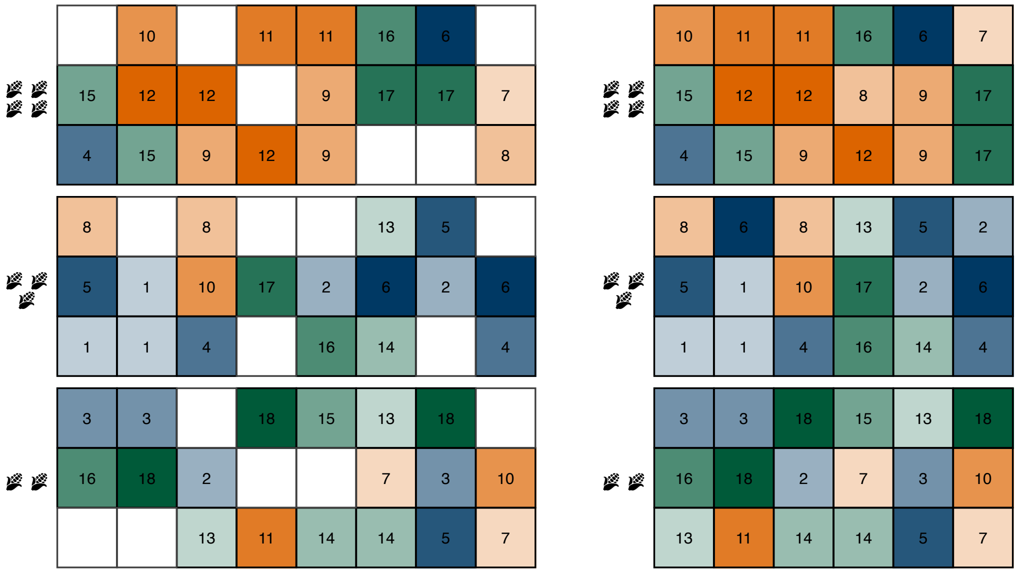


(a) Example initial allocation

(b) Example efficient allocation

Figure 1: The land trading game

Properties: (1) Players are numbered 1–18 in increasing *ability* order. Blue corresponds to the lowest ability type, Orange to the middle type, and Green to the high type. (2) There are three regions, (, ,), in increasing *land quality* order. (3) Ability and quality are complements. (4) Contiguous farms earn higher profits than fragmented farms. (5) Farmers cannot cultivate more than three plots.



(a) Complex (b) Simple

Note: numbers correspond to player IDs: 1–6 are low types, 7–12 medium types, 13–18 high types. The top region is high-quality land, the middle region medium-quality, and the bottom region low-quality.

Figure 2: Example map in complex and simple form

Properties: same as Figure 1. Simple map is constructed from Complex by moving plot owners leftwards so as to retain all existing adjacencies between plots of the same owner, while preserving as much as possible of the relative locations of different owners.

Figure 3: Efficiency in Training Games and in First and Second Week of Experiment 1

The graph shows mean efficiency in each game, with 95% confidence intervals.

(a) Example initial allocation (b) Example efficient allocation

Figure 4: Example Map from Experiment 2

10 Tables

Table 1: Fragmentation

Fragmentation in the village (yes/no)	mean	S.D.	obs
Owns two or more fragmented plots	0.64		1404
Distance between plots (in min)			
Lower bound	23.81	27.67	801
Upper bound	41.22	46.07	801
Distance to plots (in min)			
Purchased plots	19.28	26.63	1234
Inherited plots	11.85	19.22	1345
Plots received as gift	18.09	28.74	119
Costs of fragmentation (yes/no)			
Thinks having two one-acre plots better in same location	0.91		1404
Suppose that, instead of being separated, all your land was in the same place:			
Think they would earn more	0.88		896
How much more? (fraction)	0.53		792
Imagine a farmer with 2 **contiguous** acres who produces 20 bags of maize			
- He inherits one more acre **next** to the other two, so now he has three acres. Do you think he would be now be able to produce:			
Less than 30 bags	0.01		1404
30 bags	0.33		1404
More than 30 bags	0.66		1404
- He inherits one more acre **but far away** from the other two, so now he has three acres. Do you think he would be now be able to produce:			
Less than 30 bags	0.33		1404
30 bags	0.37		1404
More than 30 bags	0.30		1404

Note: We elicited walking distances from the home to each plot, and use these to compute a lower bound and an upper bound on the distance *between* the fragmented plots. The lower bound assumes both plots lie on the same bearing from home, so their separation is the difference between their distances from home. The upper bound assumes they are in opposite directions from home, so the distance between them is the sum of their distances from home.

Table 2: Complementarities Between Farmer Ability and Land Quality

Farmer heterogeneity (yes/no)	mean	S.D.	obs
Are there better farmers than others in your village?	0.99		1404
Does everyone in your village agree with who they are?	0.98		1393
How do you know these are better farmers? (select all that apply)			
Produce more yield/acre	0.93		1393
Use innovative farming techniques	0.37		1393
Have better machinery	0.05		1393
Have received farm training	0.18		1393
Are older farmers	0.11		1393
Farmers total production			
Worst farmer per acre relative to best farmer	0.28		1403
Do you think your village as a whole would produce more agricultural yield if the best farmers were cultivating more of the village's land?			
Produce the same	0.00		1404
Produce somewhat more	0.12		1404
Produce much more	0.88		1404
Land-crop heterogeneity (yes/no)			
Is some land better quality than other land in this village?	0.99		1404
Is there a crop that is good to grow in the worst land?	0.56		1404
Is there a crop that is bad to grow in the best land?	0.42		1404
Is there better/worse land for your crop?	0.93		1404
Plot-level: farmer thinks plot quality is good or very good	0.58		2726
Suppose you were thinking of buying or renting in a plot in your village. To what extent do you agree with the following statement:			
Good land is good for all crops, bad land is bad for all crops			
Strongly disagree	0.12		1404
Disagree	0.15		1404
Neither disagree or agree	0.20		1404
Agree	0.41		1404
Strongly agree	0.13		1404
Land-farmer complementarity			
Do you think your village as a whole would produce more agricultural yield if the best farmers were cultivating the best land?			
Produce much less	0.00		1404
Produce somewhat less	0.00		1404
Produce the same	0.01		1404
Produce somewhat more	0.16		1404
Produce much more	0.83		1404

Note:

Table 3: Decreasing Returns at the Farm Level

Span of Control	mean	S.D.	obs
Could you farm more land than you have now? (yes/no)	0.60		1404
Are some people in your village better at managing large farms? (yes/no)	0.99		1404
How much land can best farmer manage (acres) w/o hired labor	4.67	2.55	1403
How much land can worst farmer manage (acres) w/o hired labor	0.73	0.40	1401
Max amount of land that hh can manage (acres) w/o hired labor	2.53	1.50	1404
Max amount of land per adult in hh (acres) w/o hired labor	1.02	0.75	1404

Note: Per adult measure refers to members in the household aged 18 and older.

Table 4: Current Market Institutions: Land Markets

Land markets	Very frequently	Frequently	Occasionally	Rarely	Very Rarely	Never
How frequently do people in your village buy/sell land?	0.02	0.21	0.33	0.30	0.13	0.00
	mean	S.D.	obs			
Have you sold any land during the past 12 months?	0.02		1404			
– How many acres did you sell in total?	0.72	0.55	29			
– Price per acre sold (1000 UGX)	13456.55	36401.60	29			
Have you bought any land during the past 12 months?	0.08		1404			
– How many acres did you buy in total?	0.92	1.03	110			
– Price per acre bought (1000 UGX)	7040.85	14643.97	110			
Have you rented OUT any land during the past 12 months?	0.04		1404			
– How many acres did you rent out in total?	1.44	1.32	53			
– Price per acre rented out (1000 UGX)	241.93	189.20	53			
Have you rented IN any land during the past 12 months?	0.17		1404			
– How many acres did you rent in in total?	1.24	0.86	242			
– Price per acre rented in (1000 UGX)	185.88	161.68	242			
Would you know if someone is selling/buying/renting land?	0.89	0.31	1404			
Attempts to consolidate (yes/no)						
Tried to consolidate	0.24		1404			
– Successful? (Conditional on Trying)	0.50		341			
Land owned by people from outside village (yes/no)						
How many out of 10 plots are owned by people outside village?	1.84	1.34	1404			
Characteristics of household's own plots						
Do you cultivate this plot? yes	0.96		2726			
If plot not cultivated, is it rented out? yes	0.13		111			
Plot was purchased	0.45		2726			
Plot was inherited	0.49		2726			
Plot was given	0.04		2726			

Note:

Table 5: Current Market Institutions: Property Rights, Credit Market, Insurance Markets, and Tractors

Land Registries and Institutions (yes/no)	mean	S.D.	obs
Would you know where to register a sale/purchase?	0.94		1404
Would you know where to find a broker?	0.59		1404
Are there people in your village that help organizing land trades?	0.56		1404
Market design			
Have you ever engaged in an auction? (yes/no)	0.73		1404
What type? Land auctions	0.01		1028
Do swaps exist in your village? (yes/no)	0.02		1404
Insurance			
Do you have crop insurance? (yes/no)	0.02		1404
If not: do not know where to find/ too expensive/ do not trust or not suitable	0.81		1374
If you have bad yield, would people in the village help? (yes/no)	0.28		1404
If village has bad yield, would it receive help from outside? (yes/no)	0.27		1404
Borrowing			
Could you borrow money/loan to buy land? (yes/no)	0.54		1404
Capital markets: tractors			
Have you ever used a tractor on your land? (yes/no)	0.06		1404
Why not? no tractor available	0.31		1316
Why not?			
Land too fragmented	0.23		1316
Tractor hire is too expensive	0.65		1316
No tractor available	0.31		1316
Observations	1316		
Cost and productivity estimates of using tractor			
How much..			
.. land can be prepared by a tractor in 1 day? (in acres)	3.85	2.77	475
.. would it cost to hire a tractor for one day? (1000 UGX)	487.20	371.35	487
.. time would it take to prepare (acres 1-day tractor) of land by hand? (in days)	72.75	60.11	457
.. would it cost to hire someone to prepare land by hand? (1000 UGX, days above)	629.80	567.39	456
Suppose you had one acre of land planted with the crop you grow the most. Would it grow the same amount when prepared by hand as when prepared by tractor?			
More growth when prepared by hand than tractor	0.18		487
Same growth	0.26		487
Less growth when prepared by hand than tractor	0.56		487

Note: Answers to questions about the cost and productivity of tractor use a sample of farmers that report an estimate on the cost of hiring a tractor.

Table 6: Current Market Institutions: Culture and Attitudes Toward Land Trade

	Var mean	Very unfair (1)	Unfair (2)	Neither unfair or fair (3)	Fair (4)	Very fair (5)
Do you think it would be fair if best farmers cultivate best land?	2.52	0.38	0.23	0.06	0.13	0.19

	Var mean	Strongly disagree (1)	Disagree (2)	Neither disagree or agree (3)	Agree (4)	Strongly agree (5)
A family should never sell their ancestral land	4.53	0.04	0.04	0.01	0.14	0.76
A family should be free to trade their land if it improves their situation	3.06	0.18	0.18	0.14	0.38	0.12
It is very important to me to own some land	4.75	0.01	0.00	0.01	0.20	0.79
If I could sell my land for a good price, I would move to the city	1.62	0.61	0.28	0.04	0.04	0.04
I would like to migrate	1.68	0.53	0.35	0.05	0.05	0.02
I would like my children to be farmers	3.74	0.04	0.16	0.11	0.41	0.28
People shouldn't sell land to people within the family	1.74	0.43	0.49	0.02	0.03	0.03
People shouldn't sell land to people in the village outside their family	2.00	0.26	0.62	0.03	0.05	0.04
People shouldn't sell land to people outside the village	2.38	0.15	0.59	0.05	0.11	0.09
People shouldn't sell land to people from outside the tribe	2.71	0.12	0.49	0.08	0.18	0.13
People shouldn't sell land to foreigners	3.53	0.09	0.22	0.08	0.32	0.30
People shouldn't swap land within the family	2.05	0.33	0.50	0.03	0.06	0.08
People shouldn't swap land with the people in the village outside their family	2.59	0.16	0.53	0.03	0.12	0.15
People shouldn't swap land with people outside the village	3.15	0.07	0.41	0.06	0.24	0.23
People shouldn't swap land with people from outside the tribe	3.39	0.05	0.35	0.07	0.25	0.29
Observations: 1404						

Note:

Table 7: Adverse Selection

Adverse selection	mean					
Knows how to locate best land	0.99					
How would you check if a plot is of good quality?						
Look at what is growing	0.82					
Look at the soil	0.79					
Look at the slope	0.14					
Look at the water resources	0.12					
Look at weather in location	0.06					
Ask owner	0.06					
Ask neighbours	0.04					
Ask others in village	0.02					
Look at irrigation	0.00					
Suppose you were thinking of buying or renting in a plot in your village.	Var mean	Strongly disagree	Disagree	Neither disagree or agree	Agree	Strongly agree
To what extent do you agree with the following:		(1)	(2)	(3)	(4)	(5)
People only sell or rent out their worst plots	2.91	0.18	0.30	0.14	0.22	0.17
Thinks difficult to assess quality of plot owned by others	1.86	0.28	0.63	0.05	0.02	0.01
Knows how to assess quality of plot owned by others	4.27	0.00	0.01	0.04	0.60	0.34
Thinks difficult to assess quality of own plot	1.72	0.40	0.54	0.03	0.03	0.01
Knows how to assess quality of own plot	4.44	0.00	0.00	0.02	0.50	0.48
Observations: 1404						

Note:

Table 8: Efficiency in Uganda Decentralized Trade Experiment

		Decomposition		
	(1) Efficiency	(2) Consolidation	(3) Sorting	(4) Avoided exposure loss
Panel A: Decomposition of Efficiency				
Mean	0.230	0.306	0.127	-0.202
as % of first best		*0.611*	*0.254*	
Observations	136	136	136	136
Panel B: Efficiency & centralization				
Centralization	0.348***	0.062***	0.001	0.286***
	(0.019)	(0.008)	(0.005)	(0.019)
as % of first best		*0.124*	*0.001*	
FDR q-value: centralization		[0.001]	[0.439]	[0.001]
Control mean	0.119	0.287	0.147	-0.315
Control mean: % of first best		*0.574*	*0.294*	
Observations	136	136	136	136
Panel C: Efficiency & complexity				
Simple	0.044	0.022*	-0.018	0.040
	(0.031)	(0.012)	(0.014)	(0.028)
as % of first best		*0.045*	*-0.036*	
FDR q-value: simple		[0.232]	[0.232]	[0.232]
Control mean	0.208	0.294	0.136	-0.222
Control mean: % of first best		*0.589*	*0.272*	
Observations	136	136	136	136
Panel D: Efficiency, centralization & complexity				
Centralization	0.342***	0.059***	-0.005	0.287***
	(0.027)	(0.008)	(0.008)	(0.028)
Centralization × simple	0.013	0.006	0.011	-0.003
	(0.038)	(0.015)	(0.010)	(0.037)
FDR q-value: centralization		[0.001]	[0.211]	[0.001]
FDR q-value: centralization × simple		[1.000]	[1.000]	[1.000]
Control mean	0.111	0.284	0.151	-0.324
Observations	136	136	136	136

Note: *** denotes significance at the 1% level, ** 5% level and * 10% level. % of first best shows coefficient as share of potential gains within each category. First best for this experiment: consolidation = 0.5, and sorting = 0.5. Panels A and C include data from weeks 1 and 2, excluding the centralization treatment. Panels B and D include data from week 2, pre and post-centralization. Control group in panel B: week 2 pre-centralization, in panel C: complex maps, and in panel D: week 2, complex maps, pre-centralization. Columns (2), (3) and (4) show the decomposition of efficiency into consolidation, sorting and avoided exposure loss respectively. Regressions in panel B control for village fixed effects. Regressions in panel C control for village and week 2 × map fixed effects. Regressions in panel D control for village fixed effects. Standard errors (in parentheses) are clustered by village.

Table 9: Inequality in Uganda Decentralized Trade Experiment

	Atkinson Index (log utility)			
	(1) + 5-day wage	(2) + worst score	(3) + show-up fee	(4) rounded
Panel A: Inequality & complexity				
Simple	-0.003**	-0.011	-0.068*	-0.090**
	(0.001)	(0.007)	(0.036)	(0.043)
p-value simple: coef = 0	0.039	0.142	0.066	0.040
Control mean	0.014	0.030	0.237	0.551
Observations	136	136	136	136
Panel B: Inequality & centralization				
Centralization treatment	-0.005***	-0.008***	-0.146***	-0.304***
	(0.001)	(0.002)	(0.031)	(0.042)
Centralization × simple	0.001	0.002	0.048	0.036
	(0.001)	(0.003)	(0.044)	(0.064)
p-value centralization: coef = 0	0.000	0.000	0.000	0.000
p-value centralization × simple: coef = 0	0.394	0.379	0.286	0.579
Control mean	0.013	0.023	0.255	0.582
Observations	136	136	136	136

Note: The outcome variable is the Atkinson inequality index (equation (1)). Higher values mean greater inequality. Because many participants had negative net assets and the index is based on log assets, we explore four different corrections to ensure that the index is defined. Column (1) uses final net assets adding a five-day wage, column (2) uses final net assets adding the worst score in the sample, column (3) uses final net assets adding the show-up fee and rounds to 1 those with negative assets, and column (4) uses final net assets and rounds to 1 those with negative assets. 5.23% of the sample (player-period level) has negative net assets. Five-day wage is a total of 250,000 game shillings. Worst score is -181,000 game shillings. Show-up fee is a total of 50,000 game shillings. 1.3% of the sample has a negative final net assets after adding the show-up fee. Control group in panel A: complex, and in panel B: week 2 (no Centralization). Regressions in panel A use data from week 1 and week 2, and in panel B use data from week 2 only. Regressions in panel A control for village fixed effects, and in panel B control for village fixed effects and week 2 × map fixed effects. Standard errors clustered by village in parentheses.

Table 10: Efficiency in Kenya Package Exchange Experiment

		Decomposition		
	(1) Efficiency	(2) Consolidation	(3) Sorting	(4) Avoided exposure loss
Panel A: Comparison of mechanisms				
Package-2	0.033	0.006	0.008	0.018
	(0.033)	(0.018)	(0.012)	(0.013)
as % of first best		*0.008*	*0.031*	
Package-4	0.068**	0.019	0.032**	0.017
	(0.031)	(0.016)	(0.013)	(0.014)
as % of first best		*0.026*	*0.119*	
FDR q-value: Package-2		[1.000]	[1.000]	[1.000]
FDR q-value: Package-4		[0.184]	[0.076]	[0.184]
Control mean	0.696	0.628	0.118	-0.050
Control mean: % of first best		*0.856*	*0.444*	
Observations	318	318	318	318
Panel B: Credit constraints				
Package-2	0.023	-0.012	0.024	0.011
	(0.048)	(0.026)	(0.018)	(0.020)
Package-4	0.067	0.016	0.044**	0.007
	(0.048)	(0.020)	(0.021)	(0.021)
Low cash	-0.003	-0.016	0.015	-0.002
	(0.049)	(0.028)	(0.023)	(0.021)
Package-2 × low cash	0.019	0.036	-0.032	0.015
	(0.056)	(0.032)	(0.031)	(0.024)
Package-4 × low cash	0.001	0.006	-0.025	0.020
	(0.061)	(0.032)	(0.029)	(0.028)
FDR q-value: Package-2		[1.000]	[1.000]	[1.000]
FDR q-value: Package-4		[0.768]	[0.137]	[0.999]
Control mean	0.702	0.640	0.110	-0.048
Observations	318	318	318	318

Note: % of first best shows coefficient as share of potential gains within each category. First best: consolidation = 0.733, and sorting = 0.266. Control group: Package-1. Columns (2), (3) and (4) show the decomposition of efficiency in consolidation, sorting and avoided exposure loss respectively. Regressions in columns (1), (2) and (4) use data from all maps, sessions and randomization blocks 2-8. All columns control for low cash dummy, and auction number, map and randomization block fixed effects. Standard errors clustered by session in parentheses.

Table 11: Inequality in Kenya Package Exchange Experiment

	Atkinson Index (log utility)		
	(1) High cash	(2) Low cash	(3) High & Low
Package-2	0.0004	-0.0031***	0.0004
	(0.0006)	(0.0011)	(0.0006)
Package-4	-0.0002	-0.0019*	-0.0002
	(0.0006)	(0.0010)	(0.0006)
Package-2 × low cash			-0.0035***
			(0.0011)
Package-4 × low cash			-0.0017
			(0.0010)
p-value Package-2 = 0	0.544	0.007	0.545
p-value Package-4 = 0	0.738	0.075	0.738
p-value Package-2 × low cash = 0			0.002
p-value Package-4 × low cash = 0			0.117
Control mean	0.012	0.035	0.024
Observations	159	159	318

Note: The outcome variable is the Atkinson inequality index (equation (1)). Higher values mean greater inequality. The control means in low versus high cash auctions (columns (1) and (2)) are not directly comparable because the Atkinson inequality index is not invariant to additive changes in total wealth. Control group: Package-1. Regressions in column (1) use data from high cash group, in column (2) use data from low cash group and in column (3) use data from both high and low cash groups. All regressions control for randomization block, auction and map fixed effects, and in column (3) adds interactions each of the fixed effects with a low cash dummy. Standard errors clustered by session in parentheses.

Web Appendix
Market Design for Land Trade:
Evidence from Uganda and Kenya

A Appendix A: Implementation Details for Experiment 1

This appendix provides additional implementation details for Experiment 1.

A.1 Selection of Villages and Participants

We sampled our villages in a two-step process. First, we chose a set of villages to visit for our experiment. Second, we selected the participants to recruit.

Villages: We worked in the Masaka district, Uganda. Masaka was selected because the majority of land in Masaka is owned under freehold, i.e. it is in principle tradable by the owner. Tenure form differs in other parts of Uganda, and landholders do not have the legal or traditional right to trade land everywhere. While our experiment does not involve real land trade, we wanted to work in a region where land trade is imaginable to participants.

We selected villages using an administrative unit-level GIS file, containing census data from 2002 and 2010. We first dropped villages not listed as being in Masaka county, then dropped subcounties that subsequently joined other districts, leaving an initial sample frame of 357 villages, belonging to two counties (Bukoto and Masaka Municipality), 10 subcounties, and 39 parishes.

1. Next, we dropped 11 villages with zero population. This left us with 346 villages.
2. We dropped 4 villages with duplicate names, that would be difficult for our field team to identify reliably. This left us with 342 villages.
3. We dropped villages that were densely populated and had limited farmland. While these villages may contain many farming households, we were concerned that recruitment and attrition would be more challenging in these areas. We do this in three ways. First, we dropped villages above the 90th centile for population or population density. Second, we dropped parishes with median village above those thresholds. Third, we dropped Masaka Municipality (the main urban area). The thresholds were tuned by visual inspection of satellite images, inspecting the "marginal" villages around the threshold for whether they had significant farmland. This left us with 274 villages (Masaka municipality accounts for 53 of the 68 villages dropped).
4. We also dropped 7 villages that were previously visited for piloting. This left us with 267 villages.
5. We dropped 26 coastal villages (identified by visual inspection) that were expected to be dominated by fishing and other activities rather than agriculture. This left us with our final sampling frame of 241 villages.

The 241 villages belonged to 31 "parishes" (the next highest administrative unit), which we used for stratification (see section A.3 below).

Participants: In each selected village we first met with the village chief, and asked them to give us a list of as many households as they could think of (excluding the chief's own family members) that would be expected to be interested to participate in a sequence of trading games on three days separated by one week each.[34] The chief was also asked to attend the meetings and assist with ensuring that selected participants attend, and were compensated for their time.

We selected households randomly from the list and sought the consent of the household head to participate in the experiment. Eligibility criteria were 1) cultivation of some land, 2) reporting that at least 50% of household income was derived from farming, 3) having access to a mobile money account. Criteria 1 and 2 were intended to ensure we sample a relevant population that might be interested in real land trade, 3 ensured that participants can be paid their study earnings. If the household head is interested but not available we allow them to send another household member in their place.

We proceed this way until 22 households had been recruited. The first 18 were our intended "primary" participants, and the remaining 4 acted as reserves. The reserves were asked to attend each session, and paid show-up fees for doing so. If a primary participant did not attend a session, they were replaced by a reserve.

A.2 Payoffs and Maps

Payoffs: Each of our games consisted of 18 players and a map. Each players was assigned a numeric ability type. This ability type was private information and farmers were asked not to share it with others. Consistent with the results from the survey, the payoff function of each farmer had three key properties:

1. Ability-quality complementarities: the return to a given piece of land was the product of the player ability and the land quality. The land quality types were Low, Medium, High: $\{2,3,4\}$. The player ability types were Low: $\{0.8, 0.9, 1, 1, 1.1, 1.2\}$, Medium: $\{1.3, 1.4, 1.5, 1.5, 1.6, 1.7\}$, and High: $\{1.8, 1.9, 2, 2, 2.1, 2.2\}$.

2. Spatial complementarities: players earned an "adjacency bonus" when two of their plots shared a border, and two bonuses when three plots shared two borders (either in a vertical or horizontal strip or an"L"-shaped unit). The adjacency bonus was fixed at the player level to 10% of the player's value of a high-quality plot (e.g. a player of ability type 1 had an adjacency bonus worth $1 \times 4 \times 0.1 = 0.4$). To limit the number of payoff parameters that participants had to keep track of, the adjacent bonus was independent of land quality. Adjacency bonuses could only be earned within a land quality region.

3. Span of control: each player could farm a maximum of three plots – if they end the game with more than three they earned the return to their best three-plot combination.

Land values on maps and plot titles were represented visually with two, three, or four icons representing heads of maize. Thus, each player had four key payoff parameters to keep track of: their value for each type of land, and their adjacency bonus.

Given the payoff function, the efficient allocation was simple to compute: each player should hold three adjacent plots, positively sorted by quality-ability type. The payoff parame-

[34] In piloting we experimented with fully random sampling of participants (we attempted to obtain a full list of households from the LC1 chief and selected randomly from that list). However, this led to several of the selected participants being quite uninterested in participation, so many would send another household member, or might simply not show up. We therefore settled on giving some guidance to the chief, to suggest "interested" households. The primary goal is to ensure successful completion of the experiment since significant attrition can prevent completion of the three stages. It means the study population may be less representative of the village population, but may conversely be *more* representative of those interested in land trade.

ters were calibrated such that the gains from trade were divided approximately 50–50 between sorting and consolidation.

To generate payoff numbers that were similar in magnitude to those used by farmers in day-to-day trades, we multiplied payoffs by 20,000 and expressed values in terms of "game shillings."[35] Each player began each game with 240,000 game shillings in printed paper bills that could be used for exchange.

We also assigned each player an initial "debt," to be deducted from their final payoff when computing earnings from the games. The debt levels were calculated such that each person began the game with net assets (land value, plus initial adjacency bonuses, plus cash, minus debt) equal to 70,000 game shillings.

Final earnings (in game shillings) were calculated as

$$\text{Earnings} = \text{Final land value} + \text{Final adjacency bonuses} + \text{Final cash} - \text{Initial debt}$$

and then converted to UGX at the rate 5 game shillings = 1 UGX.

The primary role of debt was to calibrate incentives in the game. We wanted the gains from trade (in relative and absolute local currency terms) to be sufficiently large that participants payed attention and participated fully. Final earnings depended on initial assets and gains from trade. For a given average payoff, subtracting debt from initial assets increased the contribution of gains and therefore sharpened incentives. We also used the debt to start all players with the same payoff so that inequality changes could be easily compared across games.

Maps: We used the following procedure to construct the complex map and assign players to it:

1. We began with a grid of three 3*8 blocks of plots,
2. We randomly group plots into 3 groups of 24, corresponding to 18 players and 6 "non-trading" dummy players, such that:
   - Non-trading players own exactly six plots per quality region (otherwise the first-best allocation is not achievable).
   - There were no simple blocking allocations, that is, a single player that holds three plots that isolate a corner, or a combination of non-trading players that hold between them two or three plots blocking a corner.
   - The number of initial "adjacencies"[36] and "near adjacencies"[37] averaged 0.3–0.4 per player. These thresholds were set to ensure a realistic amount of clustering of initial ownership, based on visual inspection of real-world maps.
   - The contribution of land consolidation and sorting to total gains from trade was balanced, with a relative contribution range between 47.5%–52.5%.

Typically maps generated in this way have no efficient packing solution. We manually identified 10 such that (i) feasibility could be achieved by moving a maximum of 1 plot and (ii) The resulting map had a single contiguous set of land. Of these, 2 were solvable with no edits, and the remaining 8 needed one swap (exchanging a single plot between one trading

[35] This implied that the minimal land value was 32,000 ($0.8 \times 2 \times 20,000$) and the maximum land value was 176,000. The minimum adjacency bonus was 6,400 and the maximum was 17,600.

[36] A player with two horizontally or vertically adjacent plots within the same quality region counts 1, a player with three plots sharing two borders counts 2.

[37] Two plots owned by the same player that are diagonally adjacent, or separated by one plot, count as 1 near adjacency, so long as that player is not already fully consolidated. We allow near-adjacencies to span across quality types.

and one non-trading player). Swaps were implemented so as to avoid breaking or creating new adjacencies. Thus the initial payoffs were unaffected.

A.2.1 Making the simple maps

For our "simple" treatment we want to eliminate the non-trading players. We do this by manually "compressing" the complex maps, so as to preserve the adjacency structure of the map. We did this by shifting plots horizontally left, except where doing so would create or break an adjacency. Therefore, the initial payoffs are unaffected. Note that it is not possible to preserve the "near-adjacency" structure.

A.2.2 Pairing complex and simple maps

Following the above process, we generated 10 candidate maps, each with a complex and simple variant. From these we selected 8 and created four pairs, matched according to the number of possible efficient solutions in the complex form. According to our internal map numbering these are:

- Maps 69 and 148 which have on average 1.67 solutions per quality block, and 8 adjacencies among the trading players.
- Maps 74 and 149 which have on average 3 and 3.67 solutions per quality block, and 5 adjacencies among the trading players.
- Maps 93 and 130 which have on average 3.67 and 4.67 solutions per quality block, and 6 adjacencies among the trading players.
- Maps 28 and 193 which have on average 5 and 5.33 solutions per quality block, and 4 and 6 adjacencies respectively among the trading players.

A.3 Treatment assignment

Each village played the game twice, once on a simple map and once on a complex map. This section details how the ordering and the specific maps were assigned.

A.3.1 Possible assignments

As described in Appendix A.2 our map generation procedure yielded 8 maps (internal IDs 28, 69, 74, 93, 130, 148, 149, 193) each of which had a simple and a complex form. We grouped these 8 maps into 4 matched pairs according to the number of possible efficient packings available in their complex form. Accounting for possible map and complexity orderings this yielded 16 possible assignments. These are listed in Table A1.

Our field plan involved two field teams working simultaneously five days per week, covering 10 villages per week. We intended to sample 68 villages, leaving two vacant "slots" for replacement villages in case a village decides to withdraw (see section A.3.3). The 68 villages constituted four complete blocks of 16 assignments plus one randomly selected block of four (either assignments 1–4, 5–8, 9–12 or 13–16).[38]

[38] Our original sampling plan was 64 villages, we later discovered we had sufficient budget to increase to 68.

Assignment	Assignment pair	Map ordering	Complexity ordering
1	1	(69, 148)	(simple, complex)
2	1	(69, 148)	(complex, simple)
3	2	(148, 69)	(simple, complex)
4	2	(148, 69)	(complex, simple)
5	3	(74, 149)	(simple, complex)
6	3	(74, 149)	(complex, simple)
7	4	(149, 74)	(simple, complex)
8	4	(149, 74)	(complex, simple)
9	5	(93, 130)	(simple, complex)
10	5	(93, 130)	(complex, simple)
11	6	(130, 93)	(simple, complex)
12	6	(130, 93)	(complex, simple)
13	7	(28, 193)	(simple, complex)
14	7	(28, 193)	(complex, simple)
15	8	(193, 28)	(simple, complex)
16	8	(193, 28)	(complex, simple)

Table A1: Possible treatment assignments

A.3.2 Randomization

- Our primary regression specification exploit the within-village variation in complexity, but to increase power in between-village comparisons we stratified the assignment by parish and study date.
- Specifically, when selecting study villages we first randomly ordered parishes, then randomly selected pairs of villages from each parish. Each pair of villages was then assigned an *assignment pair* (see Table A1), so they differed only in their {simple, complex} ordering.
- We randomly ordered non-selected villages within each parish to act as backups in case a selected village opted not to participate.
- We had two experimental teams operating, such that each pair of villages participated in the study simultaneously, i.e. we conducted meetings 1, 2, and 3 on the same day for both villages.
- Since we have 31 parishes, we sampled all parishes once and three parishes twice.

We also stratified the assignment by four blocks of 16 assignments, i.e. we played every assignment pair once (in random order) before moving to the next block of 16.

A.3.3 Village attrition

Our protocol was designed to address attrition of individuals by replacement with reserves. We also faced two possible sources of village attrition:

1. The field team was unable to locate a sampled village at mobilization time, or the village chose not to participate. In this case the team moved to another randomly selected village from the same parish.

2. A village chose to withdraw from the study during the experiment. In this case we replaced the village with a randomly selected village from the same parish. To avoid disrupting the field work, these replacement villages were visited at the end of the experiment.

Overall, we had only one village that chose to withdraw. This village was replaced with another village from the same parish.

B Appendix B: Additional Results from Experiment 1

This appendix contains all additional analysis that was part of the pre-analysis plan for experiment 1. The pre-analysis plan can be found at https://doi.org/10.1257/rct.4581.

Week-level analysis and learning Our pre-analysis plan included specifications in which we study (i) learning across the two periods and (ii) compare efficiency, consolidation, sorting, and exposure gains and losses in each week separately. Table B2 provides the details of these regressions.

As can be seen in Panel A, overall efficiency is lower in the second week relative to the first week. This reduction in efficiency is primarily due to much larger exposure losses, which suggest that individuals in the second week traded to intermediate positions that they could not trade out of. These is also weak evidence of slightly lower consolidation gains and slightly higher sorting gains. However, we note that these measures are influenced by the reassignment of unused plots to highest value users and are both lower in the second week if we do not adjust the measures for exposure losses.

As seen in Panel B, there is no strong difference between the simple and complex treatment in the first week, the second week, or after the surprise centralization treatment. As such, the map complexity appears to be second order relative to inefficiencies that exist in both the simple and complex map.

Alternative efficiency, sorting, and consolidation measures Our pre-analysis plan also specified number of alternative efficiency, sorting, and consolidation measures. These are provided in Tables B3 and B4 below.

In Table B3, we provide alternative measures of consolidation (column 2) and sorting (column 3) in which we do not reassign unused lots to their highest value use when calculating gains. There continues to be no significant difference in efficiency between the simple and complex treatments using these alternative values. In column (4), we use an alternative adjusted efficiency measure where we reassign unused plots to their highest value use. The simple treatment is again not significant in this specification.

In Table B4, we analyze efficiency, consolidation, and sorting only in the High-quality region in the first three columns. We then report on an alternative count-based consolidation and sorting measures. For the consolidation measure, we replaced "land value" with the number of plots owned by a player of the efficient type. For the sorting measure, we counted the number of adjacency bonuses rather than using the value of these bonuses.

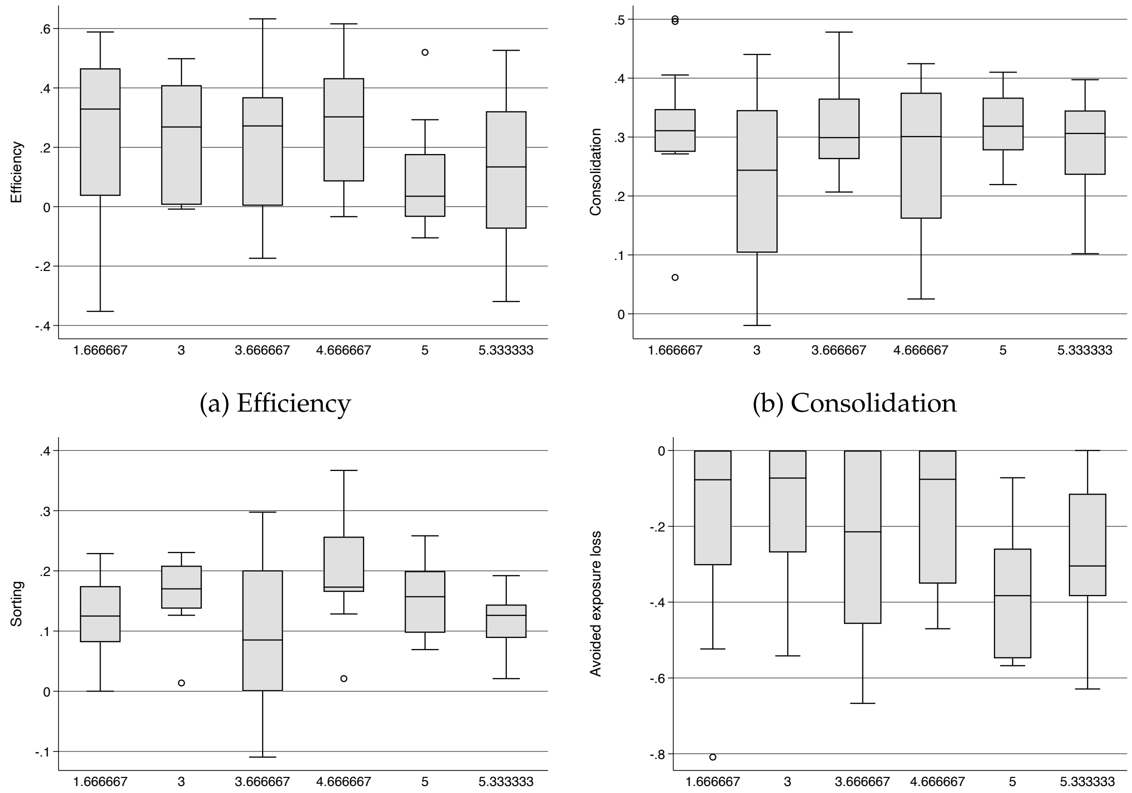
As seen in the table, the results using these alternative measures are similar to those provided in the main text. As seen in panel A, there is weak evidence that consolidation is easier in the simple treatment and no other significant differences between the simple and complex treatments. As seen in Panel B, the centralization treatment improves efficiency and consolidation, but has no impact on sorting. There is also no significant interaction between the centralization treatment and map complexity.

Alternative Inequality Measures Our pre-analysis plan discusses a potential inequality measure based on the Shapley Value. However, the Shapley analysis was sensitive to efficiency and we specified that we would only complete the analysis if efficiency was over 70%. Given the low efficiency observed in both weeks, we did not do the Shapley analysis for Experiment 1.

Alternative complexity measure Our pre-analysis plan proposes to descriptively analyze how efficiency and its decomposition depend on the number of (welfare-equivalent) efficient solutions in complex maps. Figure B1 plots these measures against the number of efficient solutions, averaged across quality regions. Consistent with our finding that the simple/complex map treatment – which substantially affects how many solutions exist – did not have large effects, we do not see any clear relationship in these graphs.

B.1 Non pre-specified analyses

Figures B2 and B3 present additional analysis that we discuss in Section 7.



(a) Efficiency (b) Consolidation

(c) Sorting (d) Exposure loss

Figure B1: Analysis of alternative map complexity measure in experiment 1

Each plot graphs efficiency (or a subcomponent) against the number of ways to efficiently "pack" consolidated three-plot farms on our complex maps, averaged across the three quality regions.

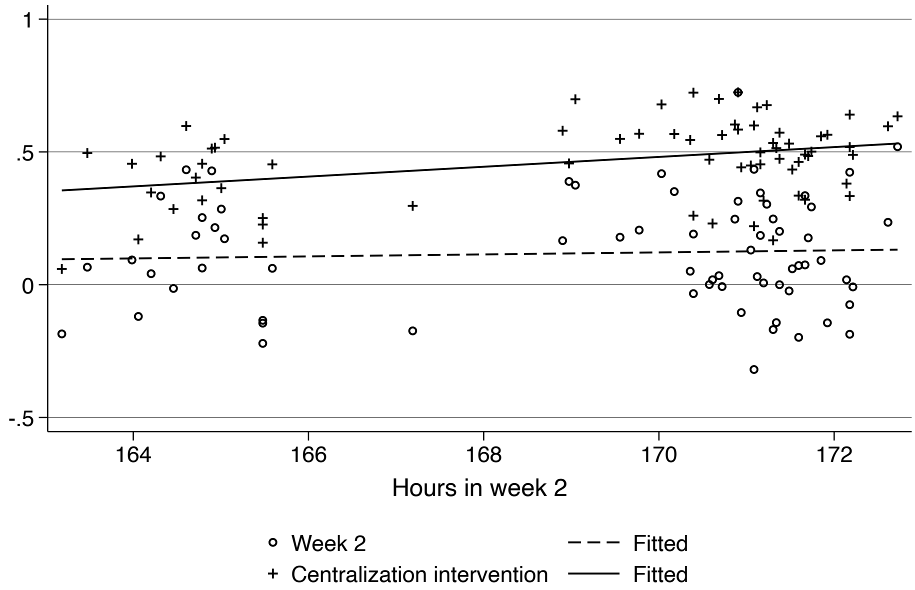


Figure B2: Relationship between time to trade and efficiency

We plot efficiency against the total time available to trade in week 2. We plot the relationship separately for efficiency measured before and after the trading day. There is no apparent improvement in week 2 outcomes for groups that had more time, and no negative relationship between the length of the week 2 window and the improvements attained during the centralization intervention.

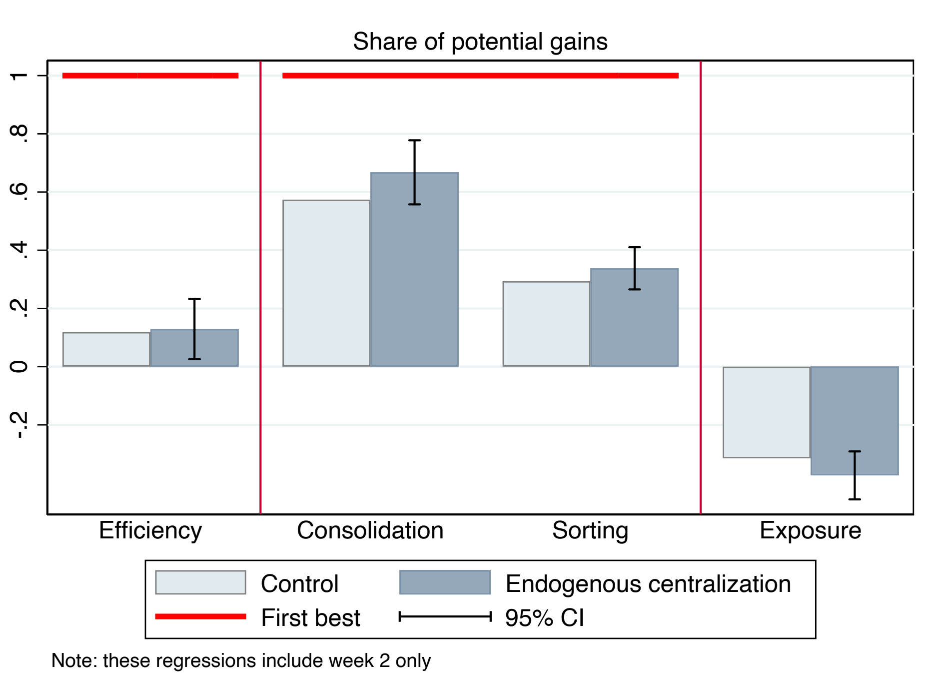


Figure B3: The Effect of Self-organized Centralization

We show mean efficiency and its subcomponents separately for the 62% of villages where 100% of respondents said that they got together in groups to trade during the experiment ("Endogenous Centralization") relative to those where less than 100% did. Consolidation and Sorting are expressed as a percentage of the maximum possible gains on each of these dimensions.

Table B1: Summary Statistics: Experiment 1

	Our sample			Uganda		
Demographics	mean	S.D.	obs	mean	S.D.	obs
Age	43.76	13.52	1404	39.11	17.48	3338
Female	0.51		1404	0.51		3338
Head of household	0.65		1404	0.38		3338
Married: monogamous	0.63		1404	0.49		3338
Married: polygamous	0.06		1404	0.11		3338
Nr adults (inc respondent)	2.99	1.54	1404	2.60	1.27	1246
Nr children in household	3.37	2.07	1404	2.97	2.13	1246
Education						
Education (years)	7.16	3.21	1404	6.34	3.24	2551
Numeracy	0.76		1224			
Farm size and income						
How many plots do you own and cultivate?	2.10	1.15	1404	1.69	0.93	1246
Total land holdings cultivated (in acres)	2.95	3.32	1349	2.94	4.22	1244
Income from agriculture (1000 UGX/season)	1482	2174	1349	897	1995	847
Income from agriculture (USD PPP/season)	1365	2002	1349	826	1837	847
Farming ability (self-evaluated, relative to best in village)						
Farmer's total production	0.47		1403			
Max farm size (w/o hired labor)	0.59		1403			
Preferences (1-5 scale)					GPS	
Patience	4.35	0.66	1404	3.52	1.17	1000
Risk tolerance	4.09	0.90	1404	3.40	0.91	1000

Note: Numeracy is the average between the following two numeracy questions (dummy = 1 if answered correctly) Q1) If one bottle of milk costs 2.480 and you give 2.500, how much change do you receive? and Q2) If 5 bottles cost 10.400, how much does one cost? Preference measures rescaled to 1–5 scale for comparability, with higher numbers indicating higher patience and lower risk aversion. Productivity relative to "best in village" is the farmer's total production and maximum farm size relative to what they think the best farmer in the village could produce/farm. Farmer's total production relative to best farmer is winsorized at the 99th percentile due to an extreme value. Household income from agriculture is the total production per season from all plots owned by household. USD purchase power parity (PPP) was 1085.85 at the end of 2019 (source: NASDAQ Data Link). Comparison demographic data is from the Living Standards Measurement Study - Integrated Surveys on Agriculture 2019 - 2020 (LSMS-ISA) and the sample is restricted to respondents aged 18 and older whose main income comes from agriculture, and cultivates one or more plots. Statistics are weighed by household. Time and risk preferences are from a nationally representative sample of Uganda, and are sourced from the Global Preference Survey (GPS). We thank Armin Falk and Markus Antony for sharing the GPS summary statistics needed for this comparison.

Table B2: Comparison of Efficiency Across Weeks and Week-by-week comparison of the Simple and Complex treatments

		Decomposition		
	(1) Efficiency	(2) Consolidation	(3) Sorting	(4) Avoided exposure loss
Panel A				
Week 2	-0.223	-0.037	0.040	-0.226
	(0.032)	(0.013)	(0.016)	(0.028)
p-value Week 2: coef = 0	0.000	0.004	0.013	0.000
Control mean	0.342	0.324	0.107	-0.089
Observations	136	136	136	136
Panel B				
Simple × Week 1	0.072	0.039	-0.028	0.061
	(0.043)	(0.019)	(0.019)	(0.035)
Simple × Week 2	0.016	0.005	-0.008	0.018
	(0.044)	(0.023)	(0.017)	(0.038)
Simple × Centralization	0.030	0.011	0.003	0.015
	(0.032)	(0.015)	(0.017)	(0.019)
p-value simple × Week 1: coef = 0	0.095	0.043	0.141	0.086
p-value simple × Week 2: coef = 0	0.717	0.810	0.647	0.629
p-value simple × Centralization: coef = 0	0.357	0.465	0.845	0.427
Control mean	0.208	0.294	0.136	-0.222
Observations	204	204	204	204

Note: Control group in Panel A: week 1. Control group in Panel B: complex maps. Column (1) shows absolute efficiency. Columns (2), (3) and (4) show the decomposition of efficiency in consolidation, sorting and exposure respectively. Panel A uses data from week 1 and week 2 (no Centralization treatment). Panel B uses data from week 1, week 2, and the Centralization treatment. Regressions in panel A control for village fixed effects. Regressions in panel B control for the centralization treatment and village pair, field team and week2 × map fixed effects. Standard errors clustered by village in parentheses.

Table B3: Alternative Consolidation, Sorting, and Efficiency Measures

	Unadjusted			Adjusted
	(1) Efficiency	(2) Consolidation	(3) Sorting	(4) Efficiency
Panel A				
Simple	0.044	0.021	0.023	0.004
	(0.031)	(0.012)	(0.026)	(0.021)
p-value simple: coef = 0	0.163	0.098	0.379	0.832
p-value simple: coef ≤ 0	0.082	0.049	0.189	0.416
Control mean	0.208	0.286	-0.078	0.430
Observations	136	136	136	136
Panel B				
Centralization	0.342	0.071	0.270	0.054
	(0.027)	(0.009)	(0.024)	(0.011)
Simple × Centralization	0.013	0.011	0.002	0.017
	(0.038)	(0.016)	(0.033)	(0.020)
p-value Centralization: coef = 0	0.000	0.000	0.000	0.000
p-value Centralization: coef ≤ 0	0.000	0.000	0.000	0.000
p-value simple × Centralization: coef = 0	0.725	0.477	0.946	0.405
p-value simple × Centralization: coef ≥ 0	0.637	0.762	0.527	0.797
Control mean	0.111	0.272	-0.161	0.435
Observations	136	136	136	136

Note: Control group in Panel A: complex maps and in Panel B: complex maps and week 2. Column (1) shows absolute efficiency. Columns (2) and (3) show the decomposition of efficiency into consolidation and sorting, unadjusted for exposure losses (this means that most avoided exposure losses are counted as sorting gains). Column (4) shows "adjusted" efficiency (efficiency after adding back all exposure losses). Panel A uses data from week 1 and week 2 (no Centralization treatment). Panel B uses data from week 2 only. Regressions in panel A control for village and week2 × map fixed effects. Regressions in panel B control for village fixed effects. Standard errors clustered by village in parentheses.

Table B4: Analysis of high-quality region and alternative count-based measures

	High quality region			Count-based	
	(1) Efficiency	(2) Consolidation	(3) Sorting	(4) Consolidation	(5) Sorting
Panel A					
Simple	-0.010	0.007	-0.026	0.049	-0.005
	(0.028)	(0.005)	(0.025)	(0.024)	(0.019)
p-value simple: coef = 0	0.726	0.124	0.307	0.045	0.788
p-value simple: coef ≤ 0	0.637	0.062	0.846	0.022	0.606
Control mean	0.275	0.081	0.217	0.573	0.116
Observations	136	136	136	136	136
Panel B					
Centralization	0.050	0.017	0.004	0.121	-0.006
	(0.014)	(0.003)	(0.013)	(0.017)	(0.009)
Simple × Centralization	0.024	0.004	0.026	0.008	0.006
	(0.025)	(0.006)	(0.018)	(0.031)	(0.012)
p-value Centralization: coef = 0	0.001	0.000	0.726	0.000	0.528
p-value Centralization: coef ≤ 0	0.000	0.000	0.363	0.000	0.736
p-value simple × Centralization: coef = 0	0.339	0.569	0.161	0.805	0.582
p-value simple × Centralization: coef ≥ 0	0.830	0.716	0.919	0.598	0.709
Control mean	0.296	0.080	0.247	0.551	0.143
Observations	136	136	136	136	136

Note: Control group in Panel A: complex maps, and in Panel B: complex maps and week 2. Columns (1), (2) and (3) show measures of efficiency, consolidation and sorting for the high-quality region of the maps. Columns (4) and (5) show measures of count-based consolidation and count-based sorting for all regions of the maps. Panel A uses data from week 1 and week 2 (no Centralization treatment). Panel B uses data from week 2 only. Regressions in panel A control for village and week2 × map fixed effects. Regressions in panel B control for village fixed effects. Standard errors clustered by village in parentheses.

C Appendix C: Implementation Details for Experiment 2

This appendix contains additional implementation details for Experiment 2. Section C.1 describes the payoffs, variation in starting maps, and procedure for randomizing map orders. Section C.2 describes how we assigned treatments to sessions. Finally, C.3 describes the algorithms and provides details of the computer interfaces used in the exchange.

C.1 Payoffs and Maps

Payoffs: Each of our games consisted of 6 players and a map. Each player was assigned a numeric ability type. This ability type was private information and farmers were asked not to share it with others.

As with the first experiment, we considered an environment where land was fragmented and where additional gains could be achieved through sorting. Land was again divided into three quality regions with high-quality land being twice as valuable as low-quality land and medium quality land 1.5 times as valuable. Farmers were also divided into three farmer types: high ability, medium ability, and low quality. In all sessions there were two of each type of farmer and medium-ability and high-ability farmers were 50% and 100% more productive than low-ability farmers. Participant earnings were calculated based on their type-specific value for their two highest-quality pieces of land, plus an adjacency bonus if their two highest-value land holdings were adjacent.

As seen in Table C1, the return to a given piece of land was the product of the farmer's ability and the land type. Adjacency bonuses were set at 40% of the value of a single piece of land for the farmer and therefore scaled with both the quality of the land and the ability of the farmer.

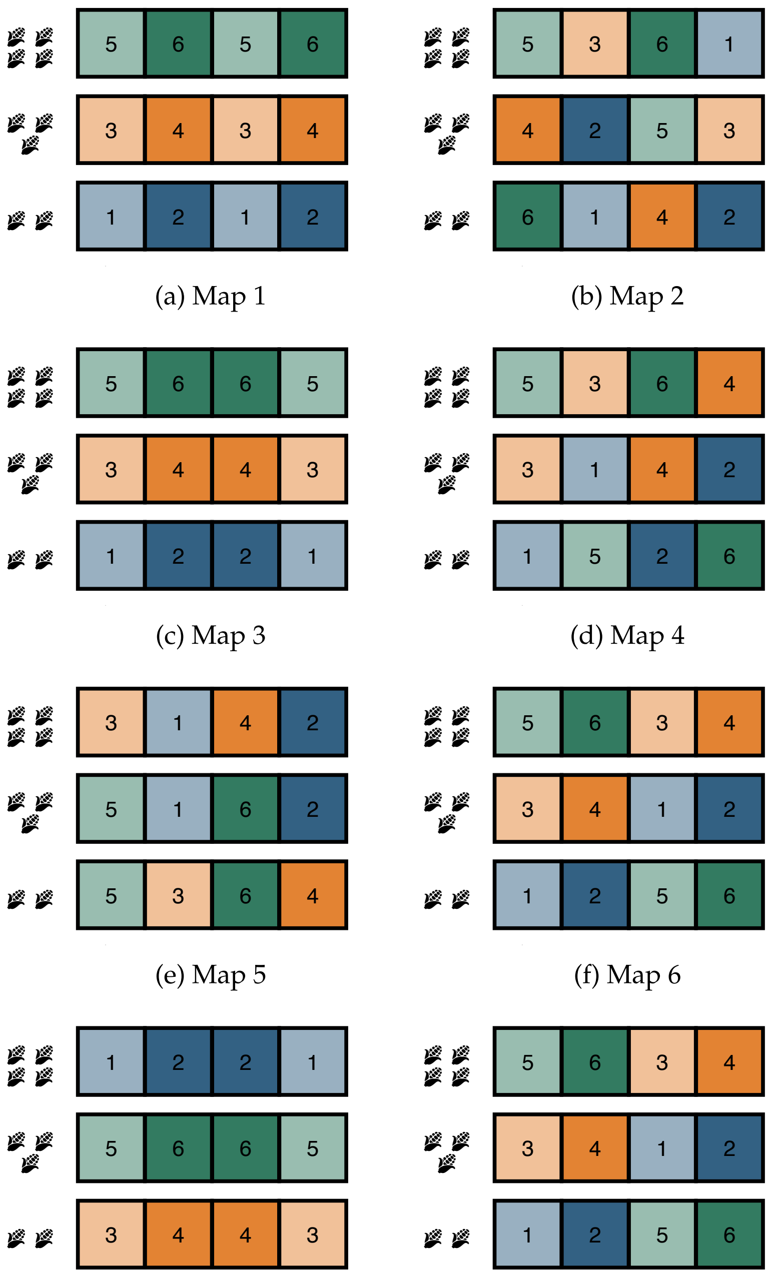
	Panel A: Land values			Panel B: Adjacency bonuses		
	High Quality	Medium Quality	Low Quality	High Quality	Medium Quality	Low Quality
High Ability	400	300	200	160	120	80
Medium Ability	300	225	150	120	90	60
Low Ability	200	150	100	80	60	40

Table C1: Land and Farmer Types in Experiment 2

Maps: We conjectured that the the initial allocation of plots would affect the ease of achieving defragmentation and efficient sorting. To study this issue, we created eight different initial land allocations, which are shown in Figure C1. In each diagram, players 1 & 2 are low, 3 & 4 are medium and 5 & 6 are high ability types.

The allocations are ordered according to our pre-experimental assessment of how difficult it would be to reach full efficiency. We considered four dimensions of difficulty. First, for each player, we determined how many *Package-1* trades were necessary to get to their efficient allocation.[39] Second, we considered how many farmers would need to be involved in any efficient *Package-2* trade. Third, we considered whether money was required to reach an efficient outcome. Finally, we considered strategic issues, for example the extent to which one farmer could holdup another farmer.

[39]Note that if an allocation required two *Package-1* trades, it required only one *Package-2* trade. If an allocation requires four *Package-1* trades, it requires two *Package-2* trades or one *Package-4* trade.



(a) Map 1 (b) Map 2

(c) Map 3 (d) Map 4

(e) Map 5 (f) Map 6

(g) Map 7 (h) Map 8

Figure C1: Initial Land Allocations, Experiment 2

C.2 Treatment assignment

We played 48 sessions in total. Each session consisted of 8 auctions, and was assigned to one trading mechanism: *Package-1*, *Package-2*, or *Package-4*. In each session, the first four auctions had the same cash treatment and the second four auctions had the alternative cash treatment. Hence, each session could be assigned to one of six possible treatments that varied by mechanism and the ordering of the cash treatments. These treatments were block randomized. The set of 48 sessions was divided into 8 blocks, each consisting of 6 consecutive sessions. Each of the 6 treatments was then randomly assigned to one of the sessions within each block.

Each lab session required one lead enumerator to introduce the environment and implement the computer programs, 6 bidding assistants, and one broker. Two labs (labeled red and black) ran in parallel, each playing one session in the morning and one in the afternoon. Lead enumerators were assigned to a specific lab (red or black) and stayed in that lab throughout. Bidding assistants were randomly assigned to a specific farmer and lab (e.g. farmer 4 red) on a session by session basis. Brokers were also randomly assigned on a session by session basis.

Because subjects arrived slowly over time (it was hard to get farmers to all arrive at 9am), the first session of the day alternated between the red and black lab. The first 6 farmers to arrive were randomly assigned to a player number between 1 and 6, and then played in the lab that was operating the first session. The next six farmers to arrive were similarly assigned a player number, and played in the second lab. Each farmer played four auctions as their initial player number, and was then moved to a different player number. This was done such that every subject had an equal chance of being assigned to play one of the six possible sequences $\{HM; HL; MH; ML; LH; LM\}$.

Finally, the 8 maps displayed in Figure C1 were assigned to sessions. Every session played every map, and they were played in one of 8 orders. These orders were devised to minimize ordering effects: we wanted to have difficulty approximately even across the session to minimize the impact of learning. To assign orders to sessions, we first randomly permuted the 8 map orders as shown in Table C2. We then assigned map orders 1 to 6 to the sessions in block 1 (in order), orders 2 to 7 to block 2 (in order), etc.

Order 1	5	1	3	7	6	2	4	8
Order 2	7	3	1	5	8	4	2	6
Order 3	6	2	4	8	5	1	3	7
Order 4	8	4	2	6	7	3	1	5
Order 5	3	7	5	1	4	8	6	2
Order 6	1	5	7	3	2	6	8	4
Order 7	4	8	6	2	3	7	5	1
Order 8	2	6	8	4	1	5	7	3

Table C2: Map Orders

Overall, this method gives assignment to the main auction and cash treatments that are orthogonal to the other elements of the design, as well as maps that are assigned orthogonally to the treatments and also randomly across time and session. We also have balance across all main elements of the experimental design.

C.3 Algorithms and Interfaces

C.3.1 The Winner Determination and Surplus Division Algorithms

Winner determination and surplus division are as outlined in Goeree and Lindsay (2019) with some modifications to impose XOR bidding. Let the set of farmers, $\mathbb{F}$, be indexed by $i \in \{1, \ldots, 6\}$ and the set of plots, $\mathbb{L}$, be indexed by $l \in \{1, \ldots, 12\}$. Farmers submit orders $o = (m, x)$ consisting of the minimum amount of money they must receive, m, and a vector of demanded plots, $x \in \{-1, 0, 1\}^{12}$. A negative number indicates that a farmer is offering money or trying to sell a plot, while a positive number indicates that a farmer must receive money, or wants to buy a plot. For instance, an order $(-500, \langle 1, 0, ..., 0 \rangle)$ indicates that a farmer is willing to pay up to 500 points to acquire plot 1, while an order $(0, \langle 1, -1, 0, ..., 0 \rangle)$ implies that the farmer is willing to buy plot 1 and sell plot 2, as long as he pays no money.

Orders placed by a farmer must be *legal*. Denote the plots owned by farmer i at time t as $\omega_i^t \in \{0, 1\}^{12}$ and denote the cash of farmer i at time t as c_i^t. A bid (m, x) is legal if at the time of placing the order, $c_i^t + m \geq 0$ and $\omega_i^t + x$ contains only zeros and ones. A bid is thus legal if the farmer has more cash than the amount of money he offers, he sells only land that he owns, and he buys only land that he does not own. Orders placed by a farmer are also restricted by the mechanism used in each treatment, as outlined above.

Legal orders are sent to the order book in the order that they arrive, and transactions occur any time there exists a set of legal orders where: (i) supply equals or exceeds demand for all plots; (ii) only a single order is used for each farmer; and (iii) the total amount of money offered is not positive. Formally, let $\mathbb{O}^t$ denote the legal orders in the order book at time t, and index its elements $o_j = (m_j, x_j)$, by $j = \{1, \ldots, |\mathbb{O}^t|\}$. Let $d = \{0, 1\}^{|\mathbb{O}^t|}$ be a vector of orders from the order book, where $d_j = 1$ if an order j is winning and $d_j = 0$ otherwise. Let $\mathbb{O}_i^t$ be the active orders of farmer i and let $\mathbb{W}_i = \{o_j \in \mathbb{O}_i^t | d_j = 1\}$ be the orders of farmer i that are winning. At each time t we find:

$$V^* \equiv \max_d \sum_j -m_j d_j$$

subject to

$$\sum_j x_j^l d_j \leq 0 \qquad \forall l \in \mathbb{L}, \quad \text{and}$$
$$|\mathbb{W}_i| \leq 1 \qquad \forall i \in \mathbb{F}.$$

Trade is triggered if $V^* \geq 0$.[40]

When a transaction is triggered, we return plots that were not demanded to their original owners, and transfer all other plots according to the set of winning orders. If there is a positive surplus (i.e., $V^* > 0$), we divide the remaining surplus amongst the winning farmers as follows: let $\mathbb{W} = \{o_j \in \mathbb{O}^t | d_j = 1\}$ be the set of winning orders and $\widehat{\mathbb{W}} = \{o_j \in \mathbb{O}^t | o_j \in \mathbb{O}_i^t, |\mathbb{W}_i| = 1\}$ be the set of all orders made by the winning farmers. Likewise, denote the set of orders made by non-winners by $\mathbb{NW} = \mathbb{O}^t \setminus \widehat{\mathbb{W}}$. Let $p \in \{0, \ldots, 10000\}^{12}$ be a vector of (integer) prices, and denote the surplus generated by order j at prices p as $s_j(p) = -m_j - p \cdot x_j$.[41] As is standard in these

[40] Note that the restriction to legal trades ensures that there is no short selling, and that all budget constraints are met. We handle these on the client side to minimize the computation time required to solve the winner allocation problem, and to make farmers aware of attempted bids that could not be exercised. Relative to Goeree and Lindsay (2019), the additional cardinality constraint prevents more than one order from a farmer being used in each transaction. This constraint ensures that orders submitted by each farmer are considered XOR. Further, we only use the bids of non-winners to set prices, while Goeree and Lindsay (2019) use all non-winning bids. This change avoids a situation that can arise in our setting, where bidders impose revealed preference constraints on themselves, and reduce their own surplus.

[41] We use integer prices in the experiment in the range of 1 and 10000 so that trade prices are similar to ones that farmers are likely to encounter when trading in Kenya Shillings on a day-to-day basis.

problems, we find the set of prices that lexicographically maximizes the minimum surplus of winning farmers, subject to the revealed preference constraints of the losing orders.[42] The revealed preference constraints ensure that a losing farmer would not prefer to have won once the surplus is reallocated given the information that was submitted to the market. Finding these prices is equivalent to solving:

$$\min_p \sum_j d_j \left(s_j(p) - \frac{V^*}{|\mathbb{W}|} \right)^2$$

subject to:

$$\begin{aligned} s_j(p) &\geq 0 \qquad \forall o_j \in \mathbb{W}, \\ s_j(p) &\leq 0 \qquad \forall o_j \in \mathbb{NW}, \quad \text{and} \\ \sum_j d_j s_j(p) &= V^*. \end{aligned}$$

Each winner pays or receives $p \cdot x_j$ and losing farmers pay and receive nothing. In the case of ties, we use the first solution found by the solver.[43]

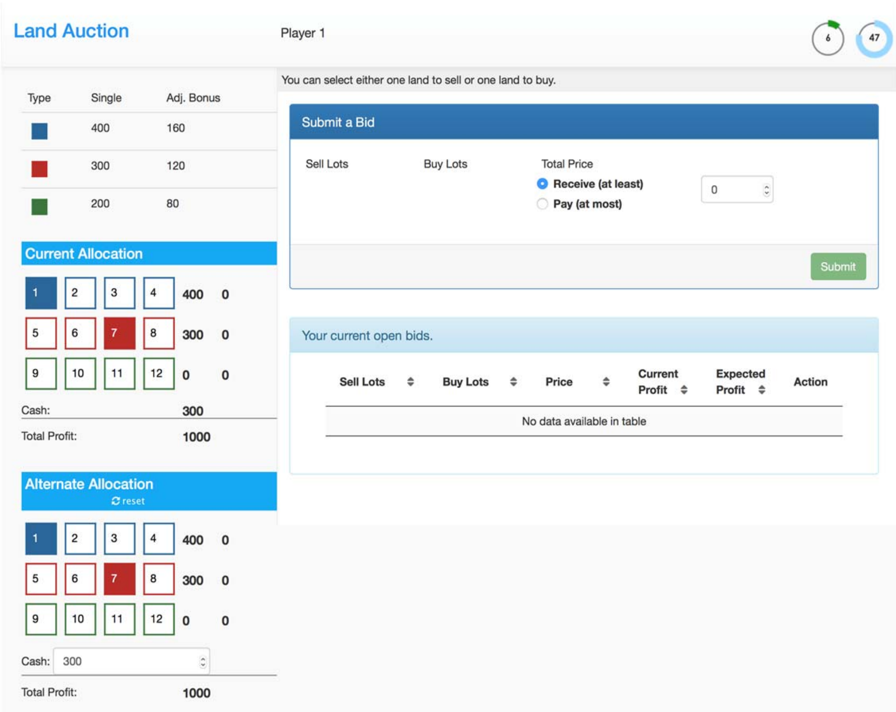
As can be seen in the optimization rule above, lexicographically maximizing the minimum surplus is equivalent to minimizing the squared difference between the surplus of each winner and the equal split subject to an additional constraint that all surplus is allocated. We explain our surplus division rule using this logic. Farmers are told that we try to split the surplus as evenly as possible between the farmers but that we want to make sure that farmers who do not trade are not disadvantaged. In training our enumerators we gave two main examples — one where there is a single buy order and a single sell order and where the surplus is divided equally, and one where there are two buy orders and a single sell order and where the non-winning buy order pins down prices.

After a transaction is triggered, all non-winning orders made by farmers in the winning coalition become inactive, and we allow farmers to renew any legal orders if they wish. Orders that are made illegal (for instance, orders that contain sale offers of objects no longer owned) are hidden from a farmer's offer book, but can be renewed if later transactions make them legal. Farmers have the ability to withdraw legal orders at any time.

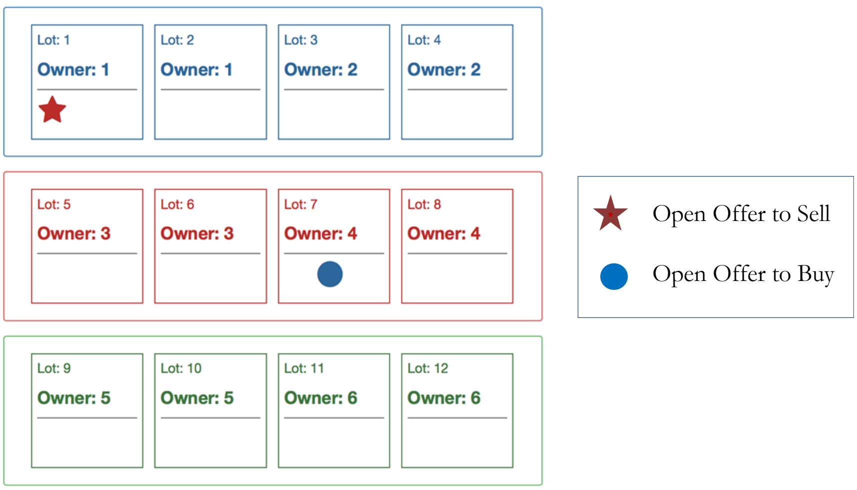
Interfaces All bids were entered through a computer interface. The interface displayed the farmer's valuations and current allocation on a geospatial map as in Panel (a) of Figure C2, and provided a calculator that could be used to determine the value of different allocations. Players (or their bidding assistant) could click on sets of plots on the map (depending on the treatment) and enter a willingness to pay, or willingness to accept to make the trade. Only legal bids were accepted by the computer. The interface also showed a list of all current bids placed by the farmer. In addition to the individual interfaces, a projector showed a map which indicated who owned each plot of land and when a plot of land was offered for sale, or had an offer to purchase. Combinatorial bids showed up on the projected interface as separate components. A screenshot of the individual and projected interfaces is shown in Figure C2.

[42] See Kwasnica et al. (2005) for a broader discussion of revealed preference constraints.

[43] The underlying algorithms were written in Minizinc, a free open-source constraint modelling language, and solved using GECODE (Nethercote et al. 2007; Stuckey et al. 2014). In general, the winner determination problem could be solved in under 200 milliseconds for order books containing under 100 legal orders. The surplus division rule was slightly slower, but usually completed in under 600 milliseconds.



Panel (a): Computer Interface Used by Farmers



Panel (b): Projected Land Market Interface

Figure C2: Computer Interfaces

D Appendix D: Additional Results from Experiment 2

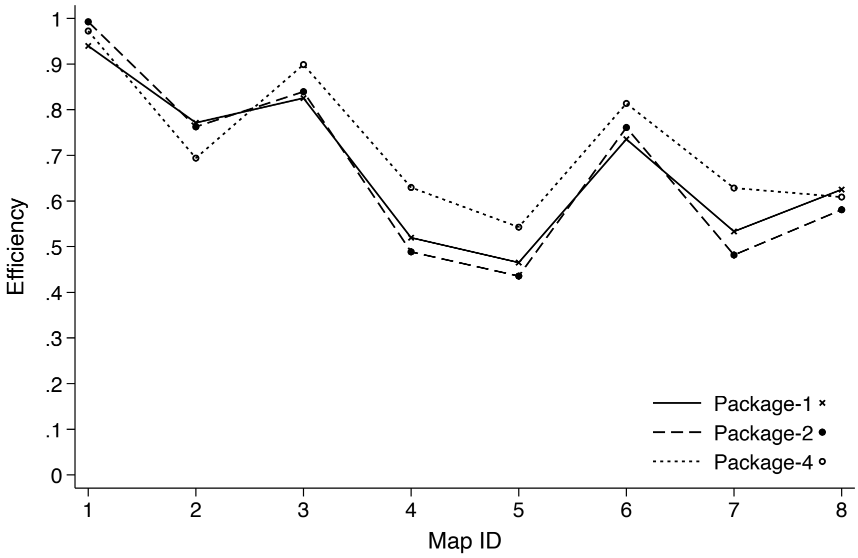
D.1 Full sample results

Our preferred regression specification for this experiment drops the first randomization block of six sessions, due to comprehension issues we encountered in these early sessions. Tables D2 and D3 report the full sample results. The efficiency results are qualitatively similar to our main results but the treatment effects are smaller, due to very low efficiency in block 1. Our inequality findings are qualitatively and quantitatively similar to the main results.

D.2 Efficiency and Initial Land Allocation

As discussed above, we conjectured that the ability to achieve full efficiency would depend on the initial allocation of plots, and we tentatively ranked our 8 initial allocations in order of perceived difficulty. Figure D1 shows that efficiency gains depend on the initial allocation of plots, but are not monotonically decreasing in our pre-experimental assessment of difficulty.

We ranked maps by a conjecture on whether or not *full* efficiency would be reached. As shown above, however, full efficiency was rarely reached, and so ease of reaching partial efficiency was more important. For example, on the basis of full efficiency, we believed that Map 8 was very hard, and Map 5 less difficult. Inspection of Figure D1, however, implies that this was not the case. One possible explanation is that for map 5, consolidation (and efficiency) requires a *Package-2* chain with three people involved. On the other hand, while full efficiency in Map 8 requires a *Package-2* chain with at least 4 people, consolidation requires only a *Package-2* chain with 2 players. Thus 8 is easy to consolidate and hard to improve sorting, but 5 is hard to consolidate. Because our auctions mostly reduced consoldiation, Map 8 turned out to be easier than Map 5.



F-test for no difference by map: $F(7,45) = 27.93$, $p < 0.01$.

Figure D1: Mean efficiency by map and treatment

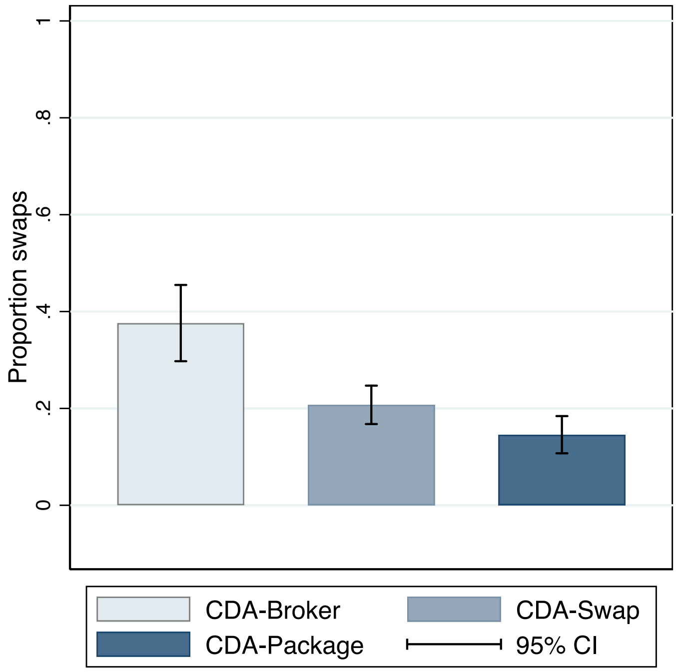


Figure D2: Proportion of Brokered Trades in Each Treatment

Table D1: Summary Statistics: Experiment 2

	Our sample			Kenya		
Demographics	mean	S.D.	obs	mean	S.D.	obs
Age	42.65	10.45	263	38.73	16.61	51535
Female	0.58		264	0.52		51535
Married	0.77		264	0.63		51535
Nr of people in household	4.06	1.71	264	4.31	2.48	23785
Education						
Education (years)	9.75	2.94	264	8.01	4.23	51416
Land tenure						
Owns two or more plots	0.22		264			
Total land ownership in acres	1.01	1.52	237	2.56	3.79	23230
Land trade						
Fraction of plots with joint ownership	0.61		303			
Fraction of plots that are far from home	0.24		303			
Fraction of plots with a title	0.64		303			
Fraction who bought a plot (last 12 months)	0.05		264			
If has bought land: How many acres	0.83	1.42	11			
Fraction who sold a plot (last 12 months)	0.02		264			
If has sold land: How many acres	7.62	11.80	4			
Fraction of sales due to emergencies	0.40		5			
Consolidation						
How important is it to have all your plots together? (1–10, 1 is better to have spread out)						
1	0.43		264			
2 – 9	0.08		264			
10	0.47		264			
Why?						
Why fragment? Less risky	0.25		264			
Why consolidate? More productive	0.38		264			
Preferences (1–5)					GPS	
Risk tolerance	3.95	1.42	264	3.49	0.93	998

Responses from the DHS survey reported for individuals aged 18 and older, and that own land suitable for agriculture. Reponses for the GPS survey are from a representative survey across all of Kenya.

Table D2: Efficiency in Experiment 2, including block 1

		Decomposition		
	(1) Efficiency	(2) Consolidation	(3) Sorting	(4) Avoided exposure loss
Panel A: Comparison of mechanisms				
Package-2	-0.002	0.002	0.004	-0.008
	(0.037)	(0.018)	(0.011)	(0.018)
as % of first best		*0.003*	*0.013*	
Package-4	0.042	0.008	0.024*	0.011
	(0.031)	(0.015)	(0.013)	(0.015)
as % of first best		*0.011*	*0.090*	
FDR q-value: Package-2		[1.000]	[1.000]	[1.000]
FDR q-value: Package-4		[0.693]	[0.255]	[0.693]
Control mean	0.677	0.614	0.114	-0.051
Control mean: % of first best		*0.838*	*0.428*	
Observations	366	366	366	366
Panel B: Credit constraints				
Package-2	-0.016	-0.013	0.018	-0.021
	(0.048)	(0.024)	(0.017)	(0.026)
Package-4	0.061	0.016	0.033	0.012
	(0.044)	(0.019)	(0.020)	(0.020)
Low cash	-0.001	-0.019	0.008	0.011
	(0.043)	(0.025)	(0.020)	(0.020)
Package-2 × low cash	0.027	0.030	-0.029	0.026
	(0.050)	(0.029)	(0.028)	(0.027)
Package-4 × low cash	-0.038	-0.017	-0.019	-0.002
	(0.058)	(0.034)	(0.027)	(0.027)
FDR q-value: Package-2		[1.000]	[1.000]	[1.000]
FDR q-value: Package-4		[0.603]	[0.480]	[0.603]
Control mean	0.678	0.624	0.110	-0.056
Observations	366	366	366	366

Note: *** denotes significance at the 1% level, ** 5% level and * 10% level. % of first best shows coefficient as share of potential gains within each category. First best: consolidation = 0.733, and sorting = 0.357. Control group: Package-1. Columns (2), (3) and (4) show the decomposition of efficiency in consolidation, sorting and avoided exposure loss respectively. Regressions in columns (1), (2) and (4) use data from all maps, sessions and randomization blocks 1-8. All columns control for low cash dummy and auction number, map and randomization block fixed effects. Standard errors clustered by session in parentheses.

Table D3: Inequality in Experiment 2, including block 1

	Atkinson Index (log utility)		
	(1) High cash	(2) Low cash	(3) High & Low
Package-2	0.0010	-0.0022**	0.0010
	(0.0006)	(0.0011)	(0.0006)
Package-4	0.0001	-0.0017*	0.0001
	(0.0006)	(0.0010)	(0.0006)
Package-2 × low cash			-0.0032***
			(0.0010)
Package-4 × low cash			-0.0018*
			(0.0010)
p-value Package-2 = 0	0.133	0.042	0.134
p-value Package-4 = 0	0.862	0.099	0.862
p-value Package-2 × low cash = 0			0.002
p-value Package-4 × low cash = 0			0.068
Control mean	0.012	0.035	0.024
Observations	183	183	366

Note: *** denotes significance at the 1% level, ** 5% level and * 10% level. The control mean in columns (1) and (2) are not directly comparable because the Atkinson inequality index is not invariant to additive changes in total wealth. Control group: Package-1. Regressions in column (1) use data from high cash group, in column (2) use data from low cash group and in column (3) use data from both high and low cash groups. All regressions control for randomization block, auction and map fixed effects, and in column (3) adds interactions of each of the fixed effects with a low cash dummy. Standard errors clustered by session in parentheses.