Complexity and Land Trade: Experimental Evidence from Uganda

Preanalysis Plan

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This pre-analysis plan was written and posted prior to the analysis of any data.

1 Research Question

We use a a land-trading game, played with farmers in rural Uganda, to ask two core questions:

- What is the effect of complexity on the *efficiency* of outcomes.
- What is the effect of complexity on the *distribution* of outcomes.

In a nutshell, the game invites participants to trade "plots" on a map. Participants begin the game with three such plots, and only earn returns on their best three ("span of control"), so at the first best they also end the game with three plots. Figure 2 shows two example initial allocations.

Gains from trade are achieved through *sorting* (player type is complementary to land type) and *consolidation* (spatially adjacent plots earn a "consolidation bonus"). The first best is achieved when all plots are consolidated into contiguous three-plot "farms", with player types assortatively matched to land quality – high types in the high-quality region, medium types in the medium-quality region, and low types in the low-quality region.

We use a precise notion of complexity which derives from the map structure. The map in Figure 2(b) is "simple," because there are a very large number of equivalent ways to reallocate plots among players that achieve the first best. The map in Figure 2(a) is "complex" because it has many "holes" – non-traded plots – such that there are only a small number of ways to pack the map with consolidated three-plot farms.

To mimic realistic status-quo trading conditions, trade is *free-form* and *decentralized*. By free-form, we mean that transactions are bargained over by the participants among themselves in whatever way they like without a tailored market mechanism. By decentralized, we mean that participants take the game home with them, and then play it among themselves over the course of a week, in the village, rather than the trade taking place in an organized experimental session.

We additionally test a simple intervention, which we call "trading day," in which participants are given additional time to trade in a session where everyone is present simultaneously.

Our ex-ante hypothesis is that in our complex maps, it will be more difficult for individuals to trade to an efficient solution without a tailored market mechanism. We believe that the complex map is more difficult to efficiently trade land for five reasons.

Holes increase the information rents required to elicit true preferences in an incentive compatible way: The mechanism design literature has identified private information about one's own valuations as a transaction cost that can prevent efficient trade. One 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 VCG 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.

In our setting, the deficit that results from the VCG mechanism being run on the complex map is always larger than the deficit that results from the VCG mechanism being run on the simple map. The intuition for this result is due to the consolidation bonuses. 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 consolidation bonuses of others are almost never impacted by one individual being excluded from the mechanism. 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 efficiently 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.

Holes increase the potential for hold-out problems to arise: A hold-out problem occurs when an initial purchase of land gives a potential trade-partner of a different piece of land bargaining power in a future negotiation. In our game, this will typically occur when an individual has consolidated two pieces of land and is trying to add a third piece of land that is adjacent to the original ones.

In our simple map, there are many ways to add a third piece of land to two consolidated pieces. Thus the purchasing party is likely to be able to negotiate against many potential sellers to ensure a low purchase price. In our complex map, however, assembled land may have only one or two adjacent plots and seller bargaining power may be high. Hold-out is predicted to prevent welfare-improving trades in some cases and is predicted to generate inequality in others.

Holes can create packing problems: In the complex map, 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. We conjecture that this may make it hard for individuals to consolidate land once others have begun to do so.

Holes increase exposure: We will define exposure as a situation where an initial purchase of land gives a potential purchaser of this same plot of land bargaining power in the future. Exposure occurs if our setting due to the span of control constraints: individuals have no value for their fourth piece of land and thus counter-parties are in a strong bargaining position when offering to purchase these plots. As with hold-out, exposure is likely to exist in the complex map since each plot has a smaller number of adjacent ones.

Holes may increase inequality: In the complex maps, some pieces of land are important for ensuring that all players can consolidate all their land. In cooperative bargaining, we would predict that individuals with such pieces of land should be able to extract more of the total surplus. We conjecture that this will increase distributional inequality for a given map and that this may make it more difficult for individuals to cooperate. More generally, complexity may increase the ability of sophisticated bargainers to better exploit their position and earn a greater share of the surplus, at the expense of less-sophisticated bargainers.

We note that consolidation is influenced by all five of these channels while sorting is likely to be influenced primarily by issues of exposure and inequality. We therefore study overall efficiency and consolidation as primary outcomes, as well as decomposing efficiency fully into consolidation, sorting, and "exposure," described below.

We expect that the trading day, which brings participants together to trade in a centralized session, will partially alleviate these issues, and therefore close the gap between the simple and complex treatments.

2 The game

The primary data for our study come from groups of 18 participants playing a "land trading game." Each player in the game is assigned an initial allocation of artificial land titles and game currency, and the group is given time to trade among themselves, bargaining over prices in whatever way they choose. Players earn financial rewards according to the gains from trade realized in the game.

The game setup is as follows. Each group is assigned a map of plots on a grid. The grid has 72 plots, of which 18 are blank "non-traded" plots and 54 are assigned as initially owned by one of the players. The basic grid is shown in Figure 1. Plots 1–24 are "low quality," 25–48 "medium," and 49–72 "high." Each player initially owns three plots and an endowment of game currency.[1] Non-traded plots are never for sale and represent obstructions in the landscape or owners who are unwilling or unavailable to trade at any price. In addition to land quality heterogeneity, players are heterogeneous in pre-assigned "ability."

65	66	67	68	69	70	71	72
57	58	59	60	61	62	63	64
49	50	51	52	53	54	55	56
41	42	43	44	45	46	47	48
33	34	35	36	37	38	39	40
25	26	27	28	29	30	31	32
17	18	19	20	21	22	23	24
9	10	11	12	13	14	15	16
1	2	3	4	5	6	7	8

Figure 1: Initial grid, with plot IDs

The payoff structure has three key properties:

1. Ability-quality complementarities: the return to a given piece of land is the product of the player ability and the land quality. The land quality types are Low, Medium, High: $\{2, 3, 4\}$. The player ability types are 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 earn an "adjacency bonus" when two of their plots

[1] Initial allocations are generated following the scheme detailed in Appendix A. We provide a summary description below.

share a border, and two bonuses when three plots share two borders (either in a vertical or horizontal strip or an"L"-shaped unit). The adjacency bonus is 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 has an adjacency bonus worth $1 \times 4 \times 0.1 = 0.4$. To limit the number of payoff parameters that participants must keep track of, the adjacent bonus is independent of land quality. Adjacency bonuses can only be earned within a land quality region.

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

Thus, each player has four key payoff parameters to keep track of: their value for each type of land, and their adjacency bonus. Land values are in fixed proportions $\{100\%, 150\%, 200\%\}$ and the maps and plot titles use two, three, or four icons representing heads of maize as a visual cue. See figure 3.

Under these assumptions the efficient allocation is simple to compute: each player should hold three adjacent plots, positively sorted by quality-ability type. The payoff parameters were calibrated such that the gains from trade were divided approximately 50–50 between sorting and consolidation, in the absence of good empirical evidence on which to tune this calibration.

In the game, all payoffs are multiplied by 20,000 and expressed in terms of "game shillings." So the minimum land value is (Low type, Low quality) $0.8 \times 2 \times 20{,}000 = 32{,}000$ and the maximum is (High type, High quality) $2.2 \times 4 \times 20{,}000 = 176{,}000$. The minimum adjacency bonus is (Low type) $0.8 \times 4 \times 0.1 \times 20{,}000 = 6{,}400$ and the maximum is (High type) $2.2 \times 4 \times 0.1 \times 20{,}000 = 17{,}600$.

Each player begins each game with 240,000 units of game currency in printed paper bills that can be used for exchange.

We also assign each player an initial "debt," to be deducted from their final payoff when computing earnings from the games. The debt levels are calculated such that each person begins 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) are calculated as:

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

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

The primary role of debt is to calibrate incentives in the game. We want the gains from trade (in relative and absolute local currency terms) to be sufficiently large that participants pay attention and participate fully in the games. Final earnings depend on initial assets and gains from trade. For a given average payoff, subtracting debt from initial assets increases the contribution of gains and therefore sharpens incentives. Our debt calibration implies that gains from trade at the first best are 50–60% of initial net assets on average.[2]

There are two kinds of map: “simple” and “complex.” Complex maps have non-trading plots scattered throughout, meaning there are only a small number of ways to pack consolidated blocks of three plots such that an efficient allocation is reached. Simple maps have all non-trading plots in the two right-most columns (i.e. all plots below numbers 71 and 72). Our complex maps average between 1.67–5.33 ways to pack six consolidated blocks in each quality region, where one packing is a unique combination of L- and l-shaped consolidated three-plot units. In the simple maps there are 134 such packings.

We generate the simple maps by moving plots so as to retain the initial ownership structure: i.e. each player in a complex map matches exactly their counterpart in the simple map in terms of the quality of their initial plots and the number of initial adjacencies, with minimal changes in the identities of the players owning the surrounding. As a result, the initial payoffs and total gains from trade are identical between the simple and complex form. An example is shown in Figure 2, and Appendix A describes in detail how we generate the maps.

The maps given to participants look slightly different to this. They list both the plot code (in the middle), the ID of the initial owner (at the bottom), and a visual cue for land quality using symbols representing heads of maize (2 icons for quality level 2, 3 for level 3, and 4 for level 4). See Figure 3.

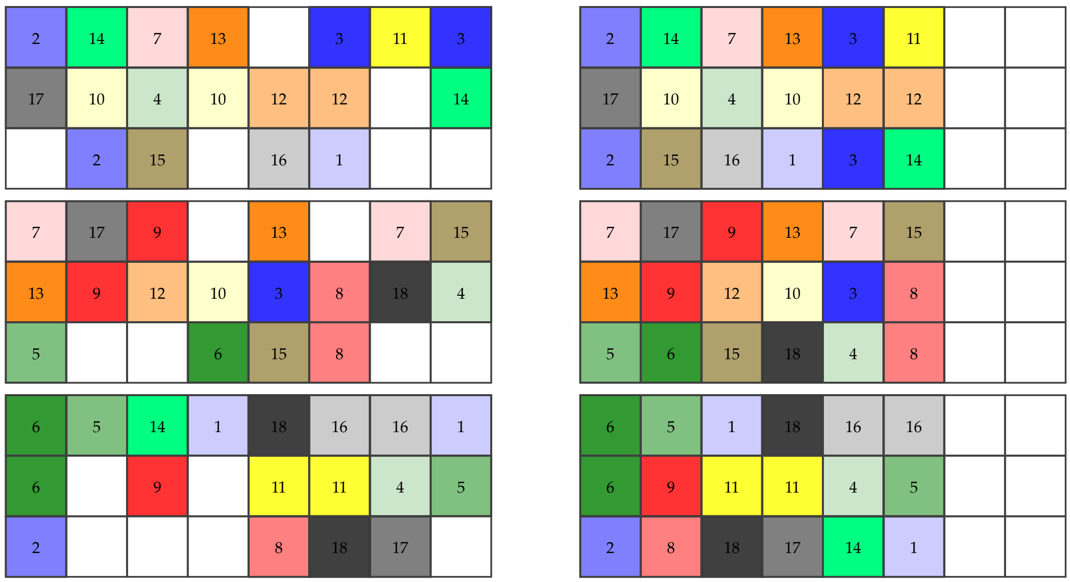
2.1 Game play

In each LC1 (village) we recruit up to 22 individuals to attend three meetings, each separated by 7 days. 18 of the players are designated as the main players, with players 19–22 as reserves, to stand in in case one of the main players does not attend a later meeting. Participants' earnings from the games are paid to them at the end of the week of the relevant session using mobile money.

Meeting 1

At the first meeting, we record basic information about the participants, and train them in the basic mechanics of play. To train the participants, and also to obtain a measure of

[2]Our parameter calibration is such that total gains from trade are around 15% of the value of initial land and adjacency bonuses, or around 8% of land plus cash, so paying participants based purely on their endowment implies relatively little marginal return to trade. An alternative approach, increasing the gains from trade by adjusting parameters is not straightforward as it also increases cash needs (as it takes more cash to buy out other players), offsetting the increase, and generating perhaps arguably implausible differences in land values between players.



(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 (map ID 74) in complex and simple form

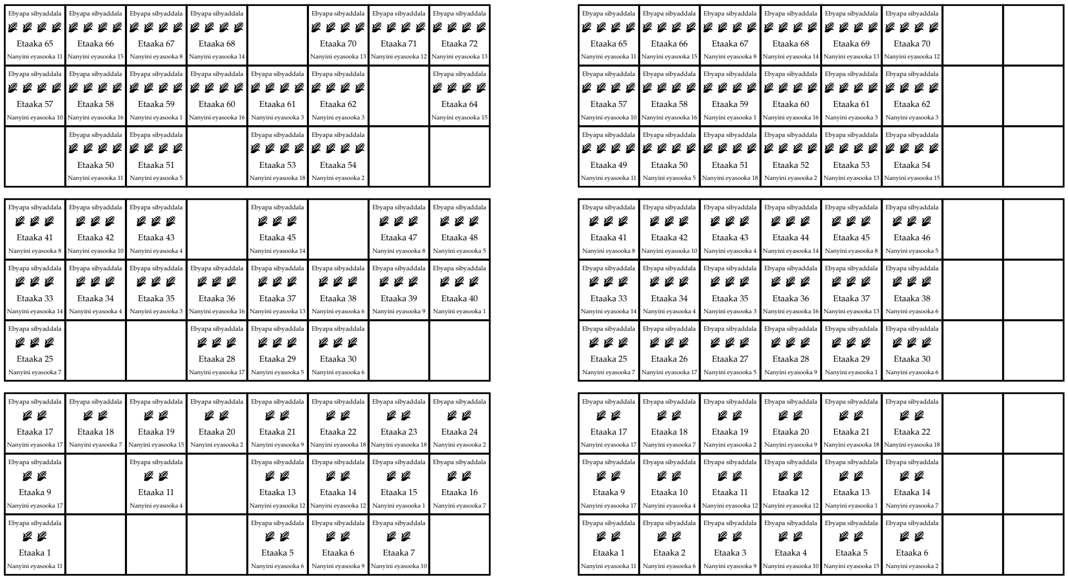
baseline trading efficiency in simple trade, we use two games, "Game 1" and "Game 2." Both games are paid at an exchange rate of 10 game shillings = 1 UGX.

Game 1 is a basic trading game in the spirit of Chamberlin (1948).[3] Participants are assigned a type corresponding to their payoff from owning a land title (holding more than one yields no additional payoff). The set of types is $\{10000, 200000, \ldots, 220000\}$.[4] Everyone receives 240,000 units of game currency and odd-numbered types $\{10000, 30000, \ldots, 210000\}$ begin with a title, 11 titles in total. Players have an initial debt such that initial net assets are 40,000 game shillings for all participants.

Trade is free-form and players are free to buy and sell as many times as they like. Play continues until nobody wishes to trade any more. Efficiency is reached if all titles are owned by the highest-type players – e.g. when there are 22 players the efficient allocation has titles owned by players $\{120000, 130000, \ldots, 220000\}$. Mean net assets at the first best are 70,000 game shillings or 7,000 UGX.

[3] Chamberlin, Edward H. 1948. "An Experimental Imperfect Market." Journal of Political Economy 56 (2): 95–108.

[4] In case of less-than full attendance we drop types 10,000, 220,000, 20,000, 210,000 in turn.



(a) Complex (b) Simple

Note: large numbers are plot IDs, small numbers are random player IDs (uncorrelated with type).

Figure 3: Example map (map ID 74) as seen by players

Game 2 is a more complex trading game in the same spirit. Participants are assigned a type (7 low types, 7 medium, and 8 high), and there are three qualities of land (low, medium, and high). Low types' payoffs are $(40000, 60000, 80000)$ depending on land type. Medium types earn 50% more and high types 100% more.[5] Everyone begins with one title (low, medium, or high, in equal proportions, 22 titles in total), 240,000 units of game currency, and an initial debt such that net assets are 40,000 game shillings for all participants. The game features span of control: players earn payoffs on their best three plots.

Trade is free-form and players are free to buy and sell as many times as they like. Play continues until nobody wishes to trade any more. Efficiency is reached if all titles are owned by the high-type players, with no more than three plots each. Mean net assets at the first best are 67,272 game shillings or 6,727 UGX.

Games 3 and 4 After playing these two games, we dismiss the reserve players, asking them to return the following week in case of no-shows by main participants. We distribute initial allocations for the main game to the 18 main participants. We also publicly show the whole group the map that they will be playing (at the front of the training session) using a clipboard, and explain that it is an abstract representation of a village's land allocation. If the participants are playing a simple map we refer to this as Game 3, if complex we refer to

[5] In case of less-than full attendance we drop first a high, then a low, then a medium, then a high type.

it as Game 4.

The 18 main participants are told they have one week to trade freely among themselves, and that they will be paid according to their profits. Before dismissing them we help them individually to calculate their initial profits, and use some comprehension questions to ensure understanding.

To avoid mixing of materials with the training games, the materials for this game are printed on colored paper.

Meeting 2

At meeting 2 we collect the final land holdings of each player and compute their earnings. We issue them with a new map and a new initial allocation, and give them another week to trade among themselves. If the village played Game 3 (a simple map) in the first week, they are assigned to Game 4 (a complex map) for the second week, and vice-versa.

To avoid mixing of materials with the previous game, the materials for this game are printed on white paper.

In the case of no-shows, we replace the player with a reserve. In piloting, missing players typically sent their final endowments with another player or the LC1 chief, otherwise we recover them by arranging a meeting time with the player.

Also at meeting 2 we conduct the "constraints survey," asking the participants questions about their own experiences of and opinions about land trade. These are to be used for descriptive work.

Meeting 3, trading day

At meeting 3 we collect the final land holdings of each player and compute their earnings so far.

Then, instead of ending the game immediately, we inform them that they have an additional hour in which to conduct additional trades among themselves, now that everybody is in the same place. This is not pre-announced, so we interpret their pre-trading day allocations as the best they could achieve in decentralized trade, and the gains during the trading day as additional gains due to centralization.

At the end of the trading day we collect their final allocations and conduct a short exit survey, asking questions about how they organized trade during the two weeks of play. These data are to be used for exploratory analysis.

Participants are paid based on their final allocation at the end of trading day.

In the case of no-shows, we replace the missing players with reserves. The reserves are issued the same land titles as the missing players. This is easy if the missing players have sent their titles with someone else, and/or if there is only one missing person (in which case we can deduce which titles they had). In the case of more than one person missing whose titles are also not available, we deduce which titles are missing and divide them randomly among an equivalent number of reserves. Reserves are issued the usual starting balance of 240,000 game shillings, for simplicity. They also receive the same initial debt as the player they replace.

Identifying other players

From the beginning of meeting 1 players are assigned a random player ID (from 1–18, uncorrelated with player ability type). These IDs are held constant throughout games 1, 2, 3, and 4 to avoid confusion. Since during trade it may important for participants to be able to find specific other players, they are given a sheet on which they can make a note of the identities of the other players. The village chief is also given such a sheet and participants can consult with the chief if they need help finding another participant. Since the village chief is typically socially connected to most people in the village in some way it is natural for them to play this role, and indeed we observed this emerging organically, unprompted, in piloting.

3 Sample

There are two levels of sampling. First, the selection of villages to visit for our experiment, second, the selection of participants to recruit.

3.1 Villages

We work in 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 drop villages not listed as being in Masaka county, then drop 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 drop 11 villages with zero population. This leaves us with 346 villages.

2. We drop 4 villages with duplicate names, that would be difficult for our field team to identify reliably. This leaves us with 342 villages.

3. We drop villages that are densely populated and have 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, by dropping villages above the 90th centile for population or population density, second by dropping parishes with median village above those thresholds, third, by dropping 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 leaves us with 274 villages (Masaka municipality accounts for 53 of the 68 villages dropped).

4. We also drop 7 villages that were previously visited for piloting. This leaves us with 267 villages.

5. We drop 26 coastal villages (identified by visual inspection) that are expected to be dominated by fishing and other activities rather than agriculture. This leaves us with our final sampling frame of 241 villages.

The 241 villages belong to 31 "parishes" (the next highest administrative unit), which we will use for stratification (see section 4 below).

3.2 Participants

In each selected village we first meet with the village chief, and ask them to give us a list of as many households as they can 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.[6] The chief is also asked to attend the meetings and assist with ensuring that selected participants attend, and is compensated for their time.

We select households randomly from the list and seek the consent of the household head to participate in the experiment. Eligibility criteria are 1) cultivation of some land, 2) reporting that at least 50% of household income is derived from farming, 3) having access to a mobile money account. Criteria 1 and 2 are intended to ensure we sample a relevant

[6] 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.

population that might be interested in real land trade, 3 ensures 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 have been recruited. The first 18 are our intended "primary" participants, and the remaining 4 act as reserves. The reserves are asked to attend each session, and paid show-up fees for doing so. If a primary participant does not attend, they are replaced by a reserve according to the protocol described in section 2.1.

4 Treatment assignment

Each village plays the game twice, once on a simple map (game 3) and once on a complex map (game 4). This section details how the ordering and the specific maps are assigned.

4.1 Possible assignments

As described in Appendix A our map generation procedure yielded 8 maps (internal IDs 28, 69, 74, 93, 130, 148, 149, 193) each of which has a simple and a complex form. We group these 8 maps into 4 matched pairs according to the number of possible efficient packings available in their complex form, which gives us a notion of complexity variation within game 4. Accounting for possible map and complexity orderings this yields 16 possible assignments. These are listed in Table 1.

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 1: Possible treatment assignments

Our field plan involves two field teams working simultaneously five days per week, covering 10 villages per week. We intend to sample 68 villages, leaving two vacant “slots” for replacement villages in case a village decides to withdraw (see section 4.3). The 68 villages constitute four complete blocks of 16 assignments (see 1), plus one randomly selected block of four (either assignments 1–4, 5–8, 9–12 or 13–16).[7]

4.2 Randomization

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

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

4.3 Village attrition

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

1. The field team is unable to locate a sampled village at mobilization time, or the village chooses not to participate. In this case the team moves to another randomly selected village from the same parish.
2. A village chooses to withdraw from the study during the experiment. In this case we will replace the village with a randomly selected village from the same parish. To avoid disrupting the field work, these replacement villages will be visited at the end of the experiment.

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

At the time of writing, one village had chosen to withdraw and will be replaced with another village from the same parish.

5 Outcomes

5.1 Efficiency

As argued in Section 1, we expect complexity to decrease efficiency, in particular by constraining gains from consolidation. This pair is our first class of primary outcomes.

We measure efficiency by the fraction of possible gains from trade realized. Defining "surplus" as the sum of the land valuations and adjacency bonuses of the land owners, we define our primary outcome variable:

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

First-best surplus is the same for all maps and equal to:

$$\begin{aligned}
\text{First-best surplus} = {} & \underbrace{6}_{\text{Players per type}} \times \Bigg[\\
& \underbrace{3}_{\text{Plots}} \times \Bigg(\underbrace{1}_{\text{Mean low type}} \times \underbrace{2}_{\text{Low quality}} + \underbrace{1.5}_{\text{Medium type}} \times \underbrace{3}_{\text{Medium quality}} + \underbrace{2}_{\text{High type}} \times \underbrace{4}_{\text{High quality}} \Bigg) && (1) \\
& + \underbrace{2}_{\text{Adjacencies}} \times \underbrace{0.1 \times 4}_{\text{Scale factor}} \times \Bigg(\underbrace{1}_{\text{Low type}} + \underbrace{1.5}_{\text{Medium type}} + \underbrace{2}_{\text{High type}} \Bigg) \Bigg] && (2) \\
& = 282.6
\end{aligned}$$

expressed in game shillings this is $282.6 * 20000 = 5{,}652{,}000$.

Efficiency equals 0 if no trade occurs, and 1 if the first-best is reached. Negative realizations are possible if trade decreases total surplus.

When computing outcomes post-trading day we take initial surplus from the beginning of that week's trading period, i.e. the initial condition from the map that was assigned. Thus the gains from trade in trading day are expressed in the same units as from the other rounds.

5.1.1 Efficiency Decomposition

The efficiency of trade can be decomposed into three composite parts.

- *Consolidation*, increasing the value of adjacency bonuses obtained.

- *Sorting*, increasing the total value of landholdings by assortatively matching player type and land quality.

- *Exposure,* losses due to players holding too much or too little land at the end of the game.

When performing the decomposition, we scale by the same denominator as our efficiency calculations (First best surplus − Initial surplus). This means that

$$\text{Efficiency} = \text{Sorting} + \text{Consolidation} + \text{Exposure}$$

The game parameters are calibrated such that, at the first best, approximately 50% of the gains are derived from sorting and consolidation, and there are no losses due to exposure.

Consolidation Our primary 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 "bonuses" are the sum of the value of landowners' adjacency bonuses values. First-best adjacency bonuses equal the result of line (2) in the above First-best calculation. We describe the calculation of Exposure bonuses below.

Sorting Our primary measure of gains from sorting is:

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

where "land value" is simply the sum of the landowners' plot values. First-best land value is equal to the result of line (1) in the above First-best calculation. We describe the calculation of Exposure value below.

Exposure The production function in our game has a sharp "span of control" feature, in that players derive zero value from their worst plots when they have more than three. This feature is included in the game for good reason: we want to avoid overwhelming participants with parameters and difficult calculations to perform (such as a smoother concave region), since we want to focus on trade complexity, not "rules of the game" complexity.

If players end the game with more or fewer than three plots, this will affect overall efficiency, and we refer to such losses as "exposure," since they reflect a source of exposure risk (buyers might find themselves unable to sell, and sellers might find themselves unable to buy).

Exposure could arise from two sources.

- Economic reasons, such as the owner failing to sell a plot due to inability to agree a price or find a buyer, reflect the kind of land market inefficiency we seek to capture. Complexity might be expected to increase the likelihood of such outcomes.
- Exposure might alternatively arise due to player miscomprehension of the game (e.g. a player that mistakenly hoards land due to misunderstanding of the rules). We expect that these mistakes will not differ between complexity treatments since they concern a fundamental rule of the game rather than a feature of the map. We include comprehension checks to try to minimize such errors.

We compute exposure losses in as minimal a way as possible, by computing the value and consolidation gains that would have been realized had each affected plot been traded one more time at the end of the game.

1. Identify all "surplus" plots that are earning zero value in the final allocation.
2. Identify all players with surplus capacity (i.e. who end the game with fewer than three plots). Order the surplus "slots" from highest to lowest type.
3. Assign surplus plots to players with surplus capacity to maximize total payoffs. We do not reallocate any other land.
4. Add the generated value to our "sorting" computation, and any additional adjacency bonuses generated to our "adjacency" computation.

We define *Exposure value* as the additional land value generated by this reassignment, and *Exposure bonuses* as the additional adjacency bonuses. We measure the loss due to exposure as:

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

Note that Exposure ≤ 0.

To the extent there are comprehension errors we do not expect them to differ across the simple and complex treatments, so we will not adjust our main efficiency results for span of control losses. We will however report adjusted efficiency for completeness.

5.2 Distribution

Our discussion of inequality in the research question section combines two conceptually distinct reasons why complexity might increase inequality. First, as noted above, complexity potentially increases inequality because it raises the chance that a given individual is pivotal, and so able to extract a large amount of rent. Second, it may be that some players perform poorly in the complex environment, perhaps failing to foresee the exposure problem and making losses. We believe that both of these possibilities are potential reasons for market designers to become particularly interested in complex markets. The first reason is the traditional reason why some people dislike markets - they are believed to create inequality

We propose to take the following approach to understanding the distributional consequences of complexity. As a starting point, we will assume that social welfare can be determined by an additively separable social welfare function

$$W(F) \equiv \int_0^{\bar{y}} u(y) dF(y) \tag{3}$$

where f is the distribution of incomes, and y_i is an individual's total payoff in the experiment (value of land, plus cash, net of debt).

Our design ensures that there are no initial social welfare differences between simple and complex, since all participants begin with the same net assets. Moreover, total potential gains from trade are equal for the simple and complex versions of the same map. Therefore, differences in final social welfare between treatments are straightforward to interpret.

It can be shown (see e.g., Cowell 'Measurement of Inequality') that if u is concave then $W(F) \geq W(G)$ if and only if F (generalized) Lorenz dominates G.

However, as is well known, Lorenz dominance is only a partial order, and so may fail to rank the two distributions. We therefore propose a more parametric approach. First, we will assume that the SWF displays constant relative inequality aversion, and so can be represented by the class:

$$u(y) = A + B \frac{y^{1-\epsilon}}{1-\epsilon}.$$

Given this assumption, we can calculate the Atkinson index for density f as

$$I^A(f) = 1 - \left[\sum_i \left(\frac{y_i}{\mu} \right)^{1-\epsilon} f(y_i) \right]^{\frac{1}{1-\epsilon}}$$

where μ is the mean of the distribution F and f is its density (in our case, the fraction of participants reaching a given y_i).

Second, for our main analysis we will assume that $\epsilon \to 1$ which implies

$$u(y) = \ln(y)$$

and

$$I^A(f) = 1 - \exp\left[\sum_i (\ln y_i - \ln \mu)\, f(y_i)\right]. \tag{4}$$

That is, the Atkinson index in this case is 1 minus the ratio of the geometric and arithmetic means. This approach has several nice features. First, the choice of a log social welfare function is readily interpretable (an intervention that increase log income for any individual by a fixed percent has the same impact on social welfare) and hence may be explainable to and acceptable to a wide audience, second, as Atkinson points out, if $I^A = 0.3$, for example, then it would be able to reach the same social welfare with 70% of the income but equally distributed. Finally, we can use the Atkinson index to decompose the change in social welfare as measured by (3) into a contribution from the increase in overall efficiency and a (negative) contribution that is due to any change in inequality, coming from the Atkinson index.

In our applications we will compute the Atkinson index of players' final assets net of debt. As described above, debt values are set such that net assets are fully equalized at the beginning of the game.

One possible channel through which complexity might affect the distribution of outcomes is by changing the "structural" inequality of the game, that is the inequality that would be expected to arise under efficient trade.

As a benchmark for structural inequality, we will compute players' Shapley values (based on final assets net of debt). Due to the large number of players in the game and the complexity of identifying the total surplus generated in some subgames, we plan to use an approximation for the Shapley value using a random sampling method. Our plan is to use the algorithm developed in Castro et al. (2009)[8] and Campen et al. (2018)[9]. These algorithms take random orders of the players and calculate the marginal contribution of each individual by adding players in order to the allocation problem. By taking the average contribution of each individual over a large number of random orders, it is possible to approximate each player's Shapley value.

In our primary analyses we will focus on the log-utility Atkinson index (4). We will give estimates with and without controlling for the Atkinson index of the Shapley values, which is our measure of structural inequality.

We will also provide a range of estimates for both inequality and social welfare using alternative values of ϵ between 0 (linear utility) and $\epsilon = 10$. This information is for the

[8] Castro, Javier, Daniel Gómez, and Juan Tejada. 2009. 'Polynomial Calculation of the Shapley Value Based on Sampling'. Computers & Operations Research 36 (5): 1726–30.

[9] van Campen, Tjeerd, Herbert Hamers, Bart Husslage, and Roy Lindelauf. 2018. 'A New Approximation Method for the Shapley Value Applied to the WTC 9/11 Terrorist Attack'. Social Network Analysis and Mining 8 (1): 3.

interested reader who has their own pre-specified social welfare function.

We will also report the number of individuals who lose through trade, ending with less than their initial assets.

6 Primary Analyses

Our treatment assignment is designed such that as much residual variation as possible can be absorbed by fixed effects. We expect that the main source of idiosyncratic variation will be at the village level, so we exploit within-village variation in treatment to estimate the effect of the simple versus complex game, while assigning maps at the pair-week level to allow us to absorb both time and map effects.

Village fixed effects additionally absorb differences in implementation between villages and over time (e.g. due to differences between our two field teams, or improvements in how they explain the games).

For within-week ("between village") analysis we cannot control for village effects but we can control for assignment pairs (which are both drawn from the same parish and so geographically similar) and field team.

Define the following variables:

- Y_{it} outcome for village i in trading period $t \in \{1, 2, 3\}$. Period 1 corresponds to trade in the first week, running from meeting 1 to meeting 2. Period 2 covers the second week, running from meeting 2 to meeting 3. Period 3 is the trading day that occurs after period 2 outcomes are recorded.

- $simple_{it}$ equals 1 if village i played the simple game in period t.

- $week2_{it}$ equals 1 if $t \in \{2, 3\}$, zero otherwise. It is a dummy variable for outcomes after the second week of trading.

- $tradingday_{it}$ equals 1 if $t = 3$.

- map_{it}^{m} equals 1 if village i played map $m \in \{1, \ldots, 8\}$ in period t.

- v_i is a dummy variable for village i (village fixed effect).

- $pair_{it}$ is a dummy variable for the pair of villages in i's treatment assignment.

- $fieldteam2_{it}$ equals 1 if the village was visited by field team 2 (there are two field teams in total).

6.1 Efficiency and Consolidation

The primary predictions that we will test are:

1. Efficiency will be lower in the complex game than the simple game.

2. Consolidation will be lower in the complex game than the simple game.

If we see an effect in 1 and/or 2, we predict:

3. Efficiency gains from trading day will be larger in the complex game than the simple game.

4. Consolidation gains from trading day will be larger in the complex game than the simple game.

We will also report the full decomposition of the estimated effects into their sorting, consolidation, and exposure components.

To test hypotheses 1 and 2 we run the following regression, using data only from periods 1 and 2 (i.e. excluding trading day).

$$Y_{it} = \beta_{simple} \times simple_{it} + \sum_{m=1}^{8} \delta_m \times map_{it}^{m} \times week2_{it} + v_i \tag{5}$$

clustering standard errors at the village level. Each village appears twice in this specification, once with a simple map and once with a complex map. In each case it is paired with another village playing the same map but the other complexity treatment. We include village fixed effects. β_{simple} captures the difference in performance between simple and complex, averaged over both trading periods, taking out village level effects and time effects.

Note that we control for $map \times week$ fixed effects, to absorb variation due to learning and due to changes in the map. The pair fixed effects absorb variation due to the first week's map which is constant within pair.

We expect that $\beta_{simple} > 0$ and will test this with one-sided statistical tests. We will additionally report p-values that control the Familywise Error Rate across these two tests.

To test hypotheses 3 and 4 we run the following regression, using only data from periods 2 and 3, (i.e. excluding week 1).

$$Y_{it} = \beta_0 \times tradingday_{it} + \beta_{diff} \times simple_{it} \times tradingday_{it} + v_i \tag{6}$$

clustering standard errors at the village level. Each village appears twice in this specification, once before the trading day and once after. We include village fixed effects. We do not control for the map or the simple treatment since these are fixed within village.

β_{diff} captures the difference in gains from trading day between the simple and complex maps. Under the assumption that no further trading would have taken place during that 1 hour period had we not congregated the players for trading day, we interpret this as the difference in the causal effect of trading day between treatments.

We expect that $\beta_{diff} < 0$ and will test this hypothesis with one-sided test. We will additionally report p-values that control the Familywise Error Rate across these two tests.

As described in Section 7 we will also report results from the same specifications for each decomposed component of efficiency.

6.2 Distribution

We will test how complexity affects the distribution of outcomes, as measured by the Atkinson index described in section 5.2.

We follow the same basic structure as our tests for efficiency, namely we predict:

5. Inequality will be higher in the complex game than the simple game.

To test hypothesis 5 we run the following regressions, using data only from periods 1 and 2 (i.e. excluding trading day).

$$I^A(f_{it}) = \beta_{simple} \times simple_{it} + \sum_{m=1}^{8} \delta_m \times map_{it}^m \times week2_{it} + v_i \tag{7}$$

$$I^A(f_{it}) = \beta_{direct} \times simple_{it} + \beta_{structural} I^A(Shapley_m) + \sum_{m=1}^{8} \delta_m \times map_{it}^m \times week2_{it} + v_i \tag{8}$$

where $I^A(f_{it})$ is the (log-utility) Atkinson index of the distribution of final assets net of debt in village i and period t. $I^A(Shapley_{it})$, defined as the (log-utility) Atkinson index of the distribution of Shapley values for the map used in village i and period t. Regression (7) estimates the total effect of the complexity treatment, while (8) decomposes it into a structural part due to how complexity changes the distribution of Shapley values, and a "direct" effect due e.g. to complexity increasing the potential for sophisticated players to capture more of the gains from trade.

We expect that $\beta_{simple} < 0, \beta_{structural} > 0, \beta_{direct} < 0$, in the sense that the simple treatment has less inequality as measured by I^A, that structural inequality tends to increase final inequality, and that the direct effect of the simple treatment is to decrease inequality. We will test these using one-sided statistical tests. We do not propose to conduct multiple hypothesis correction since (8) is a decomposition of (7).

Hypothesis 6 concerns the effect of trading day on inequality. Unlike our tests for efficiency, we do not have a clear directional prediction here. While trading day might help yield additional gains from trade, if earlier trading has generated inequality it is not clear that trading day would reverse this effect. We will report estimates using the same specification as (6) and test (two-sided) for a coefficient different from zero.

7 Exploratory analyses

7.1 Decomposition of efficiency

We will report estimates of the effect of complexity and its interaction with trading day on the full decomposition of our efficiency measure, following the specifications in Section 6.1.

7.2 Higher efficiency post-trading day

We expect the trading day to increase efficiency in general. We run the following regression, using data only from periods 2 and 3, (i.e. excluding week 1).

$$Y_{it} = \beta_{trading} \times tradingday_{it} + v_i \tag{9}$$

clustering standard errors at the village level. Each village appears twice in this specification, once before the trading day and once after. We include village fixed effects. We do not control for the map since this is fixed within village.

$\beta_{trading}$ captures the gain in efficiency during the trading day, averaging over both simple and complex maps. Under the assumption that no further trading would have taken place during that 1 hour period had we not congregated the players for trading day, we interpret this as the causal effect of the trading day.

7.3 Split effects by period

We will report the period-by-period estimates of the effect of the simple treatment. This corresponds to three coefficients, for the difference between simple and complex in efficiency (sorting, consolidation, inequality) in period 1, period 2, and trading day. We pool the data and run:

$$\begin{aligned} Y_{it} = &\sum_{\tau=1}^{3} \gamma_\tau \times simple_{i\tau} \times \mathbb{I}[t=\tau] + \beta_0 \times tradingday_{it} \\ &+ \sum_{m=1}^{8} \delta_m \times map_{it}^{m} \times week2_{it} + pair_{it} + \beta_1 \times fieldteam2_{it} \end{aligned} \tag{10}$$

clustering standard errors at the village level. Each village appears three times in this specification. Dummy variables take out time effects from week 2 interacted with map, the average effect of trading day, the average effect of field team 2, and pair fixed effects (we cannot control for village fixed effects since treatment varies at the village-period level). γ_τ is interpreted as the effect of the simple game in period τ.

7.4 Shapley value prediction

We plan on analysing inequality by comparing the gain in surplus of each participant, to the gain predicted by the Shapley value. This analysis follows closely the approach taken in Bryan et al. (2017).[10]

Under standard axioms, cooperative game theory predicts a linear relationship between payoffs and the Shapley values with a slope of one. This implies a particular division of surplus, that is, participants who unlock the most surplus (for example, because they begin with land that is highly valuable to others or in an important location) will earn the most.

We will construct an estimated Shapley value for every individual. The Shapley value assumes that participants will reach efficiency, which is not the case in most auctions. To account for this, we will scale the estimated Shapley values in each game by the total surplus gained. This is equivalent to assuming that the share of surplus given to each farmer is the same as that suggested by the Shapley value, even away from the Pareto frontier.

We hypothesize that some communities might prefer (and implement) an alternative allocation of payoffs. For example, full equality or random distribution imply a slope of zero. We will examine this by comparing the slope of the estimated relationship to the predicted slope of 1. Complexity might increase the difficulty of achieving the desired allocation, changing its slope (either toward more randomness, or toward more rent-based).

A second potential source of inequality is differences in bargaining power, bargaining ability, or strategic sophistication. Given a specific aggregate distribution of payoffs (captured by the slope coefficient), some participants might outperform their predicted earnings and others might underperform. We will measure these deviations by the squared residuals from the regression line in the Shapley value regression. Under the assumption that bargaining *power* does not vary between treatments, we interpret an increase in deviations from the line as reflecting more influence of individual traits or abilities such as bargaining power. Complexity might increase the importance of these traits.

A downside of this approach is that large differences in efficiency between simple and complex might mechanically change the slope of the relationship. For example, if participants do not trade at all, efficiency is zero and scaled Shapley values are not defined. If

[10] Bryan, Gharad, Jonathan de Quidt, Tom Wilkening, and Nitin Yadav. 2017. 'Land Trade and Development: A Market Design Approach'. CESifo Working Paper No. 6557.

most people retain their initial endowments the Shapley prediction for division of surplus will necessarily be poor. Therefore this analysis falls under the exploratory part of our plan, and will not be conducted if overall efficiency in one treatment is below 30%.

We plan to test whether:

1. Complexity changes the slope of the relationship between the Shapley value and individual final payoffs in the game.

2. Complexity changes the deviations from that linear relationship, measured by squared residuals.

7.5 Learning

We also test for learning, through an increase in average efficiency (sorting, consolidation) between periods 1 and 2. Using data only from those periods, we will run:

$$Y_{it} = \beta_{learning} \times week2_{it} + v_i \tag{11}$$

clustering standard errors at the village level. Each village appears two times in this specification.

$\beta_{learning}$ captures the average change in efficiency from period 1 to period 2, averaging over both simple and complex maps. We interpret this as learning how to trade more efficiently in the games.

7.6 Alternative efficiency, sorting, and consolidation measures

We will also report results based on

- Efficiency adjusted for exposure losses (i.e. define the outcome as Efficiency - Exposure).

- Efficiency, sorting, and consolidation only in the high quality region of the map.

- A count-based measure of sorting, replacing "land value" with the number of plots owned by a player of the efficient type.

- A count-based measure of consolidation, replacing the value of adjacency bonuses with the simple count of those achieved.

7.7 Comparison with Games 1 and 2

We will report efficiency in the games used for training participants (Games 1 and 2) and compare with the main games 3 and 4.

7.8 Continuous complexity variation

Within the "complex" treatment we have variation in the number of feasible solutions for each quality region, from an average of 1.67 solutions per quality region to an average of 5.33. We will explore descriptively the relationship between this quality measure and efficiency (sorting, consolidation).

7.9 Descriptive analyses using survey data

Our exit survey collects various measures of *how* villages played the game, such as whether an individual (endogenously) emerged as broker among the participants, or whether they organized their own trading day.

We will conduct an exploratory analysis of these variables, by regressing average efficiency at the village level on our survey measures. The most important of these is the self-reported measure of whether villages centralized trade in their own trading day.

Our "constraints survey" collects various household-level measures of land trade experience, perceived constraints on trade, and beliefs about the agricultural production function. We will use these to describe the trading environment that our participants face.

One sequence of questions attempts to identify whether constraints on trade are primarily economic (market conditions do not support trade), institutional (lack of titles), or cultural (concern that others might disapprove). In piloting we found these quite hypothetical questions very difficult to explain to participants, so we do not intend to take their findings quantitatively seriously, though the qualitative patterns are of interest.

A Map generation

A.1 Making the complex maps

We use the following procedure:

1. Begin with a grid of three 3*8 blocks of plots,
2. Randomly group plots into 24 groups of 3, 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 are no simple blocking allocations, that is, a single player that holds three plots that isolate a corner (e.g. plots 2, 9, and 10 in Figure 1), or a combination of non-trading players that hold between them two or three plots blocking a corner (e.g. 2 and 9).
   - The number of initial "adjacencies"[11] and "near adjacencies"[12] averages 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.[13]
   - 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:

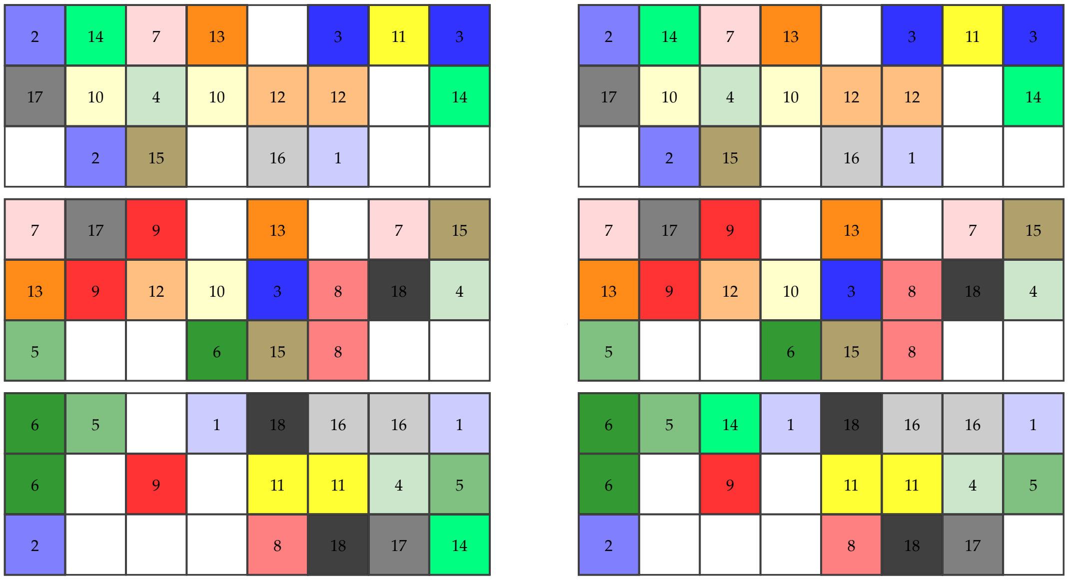
- Feasibility could be achieved by moving a maximum of 1 plot.
- The resulting map had a single contiguous set of land (i.e., no blocks of 3/6/9 were left isolated).

Of these, 2 were solvable with no edits, and the remaining 8 needed one swap (exchanging a single plot between one trading and one non-trading player). Swaps were implemented so as to avoid breaking or creating new adjacencies. Thus the initial payoffs are unaffected. See Figure 4 for the original and edited version of one such map.

[11] 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.

[12] 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.

[13] We discovered after generating the final maps that we were over counting near-adjacencies in some cases, so five of our 10 selected maps in fact average 0.29 near-adjacencies.



(a) Original

(b) Edited

Note: numbers correspond to player IDs: 1–6 are low types, 7–12 medium types, 13–18 high types.

Figure 4: Example map (map ID 74) in original and edited form. Numbers correspond to player IDs

A.2 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. See Figure 2 for the compression of the example map.

A.3 Pairing complex and simple maps

Following the above process we generated 10 candidate maps, each with a complex and simple variant. From these we 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.