# Cournot markets in the field: Dynamic decision-making in non-standard markets

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## Abstract

The Cournot economic model is very useful for representing atomized markets in laboratory experiments. Ideally, such experiments are designed following a set of conditions in order to use the model properly. However, non-standard markets and procedural concerns make it impossible for standard conditions to be adhered to in some cases. One such case is the context of rural Africa, where economic objectives of market participants differ from profit maximization, where experiments are typically conducted outdoors and where subjects have low degrees of literacy and familiarity with computers. This article describes a case study that investigated dynamic decision-making of Zambian smallholder farmers by adjusting the standard conditions of Cournot experiments to the field context of rural Africa. Both, the empirical experience from applying the proposed experimental design, as well as the insights gained based on the analysis of the experimental data highlight the usefulness and feasibility of Cournot experiments under non-standard conditions. Thus, the authors argue that the Cournot model can be used under non-standard conditions as a means to explore decision-making in contexts in which non-standard markets and procedural limitations do not permit the use of standard conditions. Furthermore, and

based on the case study, the article develops initial guidelines for further studies in contexts with similar characteristics.

**Keywords**: Cournot experiment, standard conditions, dynamic decision-making, system dynamics, food security, smallholder agriculture, rural Africa

# 1. Introduction

Laboratory experiments have long been used in the field of system dynamics to study dynamic decision-making in controlled settings (for an overview, see Arango et al., 2012). Laboratory experiments help identifying the decision rules or heuristics that people use to explain observed problematic behavior in systems, such as overshoot and collapse of natural resources (e.g., Moxnes, 2004) or oscillations in inventory-distribution systems (e.g., Sterman, 1989). When it comes to modeling market competition, some real-life markets can be represented with the Cournot oligopoly model.

The behavioral market theory behind the Cournot oligopoly model (Cournot, 1838) helps understanding the performance of a number of independent firms that compete with each other in a market through the production of a certain good. Although a number of authors have criticized the assumptions in and solutions from the Cournot model (e.g., Theocharis, 1960; Puu, 2008), the oligopoly model's adequacy for representing different types of markets is still regarded as valuable. Cournot market experiments involve several players competing to maximize a defined goal (e.g., revenues, profits, and market share) in a given market. In system dynamics, the model has been applied, for example, by Arango and Moxnes (2012), Arango et al. (2013) and Lara-Arango (2014). Their studies demonstrated that Cournot market experiments can generate valuable insights, such as the endogenous nature of commodity cycles and the effect of specific institutions on market performance, such as mothballing or capacity mechanisms.

Cournot market experiments are based on a series of fairly strict assumptions that are summarized in the standard conditions[1] that we describe in detail in the next section. From an experimental perspective, standard conditions can be thought of as a benchmark for comparisons when users vary one of the conditions. However, complying with the conditions limits the use of Cournot market experiments to conventional laboratory experiments and to standard markets, i.e., markets where players share the same objective functions as the firms in the Cournot oligopoly model, or where players have constant budgets.

These idealized conditions are often not met in reality. For example, market participants' objective functions differ from pure profit maximization in markets where local history and status interact with global and international processes (e.g., Berkes et al. 2003). It also differs from pure profit maximization in precarious situations where market participants need to focus on covering the most basic needs before optimizing their production activities according to economic logic, as for example in humanitarian operations (e.g., Carbonnier 2015). Lastly, market participants might not only maximize profits in situations where the producers are at the same time important consumers of the product as for example in small-scale agriculture in developing countries (Umar, 2014). Besides non-standard markets, i.e., markets with uncommon objective functions, there are other circumstances that require deviations from the standard conditions, such as procedural limitations regarding the feasibility or desirability of conducting conventional, fully computerized laboratory experiments. Procedural limitations can be rooted in lack of available infrastructure, participants' educational and cultural background, desire to avoid interpretations of a fully computerized laboratory experiment as a gaming session, or desire for making the experimental setting as close to the real-world decision making context as possible (Harrison and List, 2004).

[1] According to Huck et al. (2004), the standard conditions for Cournot experiments are: fixed groups; fixed number of periods; products are perfect substitutes; cost symmetry across firms; no communication between subjects; complete information about the payoff function; information about own profits, market supply and price is available to the subjects; economic framing of the experiment.

We hypothesize that a Cournot market experiment can be a useful tool to study decision-making in cases of non-standard markets and procedural limitations, even though that implies a substantial deviation from more than one standard condition. We explore this hypothesis by using a case study about smallholder farming in developing countries. Farmers in developing countries in general and in sub-Saharan Africa in particular face the challenge of considerably increasing food production for their growing and more demanding populations, while also rebuilding and maintaining the natural resource base (e.g., Campbell et al., 2014; Garnett et al., 2013; Pretty et al., 2011; Tilman et al., 2011). Smallholder farmers, who make up the vast majority of farmers in sub-Saharan Africa, struggle with combined food insecurity and natural resources based poverty traps (Stephens et al., 2012). This makes sustainable intensification particularly challenging. In this context, understanding how farmers make and adjust their decisions regarding food production and natural resource use constitutes an important precondition for the design and implementation of effective and sustainable intensification strategies.

In the case of sub-Saharan African farmers, dynamic decision-making about the choice of sustainable intensification practices deviates from the standard conditions in Cournot market experiments in two main ways. First, smallholder farmers who struggle with food security focus on maximizing their production rather than their profit (Umar, 2014). Second, it would be impractical to conduct a conventional laboratory experiment with smallholder farmers in rural areas in sub-Saharan Africa for a variety of reasons (low formal educational background of smallholder farmers, low levels of familiarity with analytical thinking in general and interaction with computers in specific, limited availability of infrastructure such as computer networks or electricity). To the best of our knowledge, there are no clearly defined guidelines for addressing such contexts.

In this article, we report a case study about dynamic farm decision-making in Zambia for which we had to adjust the standard conditions in several ways to match the local context. We focus on a budget allocation decision between two expenditure alternatives: a short-term fertilizer application strategy and a long-term soil improvement strategy. Our experiment is based on a system dynamics model developed by the second author of this article (Gerber, 2016). The model captures the essential features of the commodity market with which smallholder farmers interact. We used Cournot's market principles as a basis for our experimental design and complemented it with principles from field experiments in order to be able to carry out an exploratory study that would allow us to understand better how Zambian farmers make use of their budgets. Furthermore, this specific case is illustrative for a wide range of cases related to sustainability and production. Such cases occur when production decisions not only have economic consequences in the short-term but also wider sustainability impacts in the long-term, e.g., in energy, agri-food, renewable (water, fish, forests), as well as non-renewable resource systems (de Vries, 2013).

The outcomes of our exploratory study and the insights in terms of dynamic decision-making have been described in detail in a separate article (Gerber et al., 2017). Here, we focus on the methodological contributions of our experimental design. First, we contribute to the literature on the implementation and design of field experiments (Harrison and List, 2004). Second, we contribute to the debate on the importance of each of the Cournot standard experimental conditions, with respect to a specific problem (Huck et al, 2004). Third, we enrich the toolbox available to system dynamicists for studying dynamic decision-making in commodity markets under non-standard conditions and in a sustainability context. Based on our case study we propose initial guidelines for Cournot experiments under non-standard conditions. Fourth, and in addition to the scientific contribution of our experimental design, we found that farmers who participated in the experiments indicated that our approach is a viable means for interactive capacity building.

The remaining part of the article is organized as follows. The next section presents the standard conditions of Cournot experiments and the different types of field experiments. The third section describes an experiment conducted with Zambian smallholder farmers that frames Cournot markets as field experiments. The fourth section summarizes the results of the experiment and discusses the implications from diverging from the Cournot market experiment standard protocol. Finally, we present our conclusions in the fifth section.

# 2. Theoretical background

## 2.1 Cournot markets under standard conditions

Modern Cournot market experiments use the standard conditions proposed by Huck et al. (2004). In this section, we present each of the standard conditions and discuss their importance when running Cournot experiments.

**Interaction takes place in fixed groups**

Participants in experiments (subjects) who are randomly matched in every round of an experiment are not likely to generate high levels of collusion[2] (Holt, 1985). Lack of collusion implies that the Cournot-Nash equilibrium is a powerful predictor in these situations (Huck et al., 2001). However, real markets often consist of firms interacting with one another for long periods, which allows each of them to develop strategies based on the profiles of their competitors. Moreover, failure to consider such long-term interaction would imply that Cournot markets are only applicable when collusion is not possible (as is the case in randomly matched experiments). However, collusion is possible in practically every market. Thus, assuming there are fixed groups in Cournot markets (i.e., groups with the same subjects who interact within the market for the whole of the experiment's duration) is a standard condition and has often been found realistic.

**Interaction is repeated over a fixed number of periods**

Previous studies, such as the one conducted by Feinberg and Husted (1993), have shown that collusion is more likely to arise when Cournot games have a high continuation probability, meaning that they run for an indefinite number of rounds. In practice, however, even a few rounds can elicit collusion if other experimental factors

[2] Collusion can be understood as the extent to which a group of individuals (or groups of individuals) agree to work together to achieve a common goal.

allow for it, such as fixed groups (Holt, 1985; Huck et al., 2001) and communication (Cason and Davis, 1995; Holt and Davis, 1990). Therefore, the importance of having a fixed number of rounds does not avoid collusion per se. Nevertheless, a fixed number of rounds is recommended because it allows the experimenter to make comparisons across treatments and with other experiments.

**Products are perfect substitutes**

Differentiated products lead to more complex competition, in which firms not only compete in terms of production and costs but also in terms of product-specific issues such as branding and pricing. In this more complex environment, factors such as subjects' experiences can make significant differences. Benson and Faminow (1988) found that experience in markets with differentiated products is a crucial for reaching equilibrium through tacit collusion. In order to avoid cofounding effects from variables such as experience, perfectly substitutable goods are assumed to be a standard condition in Cournot experimental markets.

**Costs are symmetric**

Asymmetries in costs lead to a more complex competition environment. Cost advantages are likely to give more market power, which in turn, will make the market more biased. In this regard, Mason et al. (1992) and Rassenti et al. (2000) show that asymmetries in costs often lead to significantly higher outputs than expected due to the players having cost advantages. Cournot markets tend to behave more like competitive markets than oligopolies when the number of players is roughly equal to or higher than four. In a competitive market, players with a cost advantage will exercise very little constraint when the market is flooded with products; they will have lower costs and will therefore be able to take a lower price than the other players can. Given this tendency by such players, the market will end up with a "higher than normal" output. Symmetric costs are a necessary assumption to prevent this bias. Therefore, symmetric costs are a standard condition in Cournot experimental markets.

**There is no communication between subjects**

Communication between subjects is likely to lead to high levels of collusion. Previous studies of posted-offer triopolies and Bertrand markets have shown that non-binding announcements often lead to higher prices (e.g., Cason and Davis, 1995; Harstad et al, 1998; Holt and Davis, 1990). Thus, a standard condition is that subjects do not communicate with each other.

**Subjects have complete information about their own payoff functions**

This point relates to the salience principle presented by Smith (1982), which is that, in order to develop solid decision-making rules, subjects need to know exactly what the consequences of their actions will be in terms of their reward. In other words, subjects need to know how their decisions will determine their payoff. Otherwise, it will be more likely that they will develop worse performing strategies due to misunderstandings of the relationships between what they do and what they get. For example, subjects in an experimental market must understand that drastically increasing production may benefit their performance by increasing their market share, but drops in prices may harm them. Failure to see such relationships can directly hinder the external validity[3] of the experimental market because it would mean that players are not representative of real life, informed decision-makers, who know well what their performance drivers are (Smith, 1982).

**Subjects receive feedback about aggregated supply, the resulting price, and their own individual profits**

The level of information about the market and the competitors has been shown to have a significant effect on market competition. Increased information about the market (e.g., demand function) often leads to less competition and variability in subjects' actions (Huck et al., 1999). On the other hand, detailed information about competitors (e.g., individual revenues) often leads to increase competition (Huck et al., 2000). Since most firms in real markets do not have access to such detailed information, nor do they have a precise knowledge of market features (e.g., demand function), it is recommended that subjects should be assumed to have aggregate information about both the market and their competitors.

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[3] External validity refers to the extent one can generalize experimental results. That is, to what extent certain experimental results can apply to other individuals in other (similar) contexts.

**The experimental instructions use an economic frame**

An economic frame means that subjects are set in an economic situation for the experiment (e.g., by the use of economic terms such as firms, or price). Framing is an important issue in many experimental games (for example, see Franciosi et al., 1995, for a study of the Ultimatum Game). Particularly in Cournot markets, it has been found that a neutral frame can make the experiment appear as a computational problem rather than a market situation (Huck et al., 2004). Since this would directly affect the external validity of the experiment, it is important to have an economic frame.

## 2.2 Field experiments

The extent to which results from a laboratory experiment such as a Cournot market experiment can be extrapolated to a real situation is limited to the extent to which a set of experimental conditions can be generalized and to the extent that it can be argued that such experimental conditions represent a wide range of possible situations in the real system. Such realism is of especially high importance in markets with specific features, in which subjects must have specific knowledge or a specific mind frame.

Field experiments have often been regarded as a methodological way to bridge empirical and experimental research (Harrison and List, 2004). As Harrison and List (2004, pp. 1009–1010) point out: “In search of greater relevance, experimental economists are recruiting subjects in the field rather than in the classroom, using field goods rather than induced valuations, and using field context rather than abstract terminology in instructions.”

Although there does not seem to be a clear boundary between laboratory experiments and field experiments (Chamberlin, 1948; Harrison and List, 2004; Smith, 1962), a number of attempts have been made to define field experiments. In this respect, Harrison and List (2004) have provided a thorough taxonomy of field experiments. This taxonomy postulates that three types of experiments qualify as field experiments: *artefactual field experiments*, *framed field experiments*, and *natural field experiments*. The first type follows the same settings as conventional laboratory experiments, but uses a non-standard subject pool (i.e., the experiments do not use students, but rather a subject pool that is more relevant to the case e.g., traders for financial experiments, or farmers for farming experiments). The second type also depart from conventional laboratory experiment settings, but uses a non-standard subject pool and provides the subjects with a field context in the form of framed instructions as well as available information that resembles a specific field. Lastly, the third type is the same as the second type but is run in an environment in which subjects undertake their tasks as normal, unaware that they are participating in an experiment. The methodological relevance of the field experiments is centered on the idea of some real environments being hard (if not impossible) to replicate in the laboratory. Therefore, it might be better to run an experiment in the actual environment rather than trying forcibly to reproduce the actual environment in the laboratory.

# 3. Case study: Dynamic decision-making related to budget allocation by Zambian smallholder farmers

Farmers in general and smallholder farmers in particular are repeatedly confronted with budget allocation decisions that include conflicting outcome objectives. For example, short-term production activities such as fertilizer application increase food production and reduce food shortages in the short-term, but compromise future production benefits. By contrast, long-term oriented production activities, such as replenishing depleted soils, trigger sustainable food production in the future, but compromise immediate food availability.

To study how smallholder farmers dynamically decide to allocate a given budget to the two expenditure categories “fertilizer purchase” (representing a short-term production activity) and “soil improvement” (representing a long-term oriented production activity), we ran an experiment in Zambia in south-eastern Africa. The experiment used a Cournot market experiment design and procedure. However, to match the rural and cultural context of Zambia, and to ensure that the farmers could relate to their usual farm context, several adjustments had to be made to the standard protocol and these constituted a crucial basis on which to classify our approach as a field experiment, more specifically as a natural field experiment. However, we do not refer to the field experiment as a field experiment, because our subjects knew they were involved in a data gathering process for research purposes.

## 3.1 Experimental setting and procedures

We used semi-computerized experiments that each included five subjects (players). As a starting point for the experiments, we adjusted the context-specific simulation model of the Zambian maize market of Gerber (2016) to our experimental setting. The main adjustments included constant population, constant arable land area, splitting the production sector into five farms (each managed by one subject), and making soil improvement decisions endogenous. Figure 1 shows the main feedback processes of the simulation model. A detailed model description is provided in Appendix C of this dissertation.

The simulation model served as a platform where the subjects interacted. It included four main feedback processes. The two expenditure categories “soil improvement expenditure” and “fertilizer expenditure” are part of different feedback loops that both determine yield. Soil improvement slowly increases soil organic matter levels, which in turn has an increasing effect on yield (R1 feedback loop, Figure 1). Fertilizer application increases yields immediately (R2 feedback loop). In both feedback processes, yield affects the next year’s budget through the intermediate variables production, sales and farm income. Thus, these two feedback loops represent the annual farming cycle of Zambian smallholder farmers. The R3 feedback loop represents an important biological aspect of the Zambian plant production system. It adds plant residues to soil organic matter, which is a systemic leverage point for increasing food production in the long run. All the three feedback loops (R1, R2, and R3) represent processes that are specific to each experimental farm. In contrast, the B1 feedback loop represents the aggregated maize market. The sum of all farms' production results in the market supply, which determines the market price and thereby also farm income and the budget for the next growing season. This balancing feedback loop may partly offset benefits that were created through the farm specific R1-3 loops.

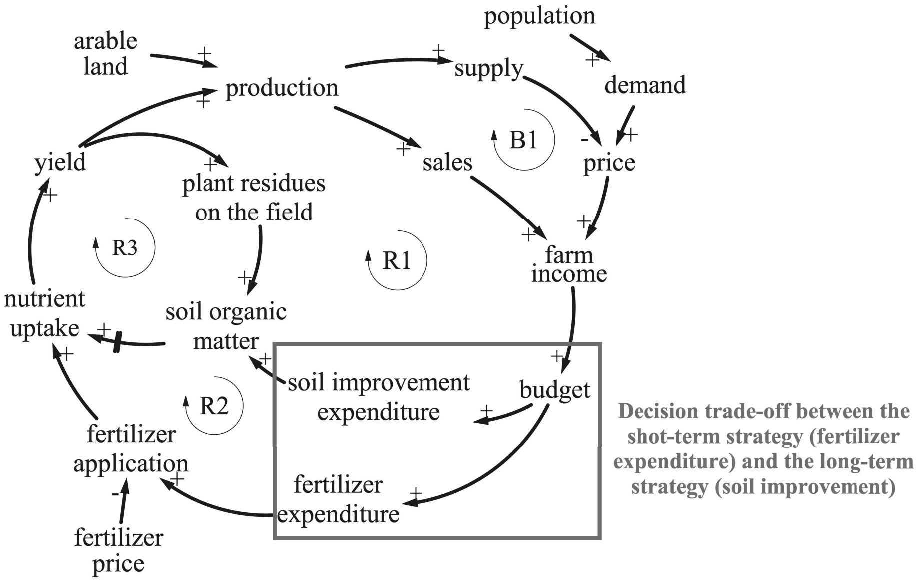


***Figure 1**. Causal loop diagram of the system dynamics model.*

*Notes: Arrows indicate causal relationships directed towards the arrowhead. A plus (+) at the arrowhead denotes a positive relationship (where the effect variable changes in the same direction as the cause variable) and a minus (-) denotes a negative causality (where the effect variable changes reversely directed to the cause variable). Feedback loops consist of circular chains of causal relationships and are either reinforcing processes (which self-reinforce the current behavior) or balancing processes (which adjust the behavior towards a goal). R1 – reinforcing soil improvement feedback loop; R2 – reinforcing fertilizer feedback loop; R3 – reinforcing soil organic matter feedback loop; B1: balancing supply feedback loop.*

*Source: Gerber et al. (2017).*

The experiment was set to last nine rounds. In each round, decisions were collected and applied for four years in the simulation model. This allowed experiments to be conducted within a feasible amount of time (ca. 90 minutes per experiment) and still covered a 35-year period, which was long enough for long-term processes such as soil dynamics to unfold. As performance indicator we used the subject's accumulated production over the total experiment duration.

We instructed the subjects verbally in their local language, following a standardized protocol (Appendix A). Important parameters to acquaint the subjects with their "experimental farm" (e.g., farm size and costs associated with the decisions) were part of the protocol and were therefore common knowledge, including symmetry across firms. The subjects were incentivized by presenting five standardized, physical rewards (2 kg sugar, 1 kg sugar, 750 ml cooking oil, big bar of laundry soap, and small bar of laundry soap) prior to the experiment. The subjects were told that performance was measured in accumulated maize production and that the best performing farm could choose a reward first, then the second, and so forth until only one reward remained for the last subject. To avoid communication, the subjects were spatially separated during the experiment. In each round of the experiment – prior to the subjects' decision – we provided the following information to each subject: the subject's budget, its yield, its production, and the market price. Based on this information, the subjects decided on how to allocate the budget to the two expenditure categories "soil improvement expenditure" and "fertilizer expenditure". In the absence of a computer network, the information was transmitted both, verbally and written (on a standardized record sheet, Appendix B). The subject's decisions were collected and entered to a central computer. After simulating four years, the current budget, yield, production and market price was noted on the record sheet and communicated to the subjects as a base for the next decision.

After completion of the nine rounds we calculated the accumulated maize production for each subject and rewards were chosen according to the rank. The experiments ended with a debriefing session where subjects expressed and exchanged their experiences, thoughts and decision rules.

The experiments were conducted in August 2016 around Mumbwa in Zambia. The subjects were recruited from smallholder farm communities and were either couples or widows that actually run and decide on a real farm in their everyday life. No subject participated in more than one experiment. The experiments were facilitated by local field assistants who spoke Tonga (the local language), and who were specifically trained. In total we conducted 15 experiments with 75 subjects, of whom 50 were couples and the remaining 25 were singles.

## 3.2 Deviations from common Cournot experiments

The experimental setting and procedures included some deviations from the standard protocol, which is normally applied in common Cournot experiments. In the following we highlight those deviations. We start with the two structural adjustments of our model to the standard protocol for Cournot market experiments (high degree of model complexity and lack of complete structural transparency; dynamic endowment).

**High degree of model complexity and lack of complete structural transparency**
Providing information about the market's mathematical representation to the subjects (i.e., providing structural transparency) gives the subjects a good understanding of their setting. Structural transparency can – in specific cases – also be an important prerequisite for arguments about the subjects' rationality. However, to qualify our approach as a field experiment, we omitted to provide structural transparency. To increase the external validity of our potential findings about smallholder farmers' decision-making, we framed the experiment so that it would be as close to a natural field experiment as possible. This implied that the subjects' decision environment needed to be as close to their normal decision environment as possible. Thus, our model structure is distinctly larger and richer in technical details than model structures in other Cournot market experiments. Consequentially, and because of the expectedly high variations in the education levels among the subjects, we did not provide full structural transparency to the subjects. Moreover, structural transparency

about the farm and agricultural markets is normally not available for Zambian smallholder farmers.

**Dynamic endowment**

Allocation decisions in Cournot market experiments are typically based on a constant budget. However, this is not the case in Zambian farmer's reality, in which budgets change over time. Thus, to ensure that the Zambian smallholder farmers' reality was reproduced as closely as possible, we applied a dynamic endowment (i.e., a budget that changes over time), based on the dynamic interactions of the subjects on the market.

**Semi-computerized setting**

To ensure the comparability of experimental data, Cournot experiments are often conducted under fully computerized settings where all subjects receive the same information. The Zambian smallholder farmers' low levels of familiarity with computers, their varying degrees of literacy, and outdoor experiments in rural villages all meant that a fully computerized setting with written instructions was not possible. Instead, we used a semi-computerized setting and tried to ensure that all subjects received the same information by using specific procedures; i.e., verbal instructions following a standardized protocol (Appendix A), spatial separation of the subjects during the experiment, and standardized communication during the experiment by using a record sheet (Appendix B) and verbal explanations. Trained field assistants facilitated the data gathering process.

**Tangible rewards**

Monetary rewards are often used in Cournot experiments to incentivize subjects, because money offers "monotonicity" (Smith, 1982). Nevertheless, we used tangible rewards instead of monetary rewards because a "game" with monetary rewards would very likely have been interpreted as gambling, for which we would have needed a concession. Additionally, a "gambling approach" would probably have distracted the subjects' focus from their farm mind-set, which was crucial for a natural field experiment. The tangible goods consisted of household items that smallholder farmers needed in everyday life (2 kg sugar, 1 kg sugar, 750 ml cooking oil, big bar of laundry soap and small bar of laundry soap).

**Payoff function**

Zambian smallholder farmers maximize production rather than profits (Umar, 2014). Thus, we used total (accumulated) maize production of each subject as a performance criterion instead of profits. The rewards were chosen according to each farmer's rank within the group. Thus, the best performing subject could choose a reward first, then the second, until only one reward remained for the least-well performing subject.

**No market supply information**

We did not provide information about aggregated market supply to the subjects because such information would normally not be available for Zambian smallholder farmers.

**Debriefing session**

After the data gathering process, we brought the subjects together for an assisted debriefing session, during which they revealed the reasoning behind their decisions and shared their thoughts.

# 4. Results and Discussion

In this section, we provide and discuss evidence from two main perspectives. The first perspective focuses on the outcome of our experiments. Thus, we briefly present the quantitative data that we gathered, the analysis that we conducted and the insights that we gained. The second perspective focuses on the experimental settings and procedures. Through the interaction with the subjects during and after the experiments we collected empirical, qualitative information about the experimental setting and procedures. We present and discuss this qualitative information, before we end this section with reporting an unintended but positive side effect of our experimental approach.

## 4.1 Outcomes: data, analysis, and insights

**Decision data**

The semi-computerized setting with a simulation model allowed to easily storing the quantitative data of allocation decisions and their impact on model variables. Figure 2 displays the trajectories of key variables for all subjects and markets. The subjects decided periodically on the allocation of a given budget to fertilizer expenditure and soil improvement expenditure, which determined production (R1 and R2 loop, Figure 1). Production in turn determined the budget for the next growing season directly and indirectly (through price, B1 loop). The model was calibrated such that maize production showed an increasing trend, which reflects the reality on many Zambian farms over the past two decades. The increasing production lead to decreasing prices, which in combination with production, determined the budget.

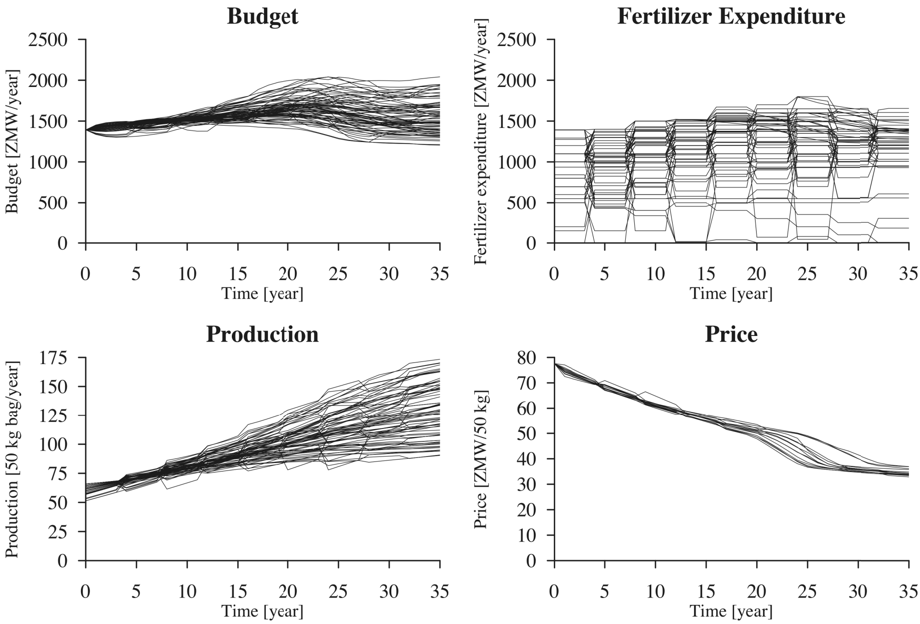


***Figure 2**. Trajectories of key variables obtained through the experiments.*

**Analysis of the data**

We analysed the data in several ways. First, we tested whether the subjects had a bias in their decisions towards one of the expenditure categories (fertilizer and soil improvement). To test for such biases we applied Mann Whitney tests to the values of fertilizer expenditure of each subject and market. Second, we were interested in detecting different decision strategies and how they affected performance (production). Thus, we applied a hierarchical cluster analysis using squared Euclidean distance as a clustering criterion in order to group the decision trajectories based on fertilizer expenditure. Then we linked the obtained clusters to performance indicators. Third, we wanted to understand how subjects that applied a certain decision strategy formed their decisions. Therefore, we ran linear regressions for each subject to identify the subject's heuristic (fertilizer expenditure was the dependent variable and the provided information cues – yield, production, budget, and price – were the independent variables). Based on the subject-specific heuristics we calculated the decision strategy's heuristic by averaging the regression coefficient of the subjects within each cluster (each cluster represented a decision strategy). Fourth, we implemented the heuristics, i.e., the mathematical decision rules, in the simulation model (Figure 1) and tested the performance implications of the heuristics by means of simulation. A detailed description of the analysis and results is presented in Gerber et al. (2017). In the following we highlight some key findings that illustrate that our experimental approach lead to valuable insights.

**Bias towards fertilizer use**

Overall, our results suggest a significant bias toward fertilizer expenditure. This outcome is consistent with findings published earlier in the literature, in which it is suggested that farmers favor strategies that give them higher short-term profitability, even at the expense of better future results (Donovan and Casey, 1998). This short-term mind-set provides an explanatory hypothesis for why long-term policies, such as conservation agriculture, do not have a high success rate in terms of scaling-up (Giller et al., 2009).

**Variation in decision strategies**

Besides the above-mentioned overall bias towards fertilizer expenditure, a substantial number of the subjects were not biased towards fertilizer expenditure. This finding shows that although fertilizer expenditure is the most common strategy, many farmers may be willing to prioritize soil improvement over fertilizer application or to implement a combination of both.

**Decision dynamics and success factors**

The results of the regression analyses showed that most of the subjects in our experiment made their decisions without taking into account the information provided to them. This finding suggests that most of the participants used pre-existing decision rules without any dynamic adjustments. A minority of the participants showed dynamic adjustment to their decisions: they changed their decisions depending on the context. We found that the initial ratio of fertilizer expenditure to soil improvement expenditure was crucial in determining how successful a given decision strategy was in terms of production, both in the case of dynamic adjustment and non-dynamic adjustment.

**Practical implications**

Our findings revealed that a mind shift from short-term production activities towards long-term production activities is required to enhance food production sustainably and increase the natural resource base in the long run. However, the question of how to achieve such a mind shift in Zambian smallholder farmers is not trivial. The variation in their decision strategies implies that there is no single solution that fits all farms, but that agricultural extension (i.e., consultancy for farmers) should focus on capacity building and the farmers’ ability to adjust adequately to changing framework conditions. Since long-term production activities provide outcomes only after a number of years, it is crucial to find means to compensate for short-term production losses.

## 4.2 Discussion of experimental settings and procedures

In this section, we discuss the ways in which our experiment differs from the standard protocol for Cournot experiments. Also, we discuss why such deviations were needed, and how they contributed to the value of our approach. This provides the basis for reflecting on the possibilities to abstract from our case study to other, related dynamic decision-making issues in commodity markets in the conclusions section.

**High degree of model complexity and lack of structural transparency**

Our experiment used a context-specific model calibrated on empirical data. For this reason, the number of variables and parameters were distinctively higher than those in the economic models commonly used for Cournot experiments. This higher complexity, coupled with procedural limitations such as varying degrees of literacy and low familiarity with computers among the subjects, meant that we did not provide full structural transparency. The field assistants who interacted directly with the farmers during the experiment reported that the farmers were well able to familiarize themselves with the provided setting and were highly engaged in the decision-making. Also, the farmers' statements in the debriefing session revealed that they could relate their experiences of the experimental setup to their real-life farms. Thus, at the cost of not being able to theorize on economic equilibriums[4], the high model complexity and the resulting lack of structural transparency was a setting that allowed the farmers to relate their experience of their experimental farms to their real-life farms.

**Dynamic endowment**

The dynamic endowment in our experimental design means that comparison of our results with previous works would be difficult. Moreover, dynamic endowment creates a high degree of autocorrelation in subjects' performance: subjects' current

[4] Market supply and demand curves have been traditionally thought to be in constant process of reaching a balance. An economic equilibrium is a point in which such balance is achieved. Well-known economic equilibriums include the Nash equilibrium, the joint maximization equilibrium and the perfect competition equilibrium.

performances will be heavily determined by their previous performances. This could lead to serious divergences across farmers' performances due to path dependence (Yesuf and Bluffstone, 2009). However, the inclusion of a dynamic endowment allowed us to explore how farmers made their decisions in light of realistic conditions, such as the possibility of falling into the poverty trap (Pugliese et al., 2017). The poverty trap is the incapability of farmers to escape poverty, once they fallen below a certain poverty threshold. Our dynamic endowment reflects this effect, and it made the economic setup more realistic for our study of Zambian farmers.

**Semi-computerized setting**

Unlike most Cournot experiments, our experimental design was not fully computerized. Given the subjects' varying degrees of literacy and low familiarity with computers, a fully computerized experiment (in which farmers would have interacted directly with the computer) was practically impossible. Under the given conditions, a fully computerized setting would only have been possible if we had used facilitators for communicating information and interacting with the computer. However, this would not have provided additional value to our setting. Moreover, an unfamiliar object such as a computer would most likely have distracted farmers' mind-sets during the decision process, which is a crucial component in field experiments as described by Harrison and List (2004). The verbal communication of the experiment instructions, as well as the standardized oral and written interaction between the farmers and field assistants ensured that the farmers understood the provided information, even though some subjects were illiterate.

While a fully computerized setting would have ensured that all subjects received the same information, we minimized information biases by specifically training the field assistants to ensure that all subjects received the same information. The geographical separation of subjects during the experiment effectively avoided communication among them, which is an important prerequisite to avoid collusive behavior. Thus, our semi-computerized setting meant that, in the given context, our experimental design was as close as possible to the standard conditions stipulated by Huck et al. (2004).

**Payoff function**

While the original Cournot oligopoly model solutions are based on the maximization of profits (given a certain production level and a resulting price), our experiment used only production as a basis for the payoff function. Consequently, maximization of production excludes any possibility of having an economic equilibrium in its pure sense and farmers will produce as much as possible even if that implies lower prices for them. While lower prices can reduce farmers' future endowments, higher production levels can compensate for them if the increase in production outweighs the price decrease. Thus, shifting the focus from profits to production may have implications for the farmers' decisions. However, having profits as a basis for payoff function in the Zambian case is unrealistic. Zambian farmers are food insecure, which means that in their daily activities their primary focus is on producing as much food as they can, instead of making as much profit as they can (Umar, 2014). Our experiment was therefore designed to be consistent with this focus, by treating accumulated production, not profit, as a basis for farmers' payoffs.

**Tangible rewards**

Subjects were paid with tangible items to avoid misinterpretations, since in the local context monetary payments could have been mistakenly associated with gambling, which might have distracted the farmers' mind set and had an adverse effect on any future research projects with the farmers. While tangible rewards have been shown to be at least as good as monetary rewards to incentivize performance (Kelly et al., 2015), this reward design poses the challenge of "monotonicity" (Smith, 1982). Monotonicity refers to the property of a good of always being equally good in incremental terms. As an example, most people are likely to agree that having USD 200 is better than having USD 100, and, in more or less the same way, having USD 300 is better than having USD 200. While we cannot guarantee monotonicity in any of the goods we used to reward subjects' performance, we mitigated the lack of it by assigning an order to choose the items instead of defining one specific item as the first prize, another item as the second prize, and so on. In other words, by giving the subject with the best performance the chance to choose among a set of items first, we expect that he or she would choose the item with the highest utility value to him or her. Although the order does not guarantee monotonicity across subjects' utilities, it certainly makes sure that the subjects with the best performance gained more value from their choices, since they had more items to choose from, compared with the farmers who performed least well. From subjects' comments made during the introduction to the experiment and the reward ceremony, it became clear that the setting with a production-based payoff function and tangible rewards motivated the subjects to perform as well as possible. Farmers saw each other when they were called to collect their prizes, which elicited another intangible reward: acknowledgement. It has been shown that acknowledgment is a powerful reward in certain communities, especially in communities in which all members know each other (Bradler et al., 2016). Since this is the case for farming communities, acknowledgment of the farmers with the best performances should reinforce the value of the tangible reward, such as the rewards received during the reward ceremony. Our results from the debriefing session also suggest there is an extra motivation for farmers, namely to learn. The Zambia smallholder farmers wanted to do their best in order to learn as much as they could from the experiment. Thus, the combination of rewards, acknowledgment and interest in learning are arguments in favor of the validity of our approach.

**No aggregate market supply information**

Our experimental design does not present aggregate market information to subjects. This can have implications for the level of competition between the subjects (Huck et al. 1999). However, it is not realistic to assume that Zambian farmers have a comprehensive understanding of the market. Furthermore, giving an additional variable to the farmers for consideration would have made our design more complex, especially when such variables do not represent a piece of information they are used to dealing with in their daily activities. In other words, not having information would be more realistic than having it. Furthermore, dealing with a piece of information they are not use to dealing with, may alter subjects' decision processes, and in our case this could have affected the external validity of our results, and thus defeating the purpose of our experiment. For these reasons, we refrained from giving the subjects information about the aggregate market supply, even though this might have implied changes in competition levels across farmers, as indicated in Section 2 above (Huck et al. 2000).

## 4.3 Implications from qualitative information by the subjects

During the debriefing session after the experiment, besides qualitative information about their decision rules and strategies, the subjects repeatedly expressed one positive, albeit unintended side effect of the experiment. Despite the unambiguous statement in the introduction to the experiment that we were gathering information for research purposes, the subjects expressed that they themselves learned a lot from the experiment. It seemed that some of the subjects had forgotten that they were part of a data collection process and thought that they had joined a capacity-building event[5]. Common learning outcomes stated by the subjects included the following: the importance of planning and making decisions as a couple (apparently, on many farms, the couples did not decide jointly in real life); the relevance of allocation decisions for production outcomes; the importance of dynamic book keeping; differentiating between short-term and long-term production activities and knowing their impacts; and differentiating between the concepts of yield and production. Thus, our experimental approach might not only serve as a method to collect decision data but also constitute a viable means for interactive capacity building.

# 5. Conclusions

Cournot market experiments propose a useful frame to analyze production related decision-making and how such decisions affect the performance of competing firms. However, common Cournot market experiments are based on a series of fairly strict assumptions (standard conditions). In reality, there are circumstances that do not coincide with all the assumptions in Cournot experiments. For example, non-standard markets (i.e., markets where participants have uncommon objective functions, such as

[5] A capacity-building event in this context is a training session for farmers.

maximizing production instead of profits) and procedural limitations (e.g., due to lack of available infrastructure, or participants' educational and cultural background) pose challenges to run Cournot market experiments. Nevertheless, there is vital research interest to also study decision-making under such circumstances. In this article we reported a case study about budget allocation decisions of Zambian smallholder farmers, which included both, a non-standard market setting and procedural limitations. To study the farmer's decision-making we used Cournot market principles as a basis for our experimental design and complemented it with principles from field experiments. Based on our case study we gained several key insights that exceed the specific case of Zambian farmers and that we believe are of general interest to researchers who want to study dynamic decision-making based on experiments.

## 5.1 Scientific value of non-standard Cournot experiments

Standard conditions provide a reliable framework with which to control important variables in Cournot markets, such as the levels of competition, cooperation, subject engagement, and salience (Huck et al, 2004). To be clear, standard conditions are not thought of as a boundary, beyond which experiments cannot be valid or valuable for different purposes. For example, standard conditions can serve as a benchmark to infer where different behaviors may arise from (e.g., a higher level of competition may be rooted in the level of information given to the subjects, instead of the actual context in which an experiment is framed). However, experiments with severe deviations from the standard conditions generate results that may be difficult to compare with other Cournot market studies that use standard conditions as a benchmark. Although standard conditions must be considered in order to study the theoretical properties of the Cournot market formulation, we found that relevant scientific knowledge, such as information about dynamic decision-making, can arise from non-standard experiments and therefore, non-standard Cournot experiments are worth considering. In particular, our experimental design and procedures allowed subjects to familiarize themselves with their experimental firm, which is a key issue when conducting field experiments with real decision makers. Furthermore, the analysis of the experimental data revealed important information about how Zambian smallholder farmers make budget allocation decisions with conflicting long- and short-term production objectives. Such trade-offs occur in several situations where production decisions not only have economic consequences in the short-term but also wider sustainability impacts in the long-term. These trade-off situations are crucial to study, even if they do not represent a standard Cournot market. Thus, we propose that standard conditions should be viewed as resources rather than limitations.

## 5.2 Initial guidelines for non-standard Cournot model applications

Using the standard conditions as resources is not trivial and giving a clear-cut generalized framework for adapting Cournot standard conditions to all problems is not entirely feasible. However, based on our case study, we suggest the following initial guidelines for non-standard Cournot model applications.

First, one needs to be clear about a study's objective. Applying severe deviations from the Cournot standard conditions may imply that it is impossible to contribute to the theoretical economic literature, for example, about economic equilibriums. However, if one wants to investigate decisions of real decision makers (i.e., to conduct field experiments), the deviation from Cournot standard conditions may be necessary. Thus, depending on the study's main objective, severe deviations form standard conditions are either essential or undesirable.

Second, one must think of how well the standard conditions represent the characteristics of the market of interest. There is a solid body of literature that analyses what deviations from each standard condition may entail (e.g., Huck et al., 2000; Huck et al., 2004). This literature can give useful indications for possible biases and compound effects that may arise when one deviates from standard conditions. Depending on a study's objectives and taking into account this literature, one can judge what deviations may be feasible. For example, a shift from fixed budgets to dynamic endowments – as in our case – is most likely adequate for a wide range of commodity markets where market participants face severe budget constraints so that the economic consequences of their decisions (at a specific point in time) directly affect their economic endowment in the subsequent decision intervals. Thus, it is important to know in which regards the market at hand is different from the standard conditions.

Third, one must think about what is meaningful for the subjects by taking into account their context. The subjects may attribute specific values to specific variables. For example, in our case, given the food insecurity faced by farmers, it made sense to shift the focus from profits to production as an objective function. In a similar way it was adequate to conduct semi-computerized experiments because fully computerized experiments would most likely have detracted the farmers from the farm setting, which was a key issue to qualify our study as a field experiment. Thus, such special conditions may require deviations from standard conditions and procedures, such as shifting the focus from profits to other variables (e.g., market share or price), or from a fully computerized setting to a semi-computerized setting.

The guidelines above are based on one case study of Zambian smallholder farmers. Many of the modifications that we performed were context-specific (i.e., they related to the case of Zambian smallholder farmers), and therefore cannot be extrapolated to all cases. This means that more research is needed to further specify and consolidate these initial guidelines. Additional case studies in different contexts can reveal other critical points. And semi-structured, post-experiment interviews could be a means to systematically collect standardized data.

## 5.3 Potential as a learning methodology for interactive capacity building

Despite the fact that farmers in our case study had been told they were going to participate in an experiment, many of them later stressed how much they had learned from the experiment. The most interesting aspects they mentioned were: the importance of joint decision-making (i.e., making decisions as couples); the relationship between their budget allocation decisions and their resulting production; the importance of keeping track of their decisions; the realization of existing biases; and learning to differentiate between agricultural concepts. Their positive feedback indicates the potential of using non-standard Cournot market games as a vehicle for simulation-based learning (Andersen et al, 1990; Davidsen and Spector, 1997; Senge, 1994; Sterman, 1992). Further research is needed to explore this potential in depth.

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# Appendixes

## Appendix A: Data Gathering Protocol

1. Gather the participants (5 couples, that in real life each actually run a farm together).

2. Introduction and Instructions: Hello and welcome everybody.

*Introduction of all that are present*

A. Purpose

Thank you for being here. Today we gather information for learning how you make different decisions. Andreas is doing a schoolwork study for his PhD in collaboration with Dr. Nyanga at UNZA[6]. He is interested in learning how you make decisions as couples. The information will be used for academic purposes and may be published in academic journals. Is that clear and ok for you?

B. Roles

We would like to gather the information through playing a game together. The roles are: I am the moderator, who will interact with you. Andreas is the computer man, who will be putting the information in the computer and giving the results. Cain and Eukeria will help me moderating the process, transmitting information between you and the computer man. You, the couples, are the players who make decisions.

C. Game

Every couple will manage a farm. You all have a common main goal for your farm. In this game the main goal is to maximize your accumulated maize production over the whole game. To reach the goal of maximize your production, you must decide how much money (Kwachas) you want to spend on two options. The first option is buying own fertilizer (not through government or NGO subsidies). And the second option is spending financial means to improve your soil through crop residue retention and manure application. In this game we just have these two options and we

[6] University of Zambia

are not considering other options such as lime application, crop rotation, Musangu tree plantation, etc.

Here is some information to understand your farm: Each couple cultivates 8 limas (equivalent to 2 hectares) of maize on its farm, so your decisions are limited to this area. The maize yield level is currently around 7 bags of 50kg per lima; the current/starting production therefore is around 60 bags of 50kg per farming season. The current/starting producer price of maize at your market is around 75 Kwacha per 50kg bag.

In the beginning your budget for the two options is 1392 Kwacha. In the first option, which is buying fertilizer, a 50 kg bag of fertilizer cost 550 Kwacha. In the second option, which is crop residue retention and manure application, a lima costs you 117 Kwacha, adding external organic matter becomes more expensive.

For you to make decisions, the moderator will come to you and give you information about your budget, yield, current production and market price. You will then decide how much of the budget you want to spend on fertilizer and how much you want to spend to improve your soils. The moderator will take note of your decision and bring it to Andreas. He will put your decision into the computer and calculate the new budget, yield, production and price. The moderator will bring this new information back to you so that you can again decide how much money you will spend for fertilizers and soil improvement. We will have 9 rounds in this game. Thus, these dynamics will continue until we complete 9 periods (you make 9 decisions). The game will be completed in 1-2 hours approximately.

At the end of the game, the computer calculates your total production for the entire game and you will be rewarded with a present depending on your results. We brought a couple of items of which the best performing couple can choose one item first, the second best performing couple second, etc.

*Show the goods (2kg sugar, 1kg sugar, 750ml oil, big laundry soap, small laundry soap)*

If you have difficulties to make your decision, think of how you decide on your own, real farm and always keep in mind that your goal is to maximize your production!

We will have the possibility to clarify procedural questions during the game, but not ask for help in decision making. So far, is the game clear to you? Are you willing to participate? If you do not want to participate or feel uncomfortable, you can withdraw.

| Remarks to the instructor:<br><br>It is ok to clarify procedural questions: e.g., what happens after we make a decision? Do we have to spend the entire budget to these two policies? Etc.<br>It is also ok to clarify the meaning of words (e.g., yield)<br>Do not give clues that may directly influence the decision making process. E.g., do not answer questions regarding what should be done such as "should I allocate more on fertilizers?" or "How can I make the highest production in the game?" |
|---|

3. Split the participants up.

In this game it is the idea that you keep your decisions and results as a secret within your farm and do not share them with the other couples. So please, keep communication between the farms at a low level. However, once the game is finished and we have all the results from everyone, you are very free to share experiences and strategies with each other!
Give your best and good luck!!

4. Start the actual rounds.

After first round: explain that yield, production, price and budget changes. Costs stay the same.

5. Save the rounds.

*Take a copy (soft or hard) from the interaction sheets and save it.*
*Give a hard copy to the farmers as a feedback.*

6. Conclude with an aftermath session.

At this point the game is over and you are free to leave if you wish. However, if you appreciate, we will have a feedback session explaining some ideas of the game.

## Appendix B: Record Sheet

Farm Number:________

Name of Participants:

_____________________

_____________________

Data Collection Set-Nr:___

Input prices:
- 50 kg Fertilizer costs 550 ZMW
- 1 lima improved soil costs 117 ZMW, for further improvement the price increases

| Round | | | | | | |
|---|---|---|---|---|---|---|
| 0 | Yield ≈7 bags/lima | Production ≈60 bags | Price ≈75 ZMK/bag | Budget 1392 ZMW | Soil | **Fertilizer** |
| 1 | Price | Yield | Production | Budget | **Soil** | Fertilizer |
| 2 | Production | Price | Yield | Budget | Soil | **Fertilizer** |
| 3 | Yield | Production | Price | Budget | **Soil** | Fertilizer |
| 4 | Price | Yield | Production | Budget | Soil | **Fertilizer** |
| 5 | Production | Price | Yield | Budget | **Soil** | Fertilizer |
| 6 | Yield | Production | Price | Budget | Soil | **Fertilizer** |
| 7 | Price | Yield | Production | Budget | **Soil** | Fertilizer |
| 8 | Production | Price | Yield | Budget | Soil | **Fertilizer** |
| 9 | Yield | Production | Price | Budget | **Total Production** | |

Date: ______________ Place: ______________