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Abstract

We experimentally study causal effects of competitive experience in markets with a short and a long side on efficiency levels attained in a subsequent social dilemma. We find that market experience affects efficiency when traders previously competed in the same market on the same side. The effect is strong for market-loser pairs and also exists for market-winner pairs, albeit to a lesser extent. Cooperation efficiency is unaffected for pairs consisting of a market-winner and a market-loser. When traders did not interact on the same market before, efficiency of cooperation is higher for market-winner pairs, but only in the short run.

Keywords: Competitive market, social dilemma, experiment
JEL: A13, C92, D30, D60

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1 Introduction

Market competition is commonly considered to be a beneficial force and there is no doubt that competitive markets are important for the efficient allocation of resources. This is demonstrated theoretically in the First and Second Welfare Theorems (see, e.g., Mas-Colell et al., 1995) and shown empirically in many field studies and in experiments with double auctions and other competitive market institutions (see, e.g., Smith, 1962; Davis and Holt, 1993). However, an important question is whether the efficiency effects of markets are not circumscribed to the market environment itself but spill over and affect efficiency in other spheres of social and economic interaction. This is especially relevant in relation to interactions through personal exchange where cooperation can not be completely regulated through formal contracts. In this paper we study, using laboratory experiments, whether and how trading experience in a highly competitive market causally affects the efficiency of cooperation in a social dilemma situation outside the market environment.[1]

Competitive market experience can have various facets. A salient feature of modern market societies is that the productive assets – including human capital – are distributed rather unequally (see, e.g., Cowell and Van Kerm, 2015). As a consequence, some people's skills or assets are in high demand in the market, with many others trying to transact with them, while those of others are in much lower demand. Some people may even have difficulties to trade at all (see, e.g., Marquis et al., 2014, for labor markets). Our focus is on whether different market experiences of 'market-winners' and 'market-losers' differentially affect the efficiency of cooperation in social dilemma situations, beyond potential income effects (see, e.g., Bowles, 1998; Smith, 1998, and our more detailed discussion below).

Another important aspect that may matter for the efficiency of cooperation in a social dilemma situation outside the market is whether agents are dealing with somebody they have to compete with in the same market or whether competition is experienced with somebody else. It may make a difference whether one has, for example, to supply a local public good jointly with a neighbor who is competing for the same job or customers, or with somebody who is not a direct competitor on the market (see, e.g., Henrich et al., 2001, and our more detailed discussion below).

In our study, market interaction takes place in a highly competitive continuous double auction (see Smith, 1962). We use this market institution because it has been shown to consistently converge to the efficient Walrasian outcome and does so through a decentral-

[1] Throughout the paper we use the term efficiency of cooperation to refer to the total surplus produced in a social dilemma situation.

ized equilibrating process in which bids and offers are made and prices and transactions emerge over time (see, e.g., Davis and Holt, 1993). It is the effect on cooperation of having experienced such highly competitive and efficient markets we are interested in.[2]

We designed the experiment in a way that allows us to study the two specific dimensions of spillover effects introduced above. First, we can investigate if and how effects differ depending on whether people are on the favorable or unfavorable side of a market. We achieve this by using the so-called box-design of a market that involves a long and a short side of the market with inelastic supply and demand curves (Holt et al., 1986). This market configuration implies that individuals on one side of the market will easily make transactions at favorable prices, whereas individuals on the other side of the market will have difficulties to make transactions and will do so at unfavorable prices, if they transact at all.

This feature represents in a stark way the very unequal opportunities that exist in some market economies or market segments (e.g., labor markets for high and low skilled workers).[3] It will also give rise to endogenous earnings differentials among agents. Thus, there will be market-winners with high earnings from market interactions and market-losers with low earnings. Our main interest is in how different market experiences affect behavior in a social dilemma game keeping everything else equal. We therefore control for earnings differences as explained further below.

The second dimension of market experience we investigate relates to whether people have to overcome the social dilemma problem together with people with whom they have had or have not had a joint market experience. In the experiment we can study this by immersing participants into the same competitive market environment but matching them for the social dilemma exogenously in a way that ensures that they do or do not share a common market experience.

The social dilemma we investigate is a repeated two-person public goods game in which pairs are fixed throughout all periods (Chaudhuri, 2011; Kagel and Roth, 2012). Our design allows us to explore how, respectively, pairs of market-losers, market-winners and mixed pairs (i.e, pairs consisting of a market-loser and a market-winner) are affected in their efficiency of cooperation. In addition, we vary whether pairings in the social dilemma game come from the same market or from different markets. For convenience we

[2]Needless to say that this does not imply that we consider other market institutions or other competitive environments to be uninteresting. However, being the first study exploring competitive market experience on non-market cooperation we chose an institution that (a) is undisputed in being a good reflection of decentralized market behavior and (b) does avoid potential confounds due to structural market imperfections and inefficiency (e.g., oligopolistic markets).

[3]Less stark representations of unequal market opportunities are conceivable. We consider our implementation as a starting point providing benchmark results for other 'less extreme' market inequalities.

will refer to the former case as Market-Partners and the latter case as Market-Strangers. Finally, within the context of our experiment, we are not only interested in the immediate impact of market interaction on cooperation but also ask whether an eventual effect fades out, persists, or is reinforced over time, a distinction we will refer to with the labels 'short run' and 'long run', for ease of exposition.

Our study also relates to the broader issue of the influence of institutions on economic and social motivations, which still is an under-explored topic in economics (Fehr and Hoff, 2011). For instance, van Winden (2012) argues that to understand economic and social interactions one needs to take into account the existence and dynamics of social ties between people and how they are affected by the context in which these interactions take place. Bowles and Polania-Reyes (2012) present an extensive survey of the evidence documenting that social motivations are not necessarily separable from the environment and experiences related to the environment.

There are two prominent contrasting views pertaining to the potential spillover effects of markets on non-market activities requiring cooperation. Vernon Smith (1998) builds on Adam Smith to postulate that people intuitively know how to behave both in a cooperative and in a competitive way depending on the context. According to this view, both behaviors grow out of a universal propensity for social exchange which "finds expression in both personal exchange in small-group social transactions and in impersonal trade through large-group markets." (Smith, 1998, p.3) Smith sees cooperative and non-cooperative behavior as peacefully coexisting, with efficiency in impersonal markets being based on competitive behavior, while efficiency in personal social exchange requires the ability to find ways to engage with others to avoid free-riding. This view implies that market experience should not affect behavior outside the market.

Relatedly, Henrich et al. (2001) report correlational evidence suggesting that market interaction can have positive effects on cooperation. They find that "the higher the degree of market integration (...) the greater the level of cooperation in experimental games." (Henrich et al., 2001, p.74) The rationale for this relation proposed by these authors is that "the more frequently people experience market transactions, the more they will also experience abstract sharing principles concerning behaviors towards strangers (...)." (Henrich et al., 2001, p.76) This is consistent with the notion of *doux commerce* as put forward among others by Montesquieu (1748) already in the eighteenth century.

In contrast, Bowles (1998) suggests that market participation can adversely affect people's personality. Specifically, he argues that "(...) there are significant differences in the personality effects on participants in markets (...) for people on the short side (...) and those on the long side of the market, some of which are simply excluded from the

exchange process, while others fear losing the transactions they have secured." (Bowles, 1998, p.78) Bowles' concerns can be seen as part of the broader question asking whether market exchange erodes moral and civic goods worth preserving (Fourcade and Healy, 2007; Sandel, 2012, 2013). This view implies an adverse affect of market experience on the efficiency of cooperation outside the market, especially for market-losers.

A priori the diverging views on potential spillover effects of market participation are both reasonable and empirical evidence is necessary to ascertain their relative merit. If the negative spillover effects of market participation discussed by Bowles indeed depress the efficiency of voluntary cooperation this would be a major challenge for societies in which markets play a central role.[4] However, as mentioned above, there are also reasons to believe that market participation is innocuous or is even beneficial for the efficiency of non-market interactions. With our study we want to contribute to shedding light on this important issue. To the best of our knowledge this is the first study doing this.

In the field non-market interactions are affected by a multitude of factors which makes it difficult to tease out the effect of market experience on the basis of field data. The use of laboratory experiments makes it possible to study spillover effects of market participation with a high degree of control under *ceteris paribus* conditions. Specifically, we are able to exogenously assign participants to the two sides of the market. Without laboratory control naturally more cooperative people might be over-represented on one or the other side. Similarly, we are able to control the composition of the groups in the subsequent social dilemma and, hence, study behavior for all possible matchings between participants with different market experiences.[5]

Our experimental set-up includes both market treatments and non-market treatments. We directly compare behavior in market treatments with that in non-market treatments. In the latter participants have no market experience and are endowed with earnings that are on average equal to the market earnings made by participants with market experience. This allows us to separate the effects of being a market-loser or market-winner from that of just having higher or lower earnings. As different market positions inevitably are associated with different earnings potentials, this separation would be virtually impossible with field data. In additional control treatments we test if it matters (a) whether or not participants are informed about (potentially) different earnings and (b) whether it makes a difference when agents have to work for their earnings instead of receiving them as windfall gains.

[4]Our focus is on spillover effects on efficiency, because they are more directly economically relevant. However, spillover effects could also be on psychological dimensions like efficacy as captured in the Rotter score (see Rotter, 1966) or social dominance orientation (see Sidanius and Pratto, 2004).

[5]Another advantage of lab experiments is the possibility of replication which allows for a systematic study of the relevant issues. See Falk and Heckman (2009) for a methodological discussion of the relevance of laboratory experiments in the economic and social sciences.

Our results show that market experience can affect the efficiency of cooperation outside the market and that the precise strength and direction of the spillover effect depends on specific market circumstances. For traders with a joint competitive market experience (Market-Partners) we find that the efficiency of cooperation decreases strongly for market-loser pairs and also, albeit to a lesser extent, for market-winner pairs. In contrast, in Market-Strangers, pairs of market-winners manage to cooperate more efficiently than comparable pairs without market experience but this holds only in the short run. Thus, having competed for scarce resources on *the same side of the same market* depresses efficiency in the social dilemma.

In two subsequently conducted treatments, we subject our Market-Partners results to additional scrutiny. First, we test if making fully transparent the earnings received before the social dilemma game affects the outcomes. Second, in a treatment that we preregistered, we replaced the market that takes place before the social dilemma game by an individual real-effort task which yielded an experience parallel to that of the market. The results from this new treatments are similar to those of the original ones, albeit the significance levels tend to be weaker. This points to the possibility that, when adding features that define market interactions compared to non-market situations, cooperation behavior after having experienced these altered non-market situations may approach cooperation behavior after market experience.

In summary, our results comparing the treatment with market experience to the different control treatments indicate that the observed differences in the efficiency of cooperation cannot be solely explained by earnings differentials or by other differences that do not pertain to market interaction per se. Hence, we can attribute a substantial part of the cooperation differences in a causal sense to different market experiences in the cases we study. We note that with our experiment we cannot uniquely identify the mechanism behind our results. However, we can offer a tentative explanation of our main results in terms of direct competition weakening social ties, and of the more general notion of state-dependent preferences, in conjunction with a positive effect of advantageous market experience on the efficiency of cooperation.[6]

2 Related Experimental Literature

There are a number of related experimental papers studying the effect of competition on behavior in a variety of environments. None of them deals with how interaction in compet-

[6]For behavioral and neuronal evidence on the existence and dynamics of social ties even in the anonymous environment of laboratory experiments, see, e.g., Sonnemans et al. (2006) and Bault et al. (2015).

itive markets under different circumstances affects subsequent efficiency in cooperation. The efficiency of markets—in the sense of the generation of economic surplus—is a central issue in economics. However, in case of spillover effects from markets to non-market interactions this may not reflect the overall efficiency effect of markets. Here we investigate how market experience under different circumstances affects after-market efficiency. We now briefly refer to some previous work and highlight the differences with our work.

Bauernschuster et al. (2013) use partner-choice games to study how competition between two investors interacts with trust and trustworthiness. In simple one-shot trust games they find that competition among trustors does not significantly increase sent amounts. However, trustees react to competition between trustors by lowering return ratios. Similarly, Huck et al. (2012) study a repeated binary trust game related to a market for an experience good with a fixed price where the buyer can choose whether to trust or not and the seller can only choose quality. Without competition, buyers are in each period randomly assigned to sellers. With competition, buyers choose in each period the seller from whom they want to buy. The authors report that the introduction of competition is highly effective, with efficiency rising from 30 to over 80 percent.

Brandts et al. (2009) also use partner-choice games to study the effects of rivalry on the disposition towards others and on subjective well-being. They use a finitely repeated prisoner's dilemma game between fixed triads of players, where one of the three players can in each period choose with whom of the other two players to interact, leaving the third player without interaction. The results show that rivalry affects individuals differently, depending on which side of the rivalry they are on. It negatively affects experienced well-being of those on the powerless side of the interaction and has a positive effect for the powerful player leading to a larger inequality in experienced well-being. Interacting under rivalry also affects negatively the disposition towards others. Interestingly, the efficiency of cooperation is the same in conditions with and without rivalry.

Herz and Taubinsky (2017) use another partner-choice game to study how experience with competition shapes fairness standards. In their experiment participants first take part in ultimatum games with either proposer or responder competition and then play the standard ultimatum game. They find that responders' acceptance thresholds are higher for responders that started in the game with proposer competition than for those who started in the game with responder competition.

Partner-choice games as the ones used in the studies just mentioned involve an element of competition. However, in our view this kind of games do not adequately represent full-fledged market competition as the one we study, with both sides of the market participating actively and multiple transactions taking place.

Carpenter and Seki (2006) report on a field experiment conducted with three groups of workers from a fishing community in Japan, where the different groups were exposed to different amounts of competition on-the-job. The results show that these differences explain differences in cooperation in an experimental setting. Specifically, fishermen and fish wholesalers, who interact in more competitive environments are significantly less cooperative than staff who faces little competition on the job. This study is perhaps closest to our work, but does not speak directly to the issue of how the effects of market interaction can be distinguished from that of income differences. Moreover, the investigated on-the-job competition varies several variables simultaneously (e.g., intensity of competition and occupation), while we investigate the effect of competition on the efficiency of cooperation for agents who have been on on either the long side or the short side of the market as well as for agents who have been on different market sides, keeping everything else equal. The mentioned study does also not speak to the effect of competition with strangers, which we investigate in one of our treatments. Lastly, as this study uses natural groups it cannot exclude selection effects.

Falk and Szech (2013) study behavior in a context in which market exchange can produce a negative externality – in their case the death of mice. They find that repeated market interaction typically yields less socially responsible behavior than one-shot non-market behavior. Bartling et al. (2015) present a comparison of social concern between Switzerland and China. They study behavior in both a non-market and a market context. They find that in both countries subjects exhibit less social concern in a market than in a non-market environment. In addition, they find that while there is no cross-country difference in behavior in a non-market context, in a market context social concern is lower in China than in Switzerland.[7] Whether market participation makes one more disposed to subsequently hurt others is in our view different from the effects of subsequent cooperation efficiency and, in addition, in these studies the relevance of the specific conditions under which one participates in the market are not studied.

Two studies compare the effects of interacting under respectively tournament and piece-rate incentives of subjects on Amazon Mechanical Turk on subsequent behavior. Buser and Dreber (2016) find that individuals are significantly less cooperative in a public goods game after having interacted under tournament incentives than under piece-rate. Chen (2011) compares the effects of interacting under competitive and piece-rate conditions on charitable donations and finds that a competitive environment leads to higher donations.

[7] In a non-market context, Peysakhovich and Rand (2015) use a repeated prisoner's dilemma to study how being in environments that are conducive to cooperation lead to higher prosociality and trust in a subsequent one-shot situation than being in environments that do not support cooperation. The authors interpret this result in terms of the creation of habits of virtue.

The focus and set-up of our study is quite different from the cited studies and complements important insights generated by this previous work. Our study reports the first causal evidence of different forms of competitive market experiences on the efficiency of cooperation controlling for the effects of differential earnings, which allows us to identify the effects of market interaction net of income effects. Specifically, we investigate the efficiency effects of experienced competition in markets with very unequal participation opportunities in which the experience of being on one or the other side of the market is vivid.[8] In comparison to other studies we investigate factors typical for markets that have not been explored earlier. In particular, we can analyze the effect of asymmetric positions in the market (market-losers vs market-winners) and the effect of more or less common experience of market competition (Market-Partners vs Market-Strangers).

3 Experiment Design

Our design has two main building blocks: (1) a highly competitive continuous double auction market (hereafter, DAM) and (2) a social dilemma game (hereafter, SDG). We implemented four main treatments: two market treatments in which the DAM is played before the SDG, and two non-market treatments, consisting of three conditions each, that control for earnings achieved in the market phase of the market treatments. All treatments also involve two measurements of subjective well-being (SWB) and of social value orientation (SVO). To keep the paper focused we describe the two main building blocks (DAM and SDG) in detail here but relegate the description of the SWB and SVO to Appendix A. In the following we first present the two market treatments followed by the two non-market treatments.[9]

3.1 Market Treatments

Both market treatments consisted of eight parts. Table 1 shows the sequence of events. At the very beginning, participants were informed that the experiment would have several parts. Instructions for the various parts were given separately for each part, except those for parts 3 and 4 which were presented together.[10]

[8] A situation reminiscent of the notion of the reserve army of labor introduced by Engels (1845). Some observers, see e.g. Standing (2011), consider that in modern globalized economies there now exists a new reserve army of labor, comprised of temporary and part-time workers, who lack any type of job security.

[9] The experiment instructions can be found in Appendix D.

[10] Sequentially presenting the different parts of the experiment involves an element of non-full immediate disclosure of information. Importantly, participants were informed beforehand that the experiment consisted of several parts and that for some parts they would receive detailed information only when the respective part starts. We used this structure because it avoids anticipation effects for the second SDG and

Table 1. Sequence of events in market treatments

1.	Self-assessment of subjective well-being	(SWB 1)
2.	Measurement of social value orientation	(SVO 1)
3.	Double auction market (18 periods)	(DAM)
4.	Social dilemma game (6 periods)	(SDG)
5.	Self-assessment of subjective well-being	(SWB 2)
6.	Measurement of social value orientation	(SVO 2)
7.	Surprise restart social dilemma game (12 periods)	(sSDG)
8.	Post-experiment questionnaire	

Note: SWB 1, SVO 1, SWB 2, and SVO 2 are described in detail in Appendix A.

In part 1 (SWB 1) all participants had to answer a self-assessment question to measure their initial subjective well-being and in part 2 (SVO 1) they had to make money allocation decisions to measure their social value orientation. In part 3 (DAM) they interacted in 18 periods of the DAM and in part 4 (SDG) in six periods of the SDG. In parts 5 and 6 (SWB 2 and SVO 2, respectively) participants had again to self-assess their subjective well-being and make money allocation decisions to measure post interaction social value orientation. Part 7 (sSDG) consisted of a 'surprise' restart of the SDG, lasting for 12 periods. In part 8, participants answered questions about their individual characteristics. As mentioned above we focus on the description of the main building blocks of the experiment, that is Parts 3, 4, and 7.

Parts 3 and 4: DAM and SDG. To explore the potential effects of market experience on the efficiency of cooperation we wanted the SDG to start immediately after the markets closed. To achieve this, participants received the instructions for DAM and SDG together. This appears to be a more natural setting than the alternative where participants are ignorant of (potential) further interactions after having traded in a market.[11] After having read the instructions and before the start of DAM participants had to answer comprehension questions about both DAM and SDG.

In each of the two market treatments participants interacted in the DAM for 18 periods and in each period there were the same three sellers and five buyers. Each seller was endowed with two units of a good which could be sold to the buyers and each buyer could buy up to two units. Thus, total market supply was six units and total market demand

in that way allows for a cleaner comparison between 'short run' and 'long run' effects of market interaction on the efficiency of cooperation.

[11] Knowing that there will be a SDG after the market may have the potential to affect market behavior. However, the literature on double auction market experiments overwhelmingly shows that market forces wipe out other concerns (Davis and Holt, 1993). Our market results reported below corroborate these findings and do not suggest that the information given on the SDG has affected market behavior in a substantial way.

ten units, implying that buyers were on the long side of the market. We chose to give every trader two units (instead of only one) to create a thicker market with more trades without having to increase the number of traders. The production costs of each unit of the three sellers was 10 and the redemption value of each unit of the five buyers was 100. This gives a so-called box design with perfectly inelastic supply and demand (Holt et al., 1986). We chose that design because it creates distinct market experiences for agents on respectively the short and the long side of the market. Moreover, as traders on each side have identical market positions their behavior can be cleanly compared.

The earnings from the sale of a unit were equal to the price at which the unit was traded minus production costs of 10, while the earnings from the purchase of a unit were equal to 100 minus the price at which the unit was traded. Not traded units created neither gains nor losses. The price was allowed to have any integer value between 10 and 95 (inclusive). We chose this upper bound on the trading price to break indifference and facilitate trade (Davis and Holt, 1993; Noussair and Tucker, 2013).

More formally, in each period the earnings of a buyer in the market were given by

$$u = \begin{cases} (100 - p_x) + (100 - p_y) & \text{if the buyer buys one unit at price } p_x \\ & \text{and another unit at price } p_y \\ (100 - p_z) & \text{if the buyer buys one unit at price } p_z \\ 0 & \text{if the buyer does not buy any unit,} \end{cases}$$

and the profit of a seller is given by

$$\pi = \begin{cases} (p_x - 10) + (p_y - 10) & \text{if the seller sells one unit at price } p_x \\ & \text{and another unit at price } p_y \\ (p_z - 10) & \text{if the seller sells one unit at price } p_z \\ 0 & \text{if the seller does not sell any unit,} \end{cases}$$

where $p_x, p_y, p_z \in \{10, 11, \ldots, 94, 95\}$.

The markets were anonymous and, depending on the market role, a trader knew her own production cost or redemption value, but did not know those of the other traders. Hence, traders did not receive information about the earnings of the other market participants. Participants were informed about the total number of buyers and sellers active in the market. We chose this information regime because it has been shown to minimize potentially confounding factors, like fairness considerations, and, thus, allows us to focus on the effects of market interaction *per se*. It also facilitates convergence to the competitive equilibrium (Smith, 1976; Holt et al., 1986) guaranteeing asymmetric market experience of buyers and sellers as intended. In the competitive equilibrium all six units are traded at

price 95. Sellers' per unit equilibrium profit is 85 $(95-10)$ and buyers' per unit equilibrium earnings are 5 $(100-95)$.

In the DAM traders had to follow particular trading rules equivalent to those used in previous double-auction market experiments:

1. Buyers make purchase offers and sellers make sale offers. A purchase offer consists of a price at which to buy a unit. A sale offer consists of a price at which to sell a unit.
2. Only the highest purchase offer and the lowest sale offer are the so-called pending prices at which transactions can take place.
3. A transaction takes place automatically if the price of a purchase (sale) offer that is made is equal or higher (lower) than the price of the pending sale (purchase) offer. The transaction price is always the pending price, regardless of the offer that leads to the transaction. A transaction also takes place if a pending purchase (sale) offer is accepted by a seller (buyer).
4. New price offers have to be improvements. That is, a new purchase (sale) offer has to be higher (lower) than the pending purchase (sale) offer.
5. If a transaction takes place the market clears and any purchase offer or sale offer in the feasible price interval is possible again.
6. The units of the good are traded one by one. That is, traders cannot make offers for or trade several units at a time.

The DAM was conducted for 18 consecutive periods with the same fixed group of eight participants. Participants in a market did not know who they were matched with. A trading period ended after three minutes or when no trades were possible any more. All participants were informed about their role in the market, buyer or seller, at the beginning of the 18 periods of the DAM and were also told that these roles would stay constant throughout these periods. During the DAM buyers and sellers could see the purchase and sale offers and transaction prices but not the identities behind the offers and transactions. Hence, traders could not track others' individual behaviors across market periods. When a trade took place, traders received information only about their own earnings. At the end of a trading period each trader received information about his or her total earnings in that period.

Immediately, after the 18 periods of the DAM, participants played six periods of the SDG. The SDG was a two-person linear public goods game and pairs stayed the same throughout the game. In each period each participant was endowed with 50 ECU and had to distribute them between a private and a public account. We used an MPCR $= 0.9$

so that for every unit that a player put into the public account both players in the pair obtained 0.9 units.[12] Formally, in each period of the SDG, earnings of a participant i were given by

$$w_i = 50 - g_i + 0.9(g_1 + g_2),$$

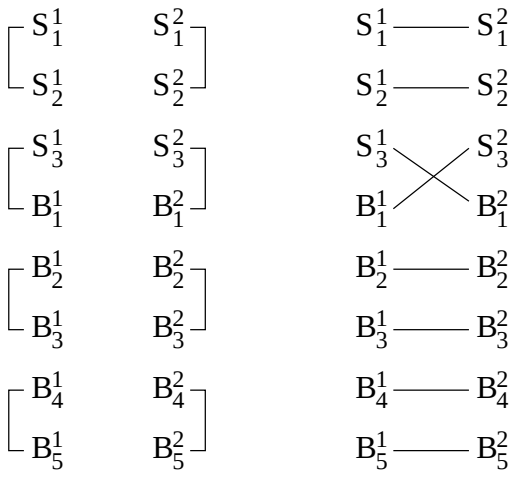
with g_i $(i = 1, 2)$ being player i's amount allocated to the public account. In the SDG, contribution decisions were made simultaneously. After each participant had made his/her decision each pair received information about decisions in their pair; that is, own contribution, other's contribution, own earnings, and other's earnings.

As already mentioned above, the matching in the SDG differed between the two market treatments, called *Market-Partners* and *Market-Strangers*. In the Market-Partners treatment each participant was matched with one of the other seven participants from the *same* DAM. Matching was done such that it led to two pairs of buyers, one pair of sellers and one pair consisting of a buyer and a seller. Specifically, the instructions specified: "You will be matched with another buyer (seller) with whom you have interacted in the market." Hence, in the SDG, participants knew the market role of the other participant they have been paired with. They were also told that they would stay matched with the same person during the six periods of the SDG. In this way we created two pairs of prospective market-losers (buyer pairs), one pair of prospective market-winners (seller pairs) and one pair consisting of a prospective market-loser and market-winner (mixed pairs).

In the Market-Strangers treatment each participant in a DAM was matched with one other participant from *another* DAM. Here the instructions specified: "You will be matched with another buyer (seller) from another market with whom you have not interacted in the market." In this case the matchings for the SDG were made using participants from two different DAMs. The sixteen subjects were matched in a way that led to four buyer-pairs, two seller-pairs, and two mixed pairs. Like in Market-Partners, market roles were known and the described matchings stayed the same for all periods of the SDG and participants were informed about this. Figure 1 provides a graphical representation of the matchings in Market-Partners and Market-Strangers, respectively.

Part 7: sSDG. After the six periods of SDG 1 (and after SVO 2) a surprise restart of the SDG was announced and participants played an additional 12 periods of the SDG. Each participant was informed that they would be matched with the same person as in the first six periods. We introduced the surprise restart to check for persistence of any

[12]The two-person version of the public goods game allowed us to obtain a relatively large number of independent observations at relatively low costs. The chosen MPCR was informed by pilot sessions with stand-alone two-person public goods experiments with the same subject pool as in the reported experiments. There we observed that an MPCR = 0.9 lead to efficiency levels of about 50 percent, leaving about the same room for efficiency improvement and worsening, respectively, in the market treatments.



(a) Market-Partners **(b)** Market-Strangers

Note: S_i^m (B_j^m) denotes seller (buyer) i (j) in market m.

Figure 1. Matchings in the SDG in Market-Partners and Market-Strangers

market experience effect and, for convenience, refer to this distinction as short run vs. long run. It allows us to see if effects on the efficiency of cooperation would be robust to a re-setting and longer lasting, an issue that is certainly relevant in market environments in the field.

3.2 Non-market Treatments

As a benchmark to which to compare contribution behavior in the social dilemma game after the market interaction, we ran treatments where participants played a SDG without having experienced market interaction before. In these treatments, except for the absence of a DAM, the sequence of events was exactly the same as depicted in Table 1. Like in the market treatments, each participant was matched with the same other person both in the first six and the second 12 periods of the SDG. We call these treatments OSDG (standing for 'Only' SDG). We have a treatment that is completely parallel to Market-Partners, which we will refer to as OSDG-MP, and one parallel to Market-Strangers, denoted by OSDG-MS.

A crucial feature of the OSDG treatments is that participants received initial lump-sum payments of money, which corresponded to the average earnings of participants in different conditions of the market treatments. As we will see in the results part, there are large earnings differences between sellers and buyers in the DAM. The initial lump-sum payments participants received were meant to control for potential effects of these differences on contribution behavior in the SDG.[13] The use of a lab experiment makes it

[13]We chose to implement average earnings as lump-sum payments instead of the exact distributions

possible to control for income differences in this way and, hence, to isolate the effects of market participation net of earnings differences.

In each OSDG treatment, each participant was in one of two payment conditions. The conditions differed with respect to the received lump-sum payment, which corresponded respectively to the average buyer and seller earnings in Market-Partners and Market-Strangers. The instructions for the SDG in these benchmark treatments were kept as close as possible to those in the DAM. Regarding the lump-sum payments and the matching with another participant in the OSDG the instructions said: "You have been assigned initial earnings of X ECU. The other group member is also assigned some initial earnings. The assignments to you and the other group member are not necessarily the same. You and the other group member will receive this amount independently of what occurs during the experiment."[14] We deliberately used a vague phrasing regarding the earnings of the other group member because in the DAM participants also only knew their own market earnings for sure. Market earnings of other traders could not be known because participants did not receive any information about traders' redemption values and production costs.[15] We describe the exact lump-sum earnings and corresponding matchings in the OSDG treatments after we have discussed behavior in markets and thus know earnings from market interaction for all types of traders (see end of Section 6.1).

4 Experiment Procedures

In total 448 subjects participated in the described main treatments of our experiment.[16] We ran three sessions with the Market-Partners treatment, four with the Market-Strangers treatment and three with the OSDG treatments. We have data from 112 subjects in Market-Partners in 14 separate markets, 192 subjects in Market-Strangers in 24 separate markets, thus 12 interlinked markets, and 144 subjects in OSDG in 72 separate pairs. For Market-Partners we have 56 pairs in the SDG (28 buyer-pairs, 14 seller-pairs,

of earnings for two reasons. First, because it highly simplifies the already complex design and, second, because within the set of buyers and sellers, respectively, earnings differences are relatively small (see Section 6.1 for average earnings and standard errors of earnings).

[14]The actual amount X of ECU used in the instructions depended on the condition the participant was assigned to (see Section 6.1 below).

[15]It has been suggested to us that in the market treatments participants could use observed transaction prices and the dynamics of the market to infer something about the earnings of the other side of the market. We acknowledge that this is not impossible, but believe that the earnings information possibly extracted is too noisy to have a significant effect. Nevertheless, to check whether full transparency regarding the lump-sum income changes contributions in the SDG, we conducted an additional treatment that exactly matches the OSDG-MP but reveals the information about lump-sum incomes to both participants in a pair. For details and results, see Section 7.

[16]In addition, respectively 90 and 174 subjects participated in two additional non-market treatments (see, Section 7) giving a total of 712 participants.

14 mixed-pairs) organized in 14 independent matching groups (markets) and for Market-Strangers we have 96 pairs in the SDG (48 buyer pairs, 24 seller pairs, 24 mixed pairs) organized in 12 independent matching groups (interlinked markets across which participants are matched in the subsequent SDG). In OSDG the 72 statistically independent observations (i.e., matched pairs of participants in the SDG) are distributed over six different lump-sum payment conditions with 12 independent pairs per condition. These lump-sum payment conditions mirror the buyer pairs, seller pairs, and mixed pairs in the Market-Partners and Market-Strangers treatment, respectively (see Section 6.2 for details). Table 2 provides an overview of the treatments, number of subjects, number of pairs in the SDG and number of independent observations in each treatment and pair, respectively.

Table 2. Summary of market treatments and main non-market treatments

Market-Partners			Market-Strangers		
$N = 112$			$N = 192$		
$n = 14$			$n = 12$		
buyer pairs	seller pairs	mixed pairs	buyer pairs	seller pairs	mixed pairs
$N_{bp} = 28$	$N_{sp} = 14$	$N_{mp} = 14$	$N_{bp} = 48$	$N_{sp} = 24$	$N_{mp} = 24$
$n_{bp} = 14$	$n_{sp} = 14$	$n_{mp} = 14$	$n_{bp} = 12$	$n_{sp} = 12$	$n_{mp} = 12$
OSDG-MP			OSDG-MS		
$N = 72$			$N = 72$		
$n = 36$			$n = 36$		
'buyer pairs'	'seller pairs'	'mixed pairs'	'buyer pairs'	'seller pairs'	'mixed pairs'
$N_{bp} = 12$	$N_{sp} = 12$	$N_{mp} = 12$	$N_{bp} = 12$	$N_{sp} = 12$	$N_{mp} = 12$
$n_{bp} = 12$	$n_{sp} = 12$	$n_{mp} = 12$	$n_{bp} = 12$	$n_{sp} = 12$	$n_{mp} = 12$

Note: $N(n)$... number of subjects (independent observations) on treatment level; $N_{xp.}(n_{xp})$... number of pairs (independent observations on pair level); 'buyer/seller/mixed pairs' indicates lump-sum payment condition mirroring buyer/seller/mixed pairs in the Market-Partners treatment and Market-Strangers treatment, respectively.

In the two market treatments, each participant's role (buyer or seller) was fixed for the duration of the session. General instructions were read out aloud at the start of each session. Instructions for the different parts were given on-screen and participants could read them at their own pace.[17] Participants could ask questions by raising a hand. All questions were answered in private.

The experiments were conducted at the LINEEX lab at the University of Valencia using the z-tree program of Fischbacher (2007). Each session involved one of the treatments and no one could participate in more than one session.[18] Performance-based earnings were

[17]The main reason for not reading out aloud all instructions was that this would have revealed information about the potential earnings of buyers and sellers in DAM, which we wanted to avoid.

[18]The market sessions have been run in February, March and June 2012 and the non-market sessions in June 2015. The reason for the relatively large time gap between sessions is that originally we had conducted a control treatment without any pre-SDG earnings. We were convinced by discussants in seminars that this was not the best control treatment as it does not control for the earnings differences generated in the

counted in ECU and total earnings consisted of the accumulated earnings across all parts. Each 100 ECU were worth €1. Participants did not receive a show-up fee. At the end of a session participants were privately paid out their earnings in cash. Average earnings were €33.00 for OSDG and €29.50 for the market treatments. Non-market sessions took about 90 minutes and sessions with market treatments took about 120 minutes.

5 Research Questions

Our research questions relate directly to the views of Smith (1998) and Bowles (1998) presented in the Introduction and to the distinction between state-dependent preferences and separable preferences between economic incentives and social preferences introduced by Bowles and Polania-Reyes (2012). The notion of separability is also implied in the view proposed by Smith (1998) stating that people are able to decouple behavior in small-group exchange from that in anonymous markets. In the context of our experiment, separability means that the ability to efficiently cooperate in a social dilemma game is independent of preceding market experience.

Alternatively, behavior can depend on the circumstances surrounding the decision situation, which can be captured by the notion of state-dependence.[19] In the words of Bowles and Polania-Reyes (2012): "State-dependence arises because actions are motivated by a heterogeneous repertoire of preferences from spiteful to payoff-maximizing to generous, for example, the salience of which depends on the nature of the decision situation" (p. 373).[20] Applied to our research this implies that preferences and behavior could be state-dependent in the general sense that market experience affects subsequent cooperation. Moreover, the effect could be positive and increase cooperation, in accordance with the idea of *doux commerce* of Montesquieu (1748) or it could be negative, in line with the social criticism of Engels (1845), and decrease subsequent cooperation.

As advanced in the Introduction, our focus is on (i) whether market interaction as

market. The control treatments OSDG-MP and OSDG-MS take care of this issue. For completeness we note that a Kruskal-Wallis test across the first OSDG and the reported OSDG-MP and OSDG-MS does not detect a difference in contributions ($p \geq 0.6648$).

[19] Bowles and Polania-Reyes (2012) distinguish between state-dependent and endogenous preferences. In their framework, the term endogenous preferences is used in relation to processes with effects that persist in the long run, typically as the result of a process of cultural transmission. In the context of our study, the effects we focus on can be better captured in terms of state-dependence.

[20] An example of how state-dependence could be incorporated into a formal model of social preferences is the general model of Charness and Rabin (2002). This two-person model has a more standard part with own and other's payoff and also incorporates a particular parameter that is said to be set to 1 when the decision-maker thinks that the counter-part is misbehaving while it is set to 0 when the counter-part is not misbehaving. The state is whether the counter-part is misbehaving or not and this gives rise to a repertoire of two different social preferences.

such affects subsequent behavior and (ii) whether particular variations in the nature of the market interaction will lead to variations in the efficiency of cooperation. Our design makes it possible to make a number of specific comparisons of interest.

First, we can separately compare behavior of agents who competed with each other on the same market (Market-Partners) and behavior of agents who experienced market interaction on different markets (Market-Strangers) to the behavior in the corresponding OSDG treatments as well as compare Market-Strangers with Market-Partners as such. One may expect the different kinds of relations in the market to differentially affect participants' attitudes towards the subsequent interaction. In Market-Partners the experience of having competed with each other for scarce resources may on the one hand inject some sense of social closeness and thus increase cooperation, but on the other hand it may also induce a competitive state that could be detrimental to efficient cooperation. The Market-Strangers setting may create an atmosphere of more anonymity and disconnectedness and thereby decrease the motivation to cooperate. On the other hand, the observations of Henrich et al. (2001) and Henrich et al. (2004) discussed in the Introduction suggest that market experience with strangers may have a positive effect on cooperation. Thus, a priori, it is an open question whether market interaction leads to more or less efficient cooperation in Market-Partners and Market-Strangers, respectively. We summarize this thoughts in our first research question.

Research Question 1. *(a) Does market experience in Market-Partners and Market-Strangers affect the efficiency of cooperation positively or negatively relative to the corresponding non-market treatments OSDG-MP and OSDG-MS? (b) Does the efficiency of cooperation differ between Market-Partners and Market-Strangers? (c) Are there differences between the short run and the long run?*

Further, continuing with the separation between Market-Partners and Market-Strangers we can disaggregate and compare behavior in different matchings of participants who have been on opposite sides or the same side of the market. In the latter case we can also compare whether the market side itself matters. These comparisons are directly related to the potentially differential effects of experiencing market interaction on respectively the long and short side of markets, as mentioned in the citation from Bowles (1998) reproduced in the Introduction. They are of particular interest, because they touch on the important societal issue whether market experience has different repercussions for those who have it easy in the market (market-winners) compared to those who have a hard time (market-losers), although the term 'personality effects' may be too strong in the context of our experiment. These disaggregated comparisons will all be made with respect to the

corresponding lump-sum payment conditions in OSDG-MP and OSDG-MS, respectively, so as to isolate the effect of market experience net of earnings differences. In addition, we can also make comparisons across market treatments to explore if the experience of being on respectively the long and short side of the market has differential effects in Market-Partners and Market-Strangers. This can be summarized in the following question.

Research Question 2. *(a) Is the efficiency of cooperation of respectively market-winners, market-losers and traders from opposite market sides each compared with the corrresponding cases in the non-market treatments affected differentially? (b) Does the efficiency of cooperation of these different pairs of traders depend on whether market interaction took place in Market-Partners or Market-Strangers? (c) Are there differences between the short run and the long run?*

6 Results

In this section, we first briefly report on market behavior to see if our markets indeed converge to asymmetric equilibrium outcomes as intended. Thereafter, we zoom in to our research questions and discuss if and how different market experiences affect behavior in the subsequent social dilemma games.[21]

6.1 Market Behavior

Figure 2 shows the average transaction price over the 18 trading periods in the two market treatments. As expected, prices in both treatments converge to the highest possible price of 95. Of the total of 4104 possible trades only 7 were not realized and overall efficiency was with 99.8% virtually optimal. Thus, markets clear, are efficient and lead to very unequal incomes. Using individual data, the averages (standard errors) of earnings are 2672 (st.dev.: 277, st.err.: 43) for sellers and 340 (st.dev.: 176, st.err.: 21) for buyers in the Market-Partners treatment and 2656 (st.dev.: 324, st.err.: 38) for sellers and 346 (st.dev.: 222, st.err.: 20) for buyers in the Market-Strangers treatment.[22]

As expected, neither buyer nor seller earnings significantly differ between Market-Partners and Market-Strangers (buyer earnings: $p = 0.6434$, seller-earnings: $p = 0.5371$; MW-tests, 2-sided). We can conclude that our manipulation worked as intended. Thus, in both implemented market environments we achieved an efficient allocation of resources

[21] Results regarding the effect of different experiences in the markets and social dilemma games on subjective well-being and social value orientation can be found in Appendix C.

[22] Note that if there are any pre-existing social preferences they apparently have little effect on the outcome of the market interaction, due to the competitiveness of the institution (see Bolton and Ockenfels, 2000).

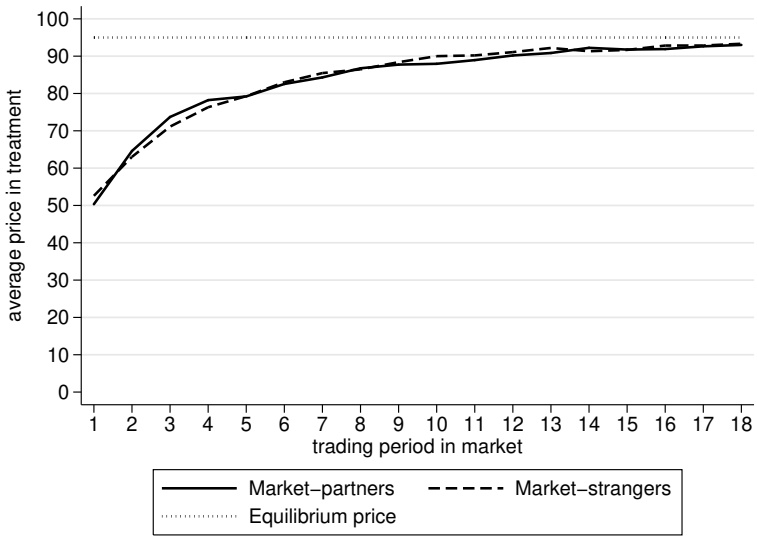


Figure 2. Average trading price dynamics in both market treatments

with very different market experiences for participants on the long and on the short side of the market.

Before moving on to the discussion if and how market experience affects the efficiency of cooperation we briefly explain how the different lump-sum payment conditions in the non-market treatments OSDG-MP and OSDG-MS were created. The idea was to match lump-sum (i.e., non-market) earnings of pairs of subjects in OSDG-MP and OSDG-MS with average earnings of buyer pairs, seller pairs and mixed pairs in Market-Partners and Market-Strangers, respectively. We have just seen that in Market-Partners sellers earned on average 2672 ECU and buyers 340 ECU. In Market-Partners the corresponding earnings were 2656 ECU and 346 ECU. To control for these income differences, in OSDG-MP and OSDG-MS we assigned participants to the following lump-sum earnings pairs: 340-340, 2672-2672, and 2672-340, respectively, to mimic buyer-buyer, seller-seller, and seller-buyer matchings in Market-Partners and 346-346, 2656-2656, 2656-346 to mimic the equivalent matchings in Market-Strangers.

6.2 Efficiency of Cooperation after Market Interaction

In our presentation of results we use the research questions posed in Section 5 as a guide. We start with Research Question 1 where we look at potential effects of market experience *per se* in Market-Partners and Market-Strangers, respectively, and do not distinguish between different trader and lump-sum payment types. Table 3 shows descriptive statistics (medians, means, and standard deviations) for the efficiency of cooperation in the social dilemma game in Market-Partners, Market-Strangers, OSDG-MP, and OSDG-MS,

respectively, as well as relevant bootstrap t-tests of differences between treatments.[23] As explained above we also distinguish between the short run and the long run.

Table 3. Efficiency of cooperation in market and no-market treatments (across trader matchings)

		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	23.740	23.025	7.556	26.120	22.172	7.502
OSDG-MP	36	33.167	30.801	13.814	41.104	32.766	15.659
Market-Strangers	12	32.146	31.418	3.154	31.357	30.422	3.612
OSDG-MS	36	27.500	28.468	12.197	34.708	31.669	14.551
MP vs OSDG-MP			$p = 0.024^{**}$			$p = 0.005^{**}$	
MS vs OSDG-MS			$p = 0.205$			$p = 0.660$	
MP vs MS			$p = 0.006^{**}$			$p = 0.001^{***}$	

Note: all statistics and tests are based on strictly independent observations; p-values come from bootstrap two-sample t-tests, two-sided; ***, **, * significant at least at the respectively 1%, 5%, 10% level with false discovery rate correction for multiple (six) comparisons (Benjamini and Hochberg, 1995); MP (MS) stands for Market-Partners (Market-Strangers).

Focusing first on the Market-Partners treatment we see that contributions in Market-Partners are significantly lower than in the corresponding OSDG-MP. This holds for the short run ($p = 0.024$) as well as the long run ($p = 0.005$).[24] Note that the average difference in the short run amounts to about 15.6 percent of the endowment (7.8 out of 50) and increases to about 21.2 percent of the endowment (10.6 out of 50) in the long run. The negative effect of market experience is thus also economically substantial. In contrast, there is no such negative effect in the Market-Strangers treatment, neither in the short run ($p = 0.205$) nor in the long run ($p = 0.660$). Finally, a comparison of Market-Partners with Market-Strangers shows that the efficiency of cooperation is substantially and significantly lower in the former than in the latter. Again this holds for the short and long run ($p = 0.006$ and $p = 0.001$).[25] We summarize in our first result.

[23] Unless indicated otherwise, all reported tests are two-sided bootstrap two-sample t-tests based on strictly independent observations (i.e., matched pairs in OSDG-MP and OSDG-MS, markets in Market-Partners, and interlinked markets in Market-Strangers). In the tables we report uncorrected p-values as well as significance levels corrected for multiple testing. For details, see the table notes. In Appendix B.1 we also provide non-parametric Mann-Whitney tests. The attained significance levels are largely the same as those provided here. In case of interesting differences we mention them in the text. We use bootstrap t-tests because t-tests also use the rich cardinal information contained in the data, whereas Mann-Whitney tests are based solely on the ordinality of the data. Applying the bootstrap technique allows us to conduct t-tests without making any assumptions about the distribution of the data. For a discussion of this method and the application to experiment data see, e.g., Moffat (2015).

[24] For ease of exposition, in the main text we refer to uncorrected $p-$values. For significance levels after correction for multiple testing, see the tables.

[25] Regression analysis controlling for time trends and initial social value orientation (SVO 1) corroborates the test results reported here. In fact, the significance levels are stronger: the comparisons MP vs OSDG-

Result 1. ***(i)*** *Market experience strongly harms the efficiency of cooperation when traders have to solve the social dilemma with other traders they had previously interacted with in the same market. This holds in the short and in the long run and the negative effect tends to get larger in the long run.* ***(ii)*** *Market experience does not have a detrimental effect on the efficiency of cooperation when traders have to solve the social dilemma with other traders they did not interact with on the market before. This holds in both, the short and the long run.*

Hence, across trader pairings, we find that market participation can be harmful for cooperation but find also that it is not harmful *per se* and that this pattern holds in the short run as well as the long run.

We next move to Research Question 2, whether the aggregate differences just discussed are similar for the different trader matchings or whether they are driven by specific matchings, and how this differs between Market-Partners and Market-Strangers. Recall that in buyer pairs the interacting participants both have had a difficult time in securing trades while in seller pairs participants have competed for trades from a relatively comfortable position. Finally, mixed pairs bring together very different market experiences. Table 4 reports descriptive statistics of contributions and corresponding tests for the three types of trader matchings, buyer-buyer, seller-seller and buyer-seller, in Market-Partners and Market-Strangers, respectively. The corresponding income matchings in OSDG-MP and OSDG-MS for convenience are called low-pay (340-340 and 346-346), high-pay (2672-2672 and 2656-2656), and mixed-pay (340-2672 and 346-2656).

Focusing on buyer-buyer pairs first we see that the pattern of contributions is the same as for the aggregate data shown in Table 3 above. Specifically, buyer-buyer pairs contribute less in Market-Partners than low-pay pairs in OSDG-MP, in the short run ($p = 0.003$) as well as in the long run ($p = 0.039$).[26] Again the difference in average contributions between Market-Partners and OSDG-MP is economically substantial amounting to 25 and 28.6 percent of the endowment in the short run and in the long run, respectively.[27] Also similar to the results across trader pairs, no such differences are found when comparing buyer-buyer pairs in Market-Strangers with low-pay pairs in OSDG-MS ($p \geq 0.311$).

MP (long run) and MP vs MS (short and long run) are significant at the 1% level and MP vs OSDG-MP (short run) at the 5% level after false discovery rate correction for six pair-wise comparisons. For details, see Appendix B.2.

[26] Interestingly, Mann-Whitney tests show even stronger significance results with $p = 0.0040$ in the short run and $p = 0.0139$ in the long run and both comparisons are significant at the 5% level after correction for multiple comparisons. See Appendix B.1 for details.

[27] Cárdenas et al. (2014) report on a somewhat related result in a field experiment. Using an ultimatum game, they find that ex-combatants (losers in the armed conflict) expect to and actually receive lower transfers from public officers and citizens than victims and control groups.

Table 4. Efficiency of cooperation in market treatments and non-market treatments for the different trader matchings

Buyer-buyer pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	22.042	21.729	8.751	15.000	18.682	11.849
OSDG-MP (low-pay)	12	33.542	34.243	9.633	41.104	33.038	15.326
Market-Strangers	12	30.229	29.236	5.854	28.729	27.990	6.707
OSDG-MS (low-pay)	12	33.125	32.597	9.083	35.875	33.674	13.375
MP vs OSDG-MP		$p = 0.003^{**}$			$p = 0.039^{*}$		
MS vs OSDG-MS		$p = 0.311$			$p = 0.269$		
MP vs MS		$p = 0.018^{*}$			$p = 0.053^{*}$		
Seller-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	18.250	21.768	12.413	21.042	20.699	14.385
OSDG-MP (high-pay)	12	26.500	29.097	15.536	38.521	34.222	15.634
Market-Strangers	12	34.396	35.486	7.428	34.573	35.168	7.323
OSDG-MS (high-pay)	12	23.833	23.785	11.064	33.917	31.649	14.522
MP vs OSDG-MP		$p = 0.249$			$p = 0.048^{*}$		
MS vs OSDG-MS		$p = 0.018^{**}$			$p = 0.505$		
MP vs MS		$p = 0.020^{**}$			$p = 0.006^{**}$		
Buyer-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	27.125	26.875	15.507	34.167	30.625	16.213
OSDG-MP (mixed-pay)	12	32.875	29.063	15.926	35.604	31.038	17.182
Market-Strangers	12	29.271	31.715	7.368	28.563	30.540	9.051
OSDG-MS (mixed-pay)	12	33.542	29.021	15.019	33.500	29.684	16.590
MP vs OSDG-MP		$p = 0.738$			$p = 0.953$		
MS vs OSDG-MS		$p = 0.597$			$p = 0.890$		
MP vs MS		$p = 0.356$			$p = 0.988$		

Note: all statistics and tests are based on strictly independent observations; p-values are from bootstrap two-sample t-tests with 999 repititions (seed = 713), two-sided; ***, **, * significant at least at the, respectivley, 1%, 5%, 10% level with false discovery rate correction for multiple (six) comparisons (Benjamini and Hochberg, 1995); MP (MS) stands for Market-Partners (Market-Strangers); in OSDG-MP and OSDG-MS 'low-pay' corresponds to income matchings 340-340 and 346-346, respectively, 'high-pay' to income matchings 2672-2672 and 2656-2656, respectively, and 'mixed-pay' to income matchings 340-2672 and 346-2656, respectively.

Together this implies that buyer-buyer pairs in Market-Partners achieve significantly lower cooperation efficiency than buyer-buyer pairs in Market-Strangers, again in the short run ($p = 0.018$) as well as the long run ($p = 0.053$).[28]

For seller-seller pairs in the Market-Partners treatment the contributions pattern is similar to the one observed for buyer-buyer pairs but appears to be less pronounced. Seller-seller pairs contribute less in Market-Partners than high-pay pairs in OSDG-MP in the short run as well as the long run, but the difference is statistically significant only in the latter case ($p = 0.249$ and $p = 0.048$). Looking at the Market-Strangers treatment we see that seller pairs contribute *more* than high-pay pairs in OSDG-MS. Now the difference is significant in the short run ($p = 0.018$) but not in the long run (0.505). Finally, when comparing the two market treatments with each other we observe that the efficiency in Market-Partners is significantly and substantially lower than in Market-Strangers in the short ($p = 0.020$) and the long run ($p = 0.006$). Together this suggests that for seller-seller pairs there is some negative effect of experienced market interaction, but only when this interaction is in Market-Partners.

For mixed buyer-seller pairs there are no significant differences detected when comparing Market-Partners with OSDG-MP, in the short run ($p = 0.738$) as well as the long run ($p = 0.953$), Market-Strangers with OSDG-MS (short run: $p = 0.597$; long run: $p = 0.890$), and Market-Partners with Market-Strangers (short run: $p = 0.356$; long run: $p = 0.988$). Thus, the efficiency of cooperation of traders who have been on opposite sides of the market is not hampered by market experience, irrespective of having interacted on the same or different markets.[29,30] We summarize in our next result.

[28] Again, Mann-Whitney tests tend to produce stronger significance results. In particular, after correction for multiple comparisons, the difference between Market-Partners and Market-Strangers remains significant at the 5% level. See Appendix B.1 for details.

[29] All test results reported her are corroborated by Tobit regression analyses where we control for initial social value orientation (SVO 1) and period effects with period dummies. Again, significance levels tend to be stronger using regression analyses: after false discovery rate correction for six pair-wise comparisons, for buyer-buyer pairings all comparisons that are significant at at least the 10% level in Table 4 are significant at the 5% level, for seller-seller pairs all comparisons that are significant at the 5% (10%) level in Table 4 are significant at the 1% (5%) level. For details see Tables B.8–B.17 in Appendix B.2.

[30] When taking the very conservative statistical approach and correcting for all 18 comparisons reported in Table 4 we find that the following comparisons remain significant at the 10% level: MP vs OSDG-MP for buyer-buyer pairs in the short run, MS vs OSDG-MS for seller-seller pairs in the short run, MP vs MS for seller-seller pairs in both the short and the long run, and MP vs MS for buyer-buyer pairs in the short run. Of these comparisons, for buyer-buyer pairs MP vs OSDG-MP (short run) and for seller-seller pairs MP vs MS (long run) just miss to reach significance at the 5% level. In addition, the comparisons of MP vs OSDG-MP and MS vs OSDG-MS in the short run just miss to reach significance at the 10% level. Again, when using Mann-Whitney rank sum tests stronger significance results are achieved. With these tests for buyer-buyer pairs the comparisons MP vs OSDG-MP in the short and long run are significant at the 5% level and for seller-seller pairs this is the case for the comparisons MS vs OSDG-MS in the short run and MP vs MS in both the short and long run.

Result 2. ***(i)*** *The observed overall adverse effects of market experience on the efficiency of cooperation in Market-Partners can be mainly attributed to market-loser pairs (buyer-buyer pairs) and, to a lesser extent, also market-winner pairs (seller-seller pairs).* ***(ii)*** *The efficiency of cooperation in Market-Strangers is unaffected for market-loser pairs and tends to be enhanced for market-winner pairs, in the short run.* ***(iii)*** *For both, market-loser and market-winner pairs, the efficiency of cooperation is lower in Market-Partners than in Market-Strangers.* ***(iv)*** *The efficiency of cooperation in trader pairs composed of market-winners and market-losers (buyer-seller pairs) is unaffected by previous market interaction.*

The result that market experience has no effect whatsoever on traders who have been on opposite sides of markets (buyer-seller pairs) raises the question whether this is due to that both traders' contributions are unaffected or that one trader type contributes more while the other trader type contributes less. To test for this we looked at contributions of both types separately in the short run and the long run. In addition, as short run and long run levels may be affected by the dynamics of interaction, we also looked at contributions in the very first period of the first social dilemma game. Table 5 reports the results, which show that in buyer-seller pairs there is virtually no difference in contributions between buyers and sellers neither in the short nor in the long run. Also in period 1 the differences are small and statistically insignificant ($p = 1.000$ in Market-Partners and $p = 0.4546$ in Market-Strangers). From that we conclude that market experience does not affect the efficiency of cooperation in groups consisting of a market-winner and a market-loser.

Table 5. Contributions of buyers and sellers within buyer-seller matchings in market treatments

	Period 1		Short run		Long run	
Treatment & role	mean	st. dev.	mean	st. dev.	mean	st. dev.
Market-Partners						
Seller	32.500	18.989	27.357	17.176	31.155	15.758
Buyer	33.286	15.529	26.393	14.135	30.095	17.076
Market-Strangers						
Seller	34.917	14.163	30.757	8.582	29.615	9.282
Buyer	37.292	15.250	32.674	7.329	31.465	9.580

Note: 'Period 1' statistics are based on individual observations ($N = 14$ in Market-Partners, $N = 24$ in Market-Strangers); 'Short run' and 'Long run' statistics are based on strictly independent observations ($N = 14$ in Market-Partners, $N = 12$ in Market-Strangers).

In Appendix B we also report comparisons of the efficiency of cooperation between different trader pairs *within* in each market treatment. The main result of these comparisons is that traders who have competed on the same market (i.e., in Market-Partners) and on the same side of the market—either on the favorable side (seller-seller pairs) or on

the unfavorable one (buyer-buyer pairs)—achieve less efficient cooperation outcomes than pairs of traders who also have been in the same market but on opposite sides of it (buyer-seller pairs). By contrast, for Market-Strangers having been on the favorable side of the market causes higher subsequent cooperation levels than having been on the unfavorable side. Thus, having competed on the same side on a market induces cooperation losses, whereas there seems to be a market-winner cooperation rent, which can however only be 'cashed in' when the cooperation problem occurs with others who have not been in the same market.

7 Additional Control Treatments and Discussion

In the previous section we have seen that relative to our control treatments market interaction has a negative effect in Market-Partners, especially for buyer-buyer pairs, a less strong and only long-run effect for seller-seller pairs, and no effect for mixed buyer-seller pairs. Moreover, in Market-Strangers no adverse effects of market interaction have been detected.

In this section we check and discuss the robustness of the results in Market-Partners by presenting the results of two additional treatments without market interaction and comparing them to Market-Partners for all three trader matchings. First, recall that in the market treatments, sellers' production costs and buyers' redemption values were private information making it difficult for participants to infer the market earnings of other buyers and sellers, respectively. Accordingly, in our OSDG treatments participants received only vague information about the lump-sum payments of other participants (see Section 3.2 for details). However, it is conceivable that in the market treatments participants could have used observed transaction prices and trading dynamics to infer something about the earnings of the other traders, especially of the opposite side. To control for this we ran an additional OSDG with income transparency. This treatment was exactly the same as the OSDG-MP except that the lump-sum payments within each pair of participants interacting in the social dilemma game was made transparent when the instructions for the SDG were given. This treatment is labeled OSDG-MP-T.

In this treatment we collected data from 90 participants who did not participate in any of the sessions reported above.[31] As in OSDG-MP participants were partitioned into three sets of pairs with lump-sum income pairs of 340-340, 2672-2672, and 340-2672, respectively, giving 15 independent observations on the pair level.

[31] In total we conducted three sessions at LINEEX in December 2018. A session lasted between 65 and 70 minutes with average earnings amounting to € 32,80.

Second, we conducted another treatment, called OSDG-MP-RE, where RE stands for real effort. This treatment was exactly the same as the OSDG-MP, except for the novel feature that the social dilemma game was preceded by an individual real-effort task in which participants had to earn their lump-sum income. The motivation for this treatment was to design an environment which had the main features of the market treatments, but without market interaction.[32]

The real effort task consisted of 18 periods each lasting for 95 seconds, which was the average length of the 18 market periods in the Market-Partners treatment. This ensured that the time participants spent in the lab before the SDG was the same as in Market-Partners. In each of these periods participants faced the task of adjusting six sliders in the well-known slider task of Gill and Prowse (2012). Each correctly adjusted slider yielded a number of points. In the task participants were randomly assigned one of two exchange rates of correctly adjusted sliders into experimental currency units (ECU) which were valid for all 18 periods. Participants were informed of their exchange rate before the start of the slider task. One exchange rate was such that if a participant adjusted correctly all sliders in all periods, the participant would at the end of the 18 periods have earned an endowment equal to the average earnings of buyers in the Market-Partners treatment. The other exchange rate was such that if a participant correctly adjusted all sliders in all periods, the participant would at the end of the 18 periods have earned an endowment equal to the average earnings of sellers in the Market-Partners treatment. We calibrated the difficulty of the slider task so that all participants indeed earned an endowment equal to those used in the OSDG-MP treatment.[33]

Participants were informed that in the SDG they were paired with another participant who did exactly the same real effort task. In pairs resembling buyer-buyer (seller-seller) matchings, participants who earned 346 ECU (2672) ECU were informed that they are paired with another participant who had the same exchange rate in the real effort task. In pairs resembling buyer-seller matchings, participants with a low (high) exchange rate in the real effort task were informed that they were paired with another participant who had a superior (inferior) exchange rate. Participants were not informed about the actual performance and thus earnings of their paired participant. In this way we keep the information about others earnings similar to what participants could reasonably deduce from trading behavior in the markets of the Market-Partners treatment. For convenience, we will refer to the different pairings in this treatment also as respectively low-pay, high-pay, and mixed-pay pairings.

[32]We thank an anonymous reviewer who pointed out the importance of controlling for these features.

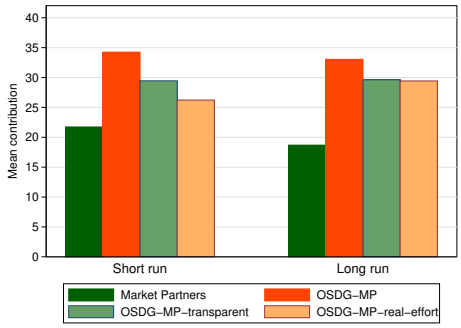
[33]The slider task was calibrated such that half of the participants earn 340 ECU and the other half 2672 ECU. Only one participant did not achieve this and earned 2647 ECU instead of the intended 2672 ECU. As the difference is minor and occurred in an early period, we do not exclude data of this participant.

Summarizing, in the OSDG-MP-T treatment (as in OSDG-MP) participants received either the buyer endowment or the seller endowment as a lump-sum payment. The difference between treatments is that in the OSDG-MP-T participants receive exact information about the earnings of their paired counterpart in the SDG. This treatment thus controls for information on the lump-sum earnings. In the OSDG-MP-RE treatment, the information on pre-SDG payments is similar to OSDG-MP but in contrast to the original control treatment participants had to exert effort to receive these earnings. This treatment controls for several aspects also present in the Market-Partners treatment. First, as in the market, participants have to exert effort to receive payments, second, the time spent in the experiment prior to the SDG is exactly the same as in Market-Partners, third, although not interacting, participants know that the other participants are also engaged in the same real effort task and, fourth, participants with low (high) exchange rates are exposed to repeated low (high) earnings and, presumably related frustration (elation) as buyers (sellers) in the markets. Thus, this treatment controls for a number of aspects that arguably are not unique to market interactions, although these aspects are inevitably linked with interaction on our markets.

We note that the OSDG-MP-RE treatment was pre-registered at the AEA registry with the following explicit directed hypotheses, which were guided by Result 2 above:

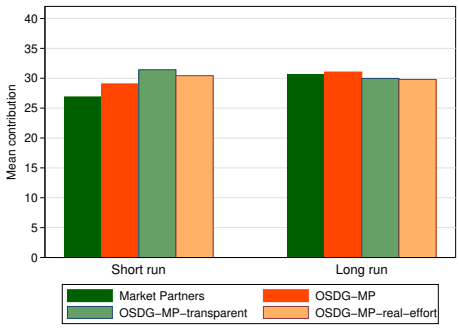
Hypothesis 1. ***(a)*** *Contributions in the buyer-buyer pairings are lower than contributions in the low-pay pairings.* ***(b)*** *Contributions in the seller-seller pairings are (weakly) lower than contributions in the high-pay pairings.* ***(c)*** *Contributions in the buyer-seller pairings are equal to contributions in the mixed-pay pairings.*

To get a first impression on how the efficiency of cooperation for the three pairings differs across treatments, Figure 3 shows average contributions for all treatments under consideration (Market-Partners, OSDG-MP, OSDG-MP-T, OSDG-MP-RE) in the short and the long run. There are several interesting observations to be made. First, for low-pay pairs (panel (a)) we see that for all three OSDG treatments contributions are higher than in Market-Partners, both in the short and the long run. There is also an interesting order within the OSDG treatments: contributions are highest in OSDG-MP and lowest in OSDG-MP-RE, with contribution in OSDG-MP-T being in-between. This order appears in the short and long run, although it is weaker in the latter case. Second, also for high-pay pairs (panel (b)) it holds that contributions are higher in all OSDG treatments than in Market-Partners, again both in the short and the long run. In contrast to low-pay pairs, however, there appears to be (almost) no difference across the three non-market treatments. Third, for mixed-pay pairs (panel (c)) there are few differences across all treatments, including the market treatment.



(a) Buyer-buyer & low-pay pairs

(b) Seller-seller & high pay pairs



(c) Buyer-seller & mix-pay pairs

Figure 3. Efficiency of cooperation in different treatments for different trader pairs

In the following we test for statistical differences in the efficiency of cooperation between Market-Partners and each of the two additional treatments separately. We base our directed hypotheses on the results observed in the previous section. That is, we test if the efficiency of cooperation is smaller in Market-Partners than in OSDG-MP-T and OSDG-MP-RE, respectively, for buyer-buyer vs low-pay pairs and seller-seller vs high-pay pairs. Additionally, we test the undirected hypothesis that for buyer-seller vs mixed-pay pairs, the efficiency of cooperation does not differ between Market-Partners and, respectively, OSDG-MP-T and OSDG-MP-RE.

Table 6 reports descriptive statistics of the two new treatments together with the descriptive statistics of Market-Partners, for convenience. It also reports the appropriate test statistics.[34] The table reports uncorrected $p-$values as well as significance levels after correcting for multiple comparisons within pairings using the false-discovery rate procedure. One can see that for buyer-buyer and seller-seller pairs, contributions in both OSDG-MP-T and OSDG-MP-RE are significantly higher than in Market-Partners, both in the short run and the long run at varying degrees of significance. By contrast, for buyer-

[34] All reported test results are corroborated by Tobit regression analyses (see Appendix B.2).

seller pairs there are no significant differences between the treatments. For the OSDG-MP-RE the results are consistent with our Hypotheses 1(a)-1(c) above, at significance levels varying between 5% and 10% (uncorrected and corrected).

Table 6. Efficiency of cooperation in Market-Partners treatment and OSDG-T and OSDG-RE for the different trader matchings

Buyer-buyer pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	22.042	21.729	8.751	15.000	18.682	11.849
OSDG-MP-T (low-pay)	15	28.333	29.450	16.268	35.833	29.622	20.295
OSDG-MP-RE (low-pay)	29	26.250	26.230	13.485	27.792	29.424	14.387
MP vs OSDG-MP-T[1]			$p = 0.066^{*}$			$p = 0.058^{*}$	
MP vs OSDG-MP-RE[1]			$p = 0.093^{*}$			$p = 0.011^{**}$	
Seller-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	18.250	21.768	12.413	21.042	20.699	14.385
OSDG-MP-T (high-pay)	15	23.083	29.150	13.767	28.125	29.625	15.679
OSDG-MP-RE (high-pay)	29	27.917	29.282	14.221	32.958	29.480	18.207
MP vs OSDG-MP-T[1]			$p = 0.082^{*}$			$p = 0.070^{*}$	
MP vs OSDG-MP-RE[1]			$p = 0.049^{*}$			$p = 0.052^{*}$	
Buyer-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	27.125	26.875	15.507	34.167	30.625	16.213
OSDG-MP-T (mixed-pay)	15	29.833	31.428	17.155	39.125	29.967	19.162
OSDG-MP-RE (mixed-pay)	29	30.417	30.431	12.592	28.958	29.802	16.251
MP vs OSDG-MP-T[2]			$p = 0.481$			$p = 0.925$	
MP vs OSDG-MP-RE[2]			$p = 0.485$			$p = 0.879$	

Note: all statistics and tests are based on strictly independent observations; p-values are from bootstrap two-sample t-tests with 999 repititions (seed = 713), $^{1\,(2)}$ one-(two-)sided; ***,**,* significant at least at the, respectively, 1%, 5%, 10% level with false discovery rate correction for multiple (four) comparisons (Benjamini and Hochberg, 1995); MP (MS) stands for Market-Partners (Market-Strangers); in OSDG-MP-RE 'low-low' ('high-high') ['high-low'] corresponds to matchings with low (high) [mixed] exchange rates in the real effort task preceding the SDG.

Summarizing the information presented in this section, the statistics shown in Table 6 are consistent with the notion that market experience leads to a decrease in the efficiency of cooperation in the SDG for participants who competed on the same side of the market but not for participants who competed on the opposite sides of the market. In addition, the order of average contribution levels shown in Figure 3 and the (partly) weaker significance levels for OSDG-MP-T and OSDG-MP-RE in comparison to OSDG-MP, suggest that incorporating dimensions that can be viewed as being typical for market experience into a non-market environment leads to lower cooperation.

8 Summary and Conclusions

We have studied whether the experience of interacting in a competitive market affects the efficiency of cooperation in a subsequent social dilemma game played in pairs. In the markets trade takes place in real time and there is a short and a long side of the market. Participants on the short side have, compared to those on the long side, a strong competitive disadvantage and it is hard for them to secure transactions. Our experimental design allows us to compare the efficiency of cooperation with and without previous market experience, holding earnings constant. We can therefore isolate the causal effect of market experience, decoupled from the effect of the earnings inequality produced in markets. In addition, we can compare the effect of market experience on the efficiency of cooperation for participants who competed on the same market with participants who had a comparable market experience but competed on different markets.

The overall picture that emerges from our experiments has many nuances. Market experience can affect cooperation negatively but it is neither market experience *per se* nor being on the long or short side of the market *per se* that is adverse to efficient cooperation. It is the fact of having competed with each other in the *same* market and on the *same* side that makes subsequent cooperation difficult, with the impact being clearer for market-losers than for market-winners. Moreover, market experience can have in the short run a positive effect for those in an advantageous market position (market-winners) but only when the social dilemma needs to be solved with somebody who has been on a *different* market before. The latter is consistent with the correlational evidence reported in Henrich et al. (2001) and suggests that there exists a 'cooperation rent' for traders who are successful in markets, but only when the market interaction is with 'strangers'.

Our study can be of general interest for economists. The results show that competitive market experience can have significant and substantial spillover effects and impose economic costs (or, in some cases, benefits) in spheres of social interaction outside of the market. It is, of course, possible that the observed spillover effects would be smaller or even disappear in a market setting less extreme that the one we studied. Nevertheless, one can argue that market conditions like the ones in our experiment are representative for some 'naturally' occurring markets, as for instance, labor markets with high job insecurity. A more specific conclusion from our results could be drawn for the voluntary provision of (local) public goods. Namely, that people who are competing on a market with each other will be less likely to contribute to the local public good efficiently. Speculatively, this may provide an argument in favor of so-called 'social mixing' in urban planning (Uitermark, 2003; Lees, 2008): social mixing could increase local social capital because it decreases the

likelihood that people who have to compete on the same side of the same market live in the same neighborhood.

We motivated our research questions using the framework of Bowles and Polania-Reyes (2012), who argue that preferences are state-dependent in the sense that "... actions are motivated by a repertoire of heterogeneous preferences the salience of which depends on the nature of the decision situation." (p.372) The nature of the decision situation can also be affected by social experiences and different experiences can trigger different states. Our results may thus be understood in terms of state-dependent preferences. They are consistent with the notion that competitive market experience triggers a less cooperative preference state towards those one has directly competed with. This occurs regardless of whether one has competed with each other on the short or the long side of the market. The fact that this effect is also present for traders on the short side (market-winners) is quite remarkable. It highlights that the issue is not whether people have had competitive experience *per se* or have been successful in terms of income, but whether one has been in competition with each other or not.

Our observations call for a refinement of the concept of state-dependence. The question is why exactly certain types of market experience lead to less cooperation. One possible explanation is that direct competition *per se* damages affective social ties or may even lead to negative ties, while successful trades *per se* have a counterbalancing effect and may create or strengthen ties (for evidence on and the effects of social ties in the lab and the field, see, e.g., van Dijk and van Winden, 1997; van Winden, 2012; Bault et al., 2015). This may explain why competition on the same market appears to be harmful for cooperation while this is not the case when this competitive experience was with somebody else. It is also consistent with the idea that a competitive seller-buyer relation even when it is asymmetric does not damage or even enhance affective ties between the traders (cf. the *doux commerce* idea of Montesquieu, 1748).

Another potentially important channel could work through how people's beliefs about others' cooperation are affected by market experience of different kinds. The result that cooperation rates of buyers and sellers in mixed pairs do not differ in the first period of the SDG suggests that differences in beliefs are not a main driving force of cooperation behavior. However, this evidence is only indirect and certainly not conclusive. The investigation of the precise role of beliefs and possible other mechanisms behind our behavioral results could be an exciting future research avenue.

Lastly we note that our results do not imply that competition other than competitive market interaction would not affect subsequent behavior. In fact, some of the studies cited in the literature section show that non-market competition can affect cooperation. In this

paper we studied a kind of market competition that is of special interest given the societal relevance of such markets. Nevertheless, future research could address the question if an environment where participants compete against each other in a non-market environment (probably with non-monetary rewards) would affect subsequent cooperation in a social dilemma similarly to what we find in our market environments.

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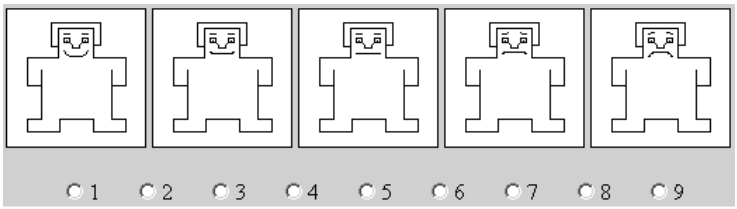
Appendix
*** For Online Publication ***

A Detailed Description of Subjective Well-being and Social Value Orientation Measurements

Parts 1 and 2: SWB 1 and SVO 1. In SWB 1 we recorded participants' response to the subjective well-being question shown in Figure A.1. These initial measurement provides the baseline to which the second measurement will be compared. Subjects were asked to mark the number related to the expression of the manikin that best corresponded to how they felt at that moment.[35] In the figure, "1" corresponds to the highest level and "9" to the lowest level of subjective well-being.



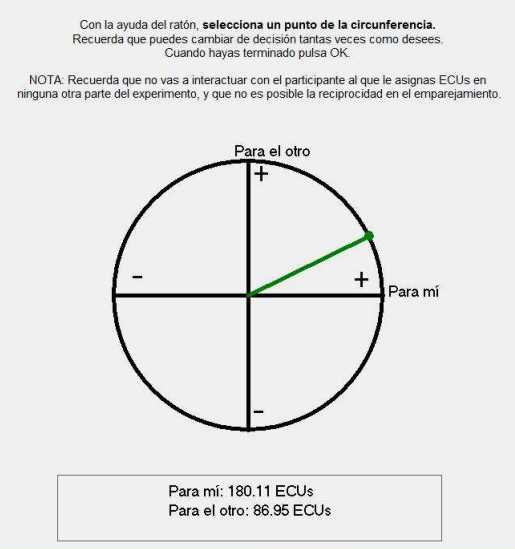
Note: "1" indicates highest level, ..., "9" indicates lowest level of subjective well-being

Figure A.1. Subjective well-being self-assessment

In SVO 1 we recorded participants' social value orientation using the so-called circle-test. The circle-test is a modified and incentivized version of the ring-test (Liebrand, 1984) and was successfully applied by, among others, Sonnemans et al. (2006) and Brandts et al. (2009). It is a simple task which allows for a quantification of individuals' social value orientation by determining the readiness of individuals to help or hurt others at some cost to themselves. Figure A.2 shows an example of a circle-test as used in the experiment.

In the circle-test a person's social value orientation is measured by a single decision which consists of the selection of a point on the circle. Each point on the circle represents an allocation S of Experimental Currency Units (ECU) to the person who makes the choice (Self) and an allocation O of ECU to another person (Other). The amounts allocated can be positive or negative, with $S^2 + O^2 = 200^2$. Note, that each point on the circle corresponds to a certain angle of the line connecting that point with the origin, which we will use as the measure of social value orientation. For instance, an angle of 0 degrees corresponds to selfishness as it allocates 200 ECU to oneself and 0 ECU to the other; an angle of 90 degrees is interpreted as altruistic as it gives 0 to oneself and 200 to the other. Generally, between 0 and 90 degrees an increasing angle is interpreted as increasing pro-sociality. A negative angle, which reduces the earnings of the other at some cost to

[35] These figures, developed by Lang (1980), are based on Sonnemans (1991).



Translation:

With the help of the mouse, **choose a point on the circle**.
Remember that you can change the decision as many times as you want.
When you are finished press OK.

NOTE: Remember that you will not interact with the participant to whom you assign ECU in any other part of the experiment and that reciprocity between matched pairs is not possible.

For myself: 180.11 ECU
For the other: 86.95 ECU

Figure A.2. Social value orientation circle test

oneself, identifies competitiveness.[36]

In the experiment the circle appeared on participants' computer screens. Participants received computerized instructions about how to make the decision and had ample opportunity to practice.[37] Decisions in the circle-test had pecuniary consequences. The chosen ECU translated into money earnings at the exchange rate of 100 ECU to €1. As matched others were random and anonymous, SVO 1 measures the social value orientation towards generalized others. Subjects were not informed about the decision of 'their' others in the circle-test until the very end of the session.

Parts 5 and 6: SWB 2 and SVO 2. After the SDG, in SWB 2 we again recorded participants' response to the subjective well-being question shown in Figure A.1 and in SVO 2 participants again made decisions in the social value orientation circle-test.[38] In SVO 2 each participant made an allocation decision with respect to him/herself and another anonymously and randomly chosen participant whom s/he did not interact with in any of the previous parts. As in SVO 1, to avoid (anticipated) reciprocity, the matched participant did not make a decision towards the deciding participant but towards another

[36]For an extensive discussion of the concept and measurement of social value orientation, see, e.g., Van Lange (1999) and Murphy et al. (2011).

[37]Each participant made a social value orientation decision with respect to another anonymously and randomly chosen participant in the lab. Importantly, the alter-participant does not make a decision towards the ego-participant but toward yet another randomly chosen participant. This was known to the participants and excludes (anticipated) reciprocity considerations.

[38]Alternatively, we could have placed SWB 2 and SVO 2 directly after the DAM. We did not do that because it could have influenced behavior in the the SDG, which is our main variable of interest.

not previously matched participant. Subjects were informed about this but did not receive information about the decision of 'their' paired others until the very end of the session.

B Efficiency of Cooperation and Market Experience: Additional Statistical Analyses

B.1 Mann-Whitney ranksum tests

Table B.1. Efficiency of cooperation in market and no-market treatments (across trader matchings)

		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	23.740	23.025	7.556	26.120	22.172	7.502
OSDG-MP	36	33.167	30.801	13.814	41.104	32.766	15.659
Market-Strangers	12	32.146	31.418	3.154	31.357	30.422	3.612
OSDG-MS	36	27.500	28.468	12.197	34.708	31.669	14.551
MP vs OSDG-MP		$p = 0.0456^{*}$			$p = 0.0299^{*}$		
MS vs OSDG-MS		$p = 0.5203$			$p = 0.3656$		
MP vs MS		$p = 0.0040^{**}$			$p = 0.0020^{**}$		

Note: all statistics are based on strictly independent observations; p-values come from Mann-Whitney rank sum tests, two-sided; $^{***,\ **,\ *}$ significant at least at the, respectively, 1%, 5%, 10% level with false discovery rate correction for multiple (six) comparisons (Benjamini and Hochberg, 1995); MP (MS) stands for Market-Partners (Market-Strangers).

Table B.2. Efficiency of cooperation in market treatments and non-market treatments for the different trader matchings

Buyer-buyer pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	22.042	21.729	8.751	15.000	18.682	11.849
OSDG-MP (low-pay)	12	33.542	34.243	9.633	41.104	33.038	15.326
Market-Strangers	12	30.229	29.236	5.854	28.729	27.990	6.707
OSDG-MS (low-pay)	12	33.125	32.597	9.083	35.875	33.674	13.375
MP vs OSDG-MP		$p = 0.0040^{**}$			$p = 0.0136^{**}$		
MS vs OSDG-MS		$p = 0.2987$			$p = 0.1659$		
MP vs MS		$p = 0.0308^{**}$			$p = 0.0270^{**}$		
Seller-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	18.250	21.768	12.413	21.042	20.699	14.385
OSDG-MP (high-pay)	12	26.500	29.097	15.536	38.521	34.222	15.634
Market-Strangers	12	34.396	35.486	7.428	34.573	35.168	7.323
OSDG-MS (high-pay)	12	23.833	23.785	11.064	33.917	31.649	14.522
MP vs OSDG-MP		$p = 0.1811$			$p = 0.0349^{*}$		
MS vs OSDG-MS		$p = 0.0130^{**}$			$p = 0.6649$		
MP vs MS		$p = 0.0075^{**}$			$p = 0.0075^{**}$		
Buyer-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	27.125	26.875	15.507	34.167	30.625	16.213
OSDG-MP (mixed-pay)	12	32.875	29.063	15.926	35.604	31.038	17.182
Market-Strangers	12	29.271	31.715	7.368	28.563	30.540	9.051
OSDG-MS (mixed-pay)	12	33.542	29.021	15.019	33.500	29.684	16.590
MP vs OSDG-MP		$p = 0.7187$			$p = 0.9590$		
MS vs OSDG-MS		$p = 0.9770$			$p = 0.8174$		
MP vs MS		$p = 0.7576$			$p = 0.6619$		

Note: all statistics are based on strictly independent observations; p-values based on Mann-Whitney rank sum tests, two-sided; ***, **, * significant at least at the, respectively, 1%, 5%, 10% level with false discovery rate correction for multiple (six) comparisons (Benjamini and Hochberg, 1995); MP (MS) stands for Market-Partners (Market-Strangers); in OSDG-MP and OSDG-MS 'low-pay' corresponds to income matchings 340-340 and 346-346, respectively, 'high-pay' to income matchings 2672-2672 and 2656-2656, respectively, and 'mixed-pay' to income matchings 340-2672 and 346-2656, respectively.

Table B.3. Efficiency of cooperation in Market-Partners treatment and OSDG-T and OSDG-RE for the different trader matchings

Buyer-buyer pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	22.042	21.729	8.751	15.000	18.682	11.849
OSDG-MP-T (low-pay)	15	28.333	29.450	16.268	35.833	29.622	20.295
OSDG-MP-RE (low-pay)	29	26.250	26.230	13.485	27.792	29.424	14.387
MP vs OSDG-MP-T[1]		$p = 0.1375$			$p = 0.0688$		
MP vs OSDG-MP-RE[1]		$p = 0.2034$			$p = 0.0116^{*}$		
Seller-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	18.250	21.768	12.413	21.042	20.699	14.385
OSDG-MP-T (high-pay)	15	23.083	29.150	13.767	28.125	29.625	15.679
OSDG-MP-RE (high-pay)	29	27.917	29.282	14.221	32.958	29.480	18.207
MP vs OSDG-MP-T[1]		$p = 0.0509^{*}$			$p = 0.0581^{*}$		
MP vs OSDG-MP-RE[1]		$p = 0.0379^{*}$			$p = 0.0537^{*}$		
Buyer-seller pairs							
		Short run			Long run		
Treatment	N	median	mean	st. dev.	median	mean	st. dev.
Market-Partners	14	27.125	26.875	15.507	34.167	30.625	16.213
OSDG-MP-T (mixed-pay)	15	29.833	31.428	17.155	39.125	29.967	19.162
OSDG-MP-RE (mixed-pay)	29	30.417	30.431	12.592	28.958	29.802	16.251
MP vs OSDG-MP-T[2]		$p = 0.2291$			$p = 0.7764$		
MP vs OSDG-MP-RE[2]		$p = 0.5167$			$p = 0.9586$		

Note: all statistics are based on strictly independent observations; p-values are from Mann-Whitney rank sum tests, $^{1\ (2)}$ one-(two-)sided; ***,**,* significant at least at the, respectively, 1%, 5%, 10% level with false discovery rate correction for multiple (four) comparisons (Benjamini and Hochberg, 1995); MP (MS) stands for Market-Partners (Market-Strangers); in OSDG-MP-RE 'low-low' ('high-high') ['high-low'] corresponds to matchings with low (high) [mixed] exchange rates in the real effort task preceding the SDG.

B.2 Tobit regressions

B.2.1 Tobit regressions related to comparison of main treatments in Section 6

Table B.4. Differences in contributions in the social dilemma game across market and non-market treatments (period dummies reported)

	Short run	Long run
OSDG-MP (const.)	41.529***	45.804***
	(4.013)	(5.975)
Market-Partners	-12.341**	-21.892***
	(5.023)	(6.801)
OSDG-MS	-4.590	-3.628
	(5.110)	(7.518)
Market-Strangers	0.255	-6.923
	(4.258)	(6.199)
Period 2	-0.179	0.622
	(0.982)	(1.124)
Period 3	-0.663	2.220
	(1.402)	(1.481)
Period 4	-3.075**	1.167
	(1.449)	(1.608)
Period 5	-5.302***	-1.511
	(1.832)	(1.905)
Period 6	-25.872***	-3.840*
	(1.999)	(2.211)
Period 7		-0.919
		(1.897)
Period 8		-0.788
		(1.814)
Period 9		-0.692
		(1.964)
Period 10		-1.177
		(2.225)
Period 11		-3.629
		(2.589)
Period 12		-26.636***
		(3.448)
N	2688	5376
F	24.201	9.891
Prob > F	0.000	0.000

MS vs OSDG-MS: F-statistics	1.870	0.407
MS vs OSDG-MS: p-value	0.172	0.523
MP vs MS: F-statistics	13.617	13.528
MP vs MS: p-value	0.000	0.000

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 98 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5. Significant comparisons (two-sided) after Benjamini-Hochberg false discovery rate correction for the six pair-wise comparisons of interest reported in Table B.4

Pair-wise comparison	Significance level
OSDG-MP > MP short run	5%
OSDG-MP > MP long run	1%
MP < MS short run	1%
MP < MS long run	1%

Table B.6. Differences in contributions in the social dilemma game across market and non-market treatments with initial social value orientation and period dummies (latter not reported)

	Short run	Long run
OSDG-MP (const.)	40.184***	45.451***
	(3.989)	(5.875)
Market-Partners	-11.851**	-21.759***
	(5.011)	(6.788)
OSDG-MS	-4.740	-3.668
	(5.059)	(7.532)
Market-Strangers	0.328	-6.897
	(4.217)	(6.196)
Initial SVO	0.071	0.019
	(0.048)	(0.048)
Period dummies	Yes	Yes
N	2688	5376
F	22.054	9.284
Prob > F	0.000	0.000
MS vs OSDG-MS: F-statistics	2.073	0.388
MS vs OSDG-MS: p-value	0.150	0.533
MP vs MS: F-statistics	12.443	13.005
MP vs MS: p-value	0.000	0.000

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 98 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7. Significant comparisons (two-sided) after Benjamini-Hochberg false discovery rate correction for the six pair-wise comparisons of interest reported in Table B.6

Pair-wise comparison	Significance level
OSDG-MP > MP short run	5%
OSDG-MP > MP long run	1%
MP < MS short run	1%
MP < MS long run	1%

Table B.8. Buyer-buyer pairs: Differences in contributions in the social dilemma game across market and non-market treatments (period dummies reported)

	Short run	Long run
OSDG-MP (const.)	46.737***	48.170***
	(5.219)	(9.292)
Market-Partners	-18.814***	-27.229**
	(6.134)	(10.625)
OSDG-MS	-3.944	-2.011
	(6.355)	(11.586)
Market-Strangers	-8.380	-11.916
	(5.705)	(9.838)
Period 2	-0.522	-0.215
	(1.253)	(1.562)
Period 3	-2.180	0.378
	(1.570)	(1.876)
Period 4	-5.702***	-3.153
	(2.103)	(2.396)
Period 5	-6.736***	-8.254**
	(2.482)	(3.205)
Period 6	-26.856***	-9.258***
	(3.426)	(3.341)
Period 7		-4.396*
		(2.522)
Period 8		-3.134
		(2.681)
Period 9		-3.881
		(2.991)
Period 10		-3.313
		(3.026)
Period 11		-7.864**
		(3.706)
Period 12		-27.165***
		(4.745)
N	1200	2400
F	10.577	5.040
Prob > F	0.000	0.000
MS vs OSDG-MS: F-statistics	0.929	1.553
MS vs OSDG-MS: p-value	0.335	0.213
MP vs MS: F-statistics	6.129	5.765
MP vs MS: p-value	0.013	0.016

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9. Significant comparisons (two-sided) after Benjamini-Hochberg false discovery rate correction for the six pair-wise comparisons of interest reported in Table B.8

Pair-wise comparison	Significance level
OSDG-MP > MP short run	5%
OSDG-MP > MP long run	5%
MP < MS short run	5%
MP < MS long run	5%

Table B.10. Buyer-buyer pairs: Differences in contributions in the social dilemma game across market and non-market treatments with initial social value orientation and period dummies (latter not reported)

	Short run	Long run
OSDG-MP (const.)	44.083***	46.442***
	(4.867)	(9.385)
Market-Partners	-17.600***	-26.427**
	(5.753)	(10.458)
OSDG-MS	-4.032	-2.115
	(5.947)	(11.423)
Market-Strangers	-8.237	-11.783
	(5.395)	(9.717)
Initial SVO	0.134**	0.087
	(0.053)	(0.085)
Period dummies	Yes	Yes
N	1200	2400
F	10.935	4.995
Prob > F	0.000	0.000
MS vs OSDG-MS: F-statistics	0.917	1.472
MS vs OSDG-MS: p-value	0.338	0.225
MP vs MS: F-statistics	5.129	5.529
MP vs MS: p-value	0.024	0.019

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11. Significant comparisons (two-sided) after Benjamini-Hochberg false discovery rate correction for the six pair-wise comparisons of interest reported in Table B.10

Pair-wise comparison	Significance level
OSDG-MP > MP short run	5%
OSDG-MP > MP long run	5%
MP < MS short run	5%
MP < MS long run	5%

Table B.12. Seller-seller pairs: Differences in contributions in the social dilemma game across market and non-market treatments (period dummies reported)

	Short run	Long run
OSDG-MP (const.)	37.035***	43.503***
	(7.530)	(10.033)
Market-Partners	-11.966	-25.981**
	(9.092)	(11.931)
OSDG-MS	-8.932	-5.831
	(8.849)	(11.949)
Market-Strangers	8.630	-0.476
	(8.406)	(10.307)
Period 2	0.851	-1.443
	(1.810)	(2.021)
Period 3	1.856	3.782*
	(2.279)	(2.007)
Period 4	0.638	4.707*
	(2.616)	(2.582)
Period 5	-3.307	4.427*
	(3.030)	(2.365)
Period 6	-20.528***	1.340
	(3.249)	(2.742)
Period 7		4.278
		(2.855)
Period 8		2.623
		(3.093)
Period 9		3.743
		(3.125)
Period 10		1.730
		(3.247)
Period 11		2.181
		(3.101)
Period 12		-21.194***
		(5.152)
N	744	1488
F	7.731	3.254
Prob > F	0.000	0.000
MS vs OSDG-MS: F-statistics	8.916	0.419
MS vs OSDG-MS: p-value	0.003	0.518
MP vs MS: F-statistics	11.022	9.885
MP vs MS: p-value	0.001	0.002

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13. Significant comparisons (two-sided) after Benjamini-Hochberg false discovery rate correction for the six pair-wise comparisons of interest reported in Table B.12

Pair-wise comparison	Significance level
OSDG-MP > MP long run	5%
OSDG-MS < MS short run	1%
MP < MS short run	1%
MP < MS long run	1%

Table B.14. Seller-seller pairs: Differences in contributions in the social dilemma across market and non-market treatments with initial social value orientation and period dummies (latter not reported)

	Short run	Long run
OSDG-MP (const.)	35.725***	43.182***
	(7.656)	(9.864)
Market-Partners	-11.225	-25.796**
	(9.023)	(11.899)
OSDG-MS	-8.535	-5.727
	(8.847)	(11.892)
Market-Strangers	8.990	-0.379
	(8.351)	(10.295)
Initial SVO	0.061	0.015
	(0.073)	(0.056)
Period dummies	Yes	Yes
N	744	1488
F	6.899	3.337
Prob > F	0.000	0.000
MS vs OSDG-MS: F-statistics	8.644	0.418
MS vs OSDG-MS: p-value	0.003	0.518
MP vs MS: F-statistics	10.952	9.833
MP vs MS: p-value	0.001	0.002

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.15. Significant comparisons (two-sided) after Benjamini-Hochberg false discovery rate correction for the six pair-wise comparisons of interest reported in Table B.14

Pair-wise comparison	Significance level
OSDG-MP > MP long run	5%
OSDG-MS < MS short run	1%
MP < MS short run	1%
MP < MS long run	1%

Table B.16. Buyer-seller pairs: Differences in contributions in the social dilemma game across market and non-market treatments (period dummies reported)

	Short run	Long run
OSDG-MP (const.)	39.734***	43.432***
	(8.024)	(12.072)
Market-Partners	-1.942	-2.735
	(11.186)	(15.835)
OSDG-MS	-0.430	-2.292
	(11.424)	(16.301)
Market-Strangers	6.377	-1.824
	(9.080)	(13.836)
Period 2	-0.490	4.954**
	(2.228)	(2.491)
Period 3	-0.024	3.892
	(3.326)	(3.745)
Period 4	-1.500	5.581
	(2.977)	(3.691)
Period 5	-4.597	4.991
	(3.682)	(3.652)
Period 6	-29.907***	0.808
	(4.434)	(4.546)
Period 7		-0.380
		(4.719)
Period 8		-0.301
		(4.723)
Period 9		0.050
		(4.577)
Period 10		-0.872
		(4.718)
Period 11		-2.556
		(4.941)
Period 12		-32.177***
		(7.514)
N	744	1488
F	6.335	2.702
Prob > F	0.000	0.001
MS vs OSDG-MS: F-statistics	0.542	0.001
MS vs OSDG-MS: p-value	0.462	0.970
MP vs MS: F-statistics	0.872	0.006
MP vs MS: p-value	0.351	0.940

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.17. Buyer-seller pairs: Differences in contributions in the social dilemma game across market and non-market treatments with initial social value orientation and period dummies (latter not reported)

	Short run	Long run
OSDG-MP (const.)	39.928***	45.068***
	(8.058)	(11.823)
Market-Partners	-1.916	-2.506
	(11.236)	(15.914)
OSDG-MS	-0.272	-0.917
	(11.617)	(16.748)
Market-Strangers	6.411	-1.478
	(9.103)	(13.893)
Initial SVO	-0.013	-0.109
	(0.115)	(0.152)
Period dummies	Yes	Yes
N	744	1488
F	5.881	2.704
Prob > F	0.000	0.000
MS vs OSDG-MS: F-statistics	0.511	0.002
MS vs OSDG-MS: p-value	0.475	0.965
MP vs MS: F-statistics	0.876	0.007
MP vs MS: p-value	0.350	0.932

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2.2 Tobit regressions related to comparison of robustness treatments in Section 7

Table B.18. Buyer-buyer pairs: Differences in contributions in the social dilemma game between Market-Partners, OSDG-MP-T and OSDG-MP-RE with period dummies

	Short run	Long run
Market-Partners (const.)	28.488***	18.442***
	(3.305)	(4.975)
OSDG-MP-T	14.393**	22.045**
	(8.309)	(12.388)
OSDG-MP-RE	7.600*	18.951***
	(5.263)	(7.613)
Period dummies	Yes	Yes
N	864	1728
F	10.032	4.724
Prob > F	0.000	0.000

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 58 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, one-sided.

Table B.19. Significant comparisons (one-sided) after Benjamini-Hochberg false discovery rate correction for the four pair-wise comparisons of interest reported in Table B.18

Pair-wise comparison	Significance level
OSDG-MP-T > MP short run	10%
OSDG-MP-T > MP long run	10%
OSDG-MP-RE > MP short run	10%
OSDG-MP-RE > MP long run	5%

Table B.20. Seller-seller pairs: Differences in contributions in the social dilemma game between Market-Partners, OSDG-MP-T and OSDG-MP-RE with period dummies (not reported)

	Short run	Long run
Market-Partners (const.)	29.284***	18.585***
	(4.454)	(7.128)
OSDG-MP-T	10.790*	16.225*
	(7.178)	(10.283)
OSDG-MP-RE	11.009**	16.925**
	(6.420)	(9.758)
Period dummies	Yes	Yes
N	696	1392
F	10.564	2.282
Prob > F	0.000	0.006

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 58 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, one-sided.

Table B.21. Significant comparisons (one-sided) after Benjamini-Hochberg false discovery rate correction for the four pair-wise comparisons of interest reported in Table B.20

Pair-wise comparison	Significance level
OSDG-MP-T > MP short run	10%
OSDG-MP-T > MP long run	10%
OSDG-MP-RE > MP short run	10%
OSDG-MP-RE > MP long run	10%

Table B.22. Buyer-seller pairs: Differences in contributions in the social dilemma game between Market-Partners, OSDG-MP-T and OSDG-MP-RE with period dummies (not reported)

	Short run	Long run
Market-Partners (const.)	38.002***	42.116***
	(7.486)	(9.790)
OSDG-MP-T	8.584	1.142
	(11.863)	(14.274)
OSDG-MP-RE	4.310	-2.890
	(8.985)	(11.273)
Period dummies	Yes	Yes
N	696	1392
F	4.618	1.779
Prob > F	0.000	0.041

Notes: Tobit regressions with lower limit of 0 and upper limit of 50; standard errors in parentheses, adjusted for 58 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, one-sided.

B.3 Efficiency of cooperation over time

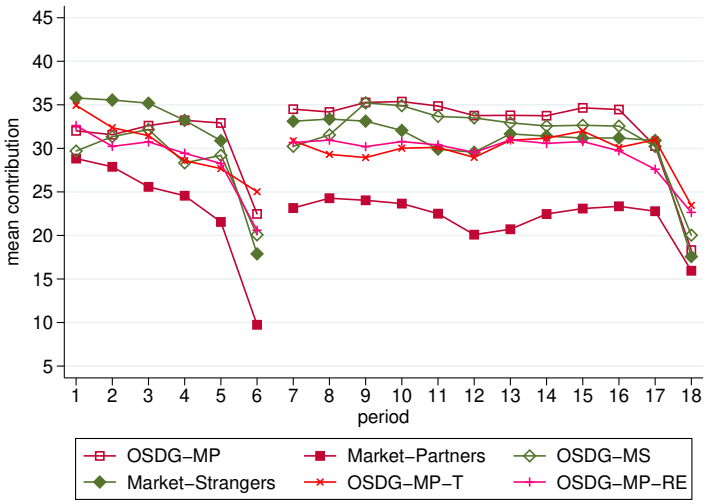
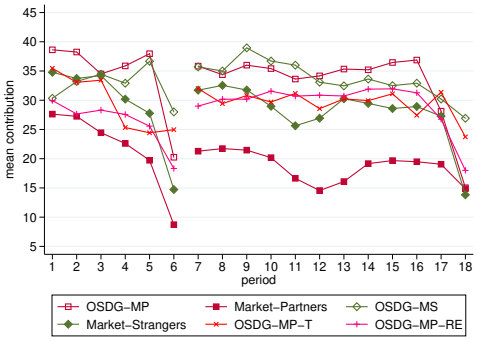
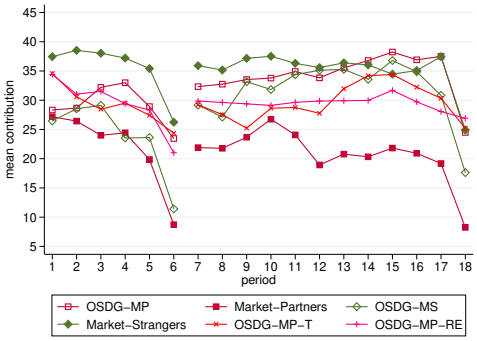


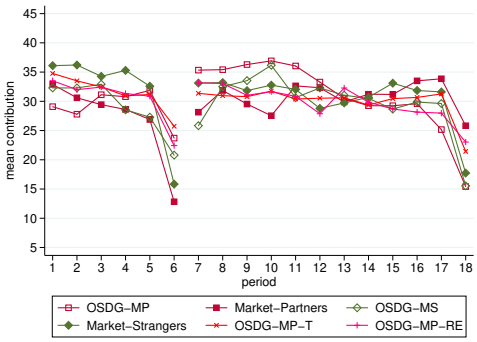
Figure B.1. Average efficiency of cooperation over periods (across traders)



(a) Buyer-buyer & low-pay pairs



(b) Seller-seller & high pay pairs



(c) Buyer-seller & mix-pay pairs

Figure B.2. Average efficiency of cooperation over periods for the different trader & pay pairings

B.4 Comparison of efficiency of cooperation of different trader pairs within each market treatment

In the main text we have compared behavior between market treatments and non-market treatments as well as between the two market treatments. Here we examine the question whether the efficiency of cooperation differs between market-winners, market-losers and mixed pairs *within* the market treatments. Table B.23 provides an overview of the results. In the Market-Partners treatment there are no differences across trader pairs in the short run ($p = 0.6404$, Kruskal-Wallis test, two-sided). In the long run, there is no difference in contributions between buyer pairs and seller pairs ($p = 0.3878$, Dunn's test, two-sided), but both these trader matchings tend to contribute less than mixed pairs ($p = 0.0678$ and $p = 0.0644$, Dunn's test, two-sided).[39] In the Market-Strangers treatment, Kruskal-Wallis tests indicate (close to) marginally significant differences across trader matchings in the short run and in the long run ($p = 0.1089$ and $p = 0.0942$, two-sided). Pair-wise comparisons show that this is driven by seller pairs who in the short run contribute marginally significantly more than buyer pairs ($p = 0.0572$, Dunn's test, two-sided) and in the long run more than both buyer pairs and mixed pairs ($p = 0.0533$ and $p = 0.0944$, Dunn's test, two-sided). All other comparisons do not return statistically significant results (see Table 5 for details). We summarize the discussed findings in the following result.

Result B.1. *With a joint market experience (Market-Partners) market-loser pairs and market-winner pairs contribute similarly and contribute less (at the 10% significance level) than mixed pairs, in the long run. In pairs without a joint market experience (Market-Strangers) pairs of market-winners contribute more (at the 10% significance level) than pairs consisting of market-losers, in the short and long run, or mixed pairs, in the long run.*

Result B.1 documents that traders who have competed on the same market (Market-Partners) and on the same side of the market—either on the favorable side or on the unfavorable one—achieve less efficient cooperation outcomes than pairs of traders who also have been in the same market but on opposite sides of it. By contrast, for Market-Strangers having been on the favorable side of the market causes higher subsequent co-operation levels than having been on the unfavorable side. Thus, there seems to be a market-winner cooperation rent, which can however only be 'cashed in' when the cooper-ation problem occurs with others who have not been in the same market.

[39] The p-values of pair-wise tests are corrected for multiple comparisons using the false discovery rate correction introduced by Benjamini and Hochberg (1995).

Table B.23. Comparison of efficiency of cooperation of different trader pairs within each market treatment

Market-Partners							
		Short run			Long run		
Trader pair	N	median	mean	st. dev.	median	mean	st. dev.
Buyer-buyer	14	22.042	21.729	8.751	15.000	18.682	11.849
Seller-seller	14	18.250	21.768	12.413	21.042	20.699	14.385
Buyer-seller	14	27.125	26.875	15.507	34.167	30.625	16.213
Across all trader matchings[a]			$p = 0.6404$			$p = 0.0956$	
Buyer-buyer vs Seller-seller			$p = 0.4449$			$p = 0.3878$	
Buyer-buyer vs Buyer-seller			$p = 0.3447$			$p = 0.0678$	
Seller-Seller vs Buyer-seller			$p = 0.5698$			$p = 0.0644$	
Market-Strangers							
Trader pair	N	median	mean	st. dev.	median	mean	st. dev.
Buyer-buyer	12	30.229	29.236	5.854	28.729	27.990	6.707
Seller-seller	12	34.396	35.486	7.428	34.573	35.168	7.323
Buyer-seller	12	29.271	31.715	7.368	28.563	30.540	9.051
Across all trader matchings[a]			$p = 0.1089$			$p = 0.0942$	
Buyer-buyer vs Seller-seller			$p = 0.0572$			$p = 0.0533$	
Buyer-buyer vs Buyer-seller			$p = 0.2367$			$p = 0.2838$	
Seller-Seller vs Buyer-seller			$p = 0.1313$			$p = 0.0944$	

Note: all statistics are based on strictly independent observations; [a] Kruskal-Wallis test; for pair-wise comparisons p-values are based on Dunn's tests (Dunn, 1964) with false discovery rate correction for multiple comparisons (Benjamini and Hochberg, 1995).

C Subjective Well-being and Social Value Orientation

Recall that we asked participants to respond to our subjective well-being and social value orientation questions at two points during the experiment: in Part 1, at the very beginning of the experiment, and in Part 5, after the market interaction (in the market treatments) and the six periods of the first SDG had taken place.

Using regression analyses, we explore if and how market experience in respectively Market-Partners and Market-Strangers affects subjective well-being and social value orientation relative to the control treatments OSDG-MP and OSDG-MS. To account for inter-individual variability, in all regressions the dependent variable is the *change* in respectively subjective well-being and social value orientation calculated as the difference between the first and the second measurement point.[40]

Table C.1 shows the results for Market-Partners and OSDG-MP. The independent variables are a dummy variable for OSDG-MP (with Market-Partners being the reference category), a dummy variable capturing whether the participant was either a buyer in market partners or received a low lump–sum income in OSDG-MP ('Buyer or Low-pay') and an interaction variable between the last two variables, which shows the effect of being a low-pay subject in OSDG-MP relative to being a buyer in Market-Partners. In addition, since the second measurement of subjective well-being and social value orientation was taken after the six periods of the first SDG, we control for individual earnings in this first SDG ('SDG 1 earnings'). For subjective well-being the regression results show no significant difference between Market-Partners and OSDG-MP *per se*. Interestingly, however, there is a significantly negative effect of having been a buyer in the Market-Partners treatment while there is no such effect of having received low income in the OSDG-MP ($p = 0.606$ for low-pay in OSDG-MP). This indicates that there is negative effect of having been on the long side of the market that is independent of the low income generated in the market. The regression for social value orientation shows no significant effects.

Table C.2 shows the results for Market-Strangers and the OSDG-MS. Here we find for subjective well-being a negative overall effect of Market-Strangers, which is not significantly different in OSDG-MS. The other variables show effects that are similar to those of Market-Partners. Having been a buyer has a significant negative impact while this is not the case for having received a low lump-sum income in OSDG-MS. For social value orientation the results show a marginally significant negative overall effect of Market-Strangers,

[40] Descriptive statistics for the initial values, final values and change values of both subjective well-being and social value orientation can be found in Tables C.3–C.5. When comparing subjective well-being and social value orientation between market and non-market treatments one should bear in mind that traders already 'worked' for about 30 minutes for their money, while the OSDG-participants just started and received this sum as Manna from Heaven.

Table C.1. Change in subjective well-being & social value orientation in Market-Partners and OSDG-MP

	Subjective well-being	Social value orientation
Market-Partners (const.)	-1.542	11.132
	(1.059)	(12.983)
OSDG-MP	-0.685	0.039
	(0.419)	(5.710)
Buyer or Low-pay	-1.287***	-0.770
	(0.449)	(3.643)
OSDG-MP * Buyer or Low-pay	1.073*	-7.329
	(0.616)	(8.405)
SDG1 earnings	0.004**	-0.032
	(0.002)	(0.033)
N	184	184
adj. R^2	0.050	0.008
Low-pay in OSDG-MP: p-value	0.606	0.295

Notes: OLS regressions; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

which is not significantly different in OSDG-MS. In addition, there is a significantly negative effect of being either a buyer in Market-Strangers or a low-pay player in OSDG-MS. In addition, there is a marginally significantly positive effect of higher earnings in the first SDG.

When planning the experiment design we expected that market experiences on the short and long side of the market will have differential effects on subjective well-being and social value orientation, which may translate to more or less success in the SDG. The above results provide evidence that subjective well-being decreases for market-losers but not for low income players in the OSDG, which is consistent with the results that market-loser pairs have difficulties to cooperate efficiently in the SDG. However, other links between changes in these individual characteristics and contribution behavior in the public goods game are not borne out by the data. For instance, the larger cooperation success in Market-Strangers is not accompanied with higher subjective well-being or more pro-social value orientation. Thus, at best the results on subjective well-being and social value orientation are partially consistent with those on the efficiency of cooperation in the SDG. One explanation of this partial disparity of results is that cooperative attitude, subjective well-being and social value orientation are simply distinct phenomena and don't have to be necessarily in line with each other. A full analysis of this issue could be a very interesting topic for future work.

Table C.2. Change in subjective well-being & social value orientation in Market-Strangers and OSDG-MS

	Subjective well-being	Social value orientation
Market-Strangers (const.)	-2.294**	-27.881*
	(0.902)	(14.734)
OSDG-MS	-0.588	0.503
	(0.380)	(4.964)
Buyer or Low-pay	-2.249***	-5.576***
	(0.341)	(1.792)
OSDG-MS * Buyer or Low-pay	1.897***	6.544
	(0.548)	(5.353)
SDG1 earnings	0.007***	0.056*
	(0.002)	(0.031)
N	264	264
adj. R^2	0.196	0.049
Low-pay in OSDG-MS: p-value	0.411	0.843

Notes: OLS regressions; standard errors in parentheses, adjusted for 50 clusters; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3. Initial subjective well-being & social value orientation: descriptive statistics

		Initial subjective well-being			Initial social value orientation		
Treatment	N	Median	Mean	St.Dev.	Median	Mean	St.Dev.
Market-Partners							
buyer-buyer	56	3.00	3.05	1.95	3.947	10.436	22.451
seller-seller	28	3.00	3.25	1.43	0.346	9.607	24.015
buyer-seller	28	3.00	3.43	1.64	26.855	15.805	35.962
OSDG-MP							
340-340	24	3.00	3.17	1.58	13.662	20.844	23.802
2672-2672	24	3.00	2.92	1.72	29.763	21.487	47.020
340-2672	24	3.00	3.38	1.79	20.115	14.562	31.870
Market-Strangers							
buyer-buyer	96	3.00	2.91	1.58	17.842	18.421	19.56
seller-seller	48	3.00	3.10	1.53	20.436	17.846	21.854
buyer-seller	48	2.00	2.73	1.70	6.026	15.947	20.827
OSDG-MS							
346-346	24	3.00	3.54	1.72	23.936	20.377	26.736
2656-2656	24	3.00	3.25	1.67	9.465	15.580	39.893
346-2656	24	3.00	3.08	1.82	32.280	27.235	17.550

Note: For subjective well-being a higher value indicates worse well-being; for social value orientation higher degrees (smaller than 90 degree) indicate stronger pro-social value orientation.

Table C.4. Final subjective well-being & social value orientation: descriptive statistics

		Final subjective well-being			Final social value orientation		
Treatment	N	Median	Mean	St.Dev.	Median	Mean	St.Dev.
Market-Partners							
buyer-buyer	56	4.500	4.214	2.417	0.346	8.302	8.312
seller-seller	28	2.000	3.107	2.132	0.000	5.423	17.233
buyer-seller	28	3.000	3.250	1.878	3.304	14.467	22.538
OSDG-MP							
340-340	24	3.000	3.333	2.200	1.565	7.736	41.410
2672-2672	24	3.000	3.375	2.300	24.257	18.938	44.631
340-2672	24	3.000	3.708	2.349	6.502	8.718	30.119
Market-Strangers							
buyer-buyer	96	4.000	4.510	2.299	0.363	8.836	23.267
seller-seller	48	2.000	2.292	1.304	2.242	13.576	18.512
buyer-seller	48	2.000	3.250	2.539	8.855	15.261	18.715
OSDG-MS							
346-346	24	4.000	3.500	1.668	24.431	21.710	25.162
2656-2656	24	3.000	3.625	2.481	7.096	10.245	41.697
346-2656	24	3.000	3.292	1.922	27.170	24.172	20.821

Note: For subjective well-being a higher value indicates worse well-being; for social value orientation higher degrees (smaller than 90 degree) indicate stronger pro-social value orientation.

Table C.5. Change in subjective well-being & social value orientation: descriptive statistics

		Change in subjective well-being			Change in social value orientation		
Treatment	N	Median	Mean	St.Dev.	Median	Mean	St.Dev.
Market-Partners							
buyer-buyer	56	-1.000	-1.161	2.499	-0.336	-2.133	15.284
seller-seller	28	0.000	0.143	2.189	-0.222	-4.184	16.420
buyer-seller	28	0.000	0.179	1.806	0.000	-1.339	22.938
OSDG-MP							
340-340	24	0.000	-0.167	2.036	-0.015	-13.108	46.900
2672-2672	24	0.000	-0.458	1.744	0.156	-2.549	26.233
340-2672	24	0.000	-0.333	2.180	-0.245	-5.844	14.865
Market-Strangers							
buyer-buyer	96	-2.000	-1.604	2.553	-0.697	-9.585	21.128
seller-seller	48	1.000	0.813	1.659	-0.001	-2.370	17.661
buyer-seller	48	0.000	-0.521	2.737	0.000	-2.585	15.540
OSDG-MS							
346-346	24	0.000	0.042	1.083	0.001	1.332	10.009
2656-2656	24	0.000	-0.375	2.183	0.002	-5.335	19.749
346-2656	24	0.000	-0.208	1.956	-0.469	-3.063	20.953

Note: Positive (negative) values indicate improved (worsened) subjective well-being and social value orientation, respectively; change in subjective well-being = initial − final; change in social value orientation = final − initial.

D Experiment Instructions

This section contains an English translation of the general instructions, which were read out aloud at the beginning of a session and which were the same in all treatments, and the translated specific instructions with the original Spanish screen shots of the Market-Strangers treatment. Differences between Market-Strangers and Market-Partners are indicated between *[ ]* (see screens 16, 26, 36, below).

The instructions of OSDG-MP and OSDG-MS were identical to those of Market-Strangers except that there were no market instructions (and comprehension questions related to markets). Some slight rephrasing of the social dilemma instructions was necessary and is indicated between *[ ]* (see screens 16, 26, below). Differences between OSDG-MP and OSDG-MP-T are also indicated there between *[ ]*. The instructions for the real effort task of treatment OSDG-MP-RE can be found after the instructions of the other treatments. Except for the real effort task instructions in this treatment were the same as in the other OSDG treatments. The full set of the original Spanish instructions is available from the authors upon request.

GENERAL INSTRUCTIONS

"READ OUT ALOUD AT THE BEGINNING OF THE EXPERIMENT"

Instructions

Welcome to this experiment. The purpose of this experiment is to study how individuals make decisions in different situations. The instructions are simple and if you follow them carefully you can earn a considerable amount of money with the decisions you make. How much you earn may also depend on the decisions of other participants and on random events. The total amount of money you earn in the experiment will be paid out to you in cash confidentially at the end of the experiment. Nobody will learn the payments received by other participants. During the experiment you can ask questions at any moment. Please do not ask the questions aloud but raise your hand. One of the experimenter team will come to you to answer your question. Apart from these questions, **any kind of communication other than specified in the instructions is not allowed** and will lead to the immediate exclusion from the experiment.

1. This experiment consists of several parts. These are the "General Instructions" which apply to all parts of the experiment. You will receive the instructions for the different parts at the beginning of each part.

2. In some parts you will learn about (parts of) the earnings received but in some parts you will not learn your earnings immediately. Only at the end of the experiment, you will be informed about all your earnings in each part and your total earnings. In the experiment we will not talk of Euro but of ECU. At the end of the experiment, all your earnings will be exchanged at the exchange rate of

   100 ECU = 1 EUR.

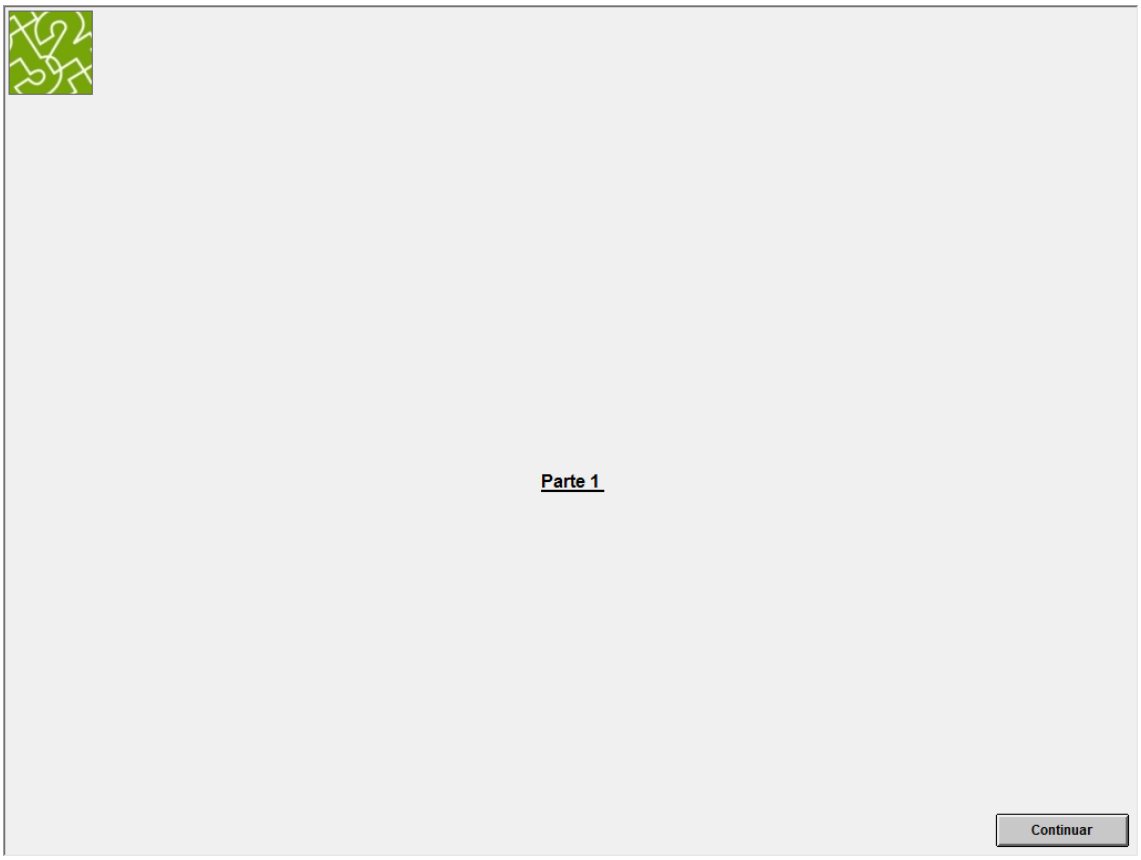
3. At the beginning of each part, you will see the corresponding instructions on the screen or you will be handed over new instructions on paper.

SPECIFIC INSTRUCTIONS AND SCREEN SHOTS

"READ BY SUBJECTS AT THEIR ON PACE"

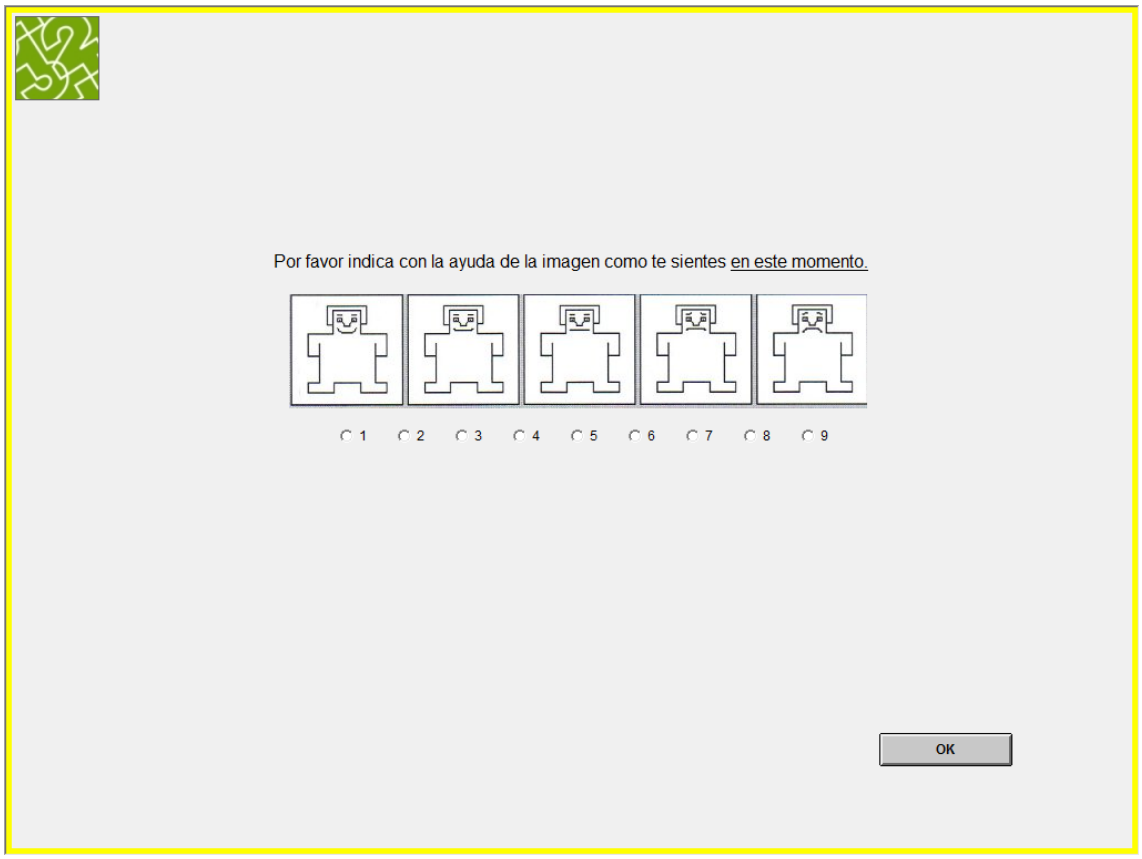
SCREENS and INSTRUCTIONS for Market-Strangers

Screen 1



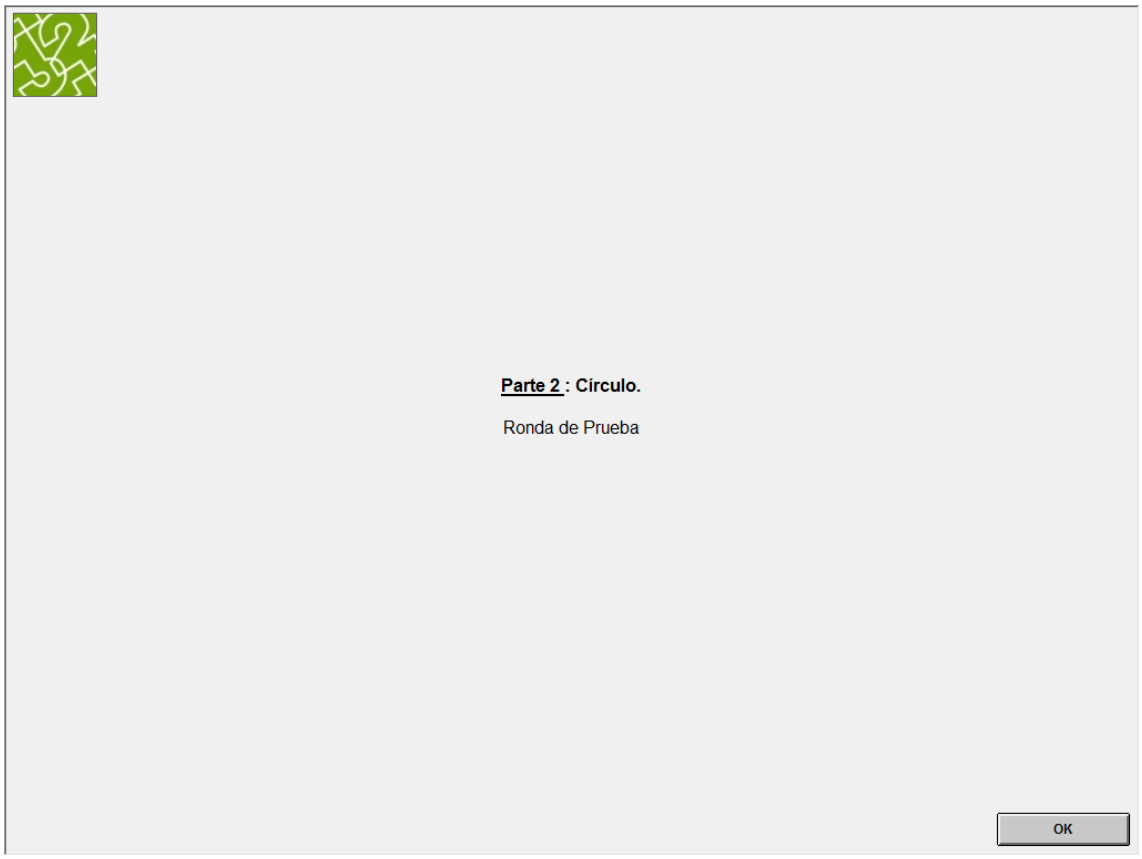
Part1

Screen 2



Please indicate with the help of the image how you feel at this moment.

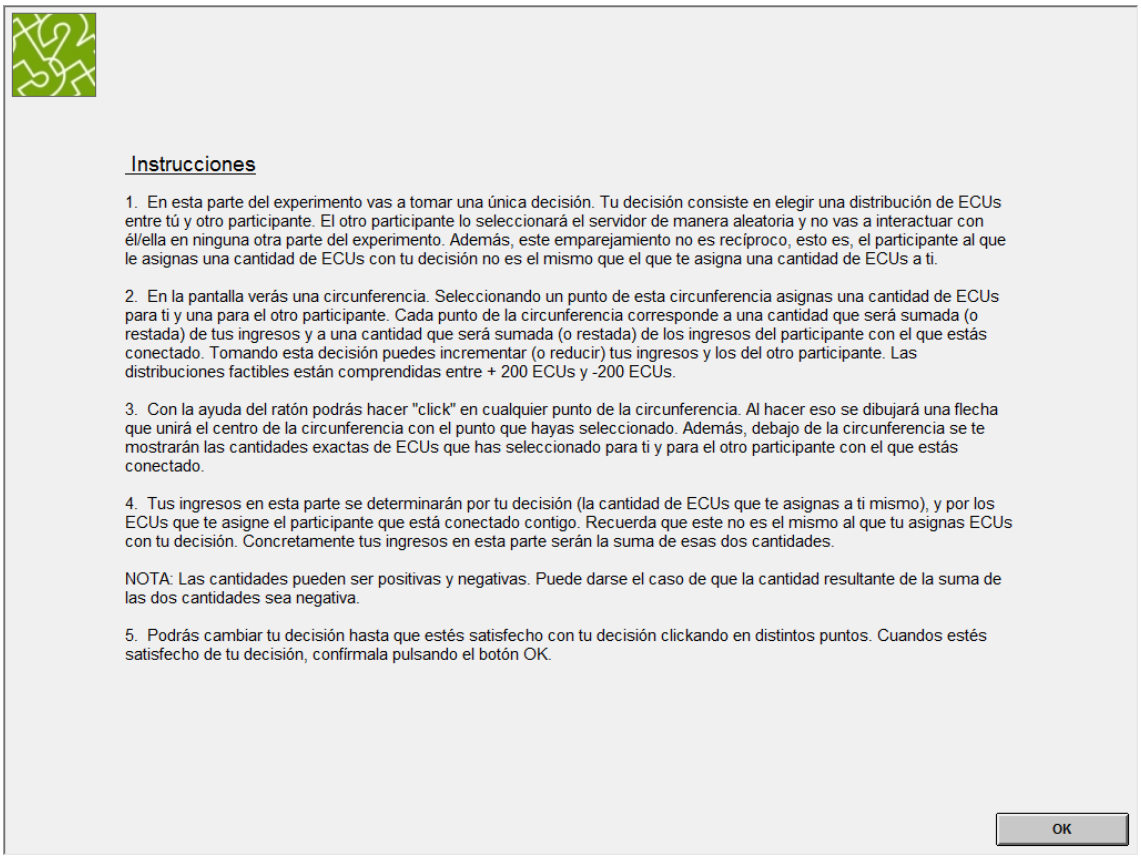
Screen 3



Part 2: Circle

Trial Round

Screen 4



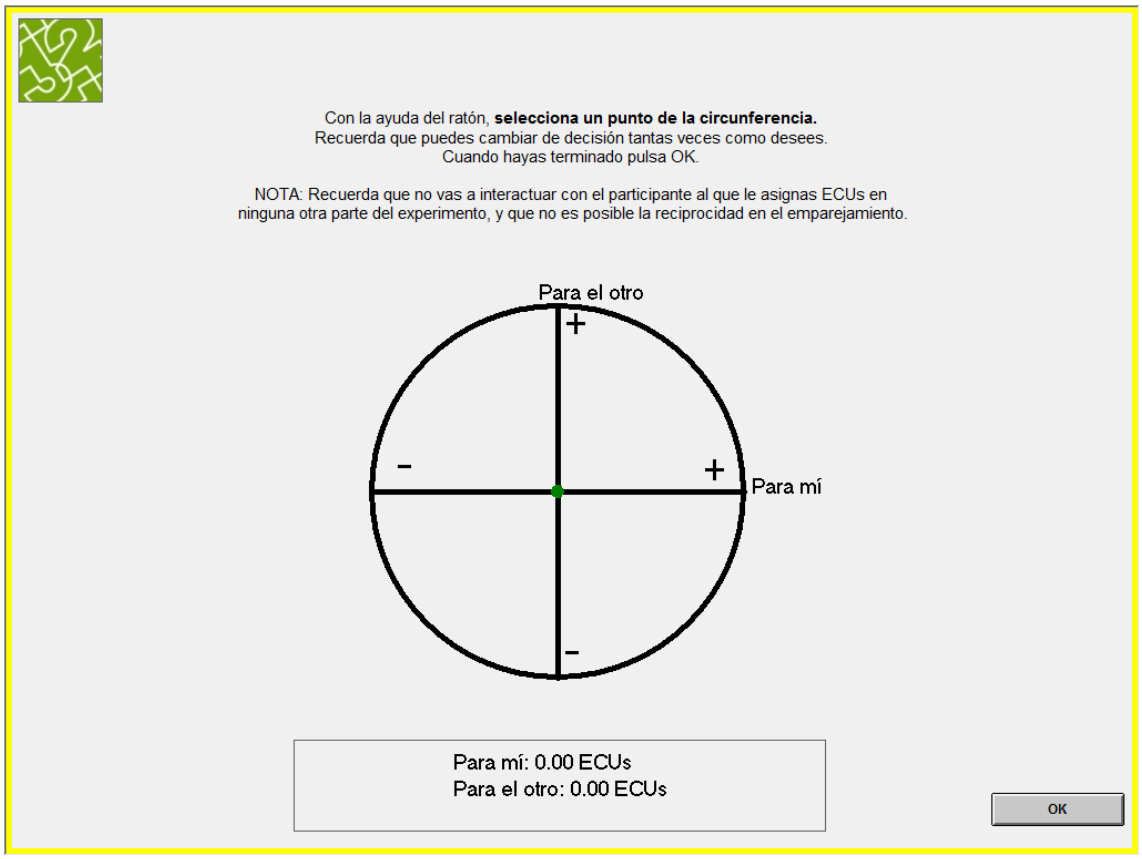
Instructions

1. In this part of the experiment you are going to take just one decision. Your decision consists in choosing a distribution of ECUs between you and another participant. The server will choose the other participant randomly and you are not going to interact with him/her in any other part of the experiment. Furthermore, this matching is not reciprocal, that is the other participant, to whom you can allocate an amount of ECUs with your decision, is not the same who can allocate an amount of ECUs to you.
2. You are going to observe a circle on the screen. By choosing a point on this circle you allocate an amount of ECUs to you and to the other participant. Each point on the circle corresponds to an amount that will be added to (or subtracted from) your earnings and the other participant's earnings with whom you are connected. By taking your decision you can increase (or reduce) your earnings and the earnings of the other participant. The feasible distributions range from +200 ECUs to -200 ECUs.
3. With the help of the mouse you will be able to click on any point on the circle. If you do that an arrow will be drawn, which connects the center of the circle with the point that you will have selected. Moreover, the exact amount of ECUs that you have selected for you and for the other participant will be shown below the circle.
4. Your earnings in this part will be determined by your decision (the amount of ECUs that you allocate to yourself), and by the ECUs that the participant who is connected with you allocates to you. Remember that the latter is not the same than the one to whom you allocate ECUs with your decision. Concretely, your earnings in this part will be the sum of these two amounts.

NOTE: The amounts can be positive or negative. It can be the case that the sum of the two amounts is negative.

5. You will be able to change your decision until you are satisfied with your decision by clicking on different points on the circle. When you are satisfied with your decision you have to confirm it by clicking on the OK button.

Screen 5



With the help of the mouse select a point on the circle. Remember that you can change your decision as many times as you wish.

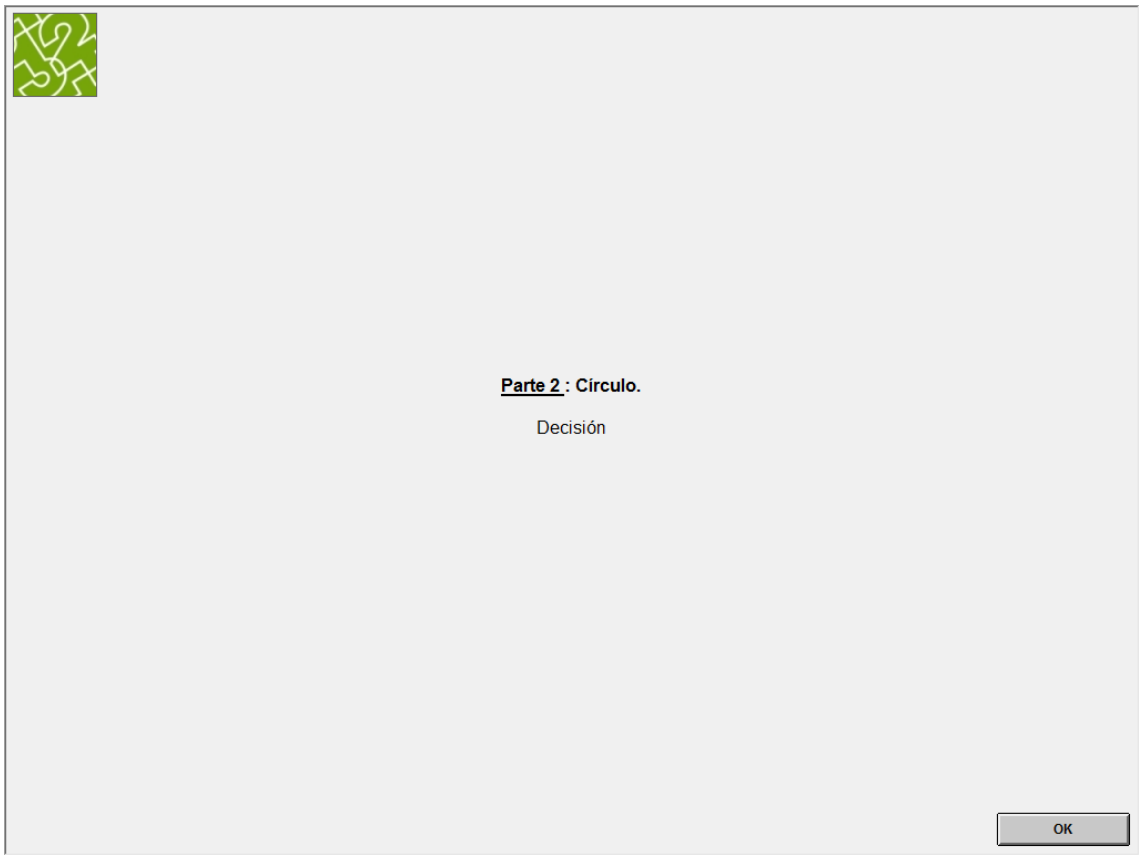
When you are finished press OK.

NOTE: Remember that you will not interact with the participant to whom you assign ECUs in no other part of the experiment and that reciprocity in the matching.

FOR ME: 0.00 ECUs

FOR THE OTHER: 0.00 ECUs

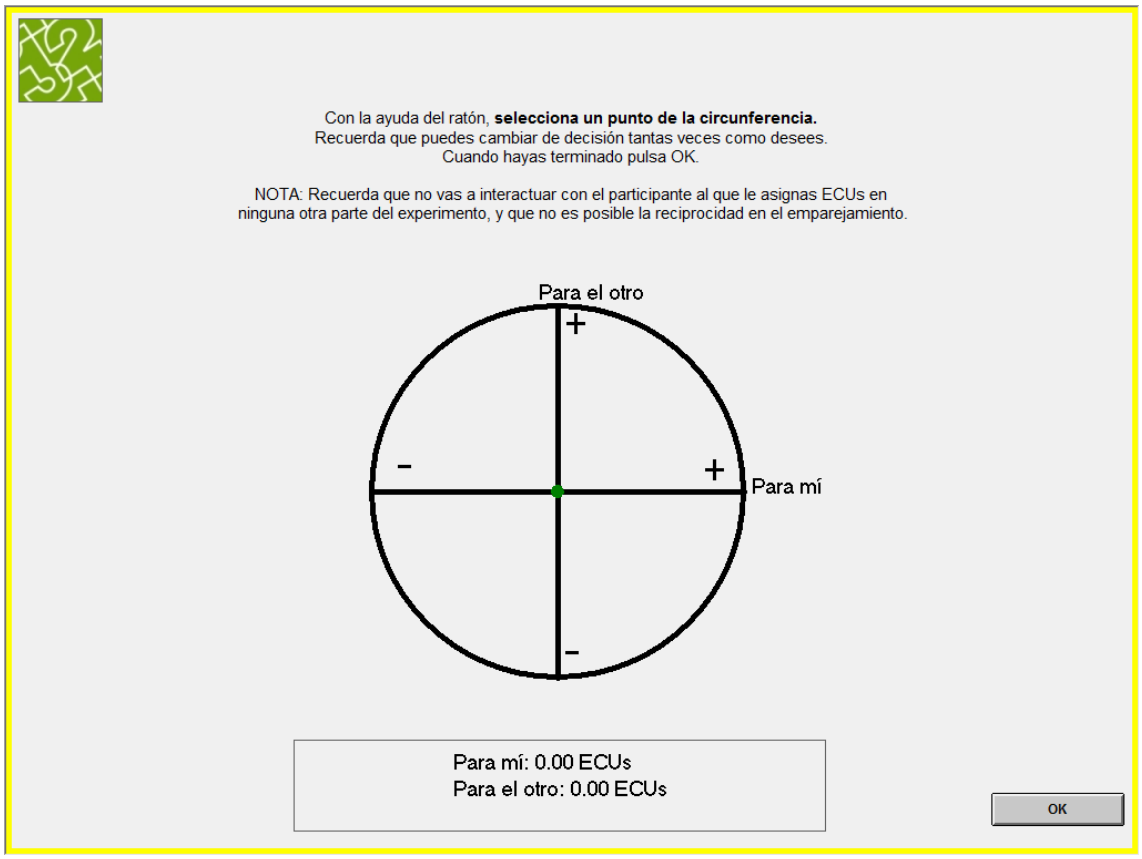
Screen 6



Part 2: Circle

Decision

Screen 7



With the help of the mouse select a point on the circle. Remember that you can change your decision as many times as you wish.

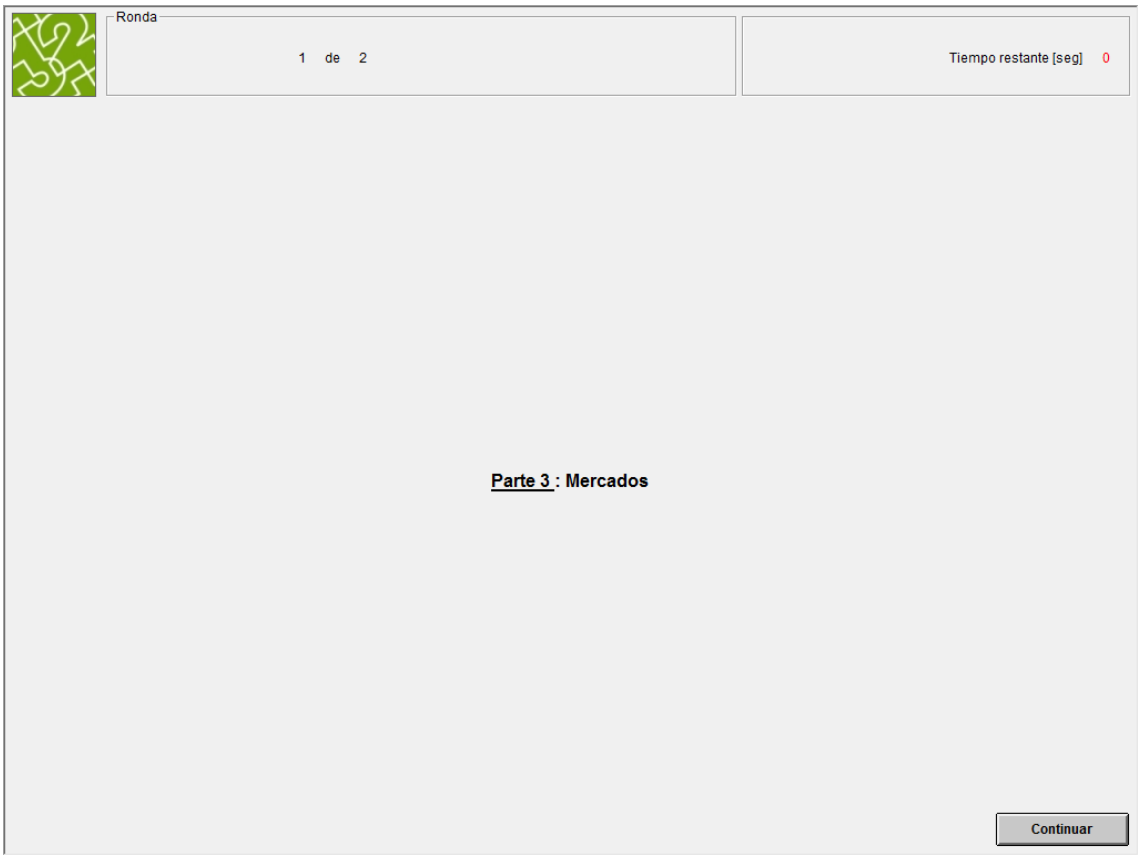
When you are finished press OK.

NOTE: Remember that you will not interact with the participant to whom you assign ECUs in no other part of the experiment and that reciprocity in the matching.

FOR ME: 0.00 ECUs

FOR THE OTHER: 0.00 ECUs

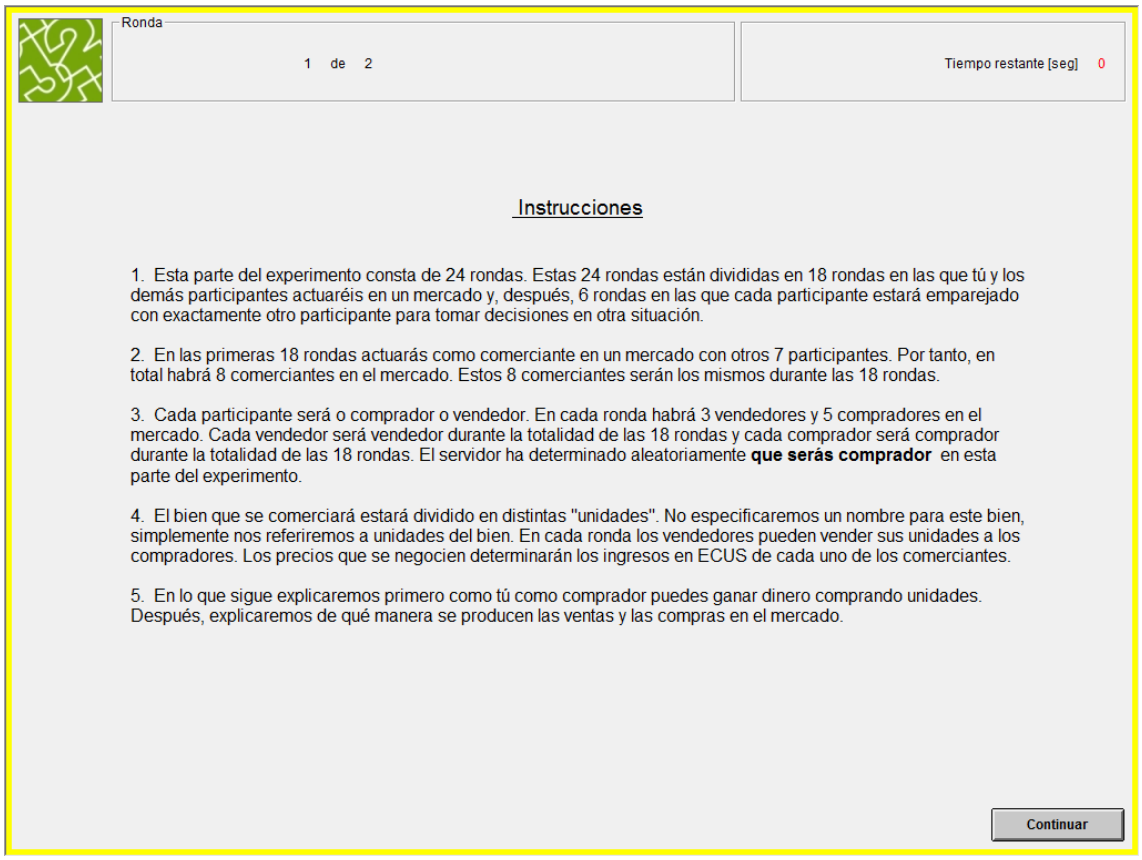
Screen 8



Part 3: Markets

BUYERS

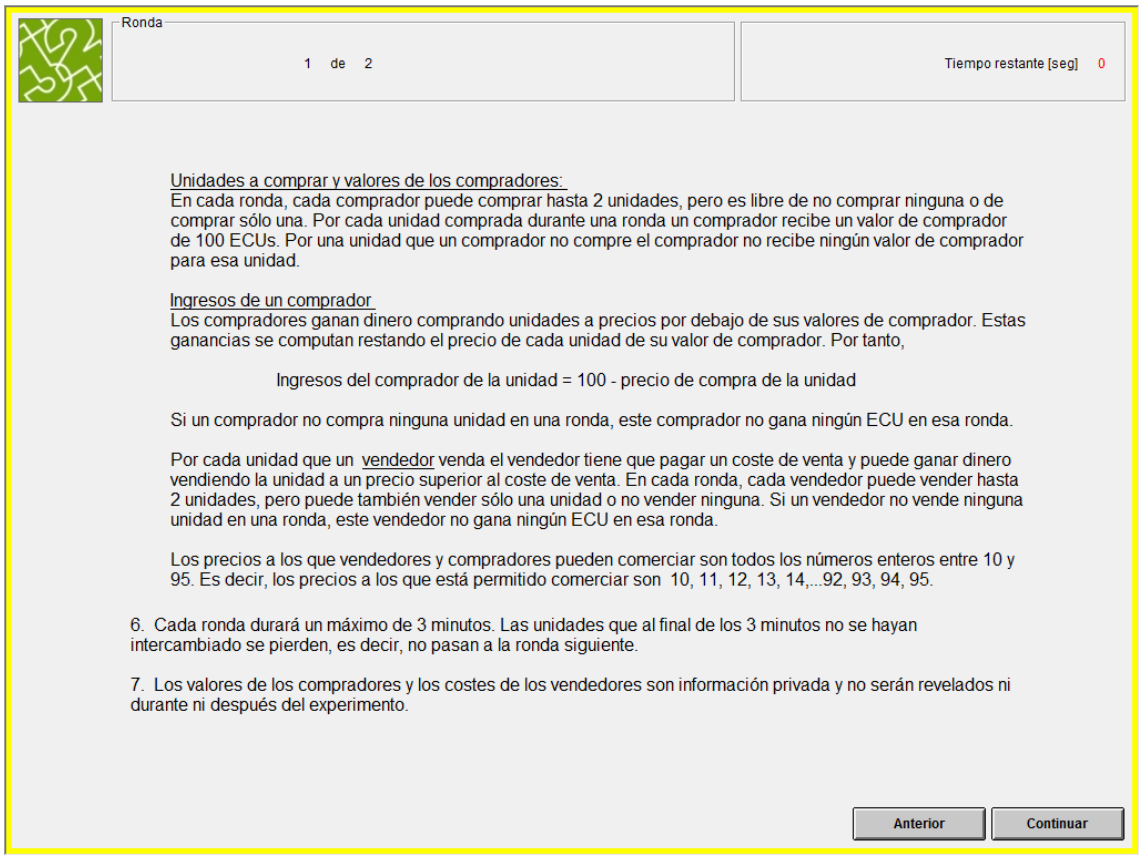
Screen 9



Instructions

1. This part of the experiment consists of 24 rounds. These 24 rounds are split into 18 rounds in which you and the other participants will act in a market and, thereafter, 6 rounds where each participant will be paired with exactly one other participant to make decisions in another situation.
2. In the first 18 rounds you will act as a trader in a market with 7 other participants. Hence, in total there will be 8 traders active in the market. These 8 traders will stay the same for all 18 rounds.
3. Each participant will be either a buyer or a seller. In each round there will be 3 sellers and 5 buyers active in the market. Each seller stays a seller throughout all 18 rounds and each buyer stays a buyer for all 18 rounds. The server has randomly determined that **you will be a buyer** in this part of the experiment.
4. The good to be traded is divided into distinct “units”. We will not specify a name for the good but simply refer to units of the good. In each round sellers can sell their units to the buyers. The prices that are negotiated will determine each trader's earnings in ECUs.
5. In the following we will first explain how you as a buyer can earn money through purchasing units. Thereafter, we will explain how sales and purchases are take place in the market.

Screen 10



Units to buy and buyer values:
In each round, each buyer can buy up to 2 units, but is free to buy no or only one unit. For each unit purchased during a round a buyer receives a buyer value of 100 ECUs. For a unit a buyer does not purchase the buyer does not receive any buyer value for that unit.

Buyer earnings:
Buyers make profits by purchasing units at prices that are below their buyer values. These profits are computed by subtracting each unit's price from its buyer value. Therefore,

buyer's earnings per unit = 100 - purchasing price for unit.

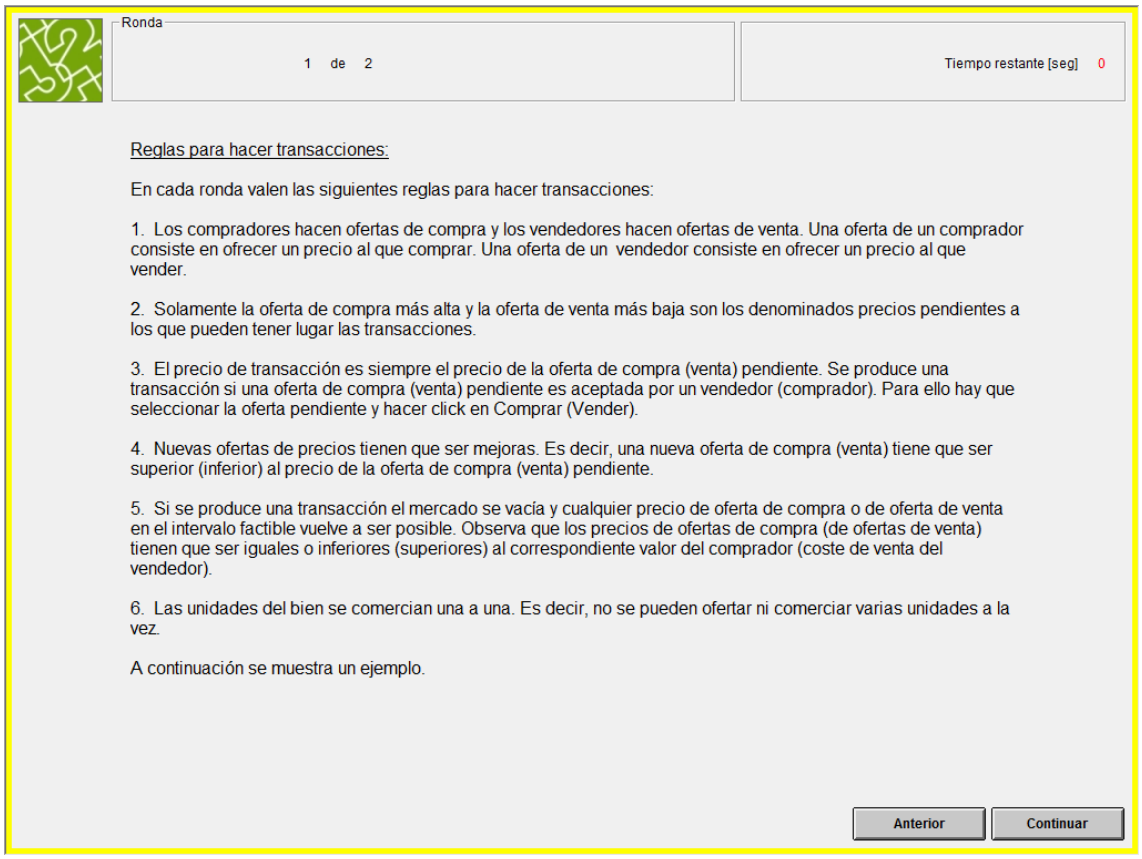
If a buyer does not purchase any unit in a round, this buyer does not earn any ECU's in this round.

For each unit a seller sells, the seller has to pay selling costs and can earn money by selling the unit at a price higher than the selling costs. In each round, each seller can sell at most 2 units, but may also sell no or only one unit. If a seller does not sell any unit in a round, this seller does not earn any ECU's in this round.

The prices at which sellers and buyers are allowed to trade are all inter numbers between 10 and 95. That is the permitted trading prices are {10, 11, 12, 13, 14, … , 92, 93, 94, 95}.

6. Each round will last a maximum of 3 minutes. The units that have not been exchanged at the end of the 3 minutes are lost, that is, they are not carried over to the next round.
7. Buyer values and seller costs are private information and will not be revealed neither during nor after the experiment.

Screen 11



Rules for making transactions

In each round there are the following rules for making transactions:

1. Buyers make buying offers and sellers make selling offers. A buying offer consists offering a price at which to purchase. A selling offer consists in offering a price at which to sell.

2. Only the highest buying offer and the lowest selling offers are the so-called standing prices at which trades can take place.

3. The transaction price is always the standing buying (selling) price. A transaction takes place if the standing buying (selling) is accepted by a seller (buyer).

4. Newly submitted price offers have to be improvements. That is, a new buying (selling) price has to be higher (lower) than the standing buying (selling) price.

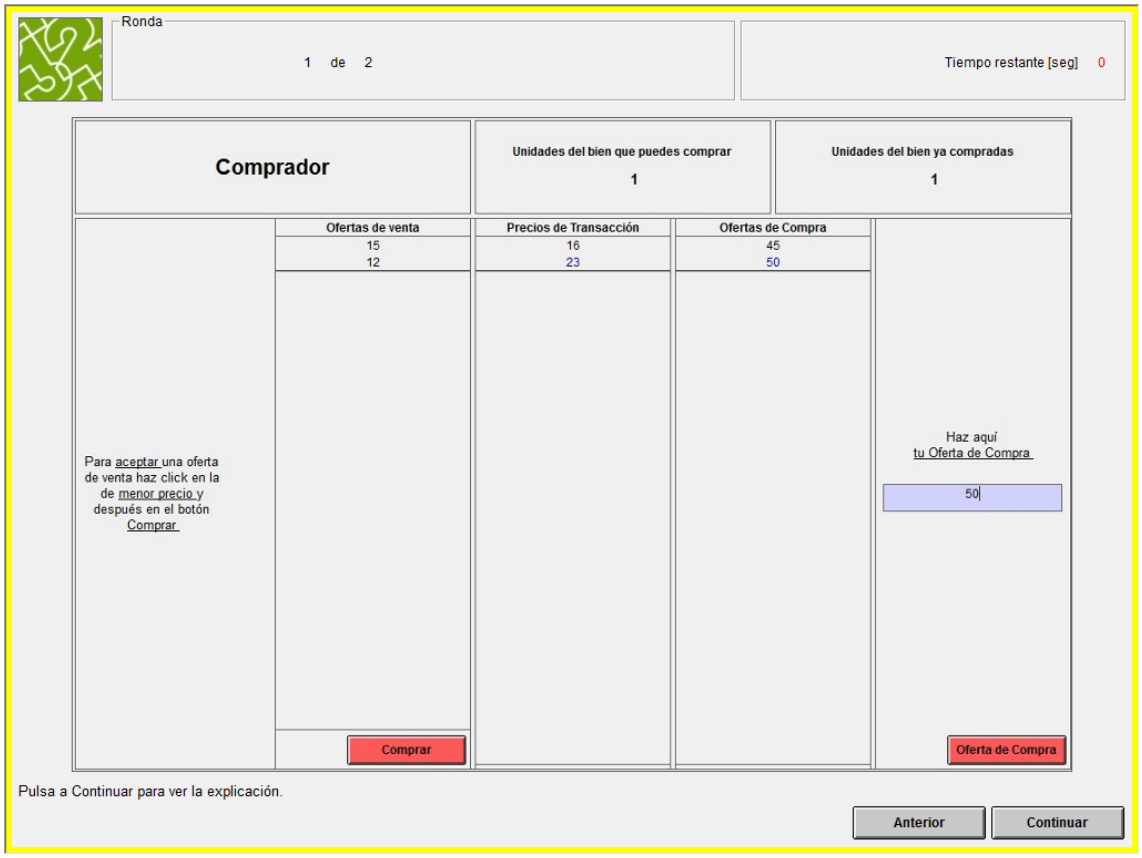
5. If a transaction takes place the market clears and any buying and selling price in the feasible range are possible again. Note, that the prices of the buying offers (selling offers) bid (ask) prices have to be equal or lower (higher) than the corresponding buyer value (seller selling cost).

6. The units of the goods are traded one by one. That is, it is not possible to offer or exchange several units at the same time.

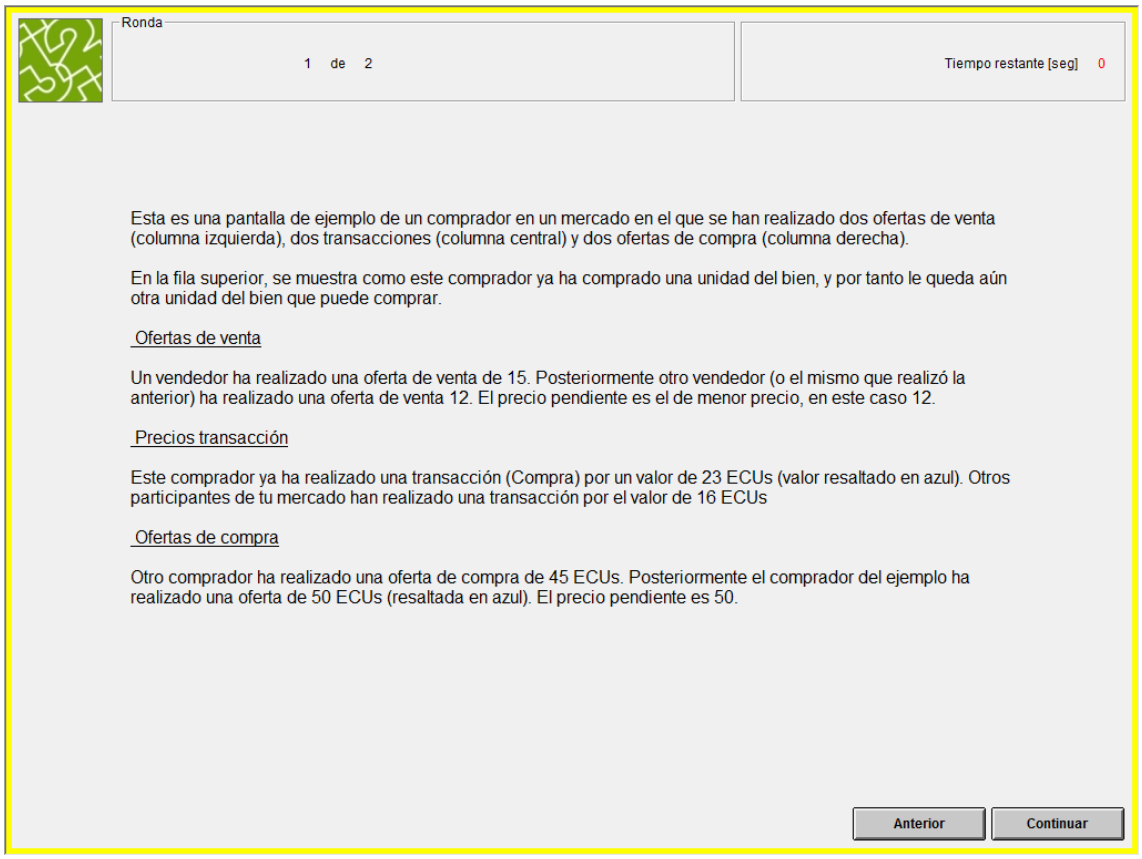
In what follows we show you an example.

Screen 12

This is the screen a buyer faces when making and accepting offers in a round, which is explained on the next screen.



Screen 13



This is an example screen of a buyer in a market in which two selling offers (left column), two transaction (central column) and two buying offers (right column) have been made.

In the top row, it is shown that this buyer has already purchased one unit of the good, and therefore he still can buy one other unit.

Selling offers

A seller has made a selling offer of 15. After that another seller (or the same that made the earlier offer) has made a selling offer of 12. The standing price is the lower price, in this case 12.

Transaction prices

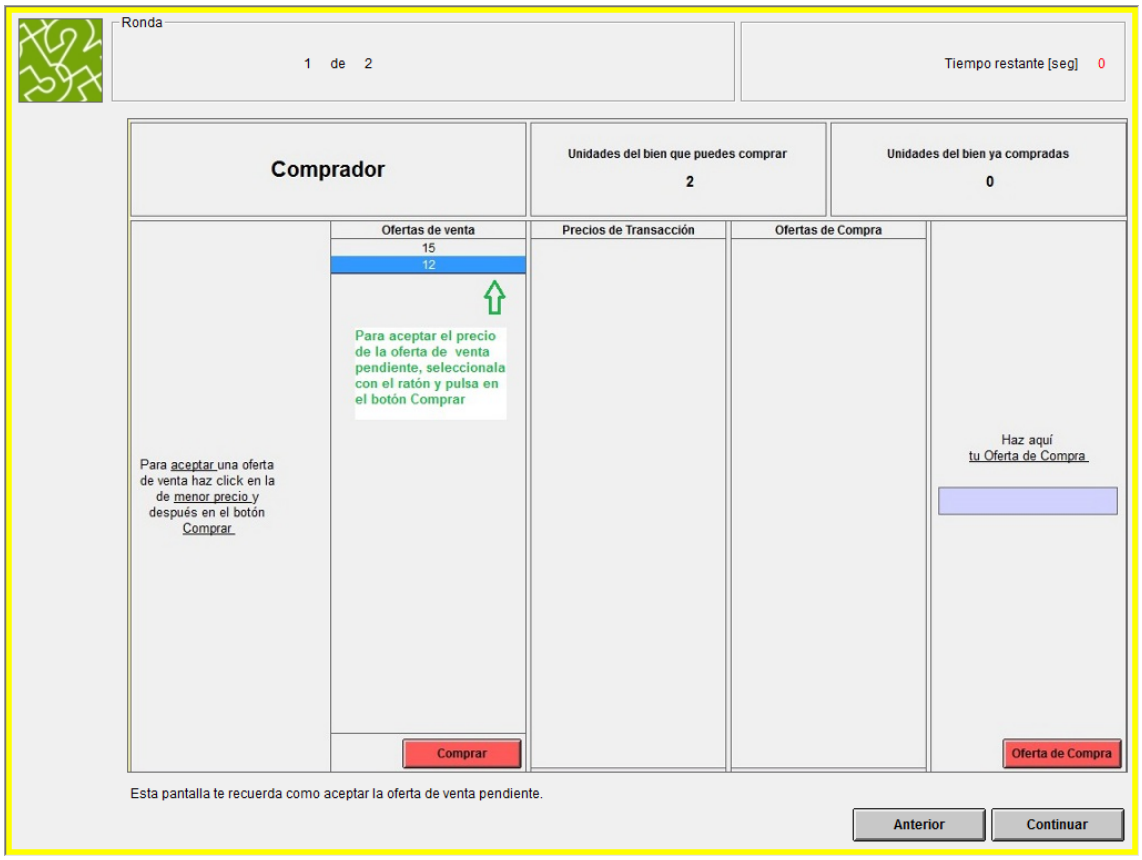
This buyer has already made one transaction (purchase) for a value of 23 ECUs (value shown in blue). Other participants of your market have made a transaction for a value of 16 ECUs.

Buying offers

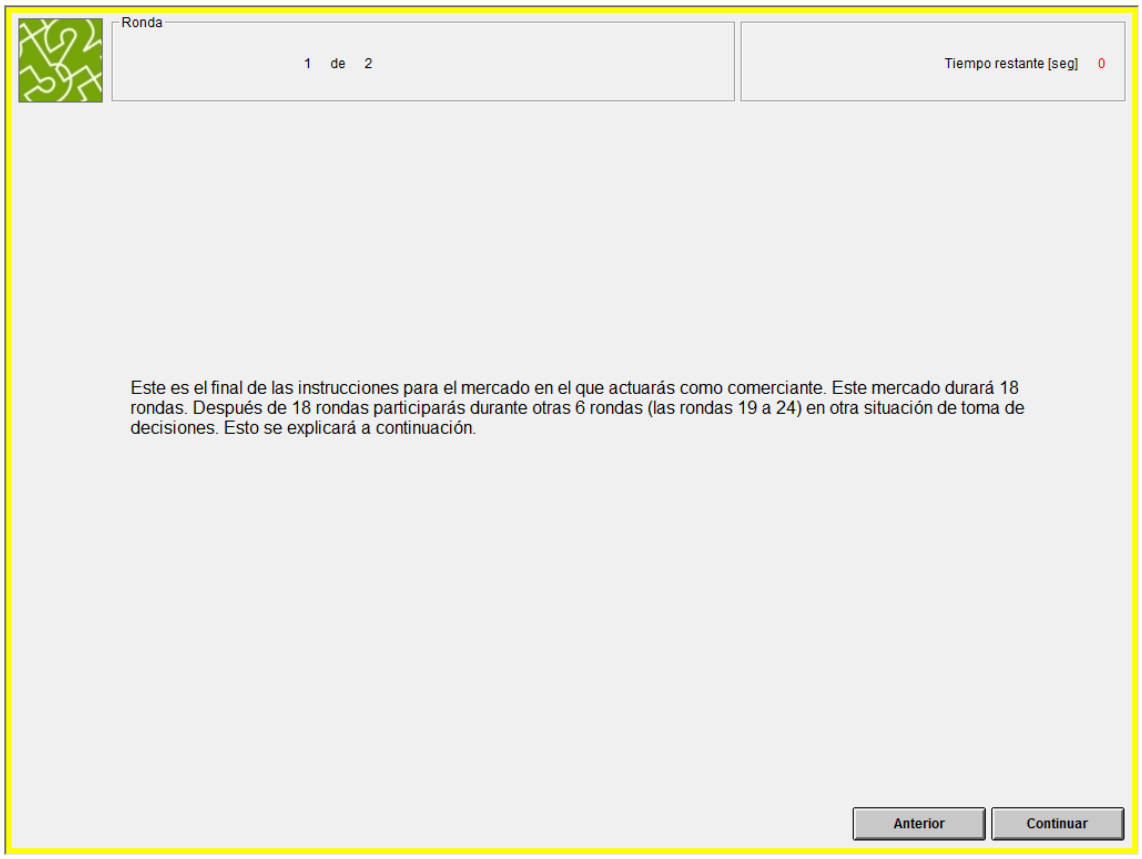
Another buyer has made a buying offer of 45 ECUs. After that the buyer of the example has made an offer of 50 ECUs (highlighted in blue). The standing price is 50.

Screen 14

This screen is the same as screen 12, but with the highlighted items mentioned on screen 13.

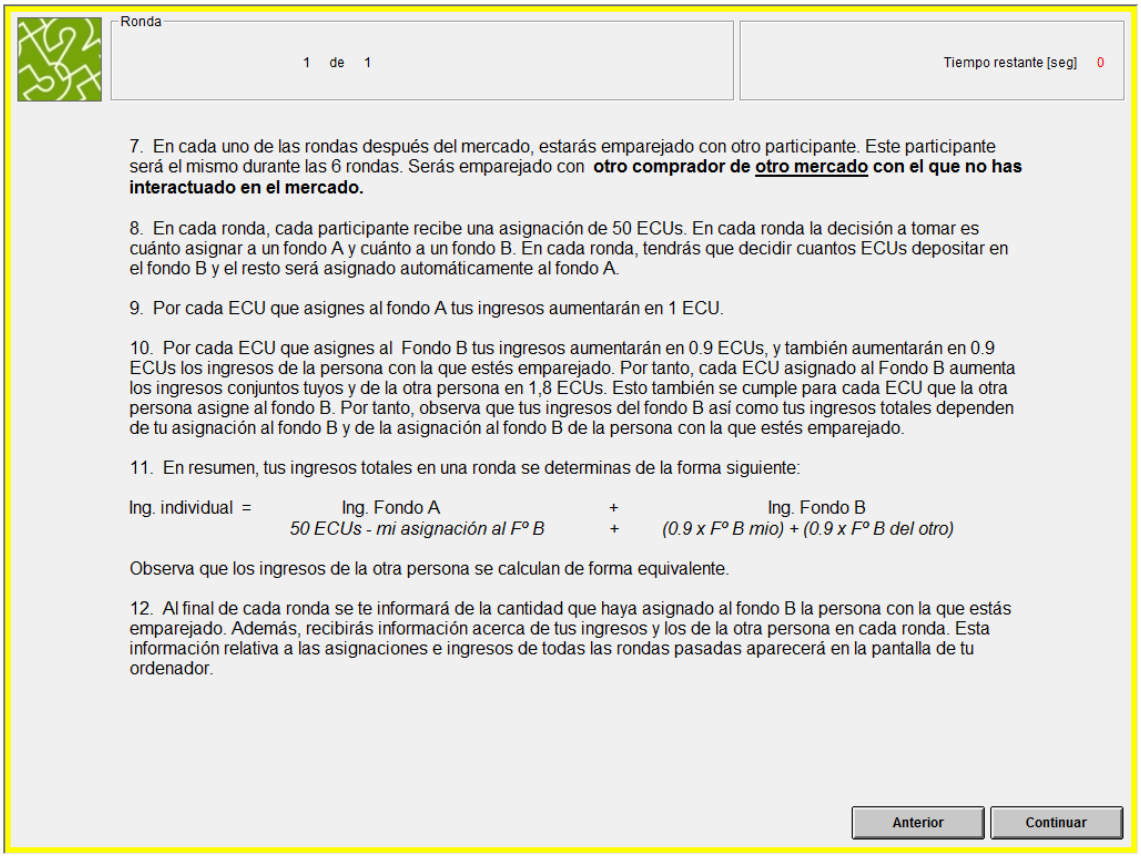


Screen 15



This is the end of the instructions for the market where you will act as a trader. This market will last for 18 rounds. After 18 rounds you will participate for another 6 rounds (rounds 19 to 24) in another decision making situation. This will be explained below.

Screen 16



7. In each of the rounds that follow the market, you are paired with one other participants. This other participant will be the same throughout these 6 rounds. You will be paired with **another buyer** *[in buyer-seller pairs: "***another seller"***]* **from another market with whom you have not interacted in the market** *[in Market-Partners:* **"another buyer** *[in buyer-seller pairs: "***another seller"***]* **with whom you have also interacted in the market."***]*

[In OSDG-MS (OSDG-MP) this item read: "In each of the rounds that follow, you are paired with one other participants. This other participant will be the same throughout these 6 rounds. You have been assigned initial earnings of 346~ECU (340~ECU). The other group member is also assigned some initial earnings. The assignments to you and the other group member are not necessarily the same. You and the other group member will receive this amount independently of what occurs during the experiment."*]*

[In OSDG-MS (OSDG-MP) with lump-sum income transparency this item read: "In each of the rounds that follow, you are paired with one other participants. This other participant will be the same throughout these 6 rounds. You have been assigned initial earnings of 346~ECU (340~ECU). The other group member is assigned initial earnings of 346~ECU (340~ECU). *[in mixed-pay pairs:* "The other group member is assigned initial earnings of 2656~ECU (2672~ECU)."*]* You and the other group member will receive this amount independently of what occurs during the experiment."*]*

8. In each round, each participant receives an endowment of 50 ECUs. In each round the decision to be made is to choose how much you allocate to a Fund A and to a Fund B. In each round, you will have to decide how many ECUs to deposit in fund B and the rest will be allocated to fund A automatically.

9. For each ECU that you allocate to fund A your earnings will increase by 1 ECU.

10. For each ECU that you allocate to fund B, your earnings will increase by 0.9 ECUs, and also the earnings of the other person you are paired with will increase by 0.9 ECUs. Hence, each ECU allocated to fund B increases your and the other's earnings together with 1.8 ECU. This also holds for each ECU the other person allocates to fund B. Hence, note that your earnings from fund B as well as your total earnings, depend on your allocation to fund B and on the allocation to fund B of the person you are paired with.

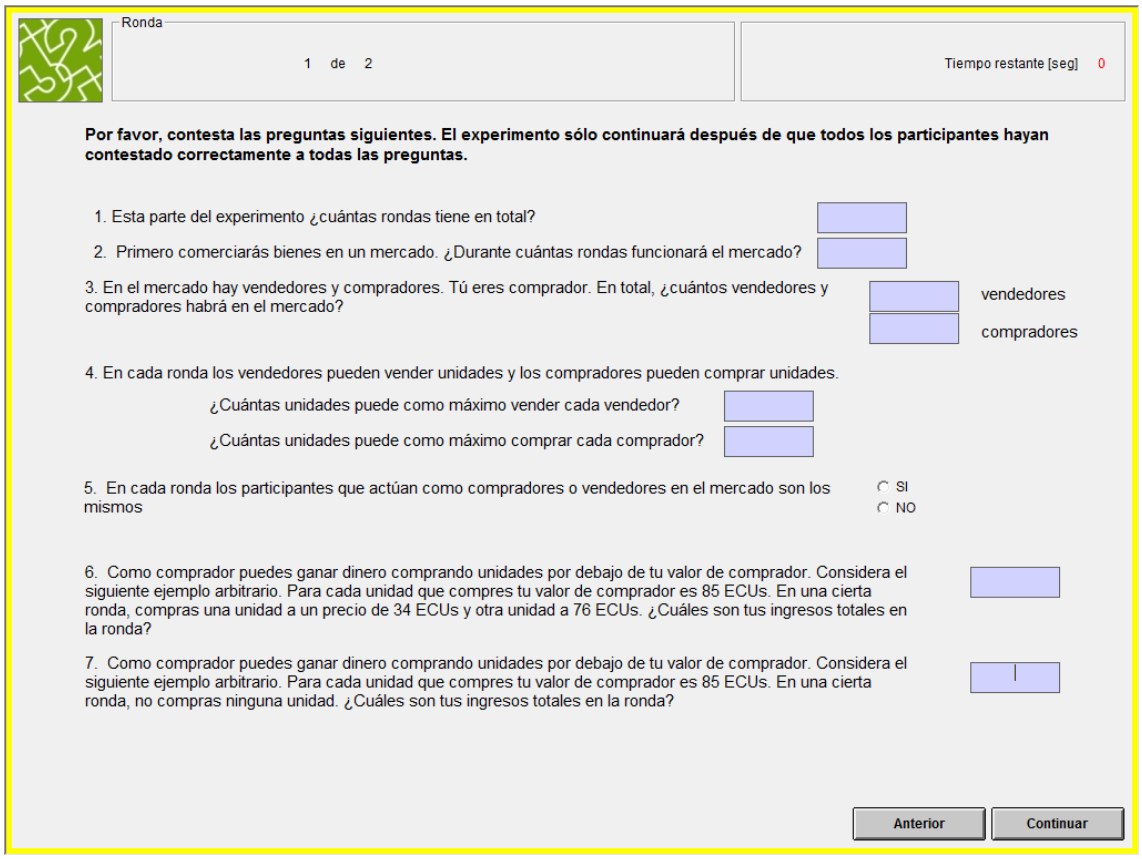
11. In summary, your total earning in one round are determined in the following way:

Individual earnings =	Earnings fund A	+	Earnings fund B
	50 ECU – my allocation to fund B +		*(0.9 x my allocation to fund B) + (0.9 x other person's allocation to fund B)*

Note, that the earnings of the other person are calculated in an equivalent way.

12. At the end of each round, you will be informed of the allocation to fund B of the person you are paired with. Moreover, you will receive information on your and the other person's earnings in each round. This information regarding the allocation and earnings of all previous rounds will appear on your computer screen.

Screen 17



Please answer the following questions. The experiment will proceed only after all participants have correctly answered all questions.

1. This part of the experiment has in total how many rounds.
   Answer: XXX rounds

2. First you will trade goods on a market. For how many rounds will the market be open?

   Answer: XXX rounds

3. In the market there are sellers and buyers. You are a buyer. In total, how many sellers and buyers will be active on the market?

   Answer: XXX sellers, XXX buyers

4. In each round sellers can sell units and buyers can buy units. How many units can each seller sell at most and each buyer buy at most?

   Answer: X units

5. In each round the same participants will be active as buyers and sellers on the market?

   Answer: YES NO

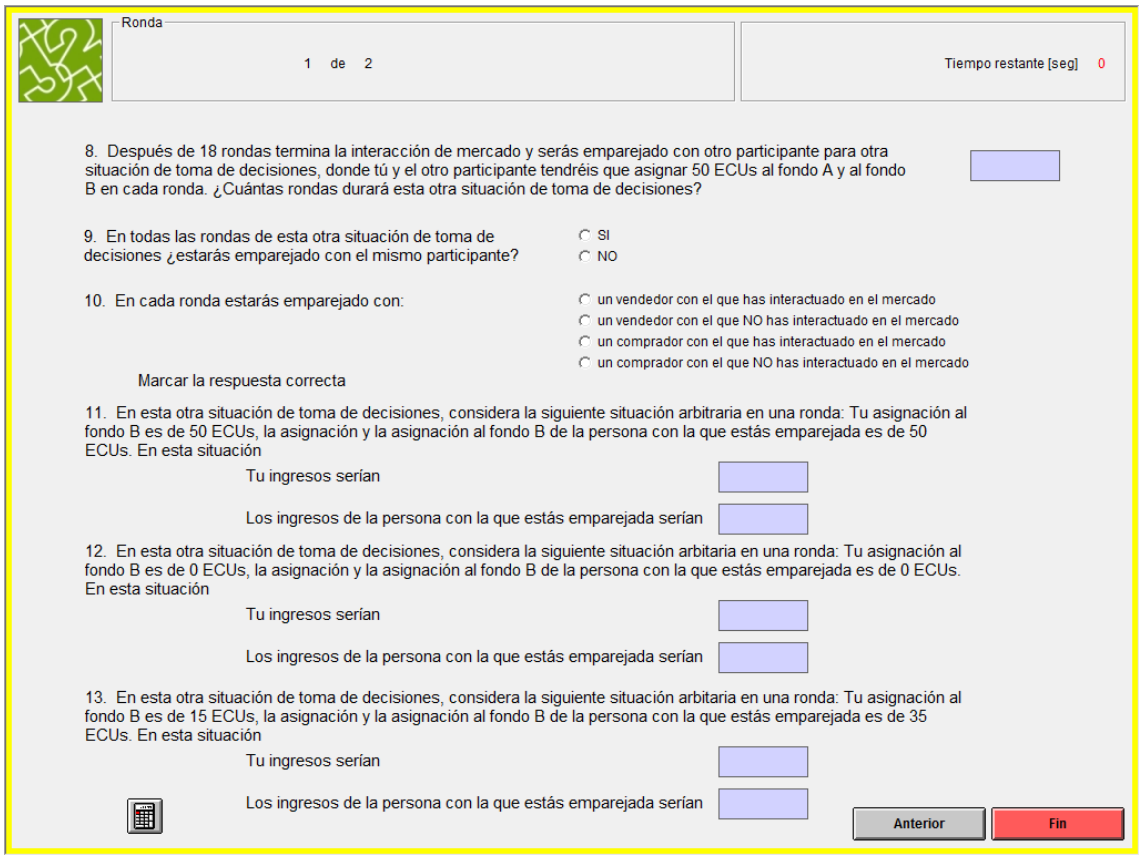
6. As a buyer you can earn money by buying units at prices below your buyer value. Consider the following arbitrary example. For each unit you buy your buyer value is 85 ECU. In a given round, you buy one unit at a price of 34 ECU and another unit at a price of 76 ECU. What are your total earnings in that round?

   Answer: XX ECU

7. As a buyer you can earn money by buying units at prices below your buyer value. Consider the following arbitrary example. For each unit you buy your buyer value is 85 ECU. In a given round, you do not buy any unit. What are your total earnings in that round?

   Answer: XX ECU

Screen 18



8. After 18 rounds the market interaction is over and you will be paired with one other participant for another decision making situation, where you and the other participant will have to allocate 50 ECU to fund A and fund B in each round. How many rounds will this other decision making situation last?

   Answer: XX rounds

9. In all 6 rounds of this other decision making situation you are paired with the same other participant?

   Answer: YES NO

10. In each round you are paired with:

    O a seller you have interacted with in the market
    O a seller you have NOT interacted with in the market
    O a buyer you have interacted with in the market
    O a buyer you have NOT interacted with in the market

    (check the correct answer)

11. In this other decision making situation, Consider the following arbitrary situation in a round:
    Your allocation to the fund B is 50 ECU, the allocation to fund B of the person you are

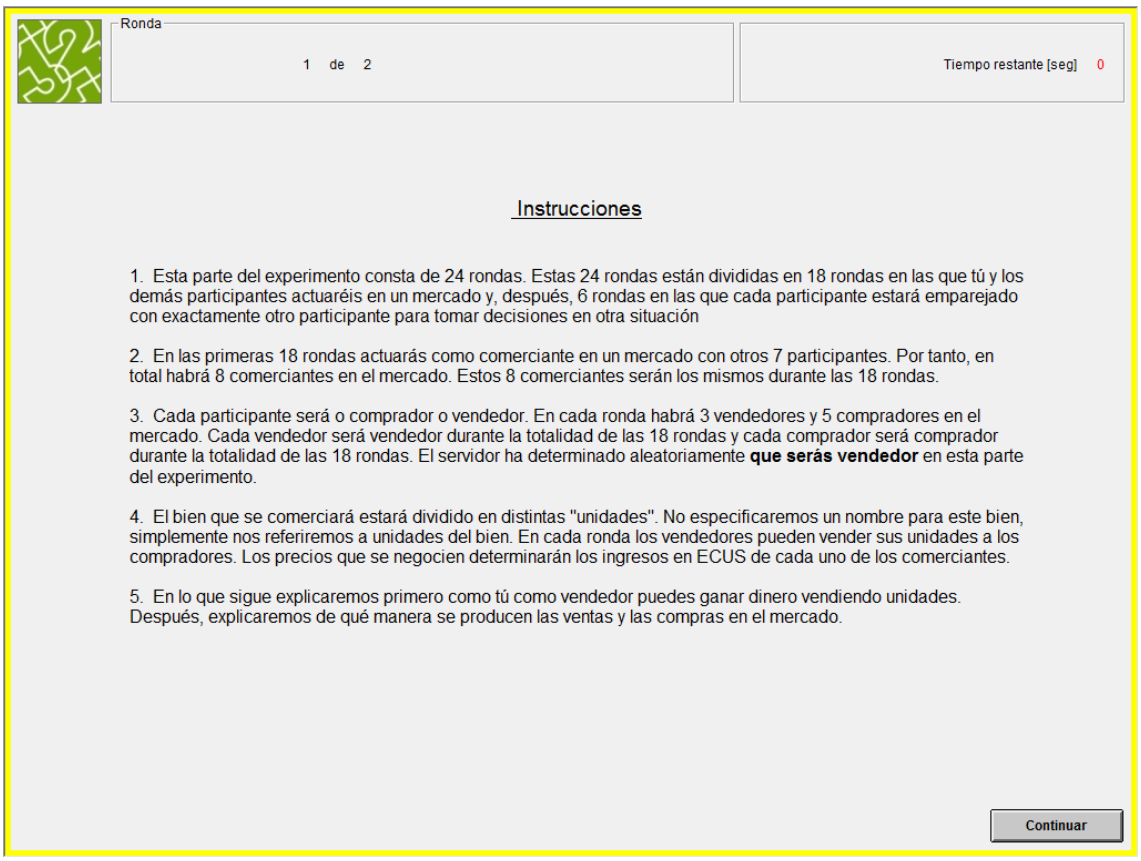
paired with is 50 ECU.
In this situation
your earnings would be: …...
the earnings of the person you are paired with would be: ….

12. In this other decision making situation. Consider the following arbitrary situation in a round:
Your allocation to the fund B is 0 ECU, the allocation to fund B of the person you are paired with is 0 ECU.
In this situation
your earnings would be: …...
the earnings of the person you are paired with would be: ….

13. In this other decision making situation. Consider the following arbitrary situation in a round:
Your allocation to the fund B is 12 ECU, the allocation to fund B of the person you are paired with is 34 ECU.
In this situation
your earnings would be: …...
the earnings of the person you are paired with would be: ….

SELLERS

Screen 19



Instructions

1. This part of the experiment consists of 24 rounds. These 24 rounds are split into 18 rounds where you and the other participants will act on a market and, thereafter, 6 rounds where each participant will be paired with exactly one other participant to make allocation decisions in another situation.

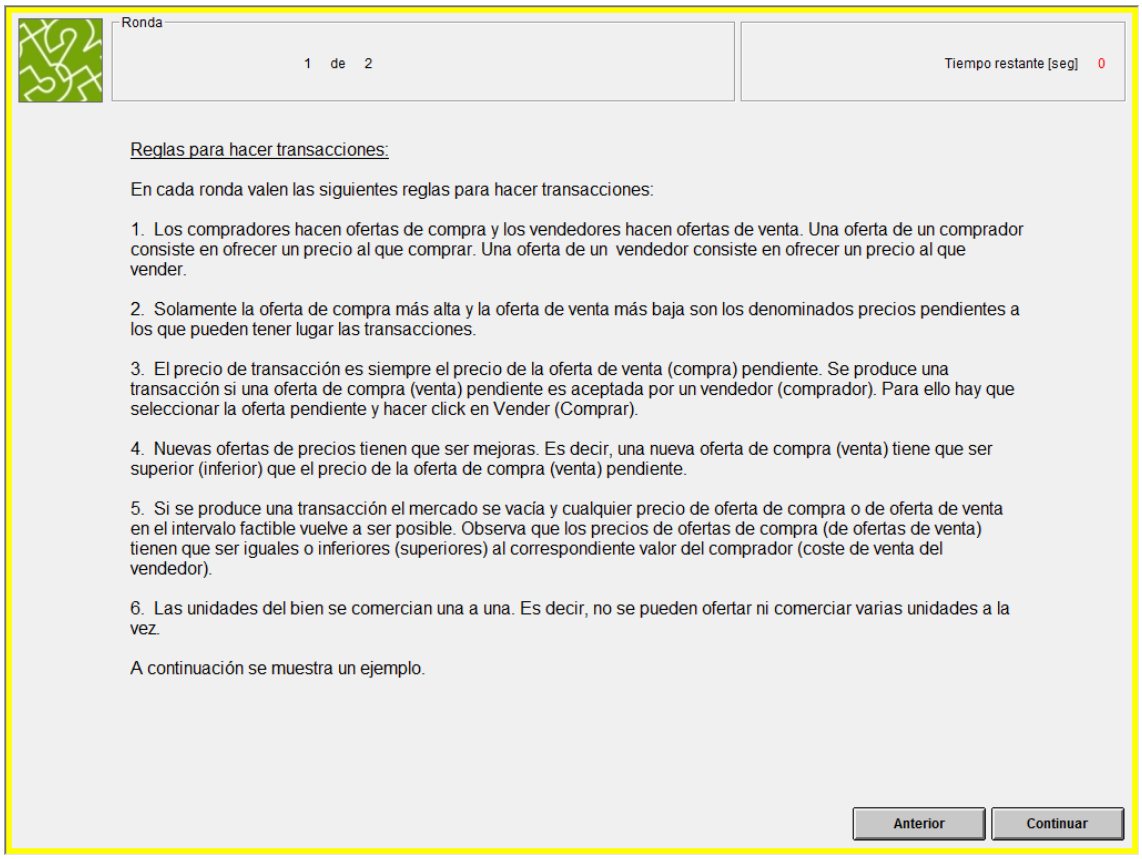
2. In the first 18 rounds you will act as a trader on a market with 7 other participants. Hence, in total there will be 8 traders active on the market. These 8 traders will stay the same for all 18 rounds.

3. Each participant will be either a buyer or a seller. In each round there will be 3 sellers and 5 buyers on the market. Each seller stays a seller throughout all 18 rounds and each buyer stays a buyer for all 18 rounds. The server has randomly determined that **you will be a seller** in this part of the experiment.

4. The good to be traded is divided into distinct “units”. We will not specify a name for the good but simply refer to units. In each round sellers can sell their units to buyers. The prices that are negotiated will determine each trader's earnings in ECUs.

5. In the following we will first explain how you as a seller can earn money through selling units. Thereafter, we will explain how sales and purchases are arranged on the market.

Screen 20



Rules for making transactions

In each round there are the following rules for making transactions:

1. Buyers make buying offers and sellers make selling offers. A buying offer consists offering a price at which to purchase. A selling offer consists in offering a price at which to sell.
2. Only the highest buying offer and the lowest selling offers are the so-called standing prices at which trades can take place.

3. The transaction price is always the standing buying (selling) price. A transaction takes place if the standing buying (selling) is accepted by a seller (buyer).

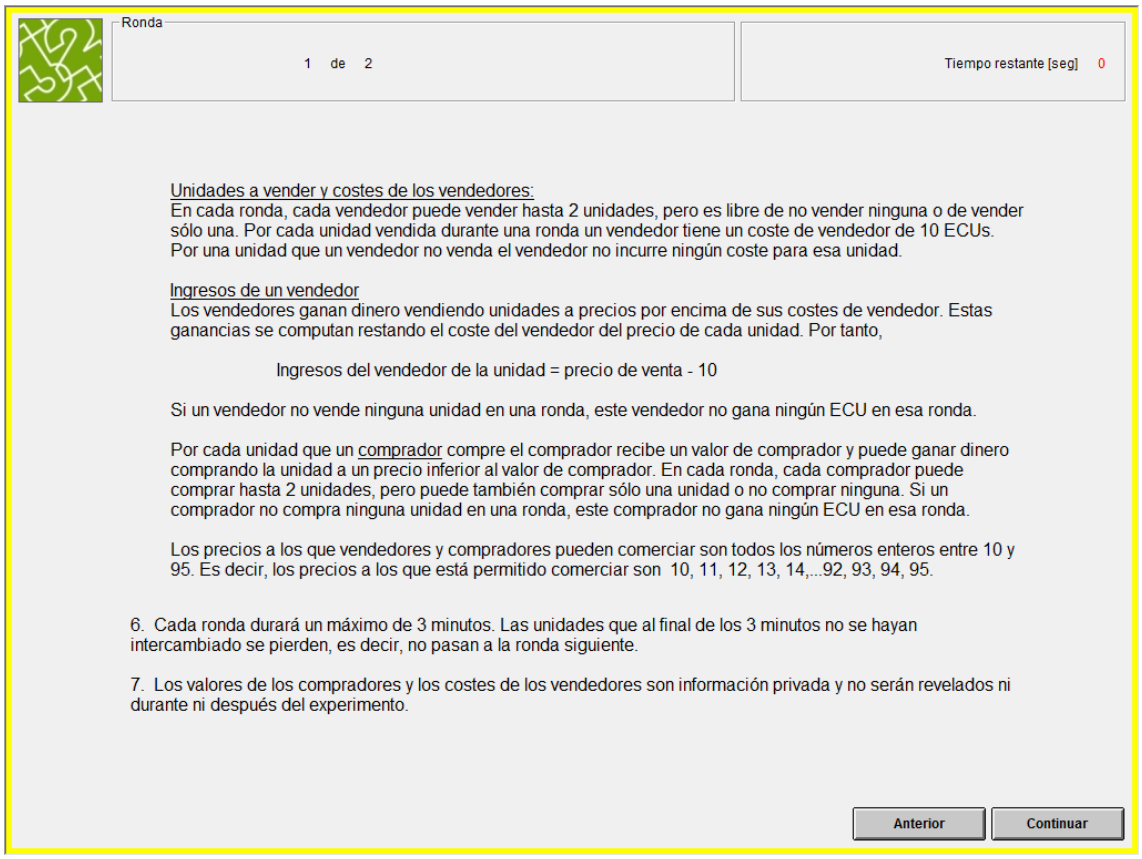
4. Newly submitted price offers have to be improvements. That is, a new buying (selling) price has to be higher (lower) than the standing buying (selling) price.

5. If a transaction takes place the market clears and any buying and selling price in the feasible range are possible again. Note, that the prices of the buying offers (selling offers) bid (ask) prices have to be equal or lower (higher) than the corresponding buyer value (seller selling cost).

6. The units of the goods are traded one by one. That is, it is not possible to offer or exchange several units at the same time.

In what follows we show you an example.

Screen 21



Units to sell and seller costs:
In each round, each seller can sell at most 2 units, but is free to sell no units or only one unit. For each unit sold during a round a seller incurs costs of 10 ECUs. For a unit a seller does not sell the seller does not incur the costs for that unit.

Seller earnings:

Sellers make profits by selling units at prices that are above their costs. These profits are computed by subtracting each unit's costs from its selling price. Therefore,

seller's earnings per unit = selling price for unit – 10.

If a seller does not sell any unit in a round, this seller does not earn any ECU's in this round.

For each unit a buyer purchases, the buyer receives a buyer value and can earn money by purchasing the unit at a price lower than the buyer value. In each round, each buyer can purchase at most 2 units, but may also purchase no units or only one unit. If a buyer does not purchase any unit in a round, this buyer does not earn any ECU's in this round.

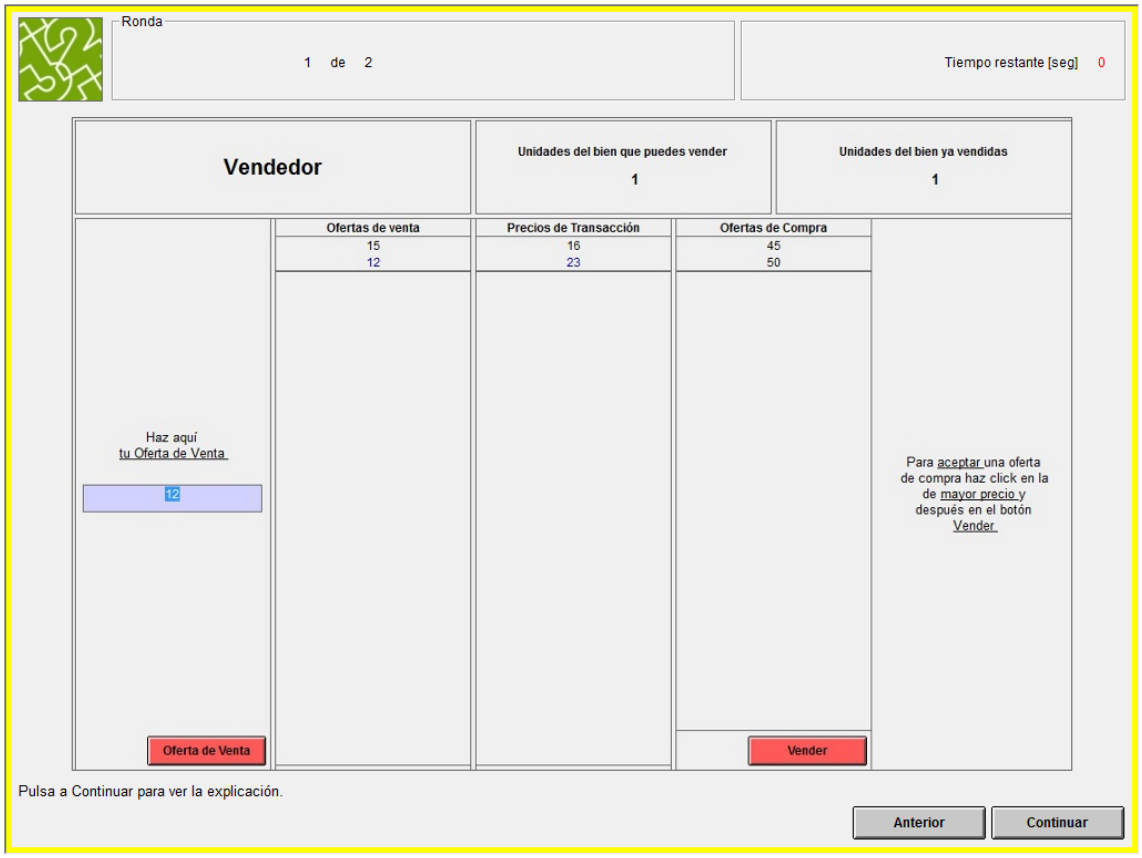
The prices at which sellers and buyers are allowed to trade are all inter numbers between 10 and 95. That is the permitted trading prices are {10, 11, 12, 13, 14, … , 92, 93, 94, 95}.

6. Each round will last for a maximum of 3 minutes. The units that at the end of the 3 minutes have not been sold are lost, that is, they are not carried over to the next round.

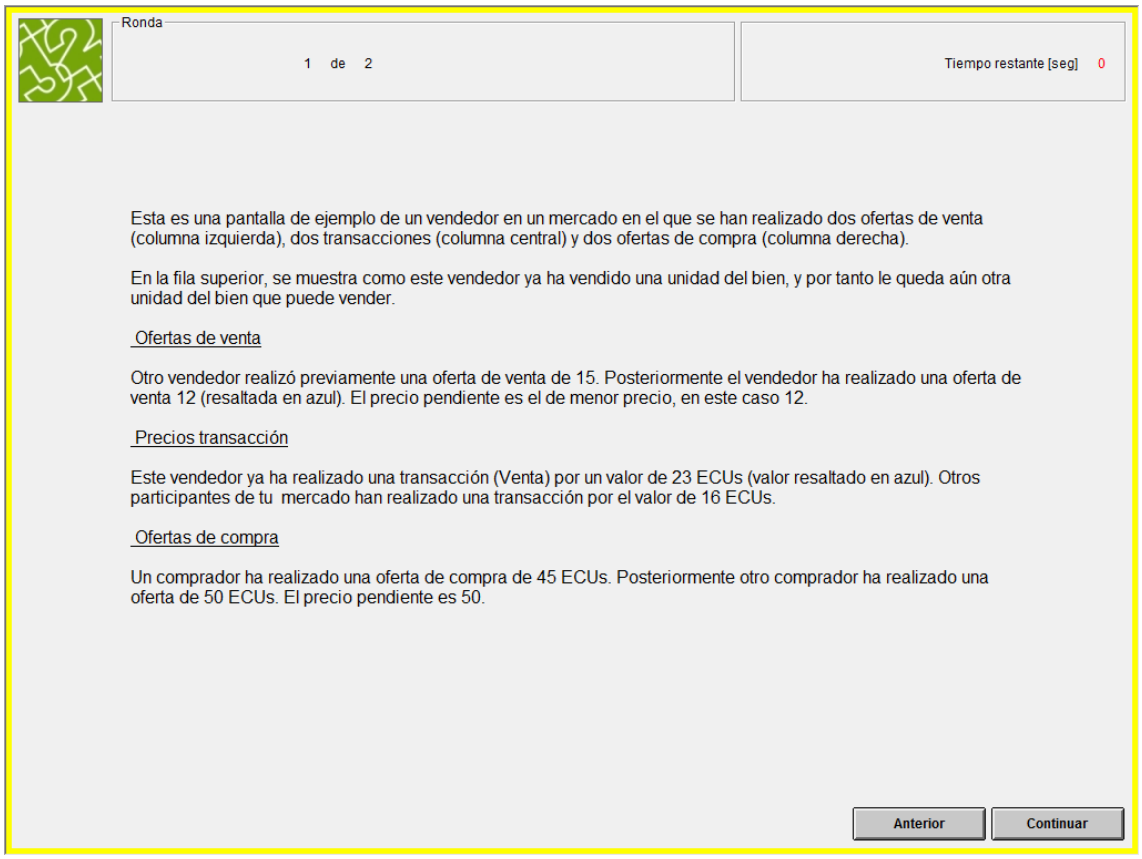
7.Buyer values and seller costs are private information and will not be revealed neither during nor after the experiment.

Screen 22

This is the screen a buyer faces when making and accepting offers in a round, which is explained on the next screen.



Screen 23



This is an example screen of a seller in a market in which two selling offers (left column), two transactions (central column) and two buying offers (right column) have been made.

In the top row, it is shown that this seller has already purchased one unit of the good, and therefore he still can sell one other unit.

Selling offers

Another seller has made a selling offer of 15. After this seller has made a selling offer of 12 (highlighted in blue). The standing price is the lower price, in this case 12.

Transaction prices

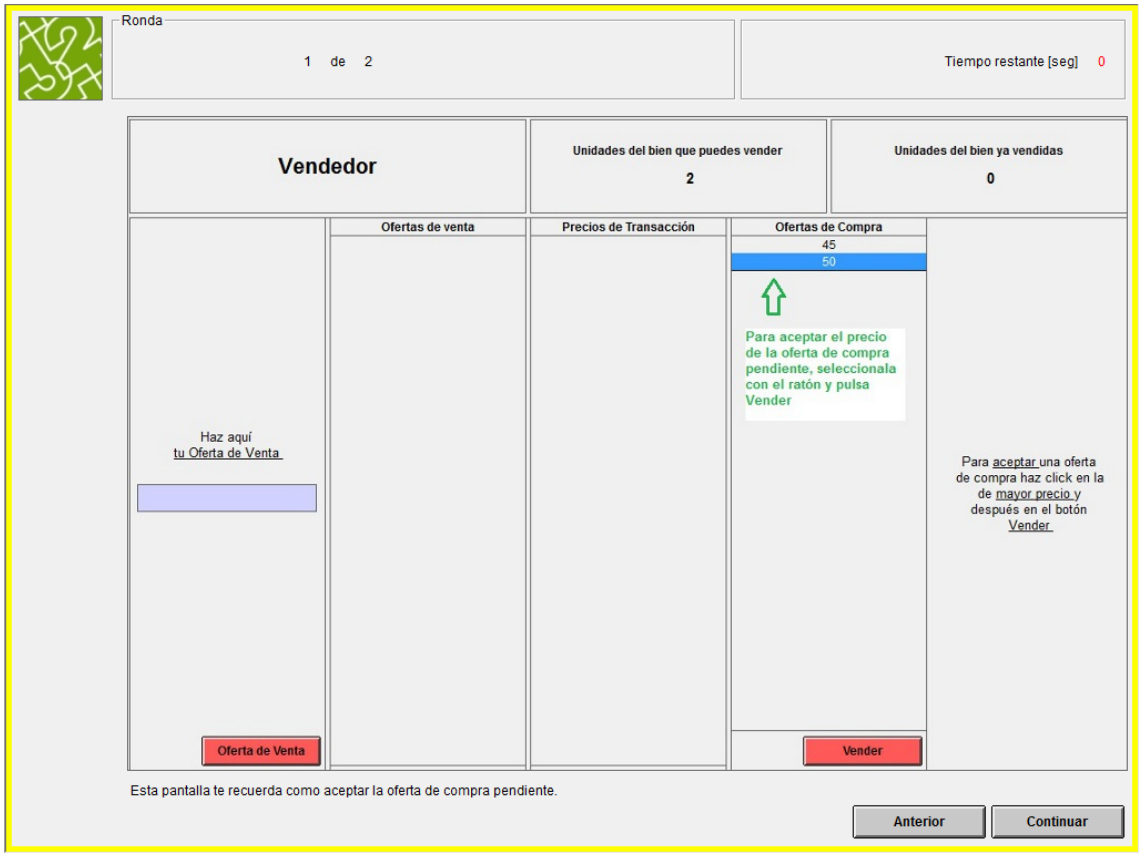
This seller has already made one transaction (sale) for a value of 23 ECUs (value shown in blue). Other participants of your market have made a transaction for a value of 16 ECUs.

Buying offers

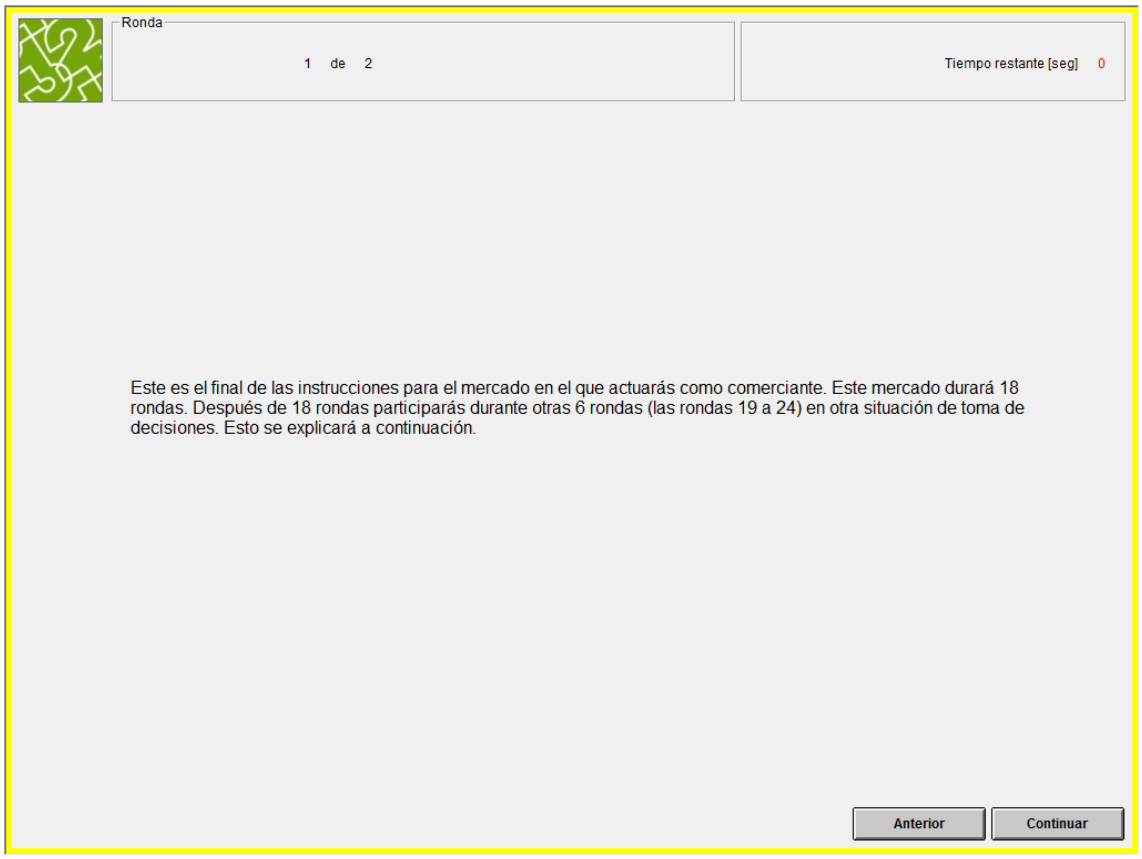
Another buyer has made a buying offer of 45 ECUs. After that another buyer has made an offer of 50 ECUs. The standing price is 50.

Screen 24

This screen is the same than screen 22, but with the highlighted items mentioned on screen 23.

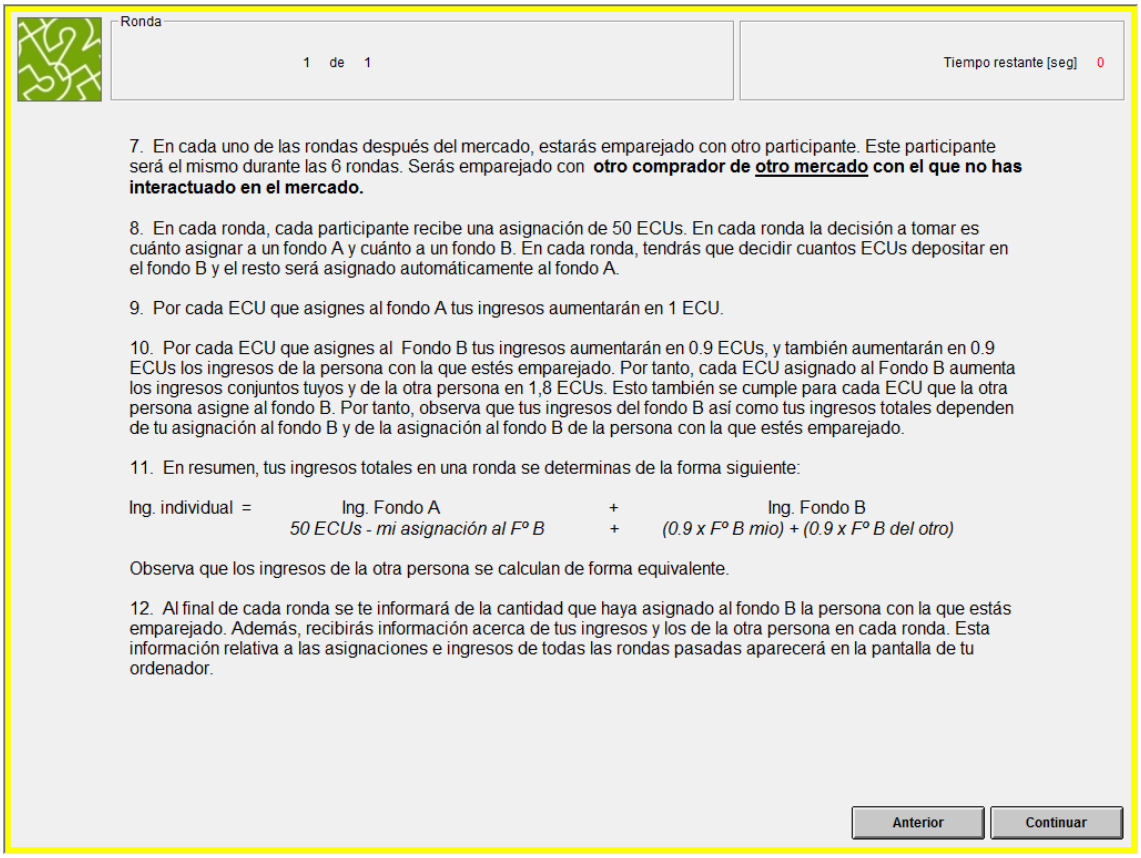


Screen 25



This is the end of the instructions for the market where you will act as a trader. This market will last for 18 rounds. After 18 rounds you will participate for another 6 rounds (rounds 19 to 24) in another decision making situation. This will be explained below.

Screen 26



7.In each of the rounds that follow the market, you are paired with one other participants. This other participant will be the same throughout these 6 rounds. You will be paired with **another buyer** *[in seller-seller pairs: "***another seller"***]* **from another market with whom you have not interacted in the market** *[in Market-Partners: "***another buyer** *[in seller-seller pairs: "***another seller"***] with whom you have also interacted in the market."].**

[In OSDG-MS (OSDG-MP) this item read: "In each of the rounds that follow, you are paired with one other participants. This other participant will be the same throughout these 6 rounds. You have been assigned initial earnings of 2656~ECU (2672~ECU). The other group member is also assigned some initial earnings. The assignments to you and the other group member are not necessarily the same. You and the other group member will receive this amount independently of what occurs during the experiment."*]*

[In OSDG-MS (OSDG-MP) with lump-sum income transparency this item read: "In each of the rounds that follow, you are paired with one other participants. This other participant will be the same throughout these 6 rounds. You have been assigned initial earnings of 2656~ECU (2672~ECU). The other group member is assigned initial earnings of 346~ECU (340~ECU). *[in high-pay pairs:* "The other group member is assigned initial earnings of 2656~ECU (2672~ECU)."*]* You and the other group member will receive this amount independently of what occurs during the experiment."*]*

8. In each round, each participant receives an endowment of 50 ECUs. In each round the decision to be made is to choose how much you allocate to a Fund A and to a Fund B. In each round, you will have to decide how many ECUs to deposit in fund B and the rest will be allocated to fund A automatically.

9.For each ECU that you allocate to fund A your earnings will increase by 1 ECU.

10.For each ECU that you allocate to fund B, your earnings will increase by 0.9 ECUs, and also the earnings of the other person you are paired with will increase by 0.9 ECUs. Hence, each ECU allocated to fund B increases your and the other's earnings together with 1.8 ECU. This also holds for each ECU the other person allocates to fund B. Hence, note that your earnings from fund B as well as your total earnings, depend on your allocation to fund B and on the allocation to fund B of the person you are paired with.

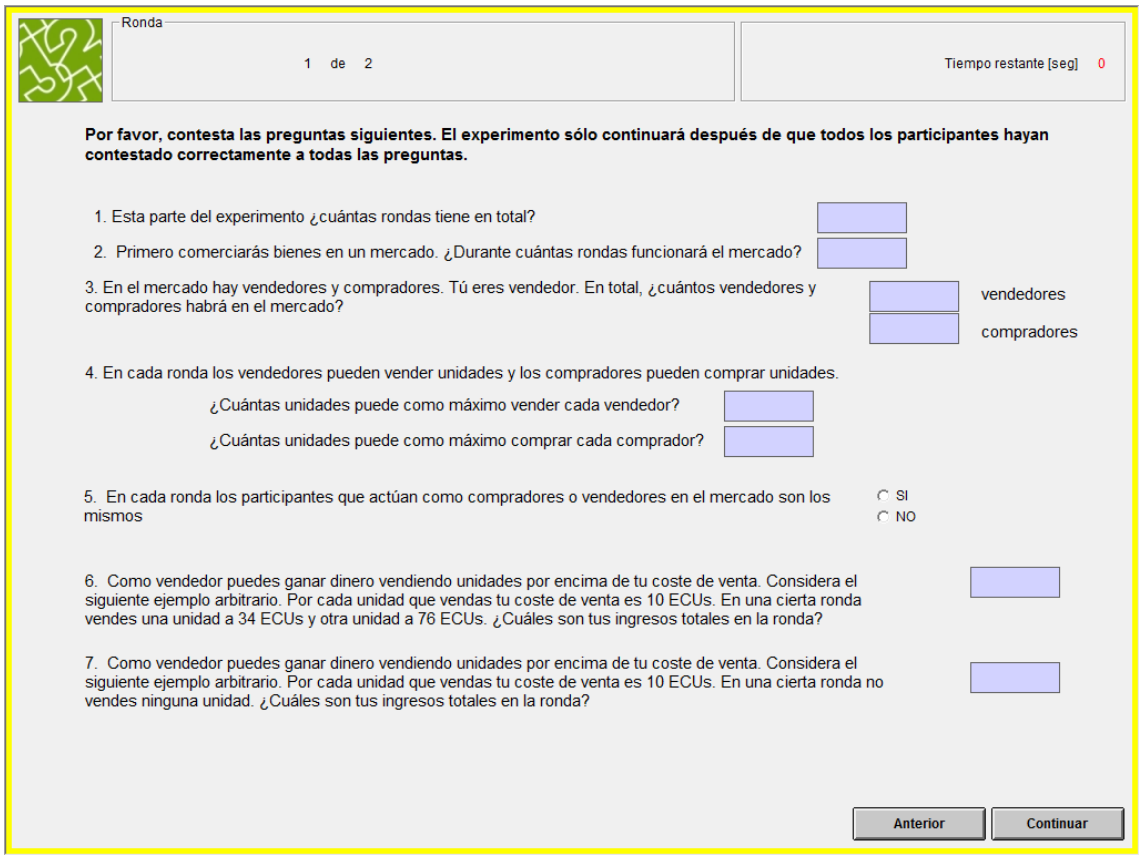
11.In summary, your total earning in one round are determined in the following way:

Individual earnings =	Earnings fund A	+	Earnings fund B
	50 ECU – my allocation to fund B +		*(0.9 x my allocation to fund B)* + *(0.9 x other person's allocation to fund B)*

Note, that the earnings of the other person are calculated in an equivalent way.

12. At the end of each round, you will be informed of the allocation to fund B of the person you are paired with. Moreover, you will receive information on your and the other person's earnings in each round. This information regarding the allocation and earnings of all previous rounds will appear on your computer screen.

Screen 27



Please answer the following questions. The experiment will proceed only after all participants have correctly answered all questions.

1. This part of the experiment has in total how many rounds.

   Answer: XXX rounds

2. First you will trade goods on a market. For how many rounds will the market be open?

   Answer: XXX rounds

3. In the market there are sellers and buyers. You are a buyer. In total, how many sellers and buyers will be active on the market?

   Answer: XXX sellers, XXX buyers

4. In each round sellers can sell units and buyers can buy units. How many units can each seller sell at most and each buyer buy at most?

   Answer: X units

5. In each round the same participants will be active as buyers and sellers on the market?

   Answer: YES NO

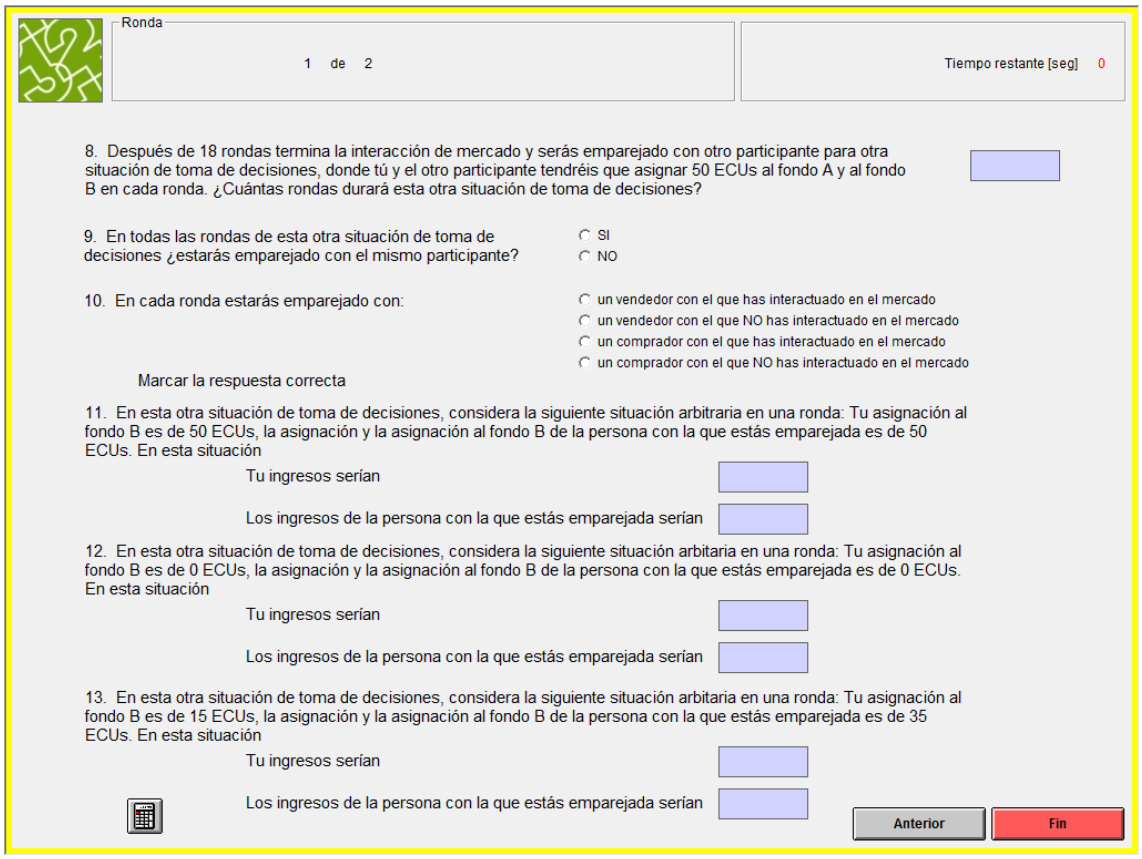
6. As a buyer you can earn money by buying units at prices below your buyer value. Consider the following arbitrary example. For each unit you buy your buyer value is 85 ECU. In a given round, you buy one unit at a price of 34 ECU and another unit at a price of 76 ECU. What are your total earnings in that round?

   Answer: XX ECU

7. As a buyer you can earn money by buying units at prices below your buyer value. Consider the following arbitrary example. For each unit you buy your buyer value is 85 ECU. In a given round, you do not buy any unit. What are your total earnings in that round?

   Answer: XX ECU

Screen 28



8. After 18 rounds the market interaction is over and you will be paired with one other participant for another decision making situation, where you and the other participant will have to allocate 50 ECU to fund A and fund B in each round. How many rounds will this other decision making situation last?

   Answer: XX rounds

9. In all 6 rounds of this other decision making situation you are paired with the same other participant?

   Answer: YES NO

10. In each round you are paired with:

   O a seller you have interacted with in the market
   O a seller you have NOT interacted with in the market
   O a buyer you have interacted with in the market
   O a buyer you have NOT interacted with in the market

   (check the correct answer)

11. In this other decision making situation, Consider the following arbitrary situation in a round:
   Your allocation to the fund B is 50 ECU, the allocation to fund B of the person you are

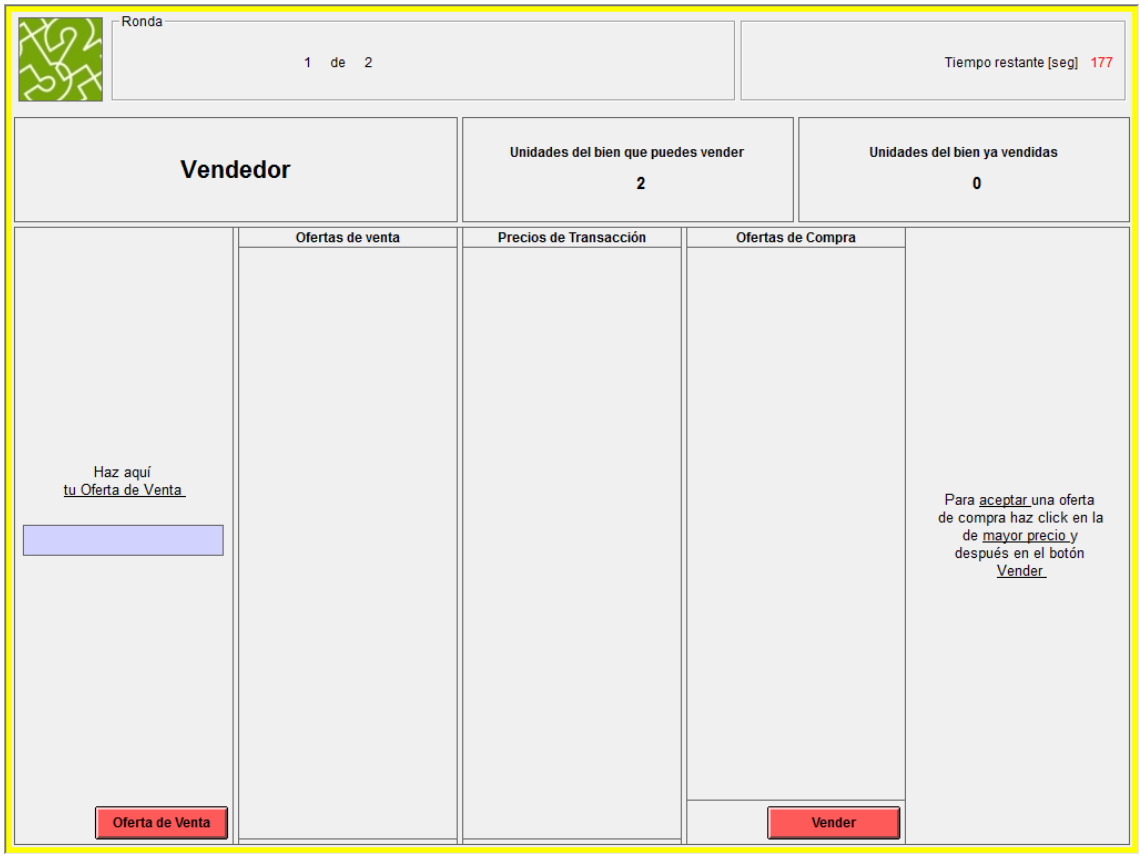
paired with is 50 ECU.
In this situation
your earnings would be: …...
the earnings of the person you are paired with would be: ….

12. In this other decision making situation. Consider the following arbitrary situation in a round:
Your allocation to the fund B is 0 ECU, the allocation to fund B of the person you are paired with is 0 ECU.
In this situation
your earnings would be: …...
the earnings of the person you are paired with would be: ….

13. In this other decision making situation. Consider the following arbitrary situation in a round:
Your allocation to the fund B is 12 ECU, the allocation to fund B of the person you are paired with is 34 ECU.
In this situation
your earnings would be: …...
the earnings of the person you are paired with would be: ….

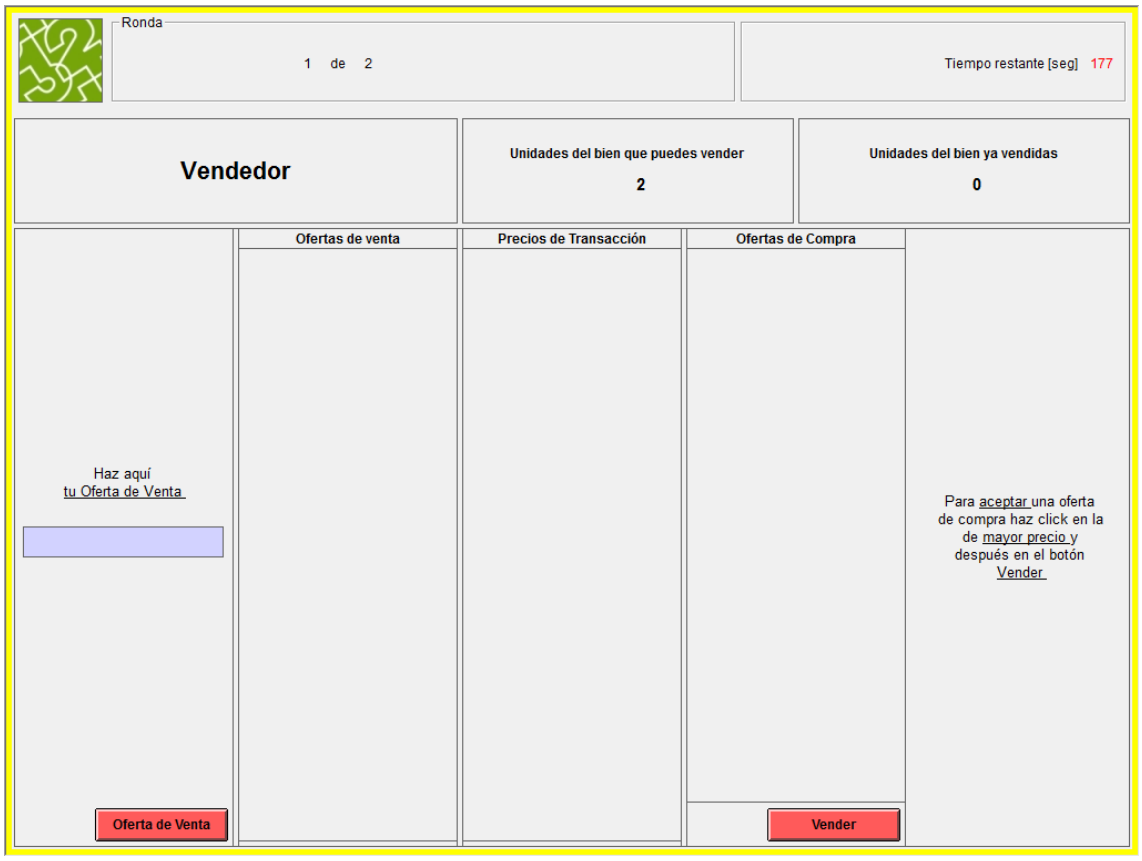
SELLERS AND BUYERS

(These screens are the ones the participants see during the experiment).



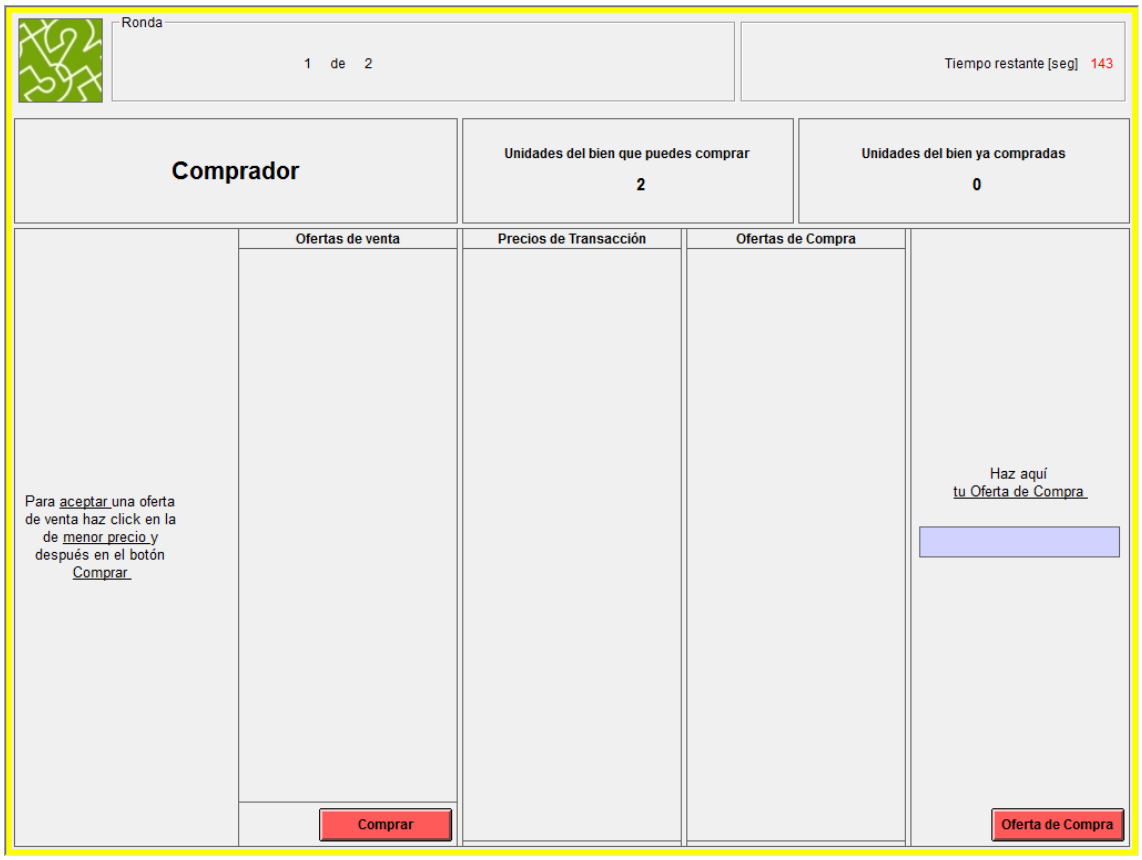
Screen 29

This screen shows the transaction screen of a seller at the beginning of a round.

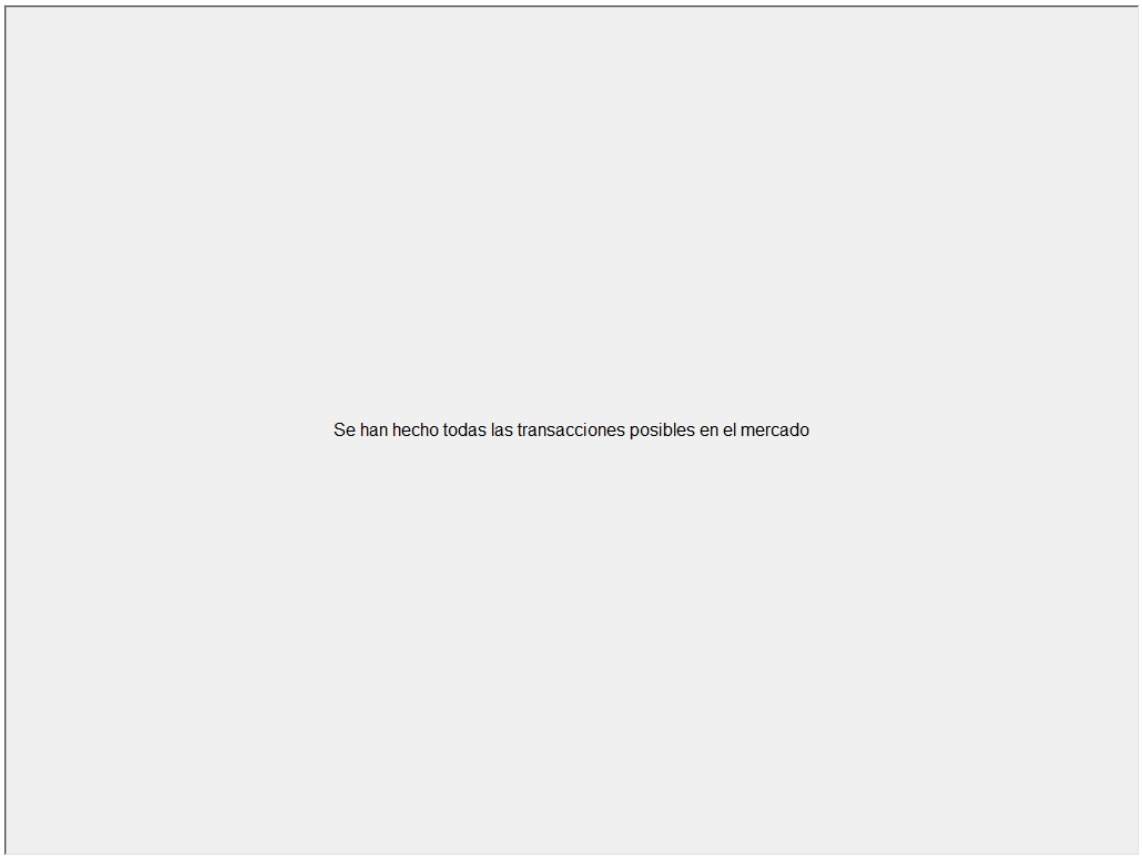


Screen 30

This screen shows the transaction screen of a buyer at the beginning of a round.

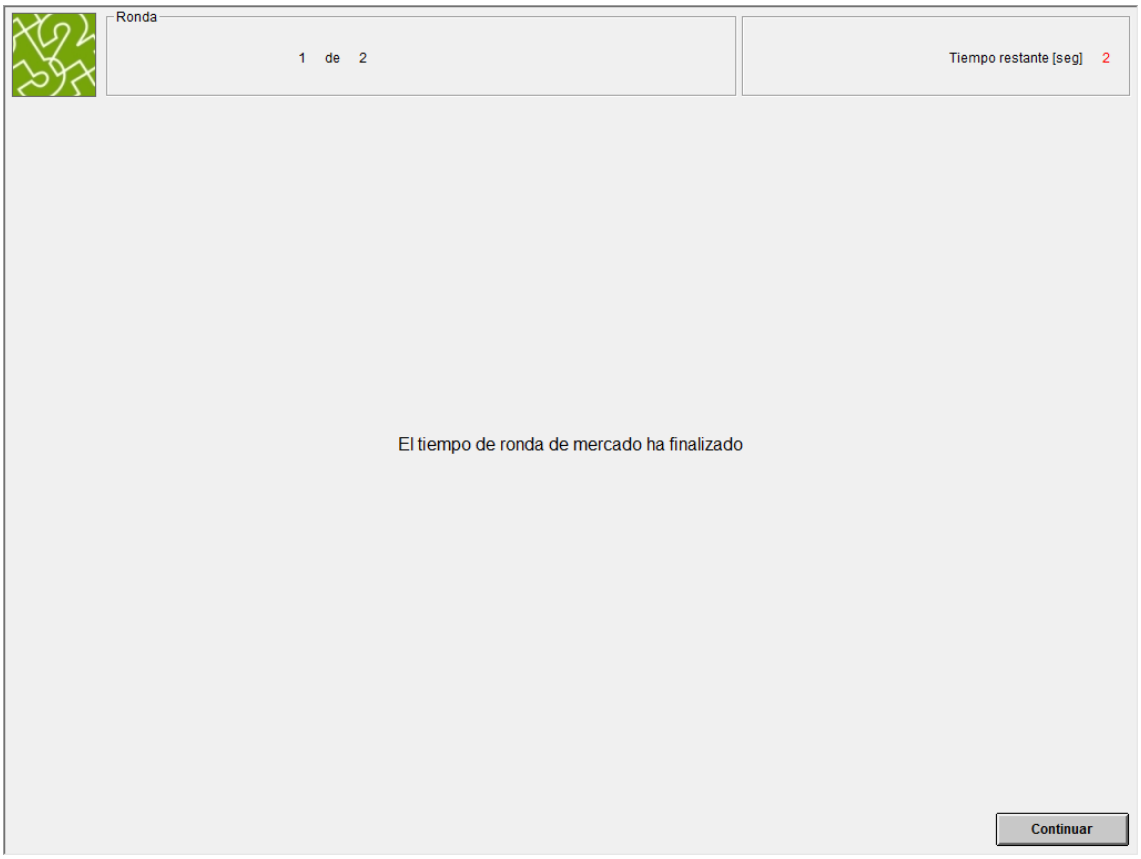


Screen 31



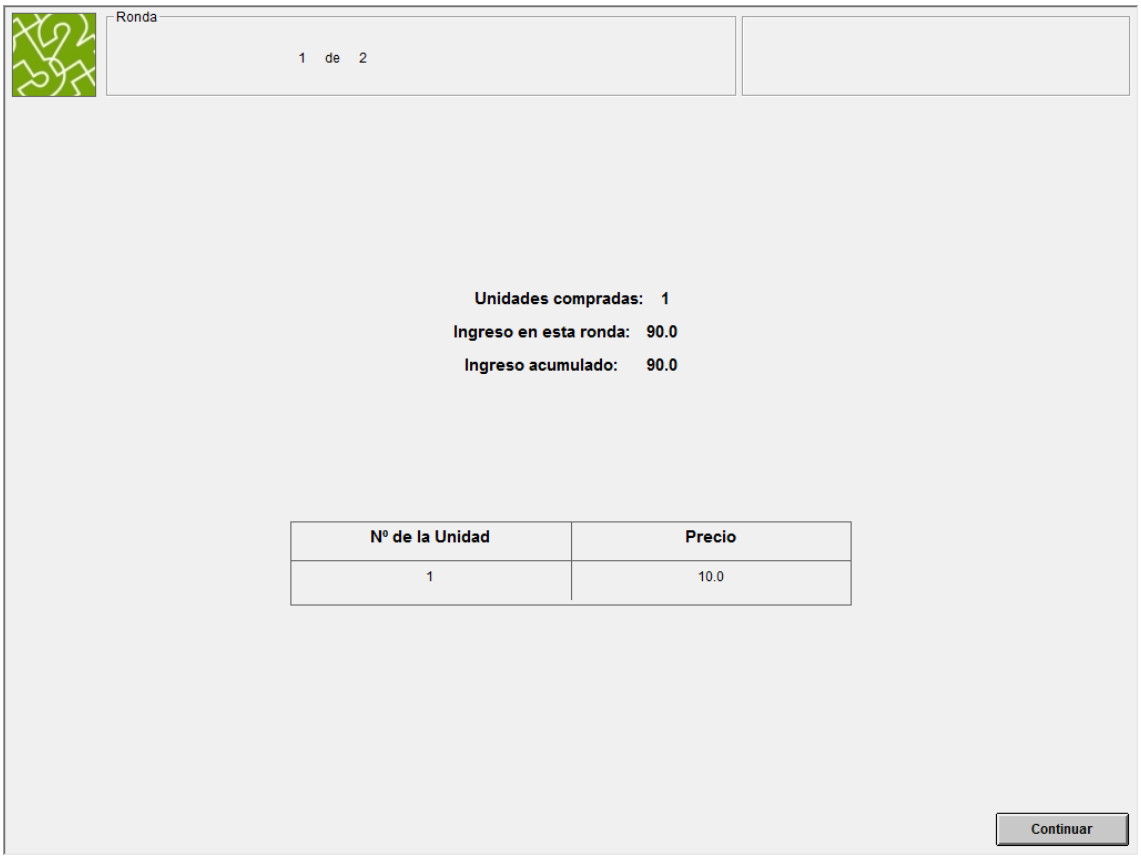
All the possible market transactions have been made.

Screen 32



The time of the market round is over.

Screen 33



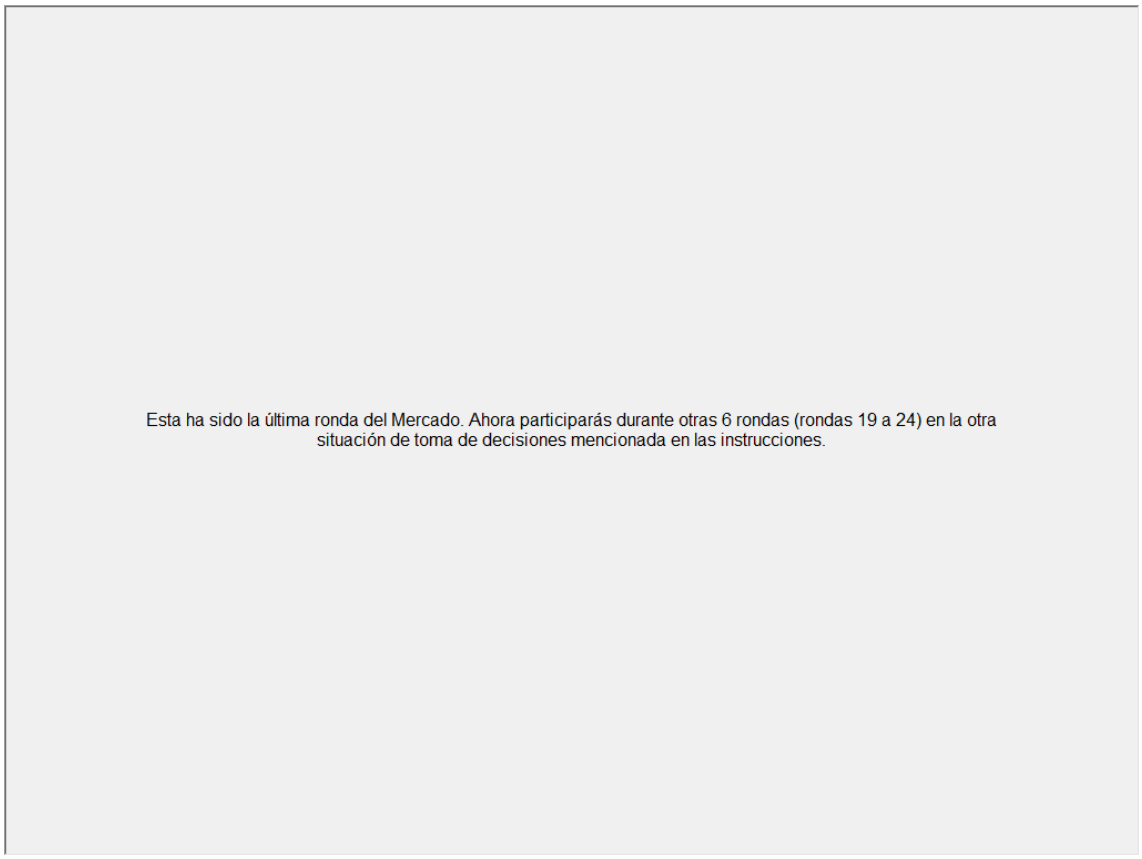
Purchased units: 1

Earnings in this round: 90

Accumulated earnings: 90

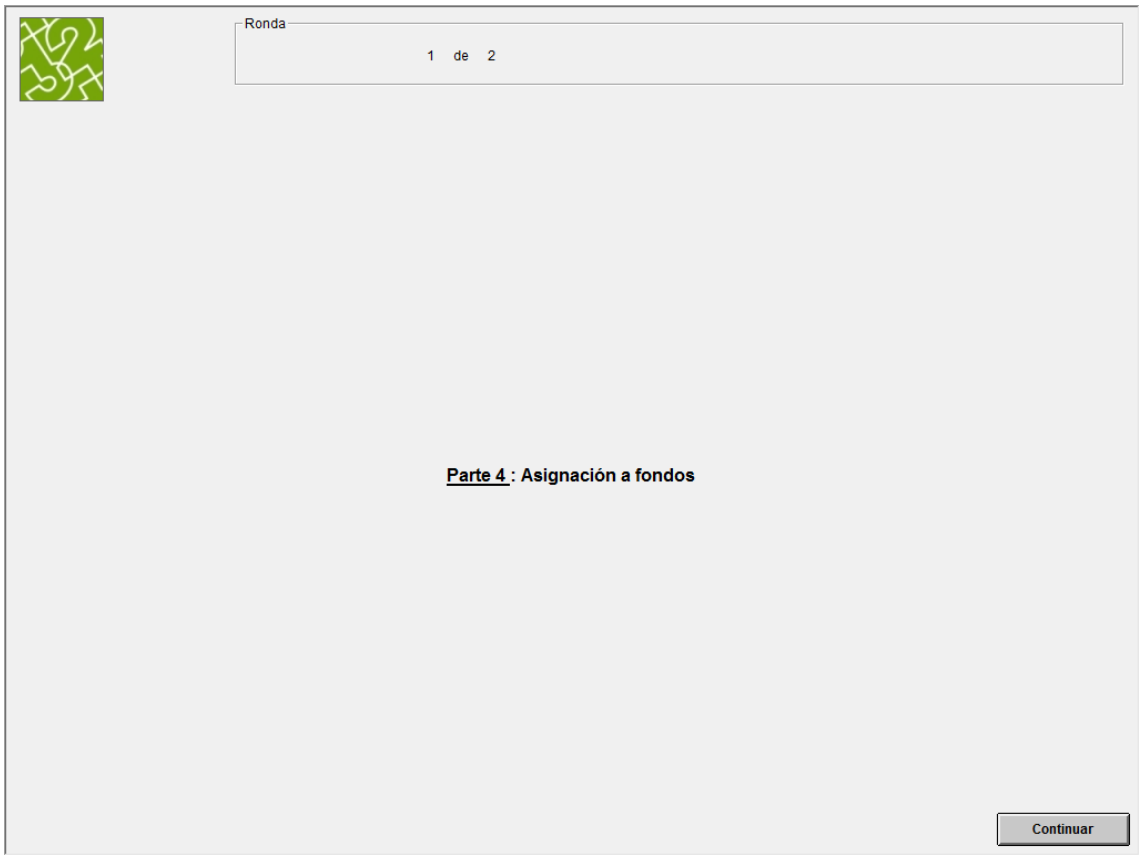
Number of unit: 1, Price: 10

Screen 34



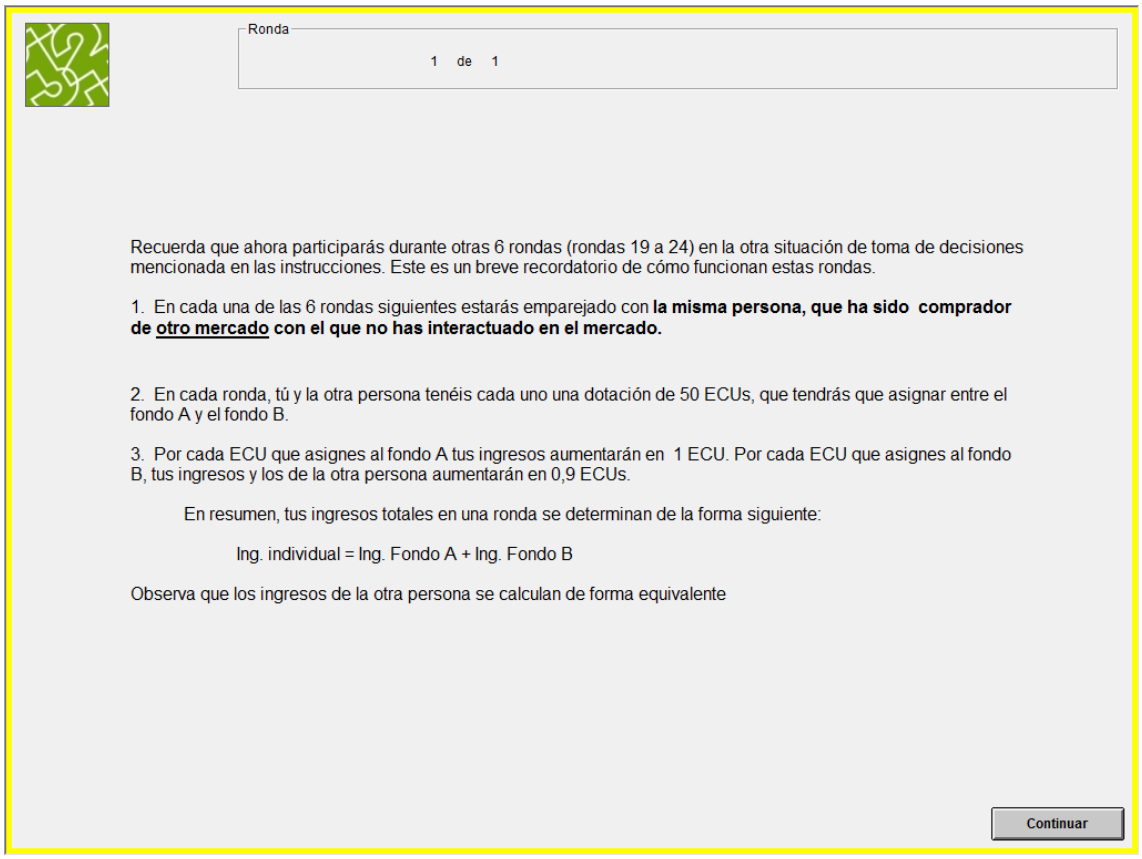
This has been the last market round. Now you will participate for another 6 rounds (rounds 19 to 24) in the other decision making situation mentioned in the instructions.

Screen 35



Part 4: Allocation to funds

Screen 36



Remember that you will now participate during 6 rounds (rounds 19 to 24) in the other decision making situation mentioned in the introduction. This is a brief reminder of these rounds work.

1. In each of the 6 rounds, you are paired with **the same person, who has been a buyer** *[in seller-seller and buyer-seller pairs: "***a seller"***]* **of the other market with whom you have not interacted in the market.** *[in Market-Partners:* **"same market with whom you have interacted in the market."***]*

2. In each round, you and the other person receive an endowment of 50 ECUs, which you will have to allocate between the fund A and the fund B.

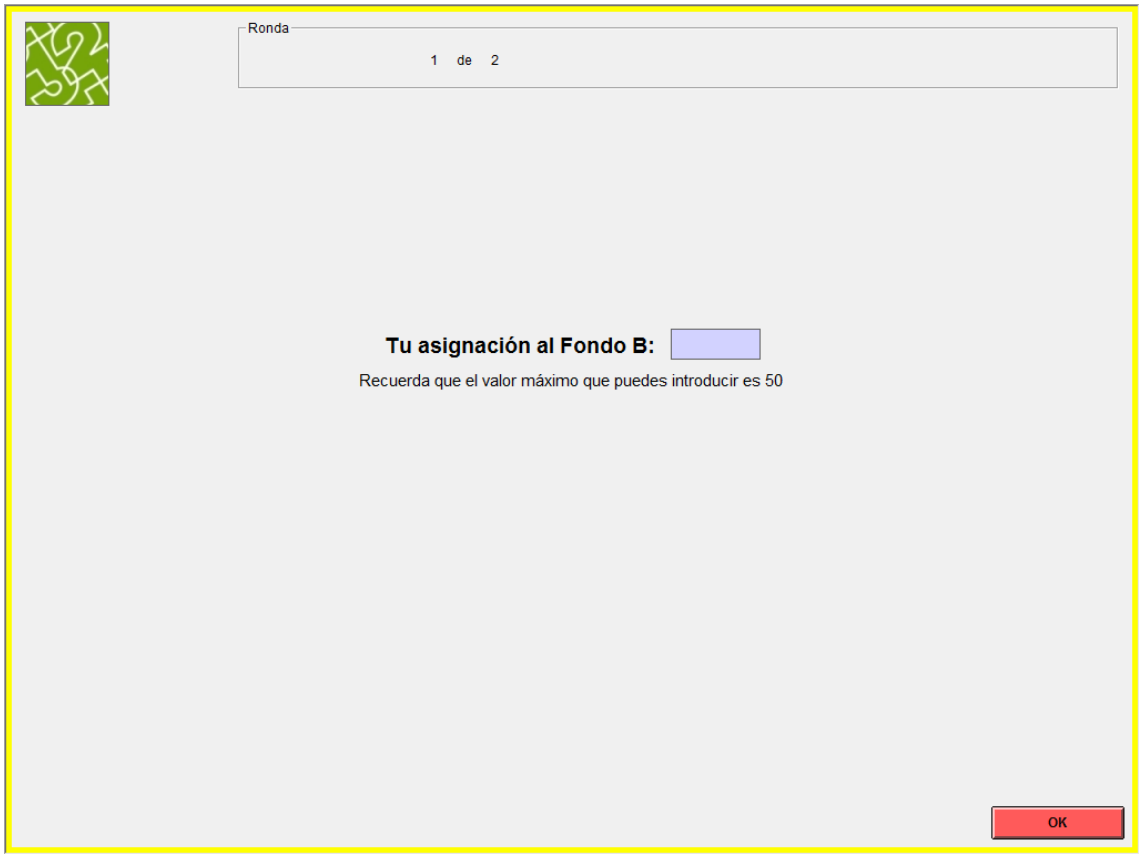
3. For each ECU that you allocate to fund A your earnings will increase by 1 ECU. For each ECU that you allocate to fund B, your earnings and the earnings of the other person will increase by 0.9 ECUs.

In summary, your total earnings in a round are determined in the following way:

Individual earnings = Earnings from fund A + Earnings from fund B

Observe that the earnings of the other person are calculated in an equivalent way

Screen 37



Your assignment to Fund B:____

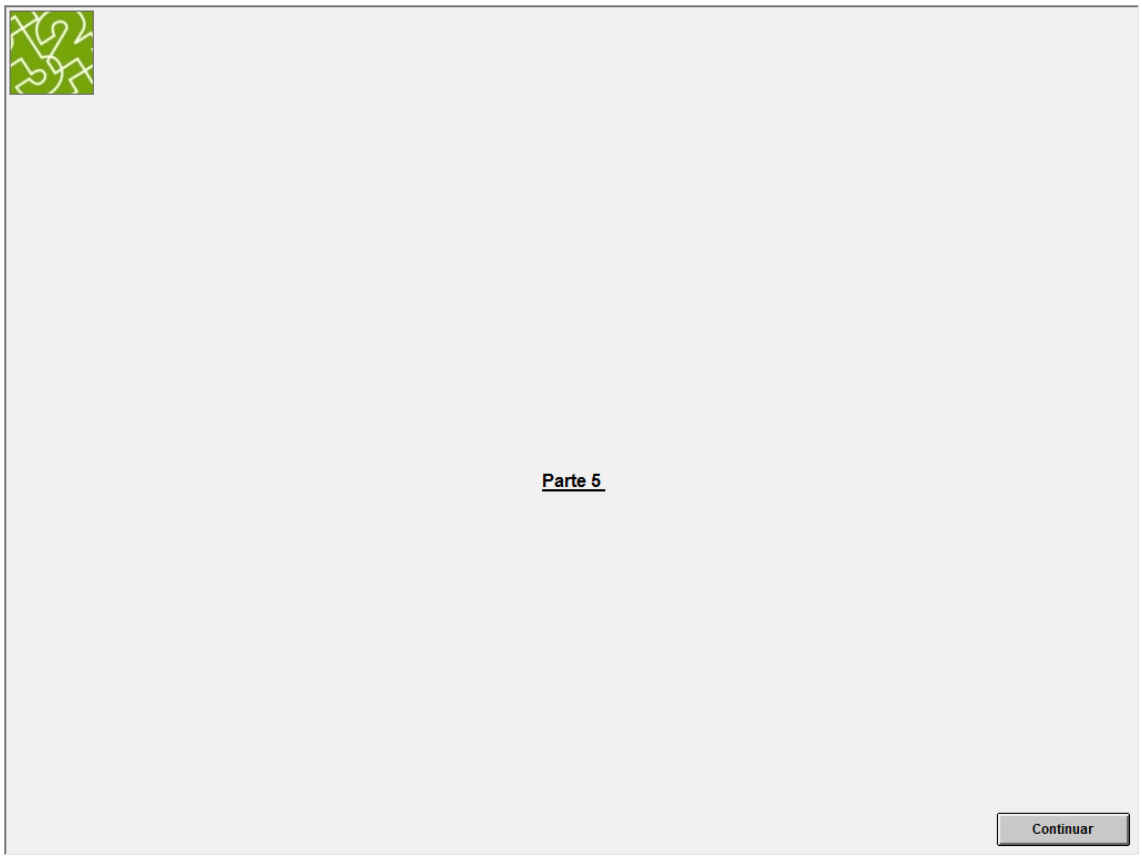
Remember that the maximum value that you can introduce is 50.

Screen 38

Results screen

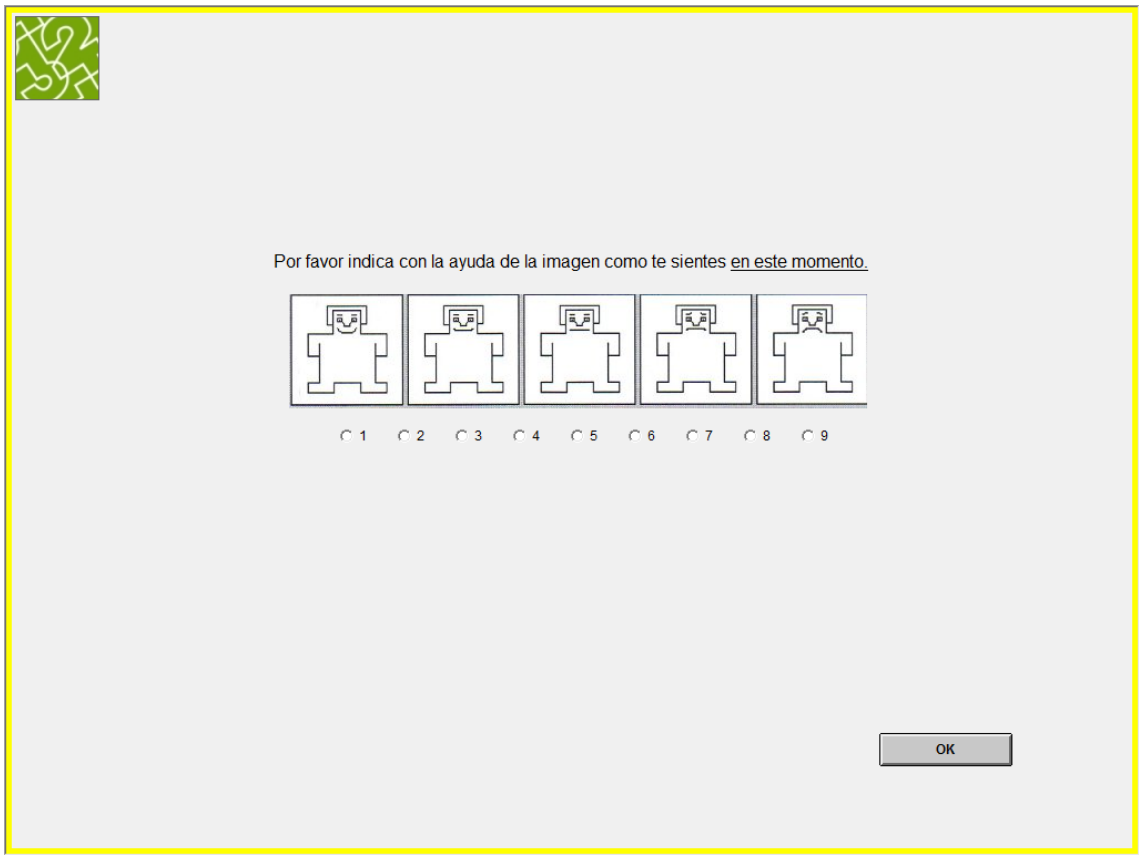
Round	My allocation	Allocation of the other	My earnings	Earnings of the other
1	10	40	85.0	55.0

Screen 39



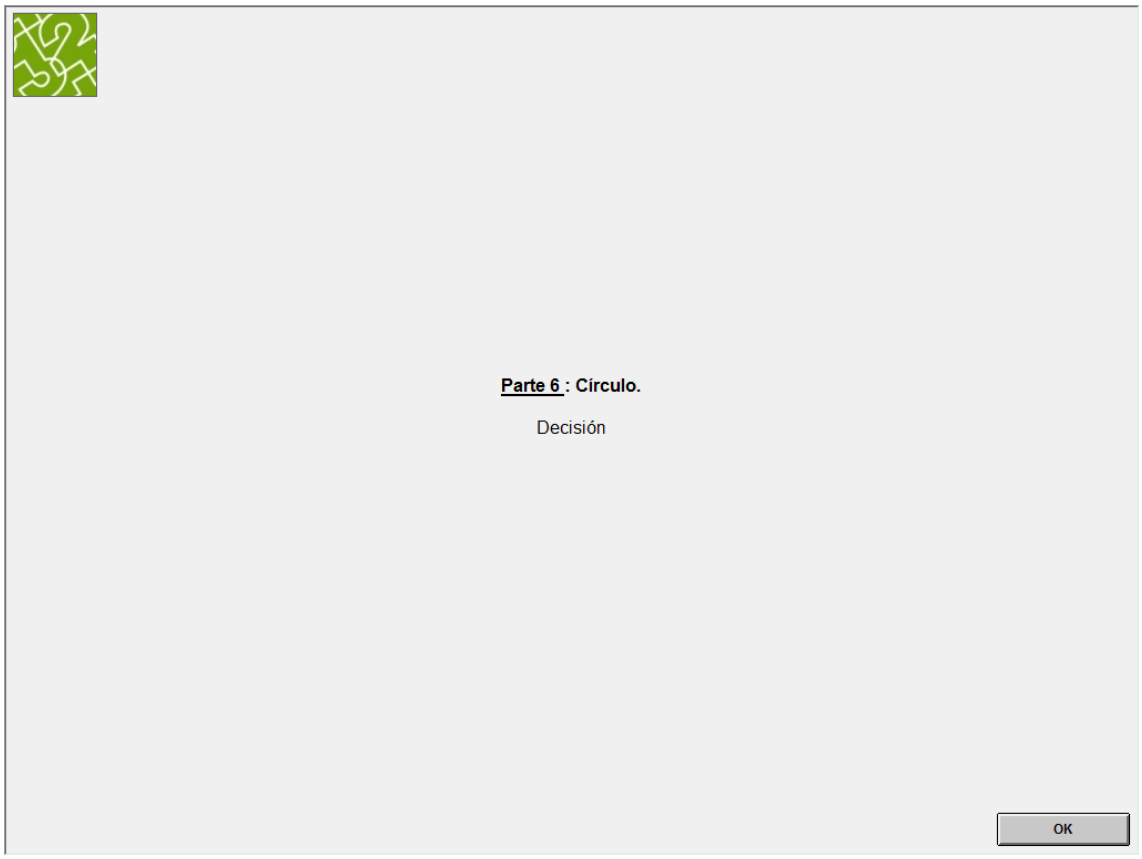
Part 5

Screen 40



Please indicate with the help of the image how you feel at this moment.

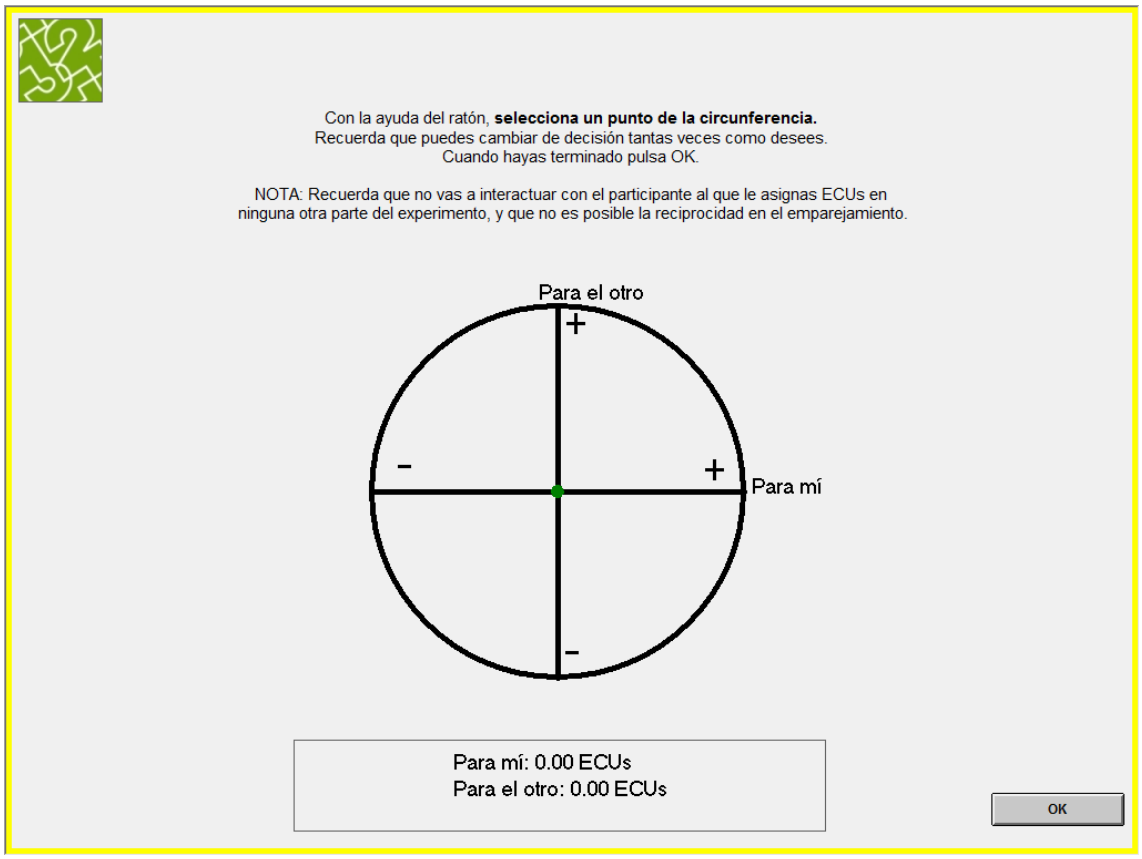
Screen 41



Part 6: Circle

Decision

Screen 42

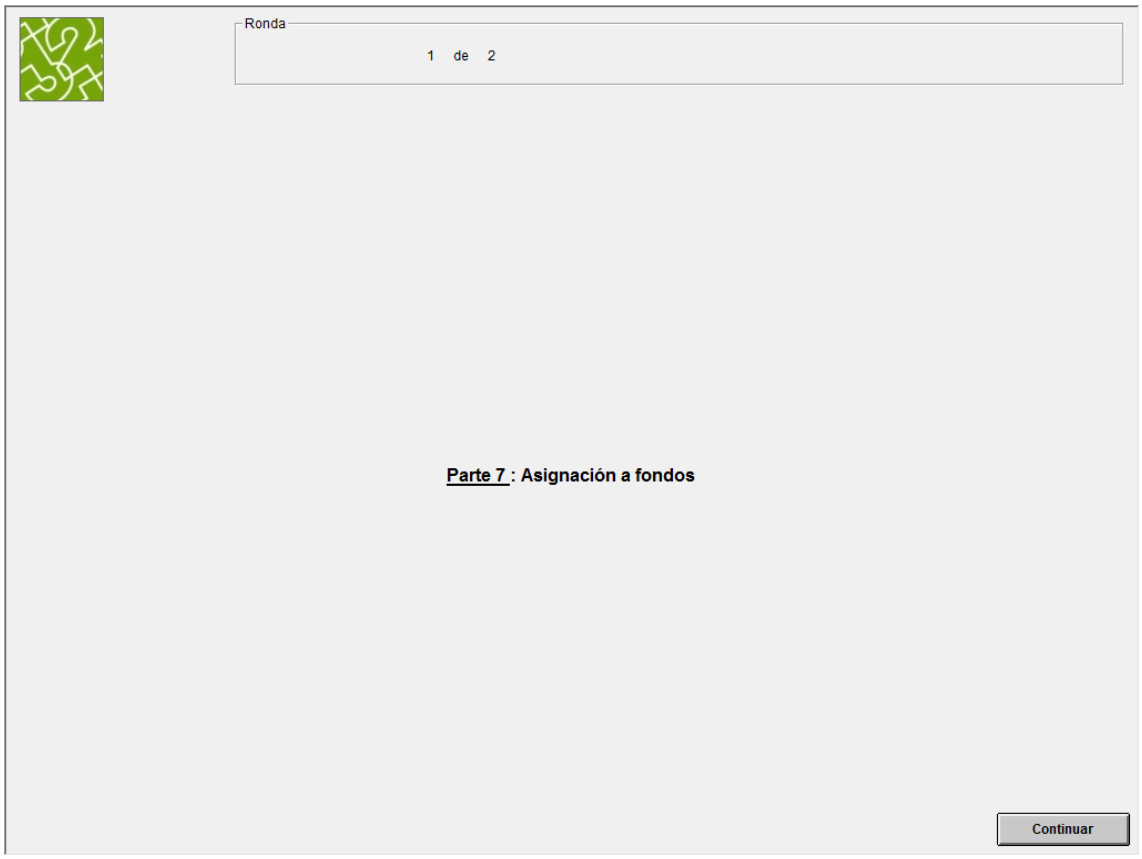


With the help of the mouse select a point on the circle. Remember that you can change your decision as many times as you wish.

When you are finished press OK.

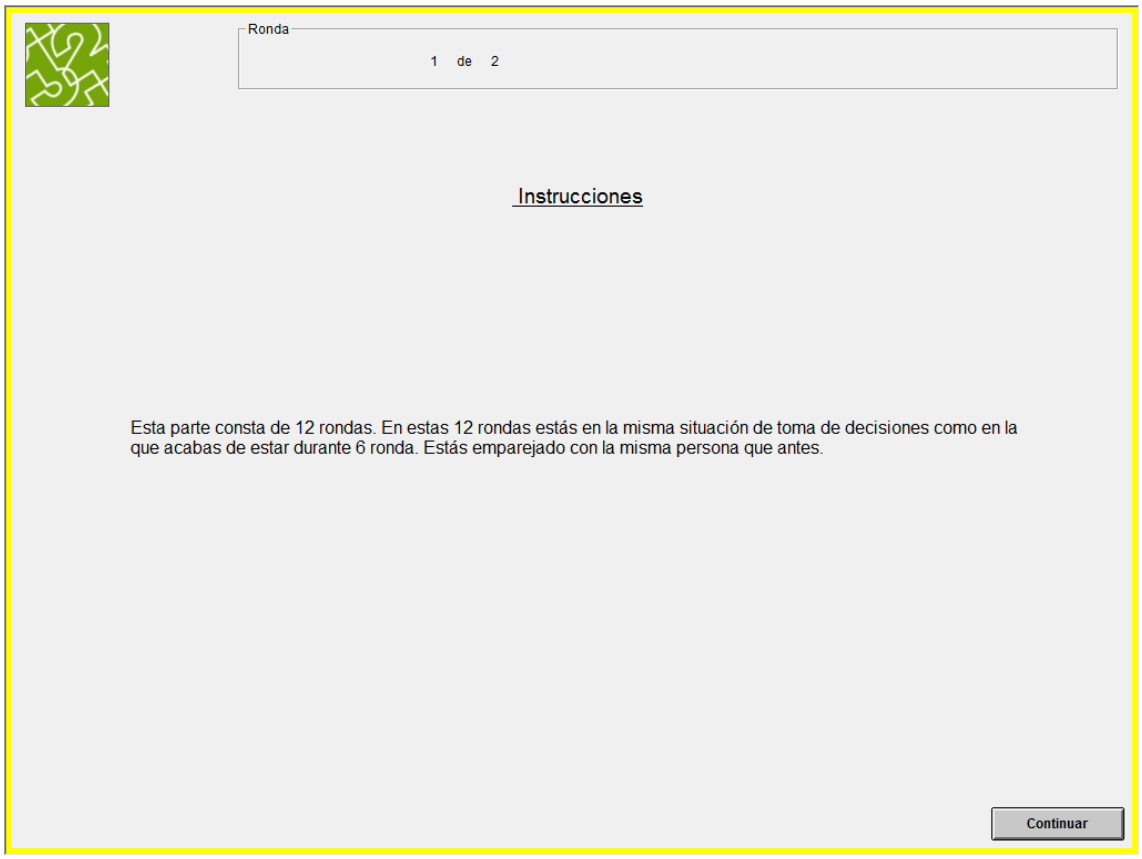
NOTE: Remember that you will not interact with the participant to whom you assign ECUs in no other part of the experiment and that reciprocity in the matching.

Screen 43



Part 7: Allocation to funds

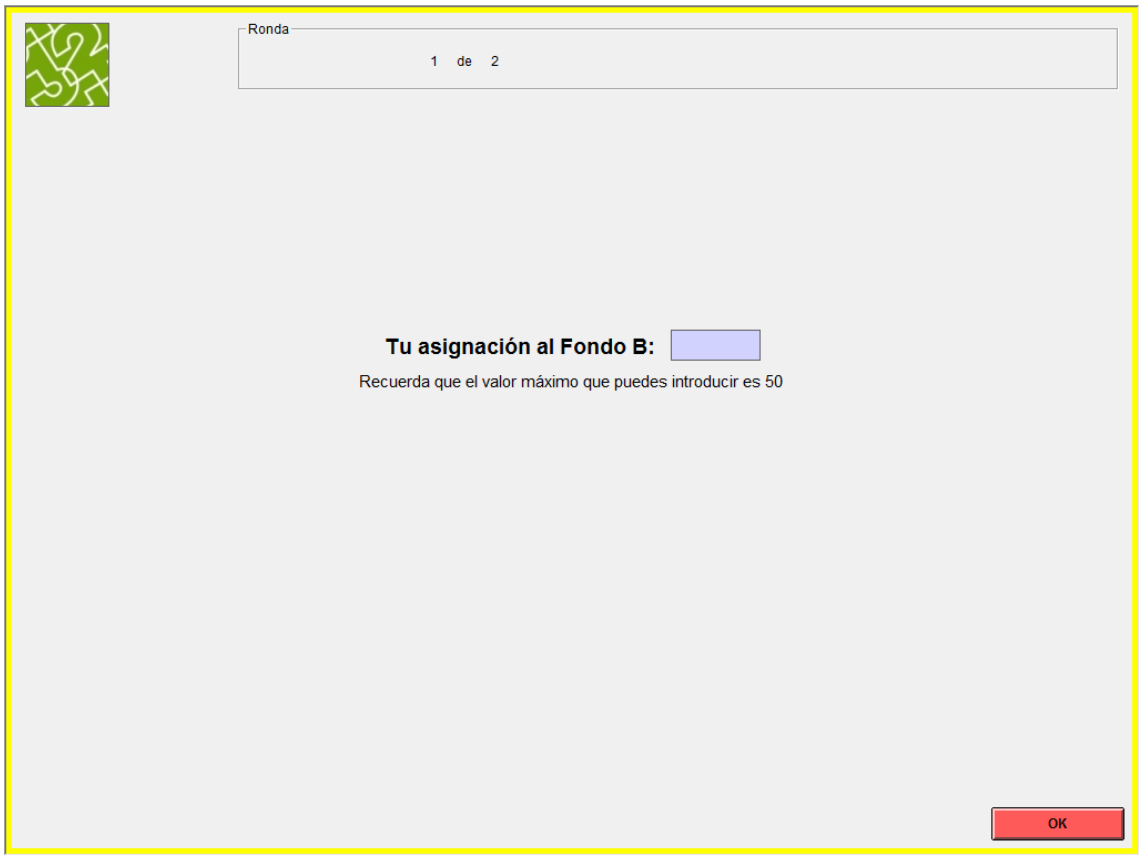
Screen 44



Instructions

This part consists of 12 rounds. In these 12 rounds you are in the same decision making situation as in the one that you have just been during 6 rounds. You are paired with the same person as before.

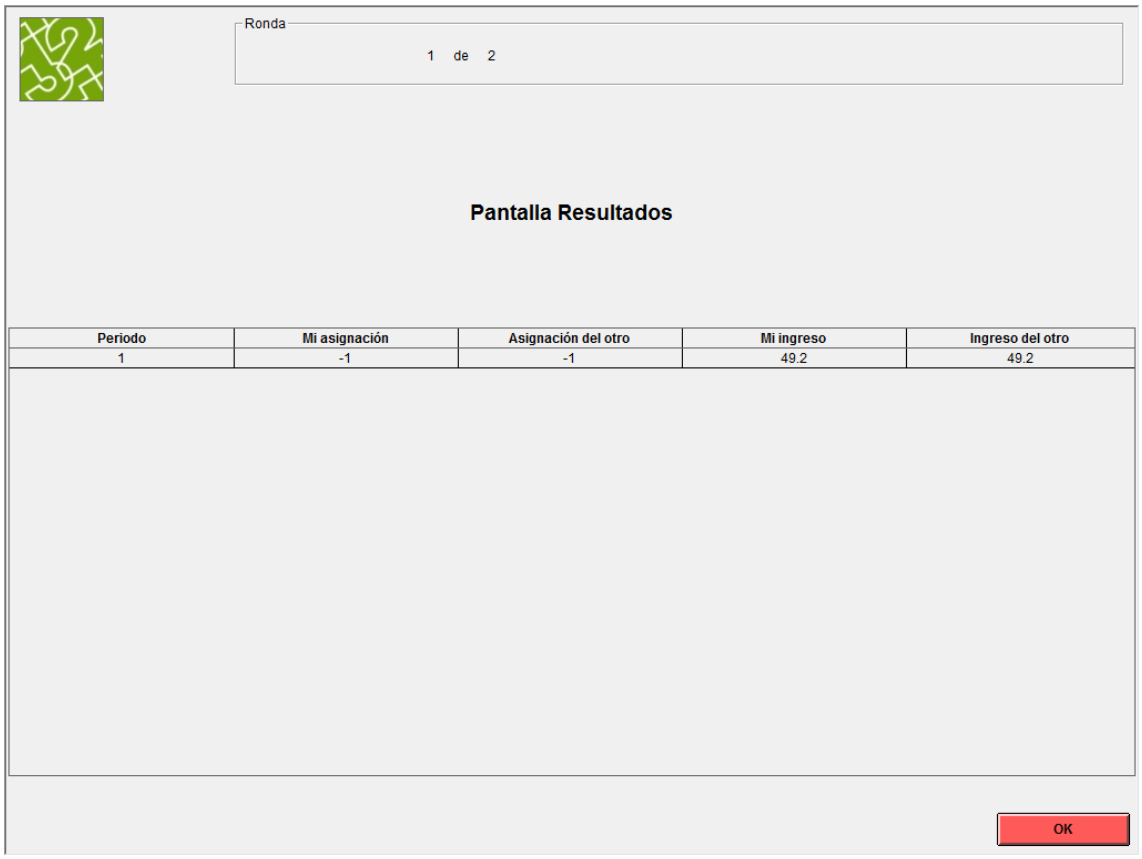
Screen 45



Your assignment to Fund B:____

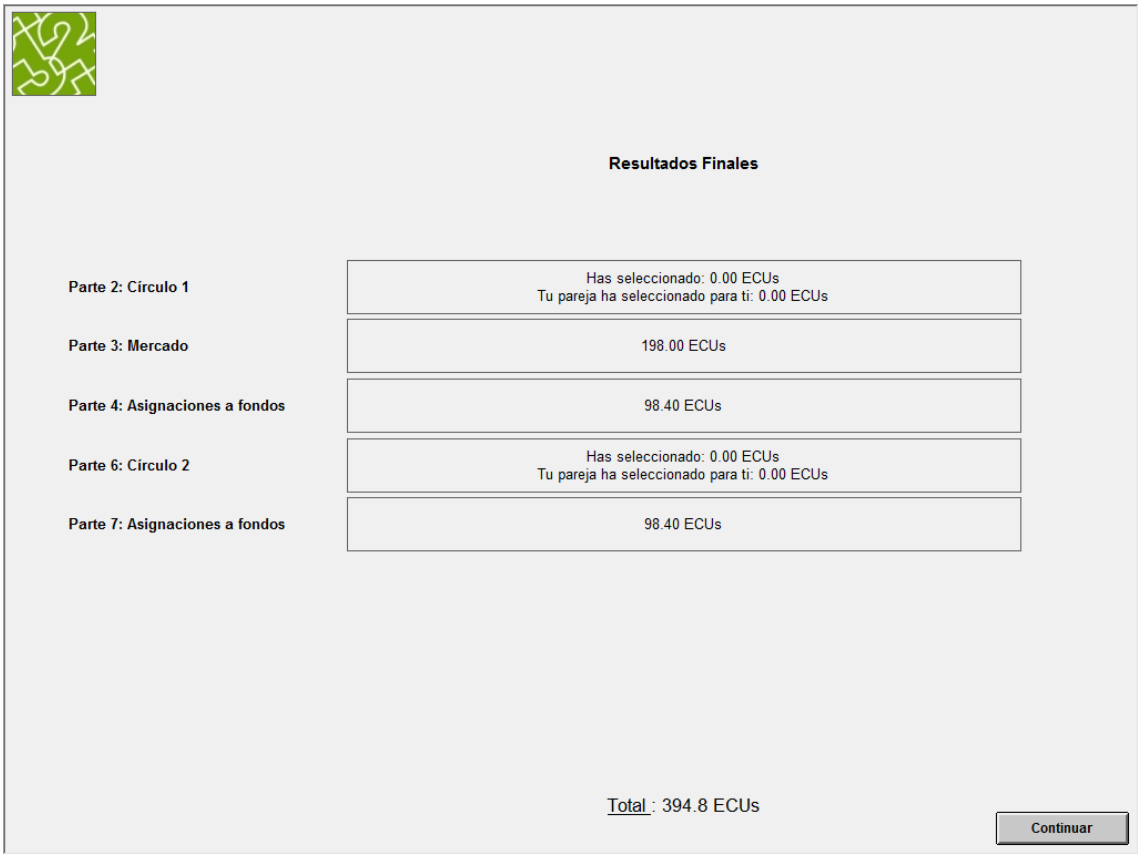
Remember that the maximum value that you can introduce is 50.

Screen 46



Round	My allocation	Allocation of the other	My earnings	Earnings of the other
1	0	0	50.0	50.0

Screen 47



Final Results

Part 2: Circle 1 You have selected 0.00 ECUS. Your partner has selected for you: 0.00 ECUs

Part 3: Market 198.00 ECUs

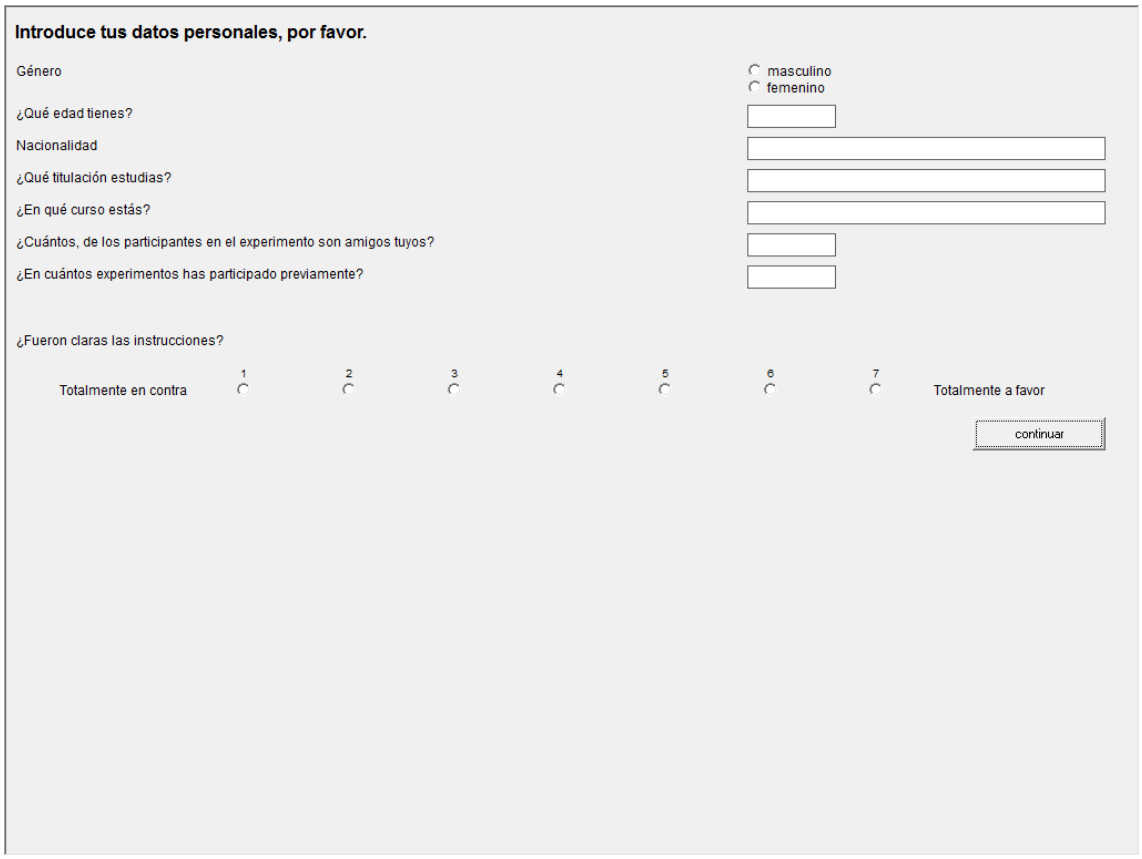
Part 4: Allocation to funds 98.40 ECUs

Part 6: Circle 2 You have selected 0.00 ECUS. Your partner has selected for you: 0.00 ECUs

Part 7: Allocation to funds 98.40 ECUs

Total: 394.8 ECUs

Screen 48



Fill in your personal data, please

Gender: male/female

What is your age?

Nationality

What is your major?

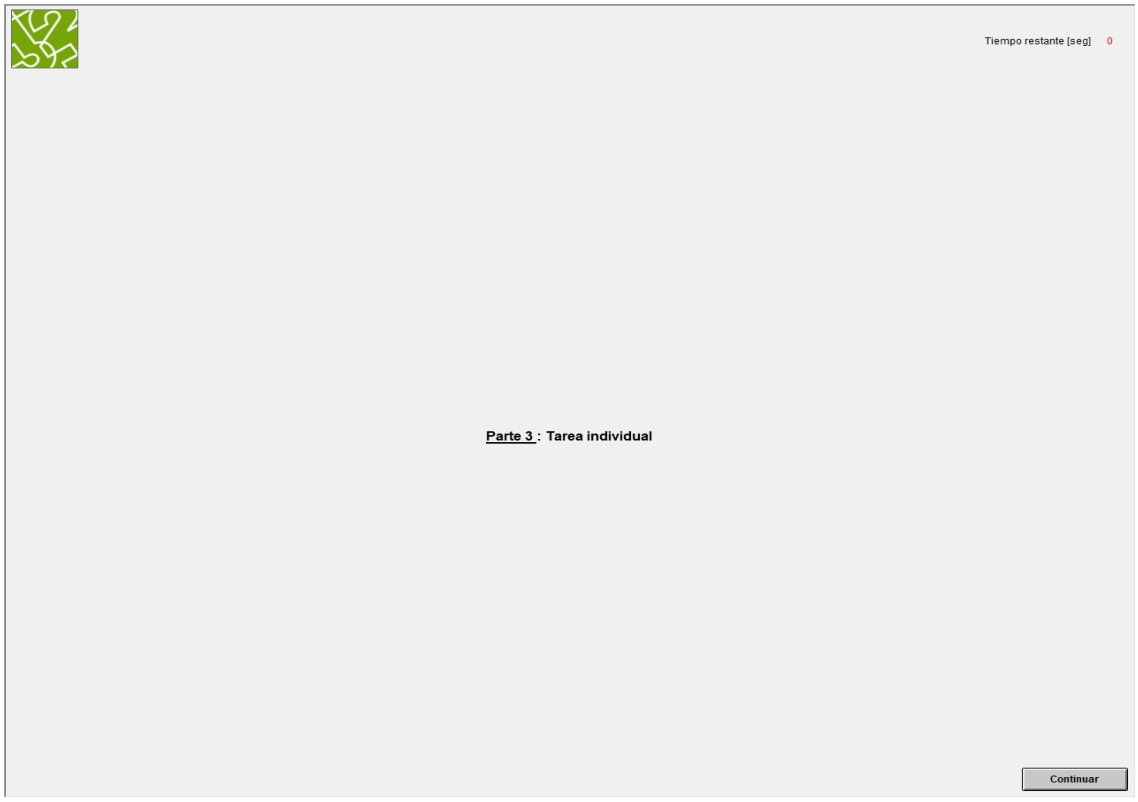
Which year are you in?

How many of the other participants are your friends?

In how many experiments have you already participates

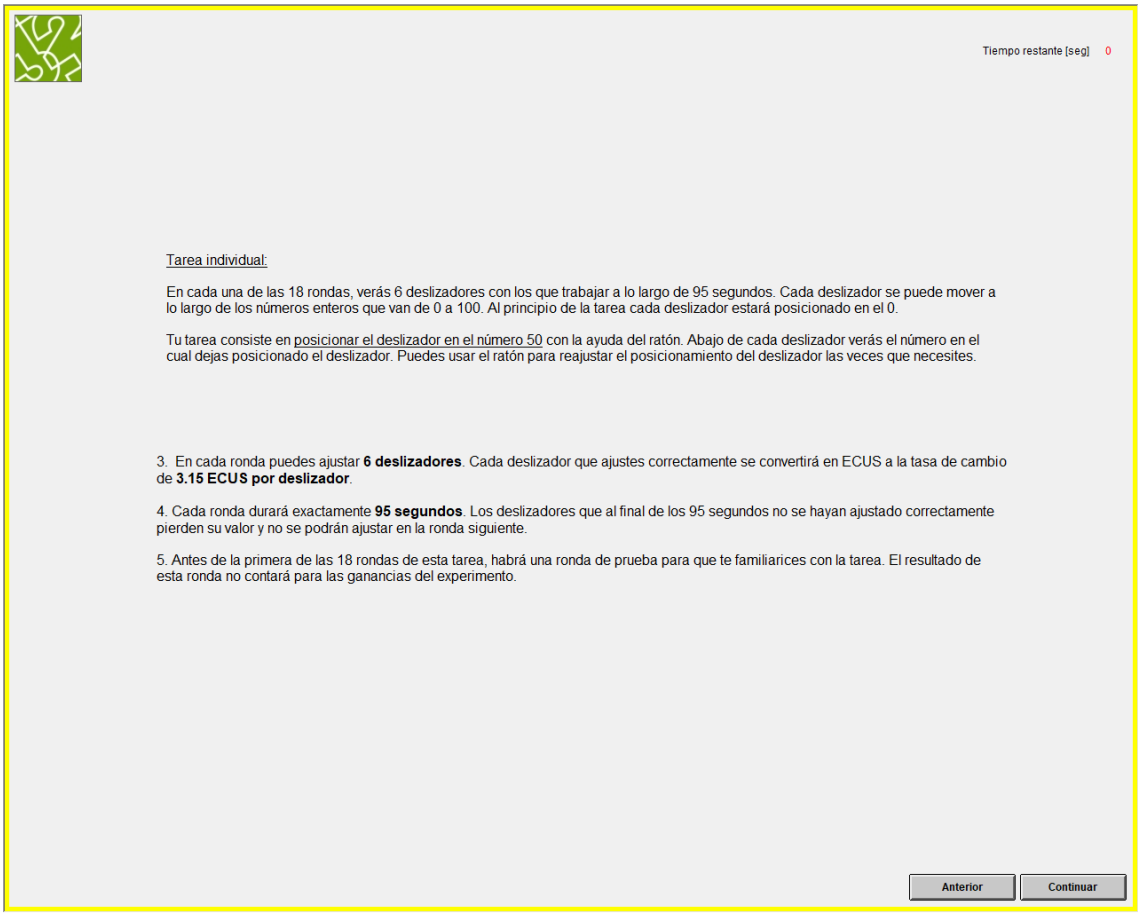
Were the instructions clear on a scale from 1 to 7?

The following screen shots show the instructions for the **real effort task in treatment OSDG-MP-RE**. In this treatment the market interaction was replaced by an individual real-effort slider task. All other instructions and screens were the same as in the other treatments



Part 3: Individual task

1. This part of the experiment consists of 24 rounds. These 24 rounds are split into 18 rounds in which you and the other participants will do an individual task and, thereafter, 6 rounds where each participant will be paired with exactly one other participant to make decisions in another situation.
2. In what follows we will explain how you can earn money in the individual task.
3. In the numerical values we will use the period as separator of decimals.



Individual task:

In each of the 18 rounds, you will see 6 sliders with which to work during 95 seconds. Each slider can be moved along the whole numbers that go from 0 to 100. At the beginning of the task each slider is positioned at 0.

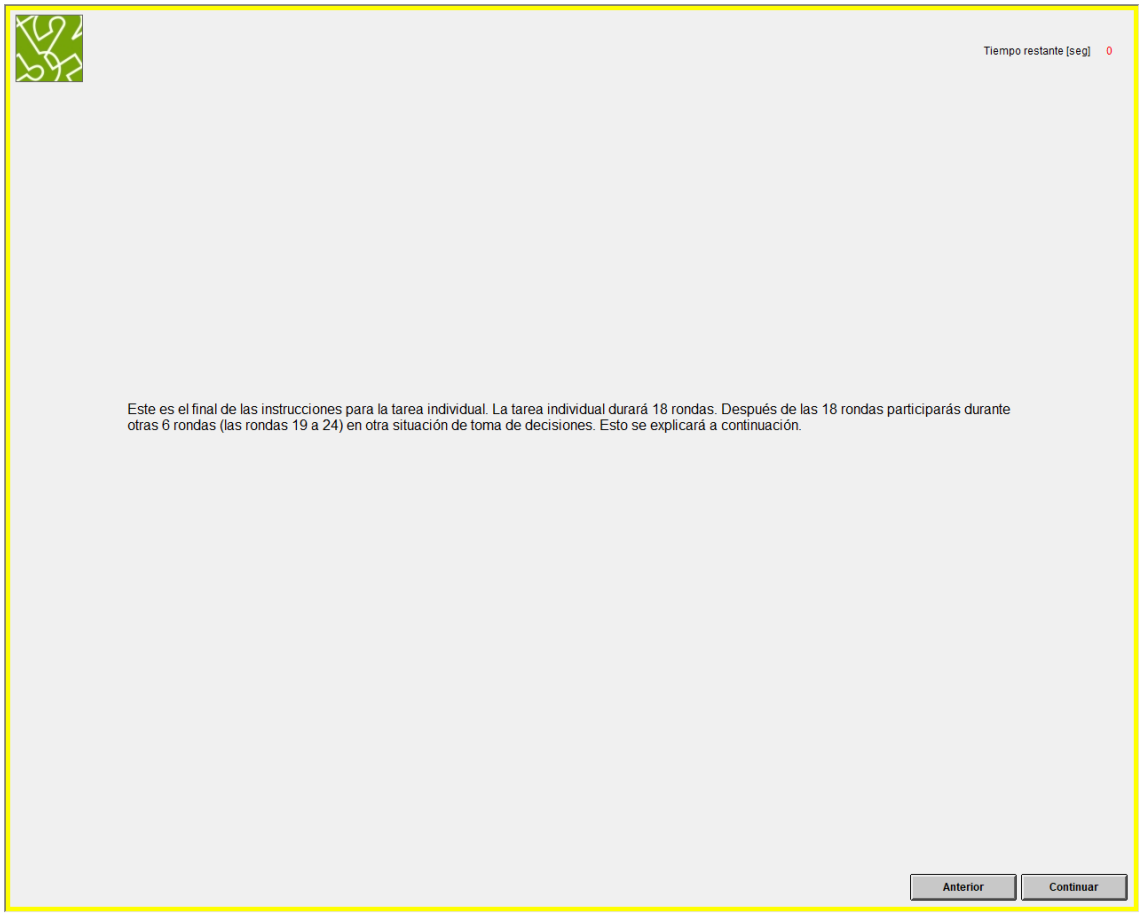
Your task consists in positioning the slide at the number 50 with the help of the mouse. Under the slider you will see the number at which you have left the slider. You can use the mouse to readjust the positioning of the slider as much as you need it.

In each round you can adjust **6 sliders**. Each slider that you adjust correctly will be converted into ECUS at the exchange rate of **3.15 ECUS* per slider**.

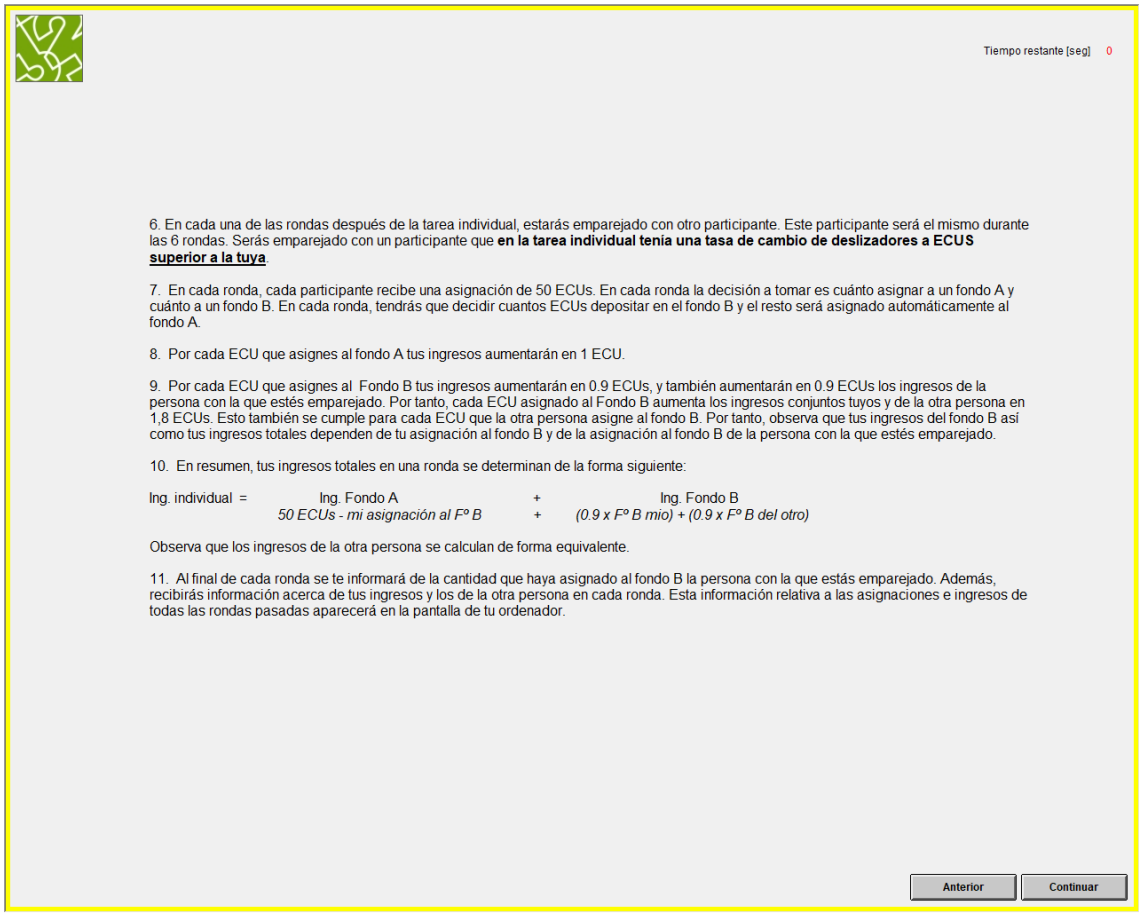
Each round will last exactly **95 seconds**. The sliders that at the end of the 95 seconds have not been adjusted correctly lose their value and can not be adjusted in the next round.

Before the first of the 18 rounds of this task, there will be a trial round so that you can familiarize yourself with the task. The result of this round will not count for the earnings of the experiment.

* For participants in the high-pay condition this was **24.74 ECUS**.



This is the end of the instructions for the individual task. The individual task will last 18 rounds. After the 18 rounds you will participate during 6 rounds (rounds 19 to 24) in another decision-making situation. This will be explained next.



This screen is the same as in the other treatments except for the third sentence of item 6, which reads:
You will be paired with a participant who **in the individual task had an exchange rate of slider to ECUS higher than yours***.

* This is the version for low-pay (i.e., low exchange rate) participants in mixed-pay pairs. For high-pay (i.e., high exchange rate) participants in mixed pairs it said "**lower than yours**" and in pairs with equal exchange rates (low and high) ist said "**equal to yours**".