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The Power of Sunspots: An Experimental Analysis

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Abstract:

The authors show how the influence of extrinsic random signals depends on the noise structure of these signals. They present an experiment on a coordination game in which extrinsic random signals may generate sunspot equilibria. They measure how these signals affect behavior. Sunspot equilibria emerge naturally if there are salient public signals. Highly correlated private signals may also cause sunspot-driven behavior, even though this is no equilibrium. The higher the correlation of signals and the more easily these can be aggregated, the more powerful these signals are in moving actions way from the risk-dominant equilibrium.

Keywords: coordination games, strategic uncertainty, sunspot equilibria, irrelevant information

JEL Classifications: C9, D5

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1. Introduction

Since Keynes (1936) compared investor behavior in stock markets to a beauty contest, the question has been asked whether extrinsic information may affect agents' behavior. Azariadis (1981) and Cass and Shell (1983) were the first to theoretically explore the influence of extrinsic information on economic activities. They showed that whenever there are multiple equilibria there are also sunspot equilibria, in which agents condition their actions on publicly observable but intrinsically uninformative signals.[1] Despite the uninformative nature of these signals, they may serve as focal points for agents' beliefs, and their public nature allows for beliefs that are conditioned on these signals to become self-fulfilling. Therefore, extrinsic events may determine on which particular equilibrium agents coordinate upon.

Whether sunspot equilibria emerge naturally and how the likelihood for observing sunspot equilibria depends on the nature of signals is an increasingly relevant question, as financial crises might emerge from sudden swings of expectations triggered by extrinsic signals. In the field, arguably it is hard to identify a particular extrinsic event that may affect an agent's choice. Even if such an extrinsic event is identified, it is difficult to establish causality between the extrinsic event (sunspot) and an economic outcome. For instance, noninformative signals might explain excessive asset price volatility when traders condition their actions on such signals, but higher volatility may also be caused by an increasing dispersion of private signals or increasing uncertainty.[2] Laboratory experiments offer a

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[1] The term sunspot originated in the work of William Jevons (1884), who proposed a relationship between the number of sunspots and the business cycle. In the theoretical literature, the term "sunspot" is a synonym for extrinsic random variables, i.e., variables that may influence economic behavior, but are unrelated to fundamentals such as payoffs, preferences, technologies, or endowments.

[2] A related recent literature in financial economics explores the impact of natural activities, such as weather conditions or lunar phases, on mood and subsequently on investment decision (see, e.g., Yuan, Zheng and Zhu, 2006; Hirshleifer and Shumway, 2007, and references therein) or on college choice (Simonsohn, 2009). Mood might also be reflected in confidence indices, such as the Michigan Consumer Sentiment Index or the Ifo Business Climate Index, which contain some information about the future path of household spending. Others have shown that sports events impact stock-market indices (Edmans, Garcia and Norli, 2007) or expectations about the future personal situation and the economic situation in general (Dohmen et al., 2006). However, in this literature it is difficult to argue that these events do not affect preferences or do not have direct effects on utility. Individuals also often show a tendency to incorporate irrelevant information into their decision, although it is not

controlled environment that permits a systemic exploration of the impact that extrinsic information has on economic behavior.

In this paper, we present an experiment that reliably produces sunspot-driven behavior without explicitly priming or recommending that subjects follow extrinsic signals. We observe that different groups of subjects coordinate on different sunspot equilibria and we test how the noise structure of sunspot variables affects behavior. While previous experimental studies, particularly Duffy and Fisher (2005), provide evidence that extrinsic random signals may indeed affect subjects' behavior (at least after some training), little is known about how the impact of extrinsic signals depends on their noise structure, meaning the number of signals, the signal distribution, and whether these signals are publicly observable. To our knowledge, no previous experiment could implement a sunspot equilibrium without a training period where subjects were exposed to the relationship of the sunspot variable with a market outcome or without framing these variables as explicit recommendations, and no previous experiment has generated different sunspot equilibria under the same external conditions.

We use a simple two-player coordination game with random matching. In this game players simultaneously pick a number from the interval between zero and 100. The two players maximize their payoffs by choosing the same number, while deviations are punished with a quadratic loss function. Each coordinated number selection constitutes a Nash equilibrium and payoffs do not depend upon the number that players coordinate on. However, the game has a risk-dominant equilibrium (picking "50") which provides a natural focal point in the absence of a coordination device.[3]

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profitable for them, for example in financial or innovation decisions (e.g., Camerer, Loewenstein and Weber, 1989, Jamison, Owens and Woroch, 2009, or Choi, Laibson and Madrian, 2010).

[3] In the pure coordination game in our baseline setting, the notion of risk dominance (Harsanyi and Selten, 1988) provides a natural way to break the payoff symmetry and thus may serve as a focal point. Indeed, Schelling's (1960) focal point concept originated from situations where the game's formal structure provides no guidance for equilibrium selection, such as in a pure coordination game. For experimental studies of the focal point concept see, for example, Mehta, Starmer and Sudgen (1994), Bosch-Domenech and Vriend (2008), Bardsley et al. (2009), Crawford, Gneezy and Rottenstreich (2009) or Agranov and Schotter (2010). Crawford and Haller (1990) show theoretically how players can coordinate via precedents when they lack a common-knowledge description of the game in a repeated setting (see also Blume and Gneezy, 2000, for an experimental test).

In our experiment, extrinsic signals (sunspots) are binary random variables unrelated to payoffs, with realizations being either zero or 100.[4] These signals have two properties that we exploit. First, signals are semantically meaningful because they clearly map to the action space. Previous research has shown that when it comes to generating sunspot equilibria, semantically meaningful signals can be much more effective in the lab. Such signals can be easily used as coordination devices and provide a second focal point in addition to the risk-dominant equilibrium. It is an empirical question whether risk dominance or (public) signals are better focal points for subjects. Second, the semantically meaningful signals are extreme in the sense that they point towards the lowest or highest possible action, thereby maximizing the tension between different focal points. As the risk-dominance criterion allows us to order the different equilibria by their distance from 50, we can measure the power of sunspots by how distant actions are from the risk-dominant equilibrium.

We systematically vary the noise structure, meaning the number of signals, their distribution, and their degree of public availability. For each treatment, we measure the average distance between the chosen actions and the risk-dominant strategy and the portion of groups that converge to sunspot equilibria. Thus, we investigate to what extent publicly available information is necessary for sunspot-driven behavior to occur, and how subjects aggregate the available information.

Our first main finding is that extrinsic public signals that are easily aggregated lead to almost perfect coordination on the sunspot equilibrium that is implied by the semantics of the signals. This salient sunspot equilibrium reliably shows up when subjects just receive public signals, even when the sunspot equilibrium is associated with higher strategic risk than any other strategy. Coordination on the salient sunspot equilibrium is less pronounced when public *and* private signals are both present, as some subjects then condition their actions on the private signal, which either prevents full coordination of actions or leads to an intermediate sunspot equilibrium. While theory predicts the same set of equilibria as in a game with just one public signal, we find that the power of sunspots is significantly lower if

[4] A sunspot may be anything that coordinates the expectations of market participants and breaks the symmetry in coordination problems. For example, Schelling speculated in his seminal book on strategic conflicts that “Most situations … provide some clue for coordinating behavior, some focal point for each person’s expectations of what the other expects him to be expected to do” (1960, 57). Such clues might include folk wisdom, public recommendations, collective perception, consensus, stereotypes, or (strategy) labels.

private and public signals are combined. When subjects receive both public and private signals, we also observe that different groups of subjects coordinate on different equilibria for the same external conditions.

In the absence of public signals, the risk-dominant equilibrium predominates. However, sunspot-driven behavior can be observed for highly correlated private signals. This observation indicates that the likelihood of sunspot-driven actions may be a continuous function of the correlation of signals, while equilibrium theory predicts that sunspot-driven behavior can occur only if the signals from different agents are perfectly correlated.

The occurrence of sunspot-driven behavior or sunspot equilibria largely depends on the distribution of strategies in the early periods of the game. In treatments in which different groups coordinate on different equilibria, we detect a significant correlation between behavior that occurred during the first and last periods of the game. This finding is in line with previous results on coordination games such as Van Huyck, Battalio and Beil (1990, 1991).

In our coordination game, sunspot equilibria are not associated with welfare losses; in fact all equilibria yield identical payoffs. However, our setup allows us to isolate the welfare effects of the miscoordination induced by extrinsic information. We find that subjects' payoffs are U-shaped in the power of sunspots, measured by how distant the actions are from the risk-dominant equilibrium, and hence, we find significant differences in average payoffs between treatments. Miscoordination arises from (i) a slower convergence process towards a common strategy or a lack of convergence and (ii) coordination on a nonequilibrium strategy, particularly if sunspot-driven behavior imposes negative externalities on agents who do not receive signals. Both channels relate to or can be thought of as the costs arising from a lack of understanding whether or not to condition actions on the signals received and how to aggregate information.

The paper is organized as follows. In Section 2, we give a brief overview of the related literature. Section 3 introduces the game and theoretical considerations. Section 4 outlines the design of the experiment and derives hypotheses from theory. The results are discussed in Section 5, and Section 6 concludes.

2. Related Literature

Although experiments provide a useful tool for investigating sunspot behavior, to date only a few studies have done so. The first attempt to investigate sunspots in the laboratory was Marimon, Spear, and Sunder (1993). They implemented an overlapping generations economy, where the sunspot was a blinking square on the subjects' computer screens that changed its color: red in odd and yellow in even periods. In the first periods the size of a generation (fundamental variable) varied between odd and even periods, which constituted an endowment shock and led to a unique equilibrium with alternating high and low prices. After 16 to 20 periods, the endowment shock was eliminated by keeping the generation size fixed, which induced multiple equilibria, one stationary and one a two-period cycle. In four out of five sessions, subjects continued to alternate their price forecasts, but the price paths substantially diverged from the predicted sunspot equilibrium. It is not clear whether the alternating predictions were just carried over from the subjects' experiences in the first phase or whether the blinking square had any effect on their behavior.

Duffy and Fisher (2005) were the first to provide direct evidence for the occurrence of sunspots. They investigated whether simple announcements like "the forecast is high (low)" can generate sunspots in a market environment with two distinct equilibrium prices. They found that the occurrence of sunspot equilibria depends on the market institution's particular information structure. Sunspots always affected behavior in less informative call markets while in more informative double auction markets sunspots mattered only in four out of nine cases. A second interesting finding of this study is the importance of the semantics of the sunspot variable. If people do not share a common contextual understanding and hence do not interpret the sunspot in the same way, it is highly likely that the sunspot variable does not matter. In order to achieve a common understanding of the sunspot variable, the subjects in Duffy and Fisher (2005) were alerted to the existence of high and low price equilibria in combination with the respective announcement in an initial training phase. In sessions with announcements like "the forecast is sunshine (rain)" and without training subjects to relate these announcements to high or low prices, sunspots did not occur. In our experiment, sunspots are semantically salient and sunspot equilibria arise endogenously without any need of training.

Beugnot et al. (2009) explored the effect of sunspots in a setting with a payoff-dominant equilibrium. They used a three-player, two-action coordination game with two equilibria—"work" or "strike"—where "work" is payoff-dominant, weakly risk-dominant, and maximin. Their sunspot variable was a random announcement of "work" or "strike." They found that subjects did not coordinate on a sunspot equilibrium. Instead, there was some convergence towards the efficient non-sunspot equilibrium. This indicates that it may be difficult to generate sunspot equilibria against a focal point that combines payoff dominance with risk dominance.

There is also some experimental work on correlated equilibrium, a concept that is closely related to sunspot equilibrium (see e.g., Peck and Shell, 1991). For instance, Cason and Sharma (2007) found that subjects tend to follow public third-party recommendations of a correlated equilibrium with higher average payoffs than any Nash equilibrium. Cason and Sharma taught subjects that it is in their interest to follow recommendations provided that their respective partners do so, and they compared strategic games with situations in which subjects played against machines known to invariably follow the "recommendation." On average, about 80% of all subjects followed the recommendations (more so when they were playing against a machine) and achieved higher payoffs than those that did not receive recommendations. In a related experiment, Duffy and Feltovich (2010) also found that subjects conditioned their behavior on a third-party recommendation if the recommendation was associated with higher than Nash equilibrium payoffs but that subjects learned to ignore bad or nonequilibrium recommendations. Both studies used binary action games. Cason and Sharma (2007) attributed the behavior of subjects who did not follow recommendations to a lack of confidence that their partner would follow the recommendations.

Some related experiments explored subjects' responses to the recommendation of a pure strategy equilibrium in games with multiple equilibria (for example, Brandts and Holt, 1992; Brandts and MacLeod, 1995; Kuang, Weber and Dana, 2007; Van Huyck, Gilette and Battalio, 1992).[5] Although third-party recommendations can be seen as extrinsic signals, they differ considerably from the signals generated in our experiment. Framing a message as

[5] While the recommendations in these experiments mostly come from the experimenter, there are also experiments where advice is given by players of a previous cohort participating in the experiment (see, e.g., Schotter and Sopher, 2003 or Chaudhuri, Schotter, and Sopher, 2009, for such "intergenerational" advice in coordination games).

advice sent by someone conveys the idea that following the recommendation is in the recipients' interest. In our experiment, the signal is just a random device without an associated intention and the payoffs are the same, regardless of whether the subjects follow a public signal or coordinate on another equilibrium. Moreover, our experiment also explores situations with privately observed signals, while in these earlier experiments recommendations are always public information. One important finding of this literature is that subjects only follow "credible" recommendations: for example, subjects tend to disregard advice to play an imperfect or a less efficient equilibrium. By contrast, our results show that subjects may follow a random coordination device, even if it is riskier to do so and even if such behavior is no equilibrium.

3. The Game

The game we will analyze is a pure coordination game. It can be considered as a reduced form of a market setting in the spirit of Van Huyck, Battalio, and Beil (1990). Two agents independently and simultaneously pick an action $a_i \in [b,c]$. Agent *i*'s payoff is given by

$$\pi_i(a_i, a_j) = f(|a_i - a_j|), \tag{1}$$

where $f: R \to R$ is a twice continuous differentiable function with $f'(x) < 0 \;\; \forall x > 0$, $f'(0) = 0$, and $f''(x) < 0 \;\; \forall x$. Therefore, agent *i* maximizes her payoff when she matches agent *j*'s action. An agent is penalized for a deviation from her partner's pick by the concave payoff function *f*; the loss grows more than proportionally in the distance between the two chosen actions. Clearly, any coordinated pick of numbers constitutes a Nash equilibrium. In a Nash equilibrium, both agents receive the same payoff and, moreover, the payoff is exactly the same in all equilibria.

3.1 Equilibria with Signals

Let us now extend this game by introducing payoff-irrelevant information which can be public, private, or both. Let Φ be the set of all the possible public signals that agents might receive and for agent *i*, let Ψ^i be the set of possible private signals. For ease of presentation, let us assume that $\Psi^i = \Psi$ for both *i* (as in the experiment), and Ψ is finite. Let $P:(\Phi,\Psi,\Psi) \to [0,1]$ be the joint probability distribution on the signals, where *P* assigns strictly positive probabilities on each element in (Φ,Ψ,Ψ). A strategy is mapping signals to the interval [*b,c*]. The following lemma shows that equilibrium strategies do not depend on private signals.

Lemma 1: *Let* $\{a^{i*}_{\varphi,\psi} | \varphi \in \Phi, \psi \in \Psi\}$ *be a Bayesian Nash equilibrium strategy profile, where* $a^{i*}_{\varphi,\psi}$ *is a Bayesian Nash equilibrium action played by agent i with public signal* φ *and private signal* ψ *. Then, equilibrium actions are the same for both agents and do not depend on the private signal, that is,* $a^{i*}_{\varphi,\psi} = a^{*}_{\varphi} \quad \forall \psi \in \Psi$ *for any given* $\varphi \in \Phi$ *.*

Proof: see Appendix A.■

Lemma 1 implies that in a game with private signals, the set of Nash equilibria is the same as in an otherwise equal game without private signals. Equilibria in these games are mappings from public signals to the interval [*b,c*]. When there is a public signal, sunspot equilibria exist in which both agents condition their actions on the public signal. Any function $f:\Phi \to [b,c]$ is an equilibrium, provided that both agents follow the same function and, thus, are always perfectly coordinated.

While it is not necessary to assume that the payoff function is continuous and differentiable, we do so for ease of presentation. The assumption that *P* assigns a strictly positive probability to each element of (Φ,Ψ,Ψ) can also be relaxed. In general, the result holds as long as one's private signal does not perfectly reveal the other agent's signal, in which case the private signal would be public information. Concavity, on the other hand, is an important assumption, as the following counter-example shows.

Suppose that the payoff function for both players is linear in differences, i.e., $\pi^i(a_1,a_2) = -|a_1 - a_2|$. There is no public signal, $\Phi = \{\emptyset\}$, and there are two possible private signals that can be 0 or 100, i.e., $\Psi = \{0{,}100\}$. Moreover, assume that *P(0,100) = P(100,0) = 1/8* and *P(0,0) = P(100,100) = 3/8*, where the numbers are the private signals for players 1 and 2, respectively. It is easy to check that in this case, playing the private signal ($a^{i*}_{\varphi,\psi} = \psi$) is one of the game's many equilibria. If player *j* is playing this equilibrium and $\psi^i = 0$, then player *i*'s expected utility of choosing a^i is equal to $E\left(\pi^i(a) \middle| \psi^i = 0\right) = -25 - a^i/2$), which is maximized at $a^i = 0$. The same reasoning applies for $\psi^i = 100$. Hence, although conditioning actions on the private signal always incurs the expected costs of mismatch and thus a welfare loss, this strategy can constitute an equilibrium if the payoff functions are not strictly concave.

3.2 Riskiness of Equilibria

Due to the large set of equilibria, it is natural to use some selection criteria. One of the most widely used criteria to assess the risk of different equilibria is given by risk dominance (Harsanyi and Selten, 1988). In its original formulation, risk dominance is a binary relation that does not provide any strict order on the equilibria of our game. There is, however, an alternative notion of risk dominance in which, according to Harsanyi and Selten's heuristic justification, the selected equilibrium results from postulating an initial state of uncertainty where the players have uniformly distributed second-order beliefs on all equilibria. Each player believes that the other players' beliefs are uniformly distributed on the set of equilibrium strategies, which in our case is the entire action space. Throughout the rest of the paper, we will refer to this alternative notion simply as risk dominance.[6]

Another alternative selection criterion is the notion of secure action (see Van Huyck, Battalio and Beil, 1990). Based on the maximin criterion of von Neumann and Morgenstern (1947), a secure action is one that maximizes the minimum possible payoff.[7]

Lemma 2 characterizes our game's risk-dominant equilibrium and secure action. Note that this equilibrium is independent of the generated signals.

Lemma 2: $a^{i*}_{\psi,\varphi} = \frac{b+c}{2} \forall \psi, \varphi$ *is both the secure action and the risk-dominant equilibrium.*

Proof: see Appendix A.■

Lemma 2 shows that the strategy of choosing the interval's midpoint is both the secure action and the risk-dominant equilibrium. By choosing this midpoint, an agent minimizes the maximum possible distance to his partner's choice and can assure himself a minimum payoff of $f((c\text{-}b)/2)$. In addition, selecting the midpoint is also the best response to the belief that the actions of others are uniformly distributed on [*b*, *c*] or, alternatively, to the belief that the strategies of others are uniformly distributed on the entire set of all possible strategies. Unlike for Lemma 1, concavity is not a necessary condition for Lemma 2 to hold.

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[6] Among other experimental studies, this alternative notion has been used in Haruvy and Stahl (2004).

[7] The secure action does not need to belong to the support of Nash equilibria. In our game, though, it trivially does, because the support of Nash equilibria coincides with the whole set of actions. The security criterion has also been applied to a game after the deletion of non-equilibrium actions (see, e.g., Haruvy and Stahl, 2004).

Both selection criteria can order the different equilibria. According to the notion of secure action, one strategy is riskier than another if it can lead to a lower payoff. According to the notion of risk dominance, one equilibrium will be riskier than another if the expected payoff against a uniform distribution over all equilibrium strategies is lower. In the absence of public signals or in the case of two public signals in which the equilibrium is symmetric with respect to the two signals, both measures of risk can be expressed as a function increasing in the absolute distance to $(b+c)/2$. Therefore, throughout the rest of the paper, we will interpret the absolute distance to $(b+c)/2$ as a measure of risk. We will say that an extrinsic signal or a combination of extrinsic signals exerts a stronger effect on behavior than another combination, if (after some convergence) the average distance of the chosen actions from the midpoint $(b+c)/2$ is higher. Alternatively, we can say that given two information structures, one structure is more likely to produce sunspot-driven behavior than the other if the fraction of groups converging to a sunspot equilibrium or to a sunspot-driven non-equilibrium strategy is larger in that structure compared to the other treatment.

4. Experimental Design, Procedures and Hypotheses

4.1 Game Setup

In all the experimental treatments, the subjects repeatedly played the coordination game explained above. The subjects were randomly assigned to matching groups of six that were fixed throughout a session. In each period, we randomly matched the subjects into pairs within a matching group. There was no interaction between the subjects from different matching groups and, thus, we can treat the data from different matching groups as independent observations. The subjects were aware that in each period they were randomly matched with another subject from their matching group and that they would never face the same subject twice in a row.

The subjects had to choose, independently and simultaneously, an integer at or between 0 and 100. The players' payoffs depended on the distance between their own and their partner's choice. In particular, the payoff function was the following:

$$\pi_i\left(a_i, a_j\right) = 200 - \frac{1}{50}\left(a_i - a_j\right)^2 \quad . \qquad (2)$$

Table 1: Treatment Overview

Treatment	Public signals	Private signals per subject	Precision *p*	Existence of sunspot equilibria	Number of sessions / number of groups
N	-	-	-	No	3 / 6
P75	-	1	75%	No	2 / 6
P95	-	1	95%	No	2 / 6
AC	-	1*	100%	No	2 / 6
C	1	-	75%	Yes	2 / 6
CP	1	1	75%	Yes	4 / 12
CC	2	-	75%	Yes	2 / 6

Note: *common signal revealed to each subject with 90% probability

Subjects could earn a maximum of 200 points if their actions perfectly matched and for a deviation between their choices they were penalized by the quadratic loss term.[8] It is easy to check that this payoff function fulfills the properties of the function characterized in the previous section, so that both Lemma 1 and 2 apply.[9]

4.2 Treatments

In the benchmark treatment (Treatment N), subjects played the coordination game with payoff function (2) and received no extrinsic information. In all the other treatments, the subjects received some extrinsic information (signals) and we varied their public nature and the number of signals. Extrinsic information was generated as follows. In each period, the computer draws a random number $Z \in \{0,100\}$. Both numbers are equally likely and the realization is not disclosed to the subjects (except for one treatment). Instead, each subject in a pair receives at least one independently drawn signal *s*.

With probability $p \in [0.5,1]$ this signal s is the same as the random number Z, that is,

$$prob(s = 0 \mid Z = 0) = prob(s = 100 \mid Z = 100) = p \,.$$

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[8] Note also that the minimum payoff is zero, since the maximum distance between two actions is 100.

[9] In contrast to the game in Section 3, subjects could only choose integers between 0 and 100 instead of choosing from an interval of real numbers. Technically, strategies assigning different numbers to different private signals are equilibria if the difference between the two chosen numbers is at most 5 in Treatments P95 and AC and 1 in Treatments P75 and CP. We do not observe these non-generic equilibria and therefore ignore them in the following analysis.

Probability p measures the precision of signals and is one of our treatment variables.[10] The more precise the signals are, the higher is the correlation between two independently drawn signals, and the greater is the likelihood that both signals are the same. In treatments with private signals, each subject received an independently drawn signal that was not revealed to the other player. Public signals were revealed to both players in a pair and the subjects knew that each of them received the same signal. We also varied the number of signals that subjects received. In some treatments, the subjects received either a private or a public signal, and in two treatments, they received two signals: either one public and one private signal or two public signals. Table 1 gives an overview of the different treatments.

In Treatments P75 and P95, both subjects in a pair received independently drawn private signals, X_1 and X_2. The only difference between these two treatments is the probability with which signal X_i coincides with the number Z. In P75, this probability is $p = 0.75$, while in P95, it is $p = 0.95$. Therefore, in P75 subjects received the same signal in 62.5% of the cases, while in P95 this probability is 90.5%. Sunspot equilibria do not exist for private signals with $p < 1$ (see Lemma 1). With $p = 1$, the private signal in fact becomes a common (public) signal and sunspot equilibria do exist. Hence, the set of equilibria is discontinuous in p and by changing the precision of signals we can test this theoretical prediction.

In Treatment AC, the random number Z was revealed to each subject with probability $p = 0.9$. We call this signal "almost common information," as it generates common p-beliefs (with $p = 0.9$) in the sense of Monderer and Samet (1989). This treatment allows an alternative test of whether behavior is discontinuous in p, as predicted by the theory. In Treatment AC, no sunspot equilibrium exists since the information is not disseminated to all subjects with probability 1.

In Treatment C, both subjects in a pair received a public signal Y with $p = 0.75$. Since it was common information that both subjects received the same signal, sunspot equilibria exist. Any function $f : Y \mapsto [0,100]$ is an equilibrium.[11] In Treatment CC, the subjects

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[10] This measure allows for comparing different distributions in a way that can be easily understood by the subjects who are not trained in statistics.

[11] Experimental economists have pointed out that common information alone does not necessarily lead to common expectations nor to common knowledge (Smith, 1991, p. 804). Equilibrium theory does not make this distinction.

received two independently drawn public signals Y_1 and Y_2, both with p = 0.75. Here, any function f mapping pairs of (Y_1,Y_2,) to the interval [0,100] is an equilibrium. In Treatment CP, the subjects received both a public and a private signal. The public signal Y and both subjects' private signals X_1 and X_2 were drawn independently. The probability of a signal coinciding with Z was p = 0.75 for each signal. Subjects were always informed which signal conveyed public and private information. Again, subjects could ignore the private signal and condition their behavior on the public signal that allowed for sunspot equilibria. As in Treatment C, any function $f: Y \mapsto [0,100]$ is an equilibrium.

4.3 Procedure

Subjects played the game for 80 periods. After each period, they learned their partner's choice, the distance between their own choice and their partner's choice, and the resulting payoff. They also learned the realization of the random variable Z, except for Treatment N. In treatments with private signals (P75, P95, CP), they never learned their partners' private signal.

The general procedure was the same in each session and treatment. At the beginning of a session, the subjects were seated at computer stations in random order. The instructions were distributed and read out aloud, and if a player had any questions, these were answered in private. Throughout the sessions, the subjects were not allowed to communicate with one another and could not see each other's screens. They were not informed about the identity of their partner or the other members of their matching group. In the instructions, the payoff function (2) was explained in detail and was also displayed as a mathematical function and as a nonexhaustive payoff table.[12] Before starting the experiment, subjects had to answer questions about the game's procedures and in particular how the payoffs were determined. We had three reasons for implementing this quiz. First, we wanted to make sure that the subjects understood how their payoff would be determined. Second, we wanted to alert the subjects to the fact that neither the number Z nor the signals would affect their payoff, and third, the quiz also ensured that the subjects could clarify any last-minute questions and gain

[12] Additionally, subjects could use a calculator during the experiment, which allowed them to enter hypothetical numbers for their own and their partner's decision and calculate the resulting payoff.

confidence that the other players understood the game.[13] Once all subjects had answered the questions correctly, the experiment started.

We ran a total of 17 sessions with 18 subjects in each session, except for three sessions of Treatment N that had only 12 subjects. The sessions took place at the Technical University Berlin and in total 288 students, mainly undergraduate majors from various fields (engineering, business administration, mathematics, chemistry, etc.) participated. They were recruited through the online recruitment system ORSEE (Greiner, 2004). The experiments were computerized using z-tree (Fischbacher, 2007). At the end of a session, we determined the subjects' earnings by randomly selecting 10 out of the 80 periods for payment. The subjects were then paid in private, where the points earned in the selected periods were converted to euros (1 point = 1 euro cent). In addition subjects received a fee of 3 euros for showing up. A session lasted about one hour and the subjects on average earned 21 euros.

4.4 Hypotheses

The results from previous studies and the game theoretic analysis in section 3 provide some hypotheses for the different treatments that we summarize in this subsection. The first hypothesis relates to the treatment with no signals:

> **Hypothesis 1 (H1):** *Without additional information (treatment N), subjects coordinate on the risk-dominant equilibrium.*

Hypothesis 1 means that in the absence of signals, subjects will converge to playing 50. As previously emphasized, any coordinated pick constitutes an equilibrium. However, different notions of equilibrium selection point to this particular equilibrium. Besides being risk dominant, choosing the midpoint of the interval is the unique symmetric equilibrium according to the theory of focal points by Alós-Ferrer and Kuzmics (2008), and constitutes the best response to a random choice by the other player.

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[13] For instance, in Treatments P75 and P95, the relevant statement was: "Your payoff in a period depends on a) the number Z, b) the distance between your chosen number and the number chosen by your partner, or c) your private hint X." Subjects had to indicate the right statement and if their answer was not correct, the experimenter once again explained the payoff function to make clear that it only depends on the distance between one's chosen number and the number chosen by the partner. The full set of questions can be found in the appendix.

Previous studies pointed out that the occurrence of sunspot equilibria hinges on agents' shared beliefs, so these studies implemented a training phase to let these shared beliefs develop (see, e.g., Duffy and Fisher, 2005). Our experiment did not include a training period. Instead, we emphasized that the signals were unrelated to the payoff function. Therefore, signals may simply be ignored and sunspot equilibria may not be observed. This gives rise to the following hypothesis:

> **Hypothesis 2 (H2):** *In treatments with public signals, subjects converge to strategies that are independent of these public signals*.

The next hypothesis refers to the treatments with private signals. Regardless of their precision, the presence of private signals does not change the set of equilibria with respect to the otherwise equal treatment without private signals. Hence, in Treatments P75, P95, and AC we should observe convergence to the same strategies as in Treatment N. Similarly, in Treatment CP, we should observe convergence to the same strategies as in Treatment C:

> **Hypothesis 3 (H3):** *In treatments with private signals, subjects converge to the same strategies as in the otherwise equal treatments without private signals.*

Our design gives us two measures for the power of sunspots: one is the number of groups in each treatment who coordinate on strategies that are driven by sunspots, the other measure is the average distance of chosen actions from 50, the midpoint. In line with H2 and H3 we state the following hypothesis regarding the power of sunspots:

> **Hypothesis 4 (H4):** *The proportion of groups converging to a sunspot driven strategy and the average distance of actions from 50 are independent of the probability that both subjects receive the same signal.*

The alternative hypothesis is that the power of an extrinsic signal rises when the probability is greater that the other player receives the same signal. This probability is 0.625 in Treatment P75, around 0.9 for Treatments P95 and AC (conditional on receiving a signal)[14], and 1.0 for Treatment C.

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[14] The probability for another subject receiving the same signal is 0.905 in Treatment P95 and 0.9 in Treatment AC. Getting these numbers as close as possible was the reason why we chose $p = 0.9$ in AC.

When two signals are received, it may be more difficult for the subjects to agree on a common sunspot strategy than if they receive one unique signal. Here, the focal sunspot equilibrium prescribes three different actions conditional on the combination of signals: 0 for $X = Y = 0$, 100 for $X = Y = 100$, and 50 for $X \neq Y$. We included Treatment CC for testing this hypothesis. Here, our null hypothesis is that the subjects simply aggregate both signals and behave as in Treatment C if both signals coincide:

Hypothesis 5 (H5): *In Treatment CC, the proportion of groups converging to a sunspot equilibrium and the average distance of actions from 50 (conditional on equal signals) is the same as in Treatment C.*

In any equilibrium, the subjects' actions are perfectly coordinated, yielding the highest possible payoff, which is the same in all treatments. Thus, if the signals' structure does not affect the ability of subjects to coordinate on an equilibrium, the payoffs should be the same across treatments:

Hypothesis 6 (H6): *The average payoffs are the same in all treatments.*

In experiments, one cannot expect that the subjects' actions are in equilibrium from the start of a session. Since the game is rather simple, we expected, however, that each matching group would converge to an equilibrium or at least to a common strategy. The speed of convergence, however, may be related to the treatments. Because more signals result in a more complex strategy set, the number of signals may reduce the speed of convergence and, thereby, the overall efficiency. For comparing public and private signals, there are two theoretical arguments pointing in opposite directions. On the one hand, any public signal increases the dimension of the equilibrium set. This may make it more difficult for group members to coordinate on any particular equilibrium. On the other hand, a private signal for each player increases the dimension of the set of states of the world (signal combination) more than a public signal. In addition, agents never learn their partner's signal—or whether their partner received a signal at all (in AC). This makes it more difficult for subjects to assign an observed action to a particular strategy.

5. Results

We start analyzing the data by checking whether the subjects within a matching group converged to a common strategy and by identifying the strategies on which they converged. This analysis also allows a first test of the hypotheses H1 to H5. Then, we show how to use the distance of choices from the risk-dominant equilibrium as a measure of the sunspots' power. We use this measure to perform a detailed analysis of the differences in behavior across treatments.

5.1 Convergence and Identified Strategies

For checking whether groups converged to a common strategy and to which strategies they converged in the different treatments, we introduce two convergence criteria. The *strong convergence* criterion requires that all six subjects in a matching group play according to the same strategy, allowing a deviation of ±1, for periods 70–79. The *weak convergence* criterion requires that at least four subjects in a matching group follow the same strategy, allowing a deviation of ±3, for periods 70–79.[15] If a group converged to a common strategy, we identify the strategy to which it converged by the choices of the majority of subjects who fulfilled the respective convergence criterion.

Table 2 gives a first impression of results in the different treatments. For converging groups, we identify four types of strategies they coordinated on: 1) "50:" the risk-dominant strategy; 2) intermediate sunspot strategies, such as "25/75" or "10/90," in which subjects choose the lower number when the signal is 0 and the higher number when the signal is 100; 3) "0/100:" follow the signal; 4) "Mean;" play the average of both signals. In Treatment CP, strategies of types 2) and 3) refer to the public signal only.

Table 2 summarizes how many groups converged according to our two criteria detailed for the identified strategies. First, we note that except for Treatment CP all groups but one converged to one of the identified strategies at least according to the weak convergence criterion. In Treatment CP only eight out of 12 groups weakly converged. While

[15] We do not include period 80, because some subjects deviate exclusively in the last period. Tables C1 and C2 in the appendix show more detailed results including the period in which a certain group convergences to a particular strategy according to the strong and weak convergence criterion.

Table 2: Coordination Summary

	Treatments without public signals				Treatments with public signals		
Treatment	N	P75	P95	AC	C	CP	CC
Total number of groups	6	6	6	6	6	12	6
Coordinated groups	5 (6)	5 (6)	3 (5)	5 (6)	4 (6)	6 (8)	4 (6)
Strategies:							
"50"	5 (6)	5 (6)	3	4	-	1	-
"25/75"	n.a.	-	-	-	-	1 (2)	n.a.
"10/90"	n.a.	-	- (2)	-	-	-	n.a.
"0/100"	n.a.	-	-	1 (2)	4 (6)	4 (5)	n.a.
"Mean"	n.a.	n.a.	n.a.	n.a.	n.a.	-	4 (6)
Average distance to 50	0.81	2.51	17.25	13.86	46.69	31.57	30.50

Note: "Coordinated groups" indicates the number of groups who converged according to the strong (weak) convergence criterion.

most groups in treatments without public signals converged to "50," most groups in treatments with public signals coordinated on a sunspot equilibrium.

The identified strategies allow a first test of Hypotheses H1 to H5. In Treatment N all six groups converged to the risk-dominant equilibrium. Hence, H1 cannot be rejected. In treatments with public signals, most groups converged to a sunspot equilibrium. Only one group in Treatment CP coordinated on a non-sunspot equilibrium. Thus, H2 can be rejected for all three treatments with public signals. Regarding the type of sunspot equilibrium, we find that in Treatment C all six groups converged to the salient equilibrium "0/100" and in Treatment CC, all groups converged to the "Mean." Thus, Hypothesis H5 cannot be rejected. In Treatment CP, however, we observe two groups converging to the intermediate sunspot equilibrium "25/75," which is not in line with Hypothesis H3.

This last finding is related to H3, the hypothesis that private signals do not affect behavior. Comparing CP and C, we see a clear difference in the fraction of converging groups and in the strategies to which they converged. For treatments with private signals only, we find that all groups converged to the risk dominant equilibrium if the correlation between signals was small (as in Treatment P75). However, we can reject H3 for Treatments P95 and AC in which the correlation of signals was high. Here, two out of the six groups in each treatment coordinated on a sunspot-driven strategy. This result also allows us to reject H4.

To summarize the evidence gained from identified convergence points: subjects may coordinate on sunspot-driven strategies whenever the signals received by different individuals are highly correlated. The subjects reliably converged to sunspot equilibria in treatments with public signals only. In these cases, all groups converged to the equilibrium that was indicated by the semantics of the sunspot variable, that is, their actions coincided with the public signal in Treatment C or the mean of the two public signals in CC. In other treatments, however, we also observed intermediate sunspot-driven strategies in which subjects chose higher numbers for higher signals without following the signals numerically.

In order to simplify our remaining analysis, we checked whether the subjects' strategies were symmetric. In other words, whether subjects who chose $a_i = m$ when they received signal s = 0 played $a_i = 100 - m$ when their signal was s = 100.[16] In Appendix B, we show that symmetry not only applies to the strategies subjects converged to; it also applies to actions played during the entire experiment. We therefore pool the data for symmetric sets of signals and measure the power of sunspots by how distant the chosen actions were from 50. If extrinsic signals affect behavior, they must raise the average distance of choices from 50.

5.2 Non-sunspot and sunspot equilibria

Figure 1 depicts the average distance to 50 over all matching groups for Treatments N and C and shows clear evidence for the convergence to the risk-dominant equilibrium in Treatment N and for the occurrence of sunspot equilibria in Treatment C. Treatment N, in which subjects did not receive any extrinsic signals, serves as benchmark for a game without sunspot equilibria. In this treatment, subjects almost immediately converged to playing $a_i = 50$. As explained above, $a_i = 50$ is not just the secure action, but it is also the risk-dominant equilibrium, results from level-k reasoning, and is the unique symmetric

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[16] In treatments CP and CC, symmetry refers to playing *m* when both signals are 0, *100-m* when both signals are 100. When the two signals are different in CP, symmetry means playing *n* when the public signal is 0 and the private signal is 100, and *100-n* when the public signal is 100 and the private signal is 0. For two distinct public signals in Treatment CC, symmetry prescribes playing 50 as in Treatment N.

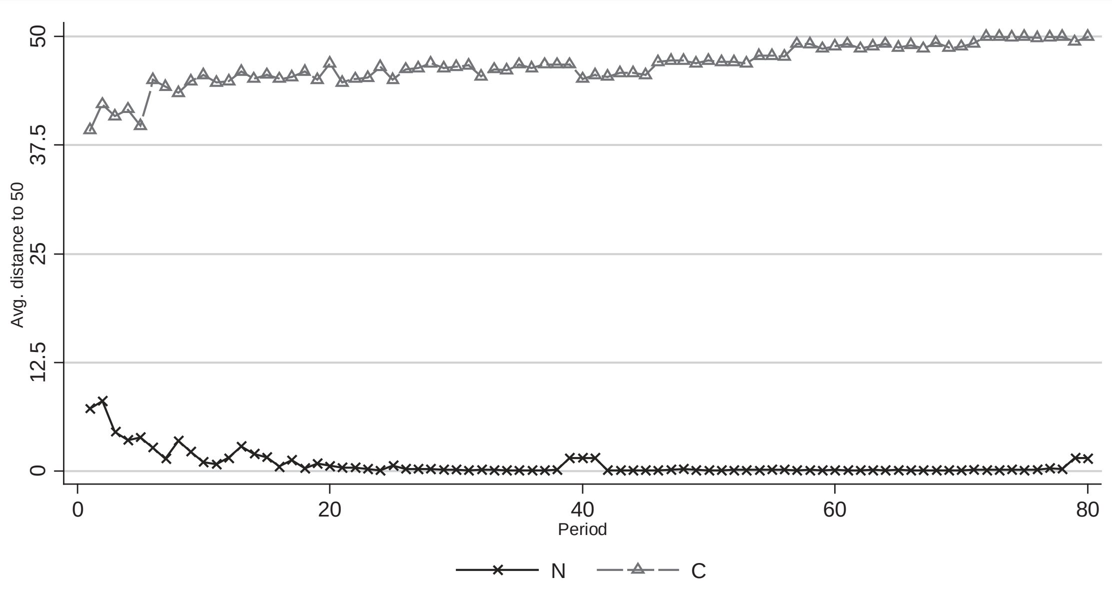

Figure 1: Average Distance to 50 over all Periods and Groups in N and C

Source: Authors' calculations.

equilibrium according to the theory of focal points by Alós-Ferrer and Kuzmics (2008).[17] The average distance of actions from 50 in Treatment N was 0.81 and all groups but one converged according to the strong convergence criterion. But even in the remaining group, the failure to achieve strong convergence was only due to one subject who deviated during the last four periods after being perfectly coordinated with all other group members on the risk-dominant equilibrium from the first period onwards. In total, 22 out of 36 subjects in Treatment N chose "50" in the first period, and 13 of them stuck to this strategy during the entire session. Thus, in the absence of public signals, it seems natural to converge to the risk-dominant equilibrium. When testing whether actual choices are distributed around 50, we cannot reject hypothesis H1 (Wilcoxon signed-rank test, $p = 0.6$, two-sided).[18]

In Treatment C, in which an extrinsic signal is publicly available, there is a clear convergence process towards choosing the action that is indicated by the signal. For Treatment C, all

[17] Van Huyck, Battalio and Beil (1990) provide the first evidence that risk dominance is a relevant selection criterion in coordination games.

[18] In all nonparametric tests we used a matching group as an independent observation, because from period 2 onwards, individual choices were affected by observing other group members. Unless otherwise noted, we aggregated the data across all 80 periods in a matching group. Note that this presents a conservative way to detect any differences in behavior. Whether we used one-sided or two-sided tests depended on the specific alternative hypothesis (see Section 4.4).

groups converged to playing $a_i = \text{Y}$. The high degree of coordination on 0/100 implies that the average absolute distance to 50 is large. It was 46.69 over all periods and rose to 49.82 during the last 10 periods. This average distance is close to the maximum possible value of 50 and is larger than in all treatments without public signals. Obviously, we can reject that the average distance is the same in N and C (Mann-Whitney test, $p < 0.01$, one-sided).

Result 1: *Sunspot equilibria emerge reliably in the presence of a salient (but extrinsic) public signal.*

5.3 Sunspot-Driven Behavior without Public Signals

Despite the different noise structures, theory prescribes that behavior in Treatments P75, P95, and AC should be the same as in Treatment N: subjects should ignore their signals, in which case we expect the chosen actions will converge to 50.

Figure 2 plots the average distance to 50 in blocks of 10 periods for all groups in P75, P95, and AC. In Treatment P75 all groups converged to the risk-dominant equilibrium. Treatments P95 and AC displayed some heterogeneity among groups. Some groups quickly converged to the risk-dominant equilibrium, while in other groups subjects seemed to condition choices on their private signals.

The most interesting finding in Treatments P95 and AC is the behavior of groups who did not converge to an equilibrium: the emergence of *sunspot-driven nonequilibrium* behavior. Unlike the theoretical prediction, highly precise private signals may not only impede coordination, but may also lead to coordination on non-equilibrium strategies. In Treatment P95, private signals affected behavior in three groups (Groups 13, 14, and 18) throughout the game and diverted actions away from the risk-dominant equilibrium. Groups 13 and 14 coordinated on 10/90. In two groups of Treatment AC (Groups 22 and 24), subjects conditioned their behavior on the signal when it was available and otherwise chose 50. Both groups converged to the sunspot-driven nonequilibrium strategy 0/100. Hence, when the correlation between private signals is high, sunspot-driven behavior can emerge. This is in contradiction to equilibrium theory which describes a discontinuous behavior, meaning that as long as the signals are imprecise, sunspot equilibria do not exist.

Result 2: *Salient extrinsic private signals of high correlation may cause sunspot-driven behavior even though this is no equilibrium.*

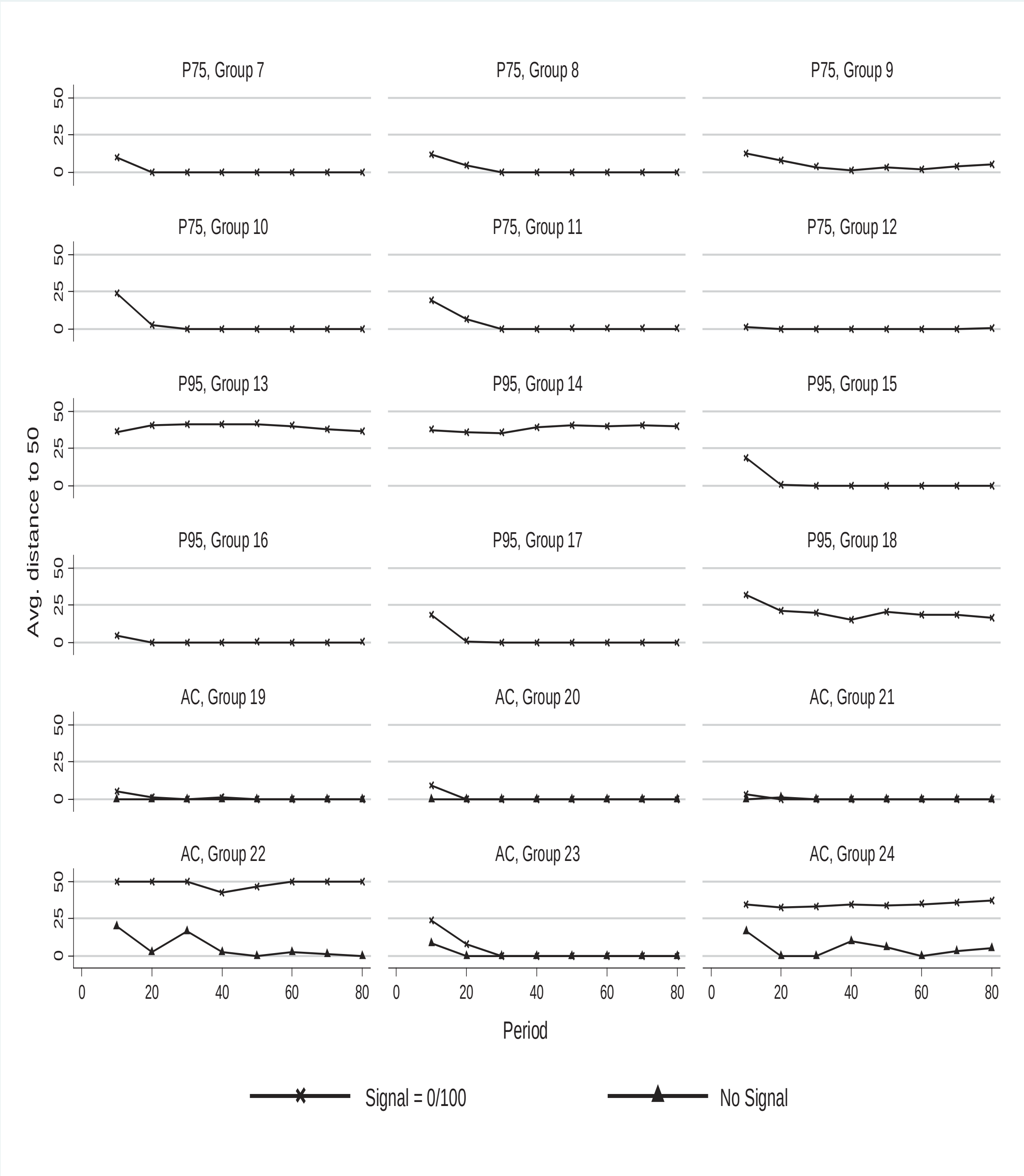

Figure 2: Average Distance to 50 by Blocks of 10 Periods in Treatments P75, P95, and AC

Source: Authors' calculations.

In Treatment P75, the average absolute distance to 50 was 2.51. The difference in distances between Treatments N and P75 is just on the edge of being significant if we use nonparametric tests (Mann-Whitney test: $p = 0.066$, robust rank-order test: $p = 0.104$, both tests one-sided). In Treatment AC, the average distance to 50 was 13.86 (14.83, conditional on receiving a signal). The difference between Treatments N and AC is significant, but only at

Table 3: Panel Regression

	Dependent variable: distance to 50		
	Period 1-80	Period 1-20	Period 61-80
P75	1.698**	5.847***	0.581
	(0.684)	(2.082)	(0.688)
P95	16.443***	18.004***	15.553**
	(6.963)	(5.933)	(7.232)
AC	14.070*	15.519**	14.140
	(8.053)	(7.546)	(8.644)
Constant	0.814***	2.507***	0.249
	(0.250)	(0.910)	(0.152)
Test P75=P95#	p=0.035	p=0.048	p=0.039
Test P75=AC#	p=0.125	p=0.210	p=0.118
Test P95=AC#	p=0.824	p=0.794	p=0.900
N	11302	2825	2825
R^2	0.17	0.14	0.19

Notes: Panel regressions with random effects with standard errors clustered at the matching group level. The significance of the coefficients of the treatment dummies is based on two-sided tests.
The *p*-values correspond to Wald tests (two-sided) based on the regression results.
* p<0.1, ** p<0.05, *** p<0.01.

the 10% level (Mann-Whitney test: $p = 0.066$, one-sided).[19] In Treatment P95, the average absolute distance to 50 was 17.26, and we can clearly reject an equal average distance in P95 and N (Mann-Whitney test, $p < 0.01$, one-sided). Thus, at least for treatment P95, we can reject the hypothesis that private signals do not affect behavior. The reason for the low significance of AC is that four out of six groups converged to the risk-dominant equilibrium. Thus, the median distance was about the same as in Treatments N and P75.

In Table 3 we present evidence from panel regressions. The dependent variable is the distance to 50, which is regressed on treatment dummies for P75, P95, and AC (Treatment N serves as the baseline).[20] Note that controlling for the period or the behavior of the opponent in the previous period does not affect the qualitative results. The results in column 1 confirm the nonparametric tests, albeit the difference between P75 and N is significant at the 5% level. Looking at the first 20 periods (column 2) we see that treatment effects emerge in the early periods but get smaller over time (column 3).

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[19] If not indicated otherwise, all results obtained by using the Mann-Whitney test are robust to using the robust rank-order test.
[20] For Treatment AC, the regression is based on data from informed subjects only.

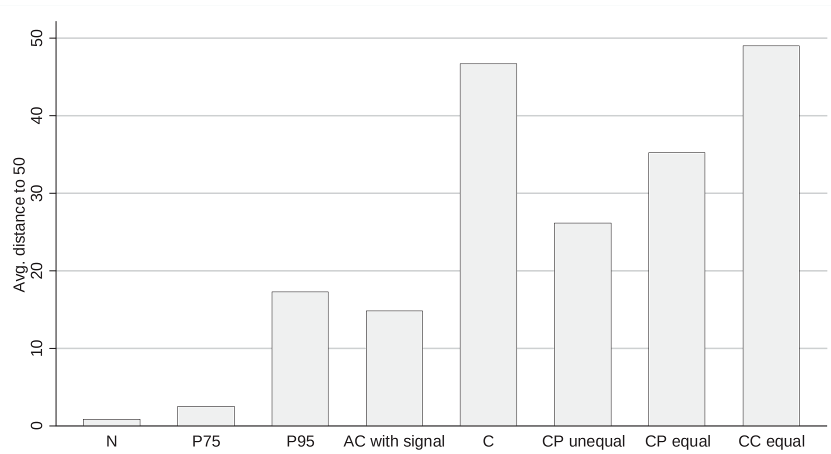

Figure 3: Average Distance of Choices from the Risk-Dominant Equilibrium

Source: Authors' calculations.

Hypothesis H4 claims that the distance of actions from the risk-dominant equilibrium is independent of the probability that another player receives the same signal. Figure 3 indicates that this is not the case. It compares the average distance of choices from 50 across treatments. Recall that for any subject receiving one signal, the probability that the other subject sees the same signal is 62.5% in Treatment P75, 90% in AC, 90.5% in P95, and 100% in Treatment C. The impression that the distance rises in the aforementioned probability is partially confirmed by a two-sided Wald test in the panel regression: the coefficients on treatment dummies P75 and P95 differ significantly. However, nonparametric tests cannot reject that the distance in Treatment P95 is the same as in P75 (Mann-Whitney test: $p = 0.13$, one-sided). P-values are even higher for a comparison between P75 and AC. In Treatment C, the average distance is significantly higher than in all treatments without a public signal. The respective p-values are all below 2%.

5.4 The Effects of Multiple Signals

In this section, we focus on the treatments with two signals, CC and CP. In both treatments, the available signals can generate sunspot equilibria. As previously shown in Table 2, subjects coordinated on following the mean of both signals in Treatment CC, while there were convergence patterns to three different equilibria in Treatment CP.[21] The most immediate observation from Treatment CP is that all groups but one departed from playing

[21] For a more comprehensive overview on each independent group, including the periods of convergence, see Table C2 in the Appendix.

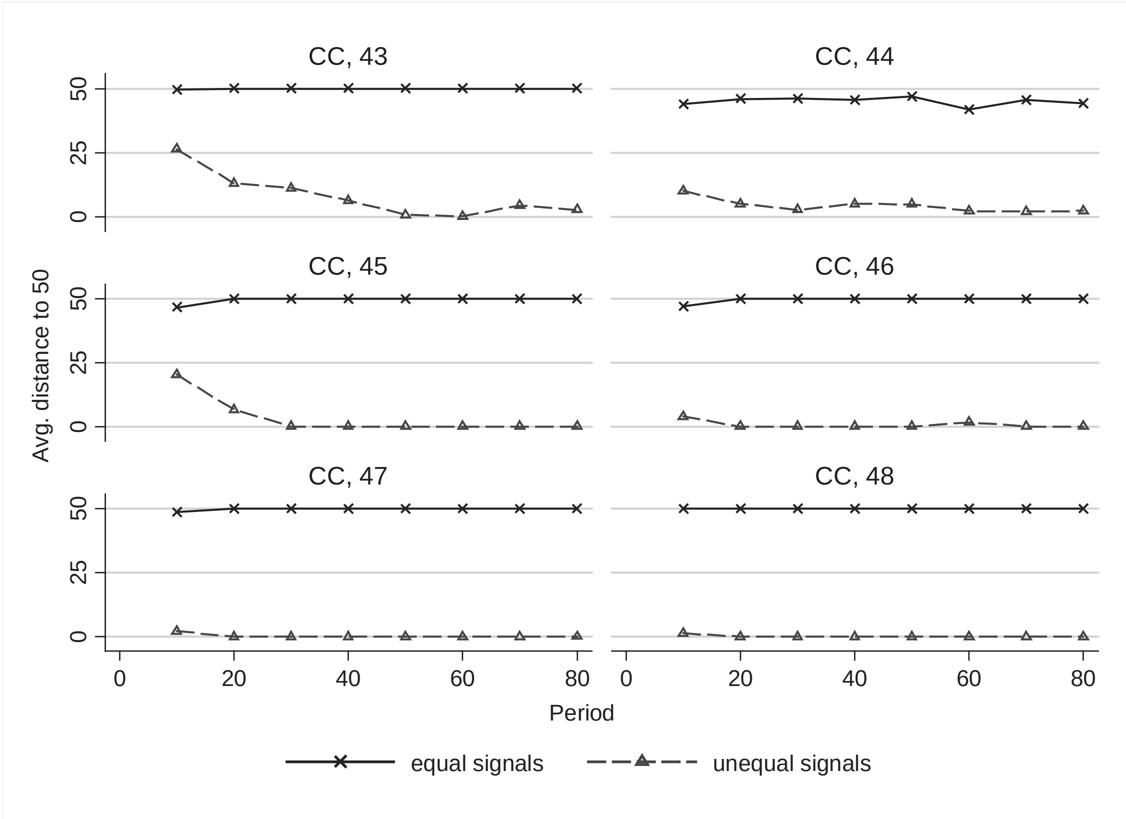

Figure 4: Average Distance to 50 by Blocks of 10 Periods in Treatment CC

Source: Authors' calculations.

the risk-dominant equilibrium and conditioned their choices on public signals. If they converged, they converged to a sunspot equilibrium.

Figures 4 and 5 show the average distance to 50 by 10-period blocks for all groups in Treatments CC and CP, respectively. Treatment CC is the treatment with the largest set of equilibria, since any function mapping a *combination* of public signals to an action constitutes an equilibrium. Figure 4 shows that all six groups converged to playing the average of the two signals, which seems the most natural focal strategy.[22] This strategy results in a three-cycle sunspot equilibrium with choices of 0 if both signals are zero, 100 if both signals are hundred, and 50 if the signals are unequal. Hence, it seems that if public signals can be aggregated in a simple way, then subjects quickly learn how to do it and respond to them as easily as to a single public signal.

[22] Five groups coordinated according to the strong convergence criterion, while one group (Group 44) converged only according to the weak convergence criterion. This was mainly due to a single subject, whose choices cannot be distinguished from random behavior.

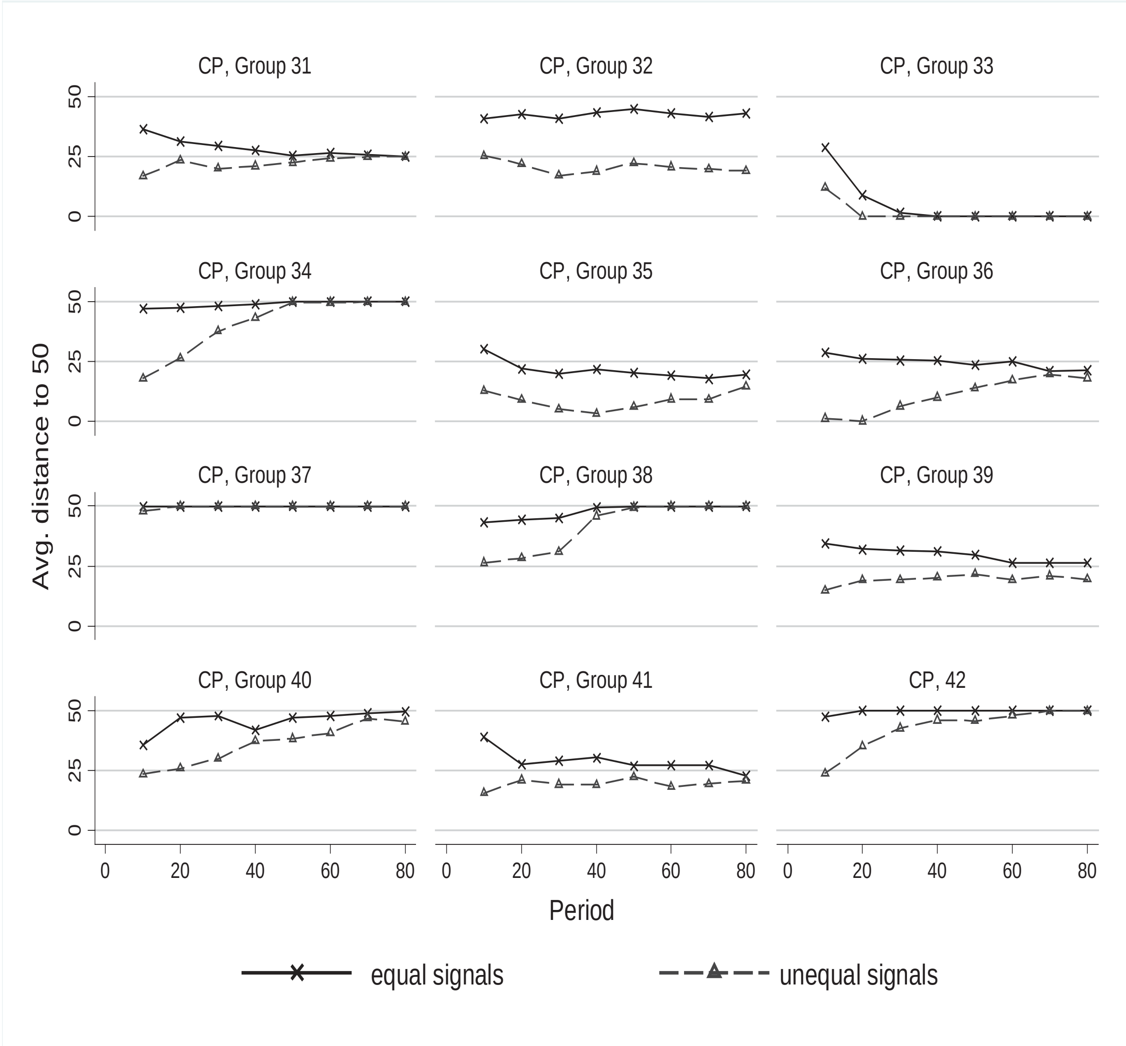

Figure 5: Average Distance to 50 by Blocks of 10 Periods in Treatment CP

Source: Authors' calculations.

Treatment CP (Figure 5) provides the most interesting results, because in this treatment different types of sunspot equilibria emerged. There are three popular strategies that are implied by the salience of signals: the risk-dominant equilibrium, following the public signal, and choosing the mean of both signals. In the first period, 32% of all subjects chose 50, 42% followed the public signal, and 60% of decisions were consistent with choosing the mean. Only one group (Group 33) resorted to the risk-dominant equilibrium and neglected both signals from early periods onwards. Five groups (Group 34, 37, 38, 40, and 42) followed the public signal as in Treatment C. Of the remaining six groups, at least three converged to an intermediate sunspot equilibrium in which subjects chose 25 whenever Y=0 and 75 when Y=100. [23] Note that "25/75" is the maximin response to any nondegenerate distribution of the three popular strategies that subjects played in the first period. This may explain why some groups converged to this particular sunspot equilibrium.

Result 3: *The presence of multiple public signals has no impact on coordination as long as signals can be easily aggregated (as in Treatment CC). However, combining public and private signals can considerably impede the convergence process (as in Treatment CP). The presence of a private signal reduces the power of the public signal.*

For statistical support we perform nonparametric tests.[24] Conditional on both signals being equal, the average distance in Treatment CC was 48.97, about the same as in Treatment C (Mann-Whitney test, $p > 0.24$, two-sided). Hence, we cannot reject Hypothesis H5. If the two signals were unequal, the average distance was 2.79, which is not significantly different from Treatment N (Mann-Whitney test, $p > 0.39$, two-sided). In total, the average distance between the chosen actions and 50 was 30.50, because in Treatment CC the two public signals coincided in 62.5% of all cases.

In Treatment CP, the average distance of actions to the risk-dominant equilibrium was 35.19 when the public and the private signal coincided, and 26.12 otherwise. The Wilcoxon matched-pairs test rejects that distances are the same for equal and unequal signals ($p < 0.01$, two-sided). This shows that private signals matter for behavior in this treatment and Hypothesis H3 can be rejected. Both distances are significantly larger than in Treatment N (Mann-Whitney tests, $p < 0.01$, two-sided), so that H2 can be rejected as well. It is also informative to compare Treatment CP to treatments with public signals only. The average distance in Treatment CP is significantly smaller than in Treatments C and CC for equal signals (Mann-Whitney tests, $p < 0.02$, two-sided). Thus, we may conclude that the public signal has less power if combined with a private signal.

The data also allow us to compare the power of private signals in Treatment CP with treatments in which the private signal is not combined with a public signal (P75 and P95): in

[23] For one group, the weak convergence criterion was met only from period 76 onwards. Two more groups show a tendency towards "25/75" but 80 periods were not long enough to reach a common strategy.
[24] We also performed panel regressions where we regress the distance to 50 on treatment dummies for CC and CP (with C as baseline) separately for equal and unequal signals. These regressions yield the same qualitative results and are thus not reported here.

CP, the difference between actions chosen in situations with equal and unequal signals was 9.07 on average. We can compare half of this value, 4.54, with the effect of private signals in Treatments P75 and P95 measured by the average distances of actions from 50. The effect of private signals in CP seems to lie between their effects in Treatments P75 and P95 (2.51 and 17.25 respectively), but these differences are not significant (Mann-Whitney test, $p = 0.15$ and $p = 0.68$, two-sided). Hence, we cannot reject that the power of the private signal in Treatment CP equals the power of a private signal without a coexisting public signal.

The evidence from Treatment CP shows that the effect of signals is not additive. While a single private or public signal does not prevent coordination, coordination becomes considerably more difficult if the two signals are displayed simultaneously. Despite some variance, convergence to an equilibrium takes a surprisingly long time, if it happens at all. This fact was exemplified by the three groups that never coordinated their actions. In Treatment CP, a subject needed to learn that (i) the private signal should be ignored and (ii) it may be good to condition one's action on the public signal, even though it is intrinsically as irrelevant as the private signal. Apparently, this learning process takes longer than learning only one of these points in the other treatments.

5.5 Welfare

The previous results clearly show that different information structures induce very different behavior. We have seen that purely public information reliably generates sunspot equilibria whereas, for instance, no information or imprecise private information leads to the risk-dominant equilibrium. For welfare considerations, it does not matter which equilibrium is eventually chosen. Hence, following or neglecting sunspots need not affect welfare. What matters, however, is whether and how fast subjects converge to an equilibrium. If a certain information structure results in a slower convergence process, we observe frequent miscoordination in the early periods and thus welfare losses.

The obvious welfare measure that we use throughout this section is the group's average payoff. Table 4 displays the average payoffs in the different treatments for the first 20 periods, for the last 20 periods, and for all periods. For statistical support we ran nonparametric tests based on all 80 periods. This gives us a rigorous test of possible welfare effects, since it requires long periods of miscoordination to generate significant differences in

Table 4: Welfare Summary

	Treatments without public signals				Treatments with public signals		
Treatment	N	P75	P95	AC	C	CP	CC
Period 1-20	197.43	188.58*	180.65**	185.17**	190.26	180.83*	187.50
Period 61-80	199.71	199.00	193.09	193.96	199.60	195.42**	197.86
all periods	199.17	196.68	188.56**	190.72**	195.37	189.56**	194.69

Note: For the nonparametric tests in this table, a matching group is treated as independent observation and Treatments P75, P95, AC, and C are compared to Treatment N, whereas Treatments CP, and CC are compared to C. Significance levels are based on Mann-Whitney tests (* p < 0.1, ** p < 0.05, *** p < 0.01). The results are robust to using the robust rank-order test.

average payoffs over all periods.[25]

Comparing payoffs in Treatments N, C, and CC, we cannot reject the hypothesis that the average payoffs over all periods are the same (Mann-Whitney tests, $p > 0.30$, two-sided). Thus, extrinsic public signals per se do not appear to be detrimental for welfare. In Treatment P75, initial payoffs were lower. But since subjects learned to ignore their signals rather quickly, the payoffs over all periods are comparable to Treatment N (Mann-Whitney test, $p = 0.13$, two-sided).

In the treatments with highly correlated private signals, P95 and AC, the average payoffs over all periods are significantly smaller than in Treatment N (Mann-Whitney, $p = 0.01$ and $p = 0.025$ respectively). In Treatment P95, this is mainly due to the late convergence—but also to the two groups who coordinated on a nonequilibrium strategy. In Treatment AC, the convergence time was comparable to Treatment C. Here, the lower average payoffs were due to the two groups who converged to a nonequilibrium strategy. The probability of not receiving a signal in Treatment AC was small (10%), but the associated losses were so large that they reduced average payoffs significantly.

Finally, we can reject the null hypothesis for equal means in Treatments C and CP (Mann-Whitney, $p < 0.04$, robust-rank order test, $p = 0.1$, both tests two sided) and in Treatments N and CP (Mann-Whitney, $p < 0.01$, two-sided). Comparing the average payoffs in Treatment CP with Treatments P95 and AC, we find no significant differences (Mann-Whitney, $p > 0.1$, two-sided). We summarize these findings in the following result.

[25] We obtained the same results by running random-effects GLS regressions on individual payoffs.

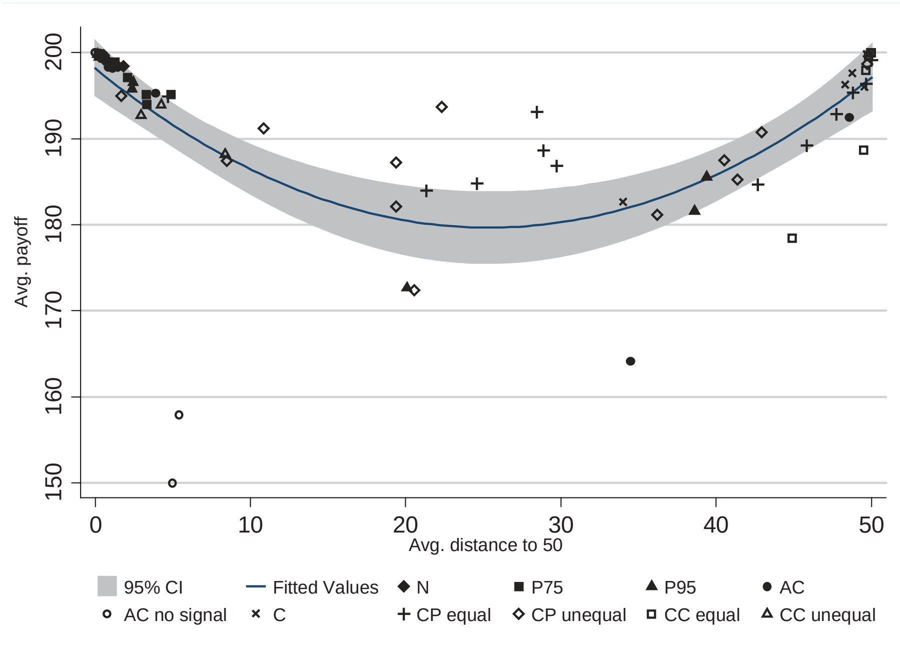

Figure 6: Relationship between Average Distance to 50 and Average Payoff

Source: Authors' calculations.

Result 4: *Extrinsic public or private signals with low correlation are not detrimental to welfare. But if extrinsic private signals are highly correlated or combined with public signals, we observe considerable welfare losses.*

The fact that average payoffs over all periods in Treatments CP were lower than in P75 (Mann-Whitney, $p < 0.01$, two-sided) and C and similar to those in Treatments P95 and AC shows that the welfare effects of imprecise private signals are larger when combined with a public signal. This is true, even though we rejected the hypothesis that private signals have a different power as measured by their effect on the distance between choices and the risk-dominant equilibrium. A possible explanation is the reduced power of public signals. While in Treatment C public signals are so powerful that they lead to a fast coordination on the salient sunspot equilibrium, this power is partially lost if other salient messages are present that should be ignored.

Figure 6 relates each group's average payoff conditional on the signal combination to the average distance of actions from 50 and reveals an interesting U-shaped pattern. The figure also displays the prediction (fitted line) from a regression of average group payoffs on

Table 5: Median Period of Convergence

	Treatments without public signals				Treatments with public signals		
Strategies	N	P75	P95	AC	C	CP	CC
"50"	3	9	8.5	2	-	8	-
"25/75"	-	-	-	-	-	59	-
"10/90"	-	-	65	-	-	-	-
"0/100"	-	-	-	35.5	4.5	22	-
"Mean"	-	-	-	-	-	-	3.5
all strategies	3	9	36.5	2	4.5	42	3.5

Note: Median period of convergence according to the weak convergence criterion.

average distance and squared average distance along with the 95% confidence interval to visualize this U-shaped pattern.

Groups that converged quickly to the risk-dominant equilibrium achieved almost the maximum payoff of 200 points. In Treatments C and CC, salient public signals were so powerful that subjects quickly coordinated on a sunspot equilibrium, resulting in payoffs that were also close to the maximum (195.37 in Treatment C and 194.69 in Treatment CC). In Treatments P95, AC, and CP, different groups coordinated on different strategies—not all of them equilibria. Nonequilibrium strategies are associated with welfare losses, even if all the subjects use the same strategy. Groups who coordinated on intermediate sunspot-driven strategies, such as "25/75," achieved lower payoffs because the convergence process was slower than in other groups. On average, the payoffs in these three treatments were well below 190 points. In Treatment AC, subjects who conditioned their choices on the signal exerted a negative externality on those who did not receive a signal. Without receiving a signal, choosing 50 is the best choice because it minimizes the loss with respect to any symmetric strategy. The externality shows up in the average payoffs for Groups 15 and 17, where subjects who did not receive a signal got average payoffs of only 150.00 and 157.23 points, respectively. These payoffs were considerably lower than in any other information condition and treatment.

The relation of payoffs to the power of sunspots has its counterpart in convergence time. For treatments in which sunspots have low power (measured by the average distance to 50), convergence to a common sunspot-driven strategy takes longer than in sessions with highly powerful sunspots. Table 5 summarizes the median period of convergence for the

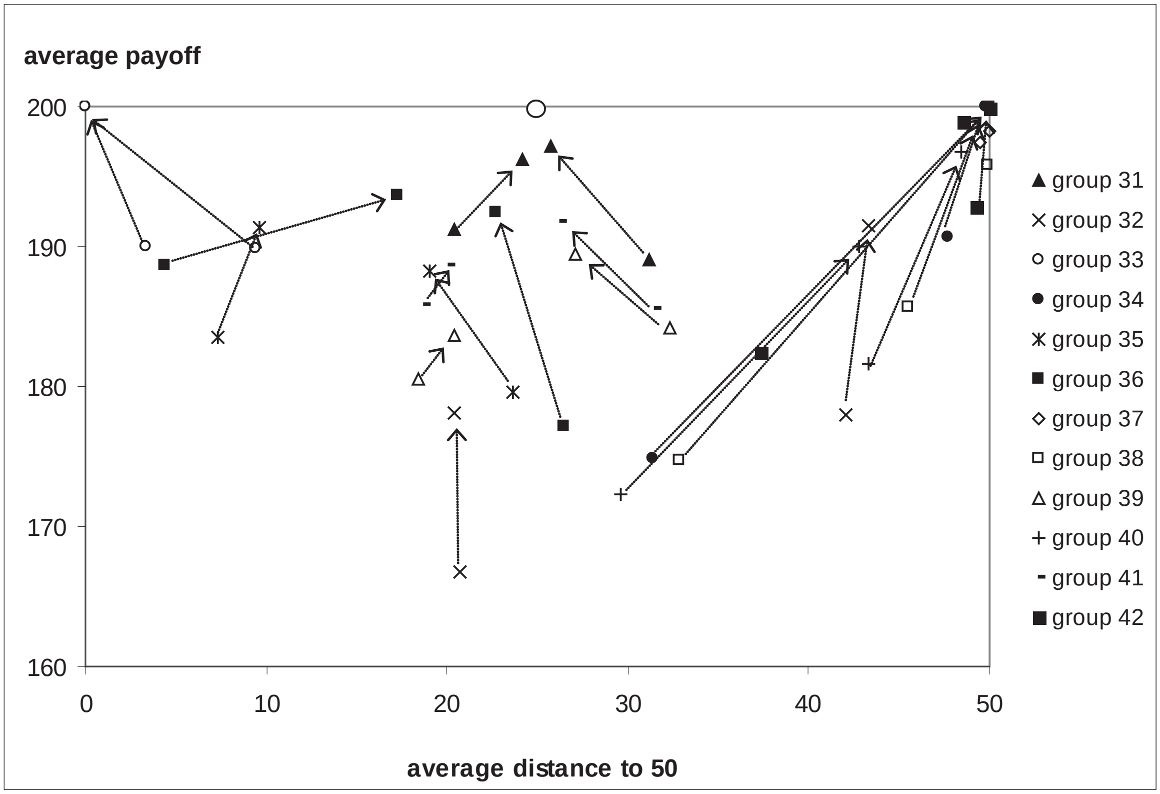

Figure 7: Change of Average Distance and Payoff from First to Second Half of Treatment CP

Source: Authors' calculations.

groups who actually converge to the respective strategy. Convergence time to "50" is short and does not seem to depend on the treatment. Similar convergence times can also be observed for salient sunspot equilibria in Treatments C and CC. But it takes longer to converge to non-equilibrium strategies in Treatments P95 and AC and to the sunspot equilibria in Treatment CP, particularly to the intermediate equilibrium "25/75."

The three convergence patterns in Treatment CP can be nicely observed in Figure 7, which shows how average payoffs and strategies changed from the first half to the second half of the experiment. Each group is represented by four identical markers that indicate the average distance of actions from 50 and the group members' payoffs conditional on receiving equal and unequal signals. The arrows show how distance and payoffs changed over time. With the exception of group 32, the difference between choices in situations with $X_i = Y$ and $X_i \neq Y$ became smaller, which means that subjects responded less to the private signal. The payoffs always increased from the first to the second half of the experiment due to the improved coordination within groups. It is interesting to note that groups starting out with distances above 25 for unequal signals converged to the sunspot equilibrium 0/100. Groups

Table 6: First Period Choices

	Treatments without public signals				Treatments with public signals				
Treatment	N	P75	P95	AC	C	CP $Y \neq X_i$	CP $Y = X_i$	CC $Y \neq X$	CC $Y = X$
# subjects	36	36	36	36	36	36	36	30	6
Distance to 50									
"negative"	n.a.	1	2	3	2	6	0	n.a.	0
0 or 1	22	13	10	14	3	19	4	16	1
2 to 19	8	2	1	0	0	0	2	0	0
20 to 30	4	7	4	1	8	2	1	4	0
31 to 48	1	4	4	2	2	3	5	2	0
49 or 50	1	9	15	16	21	6	24	8	5
Average distance to 50	7.17	23.47***	31.36***	28.47***	39.22***	15.61***	40.0	19.50***	41.67

Note: "negative" indicates the number of first period choices violating the symmetry conditions. These choices are smaller than 50 when the (public) signal is 100 or vice versa. For the non-parametric tests in this table each subject's decision is treated as an independent observation. P75, P95, AC, and C are compared to N, whereas CP and CC are compared to C. Significance levels are based on Mann-Whitney tests (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

with average distances between about 15 and 35 in the first half of the experiment seem to converge towards a 25/75 sunspot equilibrium (indicated by the large circle at the upper edge of Figure 7) during the second half of the experiment.

5.6 Behavior in the First Period

Looking at the initial choices made during the first period is interesting, because we know from previous experiments on coordination games, such as Van Huyck, Battalio and Beil (1990, 1991), that first period behavior largely determines to which equilibrium subjects converge. Table 6 summarizes distances between the subjects' initial choices and the risk-dominant equilibrium, disaggregated by treatments. In Treatment N about 60% of all subjects chose 50 from the spot. Introducing private signals substantially reduced the number of subjects who chose the risk-dominant equilibrium in favor of subjects following their signal. In Treatments P75, P95, and AC, we find that about 35% of subjects were close to the risk-dominant equilibrium and about the same number followed their signals. In treatments with public signals, the effects were reversed: in Treatments C and CP with equal signals, about 60% followed their signals.

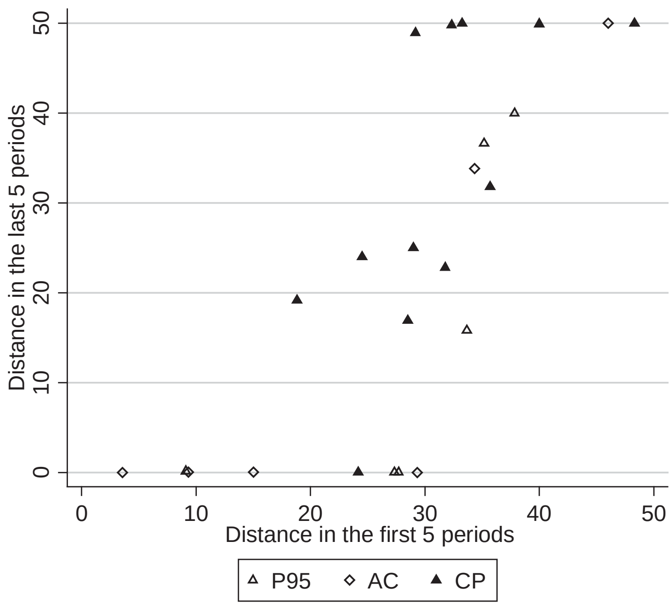

Figure 8: Average Distance to 50 in the First and Last Five Periods

Source: Authors' calculations.

In treatments in which different groups converged to different strategies (AC, P95, and CP), the distance to the risk-dominant equilibrium during the first periods was closely related to the eventual convergence points - despite the fact that convergence in these treatments took more than 30 periods on average. Figure 8 plots the average distance to 50 in the first and last five periods for all groups of these treatments. In Treatments AC and P95, the average distance to 50 over the first five periods in groups converging to the risk-dominant equilibrium is significantly lower than in groups converging to nonequilibrium sunspot behavior (Mann-Whitney test, $p < 0.01$, two-sided).[26]

Moreover, the correlation between the average distance in the first five and the last five periods is highly significant (Spearman's $\rho = 0.72$, $p < 0.01$). A similar pattern holds for Treatment CP. Again, the correlation between the distance in the first and last five periods is highly significant (Spearman's $\rho = 0.84$, $p < 0.01$). In groups converging to the sunspot equilibrium 0/100, the subjects quickly followed the public signal, and we can reject the hypothesis that the average distance from 50 during the first five periods in these groups is the same as in the other groups (Mann-Whitney, $p < 0.03$, two-sided). However, we could not

[26] We take five periods instead of one in order to capture different realizations of signals.

find a clear indication for behavior during the first periods that led groups to converge to the intermediate sunspot equilibrium 25/75.

6 Conclusion

In this paper, we have reported evidence for the occurrence of sunspots in the laboratory. In a simple game, inspired by Keynes's beauty contest, we introduced extrinsic signals and systematically varied the information structure of signals in order to control the available extrinsic information and its effect on behavior. Our findings provide direct evidence that extrinsic information can have a substantial impact on collective perceptions, and therefore show that sunspot equilibria reliably show up.

We investigated the impact of extrinsic information by manipulating the correlation of individual signals and by introducing multiple signals. As long as the signals have a small correlation, i.e., when signals are private and the conditional probability that two subjects receive the same signal is low, subjects tend to ignore these signals. But if the correlation increases, we observe sunspot-driven behavior, even though theoretically no sunspot equilibrium exists.

Two *public* signals are easily aggregated and generate an interesting three-cycle sunspot equilibrium that has not been observed in other sunspot experiments. On the other hand, if a private signal is combined with an extrinsic public signal, the impact of sunspots on behavior is smaller than in games with purely public information. The power of public signals is reduced by the presence of private signals. The ability of groups to coordinate is impeded by such a combination, which results in lower average payoffs. This finding leads to an interesting conclusion: in economies where salient private signals exist, adding an extrinsic public coordination device with similar semantics may make it more difficult to coordinate actions.

Extrinsic public information is not detrimental to welfare. However, the presence of highly correlated private information and the combination of public and private signals considerably impedes coordination and results in lower payoffs. While the individual losses arising from strategies that condition actions on private signals might be small, such behavior affects the strategies of other players and prolongs the time that subjects need to coordinate. If not all agents receive extrinsic signals, those who do condition their actions on these signals exert a negative externality on agents who do not receive signals.

Taking up the literature on focal points, we have provided evidence that the salience of a certain action—choosing 50—is no longer effective when extrinsic information is available, even in (payoff-) symmetric games. The introduction of extrinsic information influences the subjects' perceptions of focal points and may lead to considerable miscoordination. Hence, our results show the fragility of focal points.

The game form that we employed permits the use of risk dominance for measuring the power of sunspots. Whether the power of sunspots may be sufficiently strong to move agents away from a payoff-dominant equilibrium is an open question. Since risk dominance seems to work well in measuring the power of extrinsic signals, we envision that our game form may be used for testing the salience of other messages or signal combinations. Tentatively, one may use similar experiments for measuring the common understanding of messages phrased in ordinary language.

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Appendix

A. Proofs of Lemmas 1 and 2

Lemma 1: *Let* $\{a^{i*}_{\varphi,\psi} | \varphi \in \Phi, \psi \in \Psi\}$ *be a Bayesian Nash equilibrium strategy profile, where* $a^{i*}_{\varphi,\psi}$ *is a Bayesian Nash equilibrium action played by agent i with public signal* φ *and private signal* ψ. *Then, equilibrium actions are the same for both agents and do not depend on the private signal, that is,* $a^{i*}_{\varphi,\psi} = a^{*}_{\varphi} \quad \forall \psi \in \Psi$ *for any given* $\varphi \in \Phi$.

Proof: We will prove the lemma in three steps.

Step 1. We want to show that the equilibrium must be in pure strategies. For any given set of signals, it must be that

$$a^{i*}_{\varphi,\psi} = \arg\max_x \sum_{\psi \in \Psi} p^{\psi^j}_{\varphi,\psi^i} f\left(x - a^{j*}_{\varphi,\psi}\right)$$

where $p^{\psi^j}_{\varphi,\psi^i}$ is the probability that the other player receives signal ψ^j when he receives signal ψ^i in state φ.[27] The expression to be maximized is strictly concave, so the best response must be unique. Hence, it cannot be that in equilibrium they play different actions with positive probability for a same set of signals.

Step 2. Extreme actions played in equilibrium must coincide for both players, that is, $\arg\min_\psi \{a^{i*}_{\varphi,\psi}\} = \arg\min_\psi \{a^{j*}_{\varphi,\psi}\}$ and $\arg\max_\psi \{a^{i*}_{\varphi,\psi}\} = \arg\max_\psi \{a^{j*}_{\varphi,\psi}\}$ for $i \neq j$.[28] We will show that by contradiction. Suppose that, without loss of generality, $\arg\min_\psi \{a^{i*}_{\varphi,\psi}\} < \arg\min_\psi \{a^{j*}_{\varphi,\psi}\}$. Let $\overline{\psi} := \arg\min_\psi \{a^{i*}_{\varphi,\psi}\}$. Then, it must be that

$$\left.\frac{\partial \sum_{\psi \in \Psi} p^{\psi^j}_{\varphi,\overline{\psi}} f\left(x - a^{j*}_{\varphi,\psi}\right)}{\partial x}\right|_{x = a^{i*}_{\varphi,\overline{\psi}}} = \arg\max_x \sum_{\psi \in \Psi} p^{\psi^j}_{\varphi,\overline{\psi}} f'\left(a^{i*}_{\varphi,\overline{\psi}} - a^{j*}_{\varphi,\psi}\right) > 0$$

given that $f'\left(a^{i*}_{\varphi,\overline{\psi}} - a^{j*}_{\varphi,\psi}\right) > 0 \quad \forall a^{j*}_{\varphi,\psi}$. Therefore, $a^{i*}_{\varphi,\overline{\psi}}$ cannot be a best response.

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[27] See that by allowing different probabilities we implicitly allow the effects of a public signal.
[28] These expressions are well defined given the finite cardinality of private signals and Step 1.

Step 3. In this last step, we show that $\arg\min_{\psi}\left\{a_{\varphi,\psi}^{i*}\right\} = \arg\max_{\psi}\left\{a_{\varphi,\psi}^{i*}\right\}$ for both players. We will again prove it by contradiction. Suppose that it is not the case. We know by the previous step that the extremes must be the same. In that case, the derivative of the expected profits at $\overline{\psi}$ is again positive. In that case, the derivative will be zero if both players play the minimum action in equilibrium, and positive for the rest.

Therefore, it must be that $a_{\varphi,\psi}^{i*} = a_{\varphi}^{*} \quad \forall \psi \in \Psi$ for any given $\varphi \in \Phi$. ■

Lemma 2: $a_{\psi,\varphi}^{i*} = \frac{b+c}{2} \forall \psi, \varphi$ *is the both the secure action and the risk-dominant equilibrium.*

Proof. The minimum payoff that can be obtained given action *x* is the payoff given by one of the extremes, that is, *min {f(x-b),f(c-x)}* . It is trivial to see that this function is maximized at x = (b+c)/2, and therefore playing the middle action maximizes the minimum payoff. Hence, the middle point of the interval is the secure action.

In order to find the risk-dominant equilibrium, we must find the action x that maximizes the expected payoff against a player who plays a uniform distribution over all the actions, i.e.,

$$\frac{1}{c-b}\int_b^c f(x-y)dy$$

Suppose, without loss of generality, that $x < \frac{b+c}{2}$.

$$\begin{aligned}
\int_b^c f(x-y)dy &= \int_b^x f(x-y)dy + \int_x^{x+\frac{b+c}{2}} f(x-y)dy + \int_{x+\frac{b+c}{2}}^c f(x-y)dy \\
&\leq \int_b^x f(x-y)dy + \int_x^{x+\frac{b+c}{2}} f(x-y)dy + \int_{x-b}^{\frac{b+c}{2}} f(x-y)dy \\
&= \int_b^c f\left(\frac{b+c}{2}-y\right)dy
\end{aligned}$$

Hence, $\frac{b+c}{2}$ is the risk-dominant equilibrium. ■

B. Symmetry of Actions

In this section we analyze whether actions played during the experiment are symmetric, that is whether a subject choosing $a_i = m$ when she receives signal s = 0 also chooses $a_i = 100 - m$ when receiving signal s = 100. Symmetry allows us to measure the power of sunspots by the distance of chosen actions from 50, independent of whether signals are 0 or 100.

Note that for treatments with two signals, symmetry refers to playing m when both signals are 0 and *100-m* when both signals are 100. When the two signals are different in CP, symmetry means playing n when the public signal is 0 and the private signal is 100, and *100-n* when the public signal is 100 and the private signal is 0. For two distinct public signals in Treatment CC, symmetry prescribes playing 50 as in situations without signals. To test the symmetry of strategies we estimate the following model:

$$a_{it} = \beta_1 S100 + \beta_2 \, period + \alpha_i + u_{it} \qquad \text{(B1)}$$

The dependent variable is the decision of individual i. We transform this variable to $a_i = 100 - m_i$ when the private signal is s = 100 (as in P75, P95, and AC), when the public signal is s = 100 (as in C and CP), or when the public signal Y_1 is s = 100 (as in CC). Thus the dependent variable a_{it} always measures the distance to zero irrespective of the signal realization. As independent variables we include "Period" to control for the time trend and a dummy variable, "S100", which equals 1 if the private signal equals 100 (in P75, P95, or AC) or the public signal equals 100 (in C, CP, or Y_1 in CC). For Treatment AC, we consider only observations in which the random number Z was revealed to the subjects. For Treatments CP and CC we estimate separate regressions for equal signals $(X_i = Y)$ or $(Y_1 = Y_2)$ and unequal signals $(X_i \neq Y)$ or $(Y_1 \neq Y_2)$. For Treatment CC, we also test whether the constant equals 50, which amounts to both public signals having the same impact on behavior.

The regression results are displayed in Table B1. We only report the results of a random effects model as specified in (B1) in which we control for repeated decisions of the same subject as well as for dependencies within matching groups. Alternatively, we used a simple OLS model with clustering at the group level, which does not impose any restriction on the correlation within groups. Our variable of interest is the dummy for the signal "S100". If decisions are symmetric, the coefficient β_i should be close to zero and insignificant. Indeed we observe for treatments P75, P95, C and CC that the coefficient for "S100" is not significantly different from zero. The same is true for treatment CP when the signals are equal. In CP with unequal signals and in AC we find that "S100" is significant at the 5%-level, but numerically small. This is mainly due to one matching group in each of the two treatments. If we exclude these two groups the coefficient for "S100" is insignificant in both regressions. The OLS regressions with clustering at the group level yield insignificant coefficients in all treatments (including all groups).

Table B1: Symmetry of Decisions

	Dependent variable: a_{it}							
	P75	P95	C	CP		CC		AC
				Y=Xi	Y≠Xi	Y=X	Y≠X	
Signal=100 (D)	0.113	-1.086	-0.212	-0.280	-1.878**	0.019	-0.443	0.962**
	(0.517)	(1.099)	(0.511)	(0.449)	(0.939)	(0.485)	(0.447)	(0.471)
Period	0.110***	0.058*	-0.118**	0.043	-0.198***	-0.039	0.018***	0.043
	(0.034)	(0.034)	(0.055)	(0.038)	(0.062)	(0.029)	(0.006)	(0.048)
Constant	43.321***	31.245***	8.726*	13.675***	34.617***	3.662*	49.403***	33.916***
	(1.866)	(6.446)	(4.780)	(2.789)	(3.916)	(2.004)	(0.463)	(7.617)
chi^2	10.95	3.93	6.40	2.86	10.37	2.85	11.00	11.58
R^2	0.07	0.006	0.043	0.003	0.044	0.005	0.002	0.003
N	2880	2880	2880	3456	2304	1728	1152	2662

Notes: Random-effects GLS regression with robust standard errors clustered at group level in parentheses. (D) denotes a dummy variable, the equal signal refers to ($X_i = Y$) or ($Y_1 = Y_2$) and unequal signals refer to ($X_i \neq Y$) or ($Y_1 \neq Y_2$). Treatment AC only includes observations where the random number Z was revealed to subjects. For CC, the constant is not significantly different from 50 (*p=0.20*).
* p<0.10, ** p<0.05, *** p<0.01

C. Additional Tables

Table C1: Aggregate Results of Non-sunspot Treatments

Treatment	Session	Group	Strategy	T_6	T_4	Avg. coord. rate	Avg. payoff (std. dev.)	$\lvert a_i - 50 \rvert$ (std. dev.)
(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)
N	1	1	50	56	14	0.84	198.5 (6.8)	1.44 (5.9)
N		2	50	42	1	0.96	199.3 (5.2)	0.55 (4.2)
N	2	3	50	6	2	0.98	199.6 (3.82)	0.25 (3.01)
N		4	50	29	19	0.81	198.4 (8.29)	1.83 (6.15)
N	3	5	50		1	0.98	199.6 (4.55)	0.26 (3.24)
N		6	50	7	4	0.95	199.7 (2.24)	0.55 (2.69)
P75	4	7	50	10	7	0.93	198.9 (5.5)	1.27 (6.4)
P75		8	50	20	2	0.89	197.1 (11.4)	2.08 (8.6)
P75		9	50	-	25	0.58	195.1 (9.9)	4.88 (10.5)
P75	5	10	50	18	11	0.88	195.2 (20.09)	3.30 (11.16)
P75		11	50	66	14	0.88	193.95 (19.78)	3.33 (12.27)
P75		12	50	29	2	0.96	199.8 (1.87)	0.20 (1.97)
P95	6	13	10/90	-	67	0.51	185.5 (39.4)	39.39 (6.5)
P95		14	10/90	-	63	0.30	181.6 (38.7)	38.60 (12.6)
P95		15	50	13	10	0.90	195.8 (19.3)	2.37 (9.7)
P95	7	16	50	7	3	0.96	199.2 (5.1)	0.66 (4.5)
P95		17	50	12	7	0.92	196.5 (12.3)	2.44 (10.5)
P95		18	50	-	80	0.31	172.7 (33.5)	20.07 (22.4)
AC	8	19	50	32	2	0.96	198.5 (8.5)	0.79 (6.1)
AC		20	50	7	2	0.96	198.3 (9.0)	1.04 (7.1)
AC		21	50	16	1	0.97	199.5 (4.0)	0.38 (3.5)
AC	9	22	0/100	70	1	0.80	188.9 (27.4)	44.94 (14.8)
AC		23	50	19	7	0.90	195.5 (14.1)	3.71 (12.9)
AC		24	0/100	-	70	0.25	163.6 (48.7)	32.31 (21.8)

Notes: T_4 denotes the earliest period from which at least 4 subjects play the same strategy until the last but one period, allowing a deviation of ±3. T_6 denotes the earliest period from which all 6 subjects play the same strategy until the last but one period, allowing a deviation of ±1. The avg. coordination rate is the percentage of pairs choosing the same action within a range of ±1 over all periods.

Table C2: Aggregate Results of Sunspots Treatments

Treatment	Session	Group	Strategy	T_6	T_4	Avg. coord. rate	Avg. payoff (std. dev.)	$\lvert a_i - 50 \rvert$ (std. dev.)
(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)
C		25	0/100	80	10	0.74	196.3 (20.0)	48.31 (5.8)
C	10	26	0/100	6	3	0.97	199.9 (1.01)	49.60 (2.38)
C		27	0/100	33	6	0.93	197.8 (15.8)	48.76 (6.0)
C		28	0/100	2	1	0.99	199.8 (3.2)	49.88 (2.3)
C	11	29	0/100	80	55	0.44	182.6 (25.3)	33.99 (21.2)
C		30	0/100	49	3	0.94	196.0 (26.1)	49.53 (2.8)
CP		31	25/75	65	59	0.36	193.3 (11.4)	26.00 (11.8)
CP	12	32	-	-	-	0.15	179.9 (32.7)	33.83 (17.8)
CP		33	50	24	8	0.90	194.9 (15.0)	3.49 (12.7)
CP		34	0/100	54	28	0.72	192.2 (23.1)	45.49 (12.2)
CP	13	35	-	-	-	0.40	185.3 (19.3)	16.19 (19.0)
CP		36	25/75	-	55	0.41	187.3 (16.9)	19.09 (17.3)
CP		37	0/100	33	1	0.99	199.0 (13.3)	49.90 (2.3)
CP	14	38	0/100	65	29	0.72	189.8 (29.6)	45.19 (13.0)
CP		39	25/75	-	76	0.34	184.9 (25.0)	25.58 (13.6)
CP		40	0/100	77	22	0.63	186.0 (29.4)	41.98 (16.2)
CP	15	41	-	-	-	0.10	188.1 (14.5)	25.09 (16.6)
CP		42	0/100	56	9	0.84	194.1 (21.8)	46.95 (10.8)
CC		43	Mean	79	16	0.83	195.3 (14.3)	33.32 (23.0)
CC	16	44	Mean	-	3	0.66	184.6 (35.4)	28.65 (23.5)
CC		45	Mean	20	12	0.91	190.3 (38.5)	30.88 (24.2)
CC		46	Mean	58	4	0.96	198.5 (14.0)	30.08 (24.4)
CC	17	47	Mean	4	1	0.99	199.6 (4.6)	30.00 (24.5)
CC		48	Mean	2	1	0.99	199.9 (1.2)	30.06 (24.5)

Notes: T_4 denotes the earliest period from which at least 4 subjects play the same strategy until the last but one period, allowing a deviation of ±3. T_6 denotes the earliest period from which all 6 subjects play the same strategy until the last but one period, allowing a deviation of ±1. The avg. coordination rate is the percentage of pairs choosing the same action within a range of ±1 over all periods.

D. Sample Instructions for Treatment CP

The experiment in which you are participating is part of a research project. Its aim is to analyze economic decision behavior.

The experiment consists of 80 periods in total. The rules and instructions are the same for all participants. In each period, you have to make a decision. All periods are completely independent. Your income from the experiment depends on your decisions and the decisions made by the other participants. Please read all instructions carefully and thoroughly.

Please note that you are not permitted to speak to the other participants or to exchange information with them for the duration of the entire experiment. Should you have a question, please raise your hand, and we will come to you and answer your question. Please do not ask your question(s) in a loud voice. Should you breach these rules, we will be forced to exclude you from the experiment.

At the end of the experiment, the computer will randomly draw 10 of the 80 periods, which will become relevant for your payoff. Your payoff will then be determined according to the sum of your earnings from these selected periods. In addition, you will receive 3 Euro for participating in the experiment.

Description of the Experiment

At the beginning of the experiment, three groups of six participants each are randomly and anonymously formed. These groups remain unaltered for the entire experiment. At no point are you told who is in your group.

In each period, you are randomly and anonymously paired with another participant from your group (referred to as your partner from now on). This means that you can be paired with the same participant from your group several times in the course of the experiment, albeit not in two successive periods. Neither you nor your partner is told the other's identity.

1. Information at the Beginning of Each Period

At the beginning of each period, the computer randomly draws a number ***Z***. The number ***Z*** is equally likely either to have the value **0** or **100**. This means that in 5 out of 10 cases, on average, the number ***Z*** takes the value 0, and in 5 out of 10 cases, it takes the value 100. The number ***Z*** is the same for you and your partner.

At the time of the decision, the number ***Z*** is not known. Instead, you receive two independent hints for the number ***Z***:

Shared hint *Y*:

You and your partner both receive a shared hint ***Y*** for the number ***Z***. This hint can be either 0 or 100 and is randomly determined. With a probability of 75%, hint ***Y*** has the same value as the number ***Z***. With the remaining probability of 25%, the hint will have the other value. The shared hint is the same for both of you.

Private hint *X*:

In addition to the shared hint ***Y***, you will receive a private hint ***X*** for the number ***Z***. Your partner also receives a private hint ***X***.

The private hint can be either 0 or 100 and is randomly determined. With a probability of 75%, the private hint ***X*** has the same value as the number ***Z***. With the remaining probability of 25%, the private hint ***X*** will have the other value.

Your private hint and the private hint of your partner are independently drawn, i.e., both private hints can be different. You are not told which private hint your partner has received, and your partner is not told which private hint you have received.

If the shared and the private hint are the same, the probability of both being correct is 90 percent. In other words, if you have received two similar hints, then in 9 out of 10 cases these correspond to the number ***Z***.

If the shared and the private hint are different, then both values of the number ***Z*** are equally probable.

2. Your Decision

In each period, you have to decide on a number between 0 and 100 (incl. 0 and 100). Once you have made your decision, you have to click on the OK button on the corresponding computer screen. Once all participants have made their binding decisions, a period is finished.

3. Your Earnings

Your earnings depend on how close your decision has come to your partner's decision.

$$\text{Your earnings (in Euro cents)} = 200 - \frac{2}{100}\left(\textit{Your decision} - \textit{Your partner's decision}\right)^2.$$

In other words: your earnings in each period are 200 Euro cents at the most. These 200 Euro cents are reduced by the distance between your decision and your partner's decision.

The distance is squared, so that higher distance leads to a disproportionate loss compared to a smaller distance. The closer your decision is to your partner's decision, the higher your earnings are.

The following table gives you an overview of possible earnings. In this table, only distances in steps of 20 are shown. Please note that distances may be any integer between 0 and 100. In the table, you can also see that you are able to earn a maximum of 200 Euro cents (top-left field) and a minimum of 0 Euro cents (bottom-right field).

Distance from your partner's decision	Earnings
0	200
20	192
40	168
60	128
80	72
100	0

Calculator

You have a calculator at your disposal in each period. In order to use the calculator, you can note your own decision and test as many of your partner's decisions as you wish. The calculator then calculates your earnings for the relevant data entered. In the first 5 periods, the calculator is active for 20 seconds. During this time, you may carry out as many calculations as you wish. After that, the calculator becomes inactive and you must make your decision. From the 6th period onwards, the calculator is only active for 10 seconds and you can make your decision at once.

Information at the End of a Period

At the end of a period, you are given the following information:

- **The number *Z***
- **The shared hint *Y***
- **Your private hint *X***
- **Your decision**
- **Your partner's decision**
- **The discrepancy between your decision and your partner's decision**
- **Your earnings**

Control Questions

1. Is everyone given the same hint X?
   - Yes, everyone is given the same hint X /
   - No, everyone receives his own hint, i.e., your hint X can be different from your partner's hint X.
2. Is everyone given the same hint Y?
   - Yes, everyone is given the same hint Y
   - No, everyone receives his own hint Y, i.e., your hint Y can be different from your partner's hint Y.
3. Your earnings in a period depend on ...
   …the distance between your chosen number and your partner's chosen number
   …the number Z
   …the private hint X
4. Are you always paired with the same partner? *Yes / No*
5. How many of the 80 periods are randomly chosen by the computer in order to determine your earnings?