Bandits in the Lab[*]

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Abstract

We experimentally implement a dynamic public-good problem, where the public good in question is the dynamically evolving information about agents' common state of the world. Subjects' behavior is consistent with free-riding because of strategic concerns. We also find that subjects adopt more complex behaviors than predicted by the welfare-optimal equilibrium, such as non-cut-off behavior, lonely pioneers and frequent switches of action.

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1 Introduction

Economists have long been concerned with social dilemmas related to the production of public goods. There exists a vast experimental literature on games which examines the willingness to contribute to public goods (for surveys, see Ledyard 1995 and Chaudhuri 2011; and for a meta-analysis, see Zelmer 2003). In these environments, payoff-maximizers' dominant strategy is to contribute none of their endowment to a group activity. The typical environment is such that it creates a social dilemma, leading to zero contribution to the group activity, while, in the efficient outcome, each player contributes his entire endowment. For many decades, economists have attempted to experimentally test this trade-off and to analyze factors that facilitate increased cooperation in such social-dilemma situations in the lab (e.g., Fehr and Gächter 2000; Ambrus and Greiner 2012; and Fenig, Gallipoli, and Halevy 2018).[1]

Situations in which the public good is dynamically evolving, however, have received scant attention from experimental economists so far. This is the case, for instance, when the public good in question is information. By contrast, purely informational social dilemmas are the object of the so-called strategic multi-armed bandit problems, which have received a lot of attention in the recent theoretical literature. By abstracting from payoff externalities, these settings reduce players' scope for intertemporal incentives, as punishments and rewards can only be informational in nature. In this paper, we offer one of the first experimental investigations of the provision of a dynamically evolving public good,[2] and, to the best of our knowledge, the first to implement an experimental investigation of a dynamic social dilemma where externalities are purely informational.

The trade-offs involved in the production of public information are important to understand. Indeed, innovation and social learning are often the work of pioneers, who, by bearing the costs of experimenting with a new approach, create informational spill-overs for others. Whether we consider R&D, resource exploration, or the testing of a new drug, the information produced by a relatively small set of agents benefits a much larger group of agents. R&D is universally recognized as an important factor of economic growth (Romer 1990; Grossman and Helpman 1993). An economy's productivity level depends on innovation, which is driven by knowledge emerging from cumulative R&D experience as well as an economy's overall knowledge stock (Griliches 1988; Coe and Helpman 1995). Situations in which the informational benefits of experimentation are shared abound: for example, the decision of where to fish when others can see one's boat and haul,[3] consumers searching for the right car or cell-phone to buy, farmers deciding whether to grow a traditional or a gene-modified crop, graduate students selecting their field of research, etc.

In the multi-armed bandit models, which have become canonical to study information producers' dynamic trade-offs, a decision maker, at each point in time, either optimally exploits the information he already has, or he decides to invest in exploration in order to

[1]For early experimental studies, see Kim and Walker (1984), Isaac, Walker, and Thomas (1984), Isaac, McCue, and Plott (1985), Isaac and Walker (1988a,b), and Andreoni (1988). For early studies embedded in the sociology literature, see Marwell and Ames (1979, 1980, 1981).

[2]Battaglini, Nunnari, and Palfrey (2016) is the only other paper we are aware of in this category.

[3]We are indebted to an anonymous referee for this example.

make better future decisions. Until fairly recently, the literature focussed on the trade-off of an individual decision maker acting in isolation. Bolton and Harris (1999) and Keller, Rady, and Cripps (2005) (subsequently: KRC) have extended the individual choice problem to a multi-player continuous-time framework. The simpler exponential model of KRC especially has since been used to analyze a wide array of applications, such as, for instance, R&D races.[4]

Thus, the object of this paper is to experimentally examine a novel social dilemma over the exploration of information with common value. In order to do so, we base ourselves on KRC's exponential model. We chose this particular model for two reasons. Firstly, its setup is simpler than that of the other strategic-experimentation papers, and, secondly, as Hörner, Klein, and Rady (2020) (subsequently: HKR) have shown, the welfare-optimal equilibrium[5] has a particularly simple structure in this model. Indeed, while it is non-Markovian, it is strongly symmetric and players play a cut-off strategy[6] (on the path of play), applying the same cut-off as a single agent. Given the simple structure of the best equilibrium, it can reasonably be expected to be focal among the continuum of equilibria that exist in the model. Moreover, it gives us a very clean empirical test: if they (want to) play the equilibrium, subjects should behave in the same way as when they solve the single-agent problem.

To make the problem tractable, the strategic-experimentation literature is by and large focussing on the choice between a safe arm, yielding a known payoff, and a risky arm, which yields payoffs following a stochastic process. The time-invariant quality of this risky arm can be good or bad. If it is good (bad), it dominates (is dominated by) the safe arm. Whether the risky arm is good or bad is initially unknown and can only be found out by trying it out over time. Trying it out is costly, however, as it means forgoing the safe payoff. As the quality of the risky arm is assumed to be the same across players, and players can observe each other's actions and payoffs, there is a positive informational externality associated with a player's use of the risky arm. This gives rise to a dynamic public-good problem in the form of dynamically evolving information about the agents' common state of the world.

As hinted at above, our analysis relies on comparing the behavior of our experimental subjects in groups where the quality of the risky arm was known to be the same for all partners (which we call the *strategic treatment*) to that of groups where its quality was iid across members, the *control treatment*. When the quality of the risky arm is known to be

[4]See e.g. Besanko and Wu (2013), Akcigit and Liu (2016) or Das and Klein (2020). Besanko, Tong, and Wu (2018) uses the exponential bandits framework to analyze optimal subsidies for R&D.

[5]We are committing a slight abuse of terminology here by referring to the "welfare-optimal," "average-payoff maximizing" or "best" equilibrium, which we shall maintain throughout this paper. HKR in fact analyze a setting in continuous time in which actions are frozen for small intervals of time of length $\Delta > 0$. They show that, for small enough Δ, there exists a perfect Bayesian equilibrium (PBE), which happens to be strongly symmetric, with payoffs that, as $\Delta \to 0$, converge to the payoff from all players playing risky above the single-agent threshold and safe below it, and that it is not possible to achieve higher limit average PBE payoffs. Our experimental implementation is, of course, strictly speaking, in discrete time, with information and action choices being updated every second.

[6]A cut-off strategy is defined by a unique threshold belief above which it prescribes risky play, while prescribing safe play below it.

the same across players, rational agents will take into account the result of their partners' experimentation when updating their beliefs. As they can learn from what others are doing, they have an incentive to induce others to behave in certain ways so they may learn from it. There is thus some strategic interaction across players, even though a player's payoffs depend only on his own action and the common state of the world, i.e., there are no payoff externalities.

In a first step, we show that the informational externality impacts subjects' behavior. Average experimentation intensities are lower in the strategic treatment, and, in particular, in the belief region for which theory predicts free-riding to be an issue, subjects experiment significantly less in the strategic treatment, suggesting they are free-riding because of strategic concerns.[7] Moreover, subjects' payoffs are higher in the strategic treatment, suggesting that they are taking advantage of the information produced by their partners. Further, subjects are adopting more sophisticated behaviors in the strategic treatment than in the control treatment. Players switch much more between safe and risky, and use cut-off strategies much less frequently, in the strategic treatment. Additionally, there is a larger proportion of time during which exactly one player is playing risky in the strategic treatment.[8] If subjects were playing, or aiming to play, the best PBE, we should observe none of these differences. In this case, the only difference between the two treatments should consist in the increased speed of learning in the strategic treatment, with subjects' behavior otherwise the same across treatments.

We interpret our findings as showing that, while understanding the informational externality (since they achieved higher payoffs in the strategic treatment), subjects were *not* behaving, or aiming to behave, as in the best PBE. Rather, the behaviors we document are consistent with the qualitative predictions of *any* of KRC's (infinitely many) Markov perfect equilibria (MPE), which feature players' taking turns and alternating in the roles of free-riders and pioneers for some intermediate range of beliefs. Further, the differences between the two treatments tend to be more pronounced for two-player groups than for groups of size three.

Our game is of course very complicated, so that we cannot reasonably expect subjects to be able to compute equilibrium strategies. Yet, subjects' experimentation efforts are clearly decreasing with the incremental arrival of bad news in the form of unsuccessful previous experimentation. This would suggest that, even though subjects could of course not be expected continually to update their beliefs using Bayes' rule precisely at lightning speed, they were nonetheless reacting to the dynamically evolving incentives. Furthermore, we are documenting behavior that is very different from the simple structure of the best PBE, and arguably more in line with the sophisticated coordination required by MPE play.

The rest of the paper is organized as follows: Section 2 reviews some additional related literature; Section 3 explains the KRC model in more detail; Section 4 sets out our

[7] *Free-riding* in our setting refers to a subject opportunistically using the safe arm while efficiency would require the use of the risky arm. Players have no incentives to do so at very optimistic beliefs, where risky is a dominant action. Subsequently, we shall therefore use the phrase only with respect to those belief regions where there is a strategic rationale for players to deviate from efficient behavior by playing safe.

[8] We refer to such players as *pioneers*.

experimental implementation; Section 5 presents our main findings; Section 6 provides additional results and robustness checks; and Section 7 concludes. Appendix A breaks down the analysis to the individual games subjects played and Appendix B exhibits and explains the interface our experimental subjects were using. Appendix C reproduces the instructions the subjects received.

2 Literature Review

The bandit problem as a stylized formalization of the trade-off between exploration and exploitation goes back to Thompson (1933) and Robbins (1952). It was subsequently analyzed, amongst others, by Bellman (1956) and Bradt, Johnson, and Karlin (1956). Its first application to economics was in Rothschild (1974), who analyzed the price-setting problem of a firm facing an unknown demand function. Gittins and Jones (1974) showed that, if arms are stochastically independent of each other and the state of only one arm can evolve at any one time, an optimal policy in the multi-armed bandit problem is given by the so-called "Gittins Index" policy. For this policy, one can consider the problem of stopping on each arm in isolation from the other arms. The value of this stopping problem is the so-called *Gittins Index* for this arm. Now, an optimal policy consists of, at each point in time, using the arm with the highest Gittins Index. Presman (1990) calculated the Gittins Index for the case in which the underlying stochastic process is a Poisson process. Bergemann and Välimäki (2008) give a survey of this literature.

Bolton and Harris (1999, 2000) were the first to consider the multi-player version of the two-armed bandit problem. While they assumed that the underlying stochastic process was a Brownian motion, KRC analyzed the corresponding problem with exponential processes. This model proved to be more tractable and is underlying our theoretical hypotheses. While the previous papers focussed on MPE, HKR extended the equilibrium concept beyond Markov perfect equilibrium.[9]

We are aware of only one other experimental investigation of a strategic-experimentation problem with bandits, by Boyce, Bruner, and McKee (2016). Their setting is specifically designed to test for strategic free-riding in a two-player, two-period context. Coordination issues are assumed away in that one player was known to have lower opportunity costs for playing risky than the other, so that it was clear which player ought to play the role of pioneer (and that of free-rider respectively) in the first period. Moreover, in Boyce, Bruner, and McKee (2016)'s experiment, subjects faced ambiguity concerning the type of the risky arm. Indeed, they were not told a prior probability of the risky arm's type, which allows for an explanation of subjects' behavior that relies on their priors and ambiguity attitudes. Our investigation, by contrast, is focussed on how players resolve the coordination

[9] Many variants of the multi-player bandit problem have been analyzed since. In Keller and Rady (2010), a bad risky arm also sometimes yields a payoff. In Klein and Rady (2011), the quality of the risky arm is negatively correlated across players. Klein (2013) introduces a second risky arm, with a quality that is negatively correlated with that of the first. In Keller and Rady (2015), the lump-sum payoffs are costs to be minimized. Rosenberg, Solan, and Vieille (2007) and Murto and Välimäki (2011) analyze the case of privately observed payoffs, while Bonatti and Hörner (2011) investigate the case of privately observed actions. Bergemann and Välimäki (1996, 2000) consider strategic experimentation in buyer-seller settings. Hörner and Skrzypacz (2016) give a survey of this literature.

problems arising from strategic interaction. Indeed, our subjects all face the same decision problem and are given a Bayesian prior at the outset. As they interact many times with a stochastic and unknown deadline, their action spaces are very rich.

The only other papers we are aware of that conduct experimental tests of bandit problems consider exclusively various single-agent problems without strategic interdependencies among experimental subjects. Banks, Olson, and Porter (1997) experimentally implement bandits with simple win-lose (Bernoulli) payout distributions, and test whether subjects value information gained through experimentation. In their experimental design, the expected payoff of one arm is known, while the other is unknown. Experimentation is observed more in one treatment where initial selection of the unknown arm is optimal compared to the treatment where experimentation is suboptimal. These results suggest that subjects' behavior is consistent with the normative predictions and that subjects value the information gained through costly experimentation.

A couple of papers by Meyer and Shi (1995) and Gans, Knox, and Croson (2007) employ a different experimental approach, aiming at identifying choice patterns that are consistent with a list of simple decision rules. Meyer and Shi (1995) test decision-making under ambiguity and use experimental data to generate hypotheses about subjects' possible heuristics. While observed choice behavior indicates Bayesian updating of priors, their experimental subjects also exhibit a strong bias toward myopic choices. Among all decision rules considered, the simple stick-with-a-winner strategy fits the data best. Gans, Knox, and Croson (2007) consider a list of simple discrete-choice models in a two-armed bandit set-up. The optimal choice model could not explain their experimental data well. To predict choice behavior, simpler heuristic models are proposed. Indeed, backward-looking strategies which predict switching arms after a fixed number of consecutive failures best explain the observed choices.

Anderson (2001, 2012) uses arms with payout distributions, e.g., simulated dice rolls and normally distributed rewards. He finds that subjects experiment less than would be optimal, and are willing to pay more for getting perfect information than theory would predict. In this set-up ambiguity aversion along with diffuse priors is identified as a driver of the observed behavior in the laboratory.

Oprea, Charness, and Friedman (2014) study experimentally a standard pubic-goods game with a rich communication protocol in both discrete and continuous time. They find that voluntary provision of the public good is higher in continuous than in discrete time. This, however, is only the case if subjects have the possibility to communicate freely to coordinate their contributions.

The only other paper we are aware of that studies a dynamic social dilemma in the laboratory is Battaglini, Nunnari, and Palfrey (2016), which investigates a game of dynamic contributions to a *durable* public good in the laboratory; i.e., the stock of the public good builds up over time. Theirs is thus a setting of conventional payoff externalities, while, in our setting, externalities are purely informational in nature; i.e., the presence of the other players impacts a given player only via the information they produce over time. Battaglini, Nunnari, and Palfrey (2016) find that subjects' qualitative behavior is by and large consistent with the predictions of the Markov Perfect Equilibria that were characterized in Battaglini, Nunnari, and Palfrey (2014), although they find some evidence of non-Markovian history dependencies.

Chernulich, Horowitz, Rabanal, Rud, and Sharifova (2020) study an individual decision problem of market entry and exit decisions under varying informational uncertainty in an environment where the trade-off is similar to a two-armed bandit problem with a safe arm and a risky arm. Hudja (2018) experimentally implements Strulovici (2010)'s collective experimentation model. An individual experimentation problem is compared to a collective experimentation problem where groups of three players face a majority-vote. Fudenberg and Vespa (2019) analyze a signaling-game experiment and focus on the effect of how types are assigned. A bandit problem of their signaling game is employed as a robustness test in which subjects play against a computer.

The prevalence of MPE-type behavior in dynamic games is investigated by Vespa and Wilson (2015, 2019) in the context of a prisoners' dilemma where payoffs depend on a binary state. Their game is set up in such a way that higher efficiency can be achieved by symmetric non-Markovian play, while full efficiency can be achieved by asymmetric SPE. They show that a substantial fraction of subjects behaved in a Markovian fashion. Those who did not tended to aim for higher efficiency. As the complexity of the coordination required to achieve more efficient outcomes than the best MPE increased, the prevalence of MPE play increased. Their findings thus suggest that one of the main draws of Markovian behavior is the simplicity of the coordination required. In our setting, by contrast, MPEs require more complex coordination than the best PBE.

On the other hand, MPEs feature turn-taking in our setting, while the best PBE does not. Cason, Lau, and Mui (2013) study the emergence of turn-taking in the dynamic assignment game and highlight the importance of being able to teach dynamic strategies to others as well as the importance of using strategies that allow for teaching and learning. Leo (2017) studies both theoretically and experimentally flexible turn-taking. He shows that turn-taking leads to substantial efficiency gains and efficiency achieved by subjects is close to that expected in theory. Nevertheless, robust anomalies in subject behavior, which cannot be attributed to pro-social behavior or strategic concerns, are prevalent in his experimental implementation.

3 The Theoretical Framework

We borrow our theoretical reference framework from KRC. There are $n \geq 1$ players, each of whom plays a bandit machine with two arms over an infinite horizon. One of the arms is safe, and yields a known flow payoff of $s > 0$ whenever it is pulled. The other arm is risky and can be either good or bad. If it is bad, it never yields any payoff. If it is good, it yields a lump sum of $h > 0$ at the jumping times of a Poisson process with parameter $\lambda > 0$. It is assumed that $g := \lambda h > s$. Players decide in continuous time which arm to pull. Payoffs are discounted at a rate $r > 0$. If they knew the quality of the risky arm, players would have a strictly dominant strategy always to pull a good risky arm and never to pull a bad one. They are initially uncertain whether their risky arm is good or bad. Yet, the only way to acquire information about the quality of the risky arm is to use it, which is costly as it implies forgoing the safe payoff flow s. The n players' risky arms are either all good or all bad. Players share a common prior belief $p_0 \in (0,1)$ that their risky arms are good. Every player's actions as well as the outcomes of their actions are publicly observable; therefore, the information one player produces benefits the other players as well, creating incentives for players to free-ride on their partners' efforts. Players thus share a common posterior belief p_t at all times $t \in \mathbb{R}_+$. All the parameter values and the structure of the game are common knowledge.

The common posterior beliefs are derived from the public information via Bayes' rule. As a bad risky arm never yields any payoff, the first arrival of a lump sum fully reveals the quality of *all* players' risky arms. Thus, if a success on one of the players' risky arms is observed at instant $\tau \geq 0$, the common posterior belief satisfies $p_t = 1$ for all $t > \tau$. If no success has been observed until instant t, the common posterior belief satisfies

$$p_t = \frac{p_0 e^{-\lambda \int_0^t \sum_{i=1}^N k_{i,\tau}\, d\tau}}{p_0 e^{-\lambda \int_0^t \sum_{i=1}^N k_{i,\tau}\, d\tau} + 1 - p_0},$$

where $k_{i,\tau} = 1$ if player i uses the risky arm at instant τ and $k_{i,\tau} = 0$ otherwise.

KRC show in their Proposition 3.1 that, if players are maximizing the sum of their payoffs, all players $i \in \{1, \cdots, n\}$ choose $k_{i,t} = 1$ if $p_t > p_n^* := \frac{rs}{(r+n\lambda)(g-s)+rs}$, and $k_{i,t} = 0$ otherwise. Note that p_n^* is strictly decreasing in the number of players n. In particular, in the single-agent case ($n = 1$), the decision maker optimally sets $k_{1,t} = 1$ if $p_t > p_1^* := \frac{rs}{(r+\lambda)(g-s)+rs}$, and $k_{1,t} = 0$ otherwise.

KRC go on to analyze the game of strategic information acquisition, where each player maximizes his own payoff, not taking into account that the information he produces is valuable to the other players as well. They analyze perfect Bayesian equilibria in Markov strategies (MPE), i.e., strategies $k_i : [0,1] \to \{0,1\}$, $p \mapsto k_i(p)$, where a player's action after any history can be written as a time-invariant function of the common belief at that history.[10] Thus, the action of a player playing a Markov strategy depends on the previous history only via the current belief. It is shown that, for beliefs close to 1 (0), playing risky (safe) is a dominant action; for intermediate beliefs, players' effort levels are strategic substitutes. In any MPE with a finite number of switches, all players will set $k_i(p) = 0$ for all $p \leq p_1^*$ (see Subsection 6.1 in KRC). Moreover, it is shown that there exists no MPE in which all players play a cut-off strategy, i.e., a strategy that prescribes the use of the risky arm for beliefs above a single cut-off and that of the safe arm below. Thus, the roles of pioneer and free-rider must switch at least once in Markov equilibrium.[11] HKR extend the

[10]In KRC, Markov strategies are actually defined as functions $k_i : [0,1] \to [0,1]$. In order to make the decision problem easier for our subjects, we have restricted the action space to $\{0,1\}$ rather than $[0,1]$. Of course, all equilibria in the game with the larger action space that only use actions in $\{0,1\}$ (called "simple equilibria") remain equilibria in the game with the smaller action space. We, however, lose KRC's (unique) symmetric MPE, which involves interior action choices on some open subinterval of beliefs.

[11]The intuition for this result is best described in the context of a two-player game. Indeed, suppose to the contrary that there existed an equilibrium in cut-off strategies. As there is a region of beliefs in which safe and risky are mutually best responses, both players cannot use the same cut-off in equilibrium; i.e., one player

analysis to non-Markovian PBE. They show that on the path of play in the average-payoff maximizing PBE, all players set $k_i(p) = 1$ for all $p > p_1^*$, and $k_i(p) = 0$ otherwise.

4 Parametrization and Experimental Design

4.1 Experimental Implementation

In our experimental treatments, the number of players will be $n = 2$ or $n = 3$.We choose the discount rate $r = \frac{1}{120}$. To implement the infinite-horizon game in the laboratory, we end the game at the first jump time of a Poisson process with parameter r.[12] With one unit of time corresponding to a second in our experimental implementation, games thus last 120 seconds in expectation. Ours being a rather complicated game that places high demands on subjects' concentration, our goal was to limit the duration of the game, while at the same time allowing for the collection of a wealth of data. We set the probability that the risky arm is good $p_0 = \frac{1}{2}$, the safe payoff $s = 10$, the lump-sum amount paid out by a good risky arm $h = 2500$, and the arrival rate of lump sums on the good risky arm $\lambda = \frac{1}{100}$. Thus, $25 = g > s = 10$. With this parametrization, the game starts in the belief region where risky is a dominant action; if no breakthrough arrives, play then moves into the belief region where safe and risky are Markovian mutually best responses, before entering the region where safe is dominant. The realizations of all random processes were simulated ahead of time.[13] We generated six different sets of realizations of the random parameters controlling the length of the game, the quality of the risky arm, and the arrivals of the good risky arm. These corresponded to six different games that each of our subjects played. To make our findings more easily comparable, we have kept the same realizations for both the strategic and the control treatments.[14] Participants' interfaces (see Appendix B) were updated every second.[15]

Subjects were randomly assigned to groups of $n = 2$ or $n = 3$ players. We used a between-subject design: Each group was randomly assigned either to a control treatment

plays the role of pioneer, while the other one free-rides, throughout the belief region where safe and risky are mutually best responses. As he gets all his information for free in the relevant belief region, the free-rider's payoff function will be higher than the pioneer's. As a player's propensity to play risky is increasing in his own payoff, however, this would imply that the free-rider entered the region in which risky is dominant at a more pessimistic belief than the pioneer.

[12]Subjects knew that the end time of the game corresponded to the first jumping time of a Poisson process with parameter r but did not know the realization of this process at any time before the game ended. In particular, the time axis they saw on their computer screens gradually grew longer as time progressed, so that they could not infer the end date. Please see Appendices B and C for details and for the instructions the subjects received.

[13]As all our stochastic processes are Lévy processes, simulating their realizations ahead of time is equivalent to simulating them as the game progresses. In order to increase the computational efficiency of the implementation, we chose to simulate them ahead of time.

[14]Details are available from the authors upon request.

[15]Thus, our setting approaches the "Inertial Continuous-Time" setting in Calford and Oprea (2017). We were able to optimize at the margin so as to decrease the lag times in zTree to be less than 250 milliseconds, i.e., shorter than human reaction time, by constraining the maximum number of switches, game length etc. allowed. These constraints were unproblematic in all of the sessions.

or to a strategic treatment, and played the six games in random order. To ensure a balanced data-collection process, we replicated any order of the six games that was used for k ($k \in \{1, \cdots, 10\}$) groups in the strategic treatment for k groups in the control treatment as well. Subjects could see their fellow group members' action choices and payoffs on their computer screens. They had to choose an action before the game started and could switch their action at any point in time by clicking on the corresponding button with their mouse.[16]

All experimental sessions took place in July and August 2017 at the BizLab Experimental Research Laboratory at UNSW Sydney. All subjects were recruited from the university's subject pool and administered by the online recruitment system ORSEE (Greiner 2015). All participants were native speakers of English. In total, 100 subjects, 46 of whom were female, participated in 40 sessions. The participants' age ranged from 18 to 35 years, with an average of 20.78 and a standard deviation of 2.43. Because the implementation was programatically very intensive and because we wanted to collect eye-tracking data, only between 2 and 3 subjects participated at a time in each session. Upon arrival, participants were seated in front of a computer at desks which were separated by dividers to minimize potential communication. Participants received written instructions and had the opportunity to ask questions.[17] After the subjects had successfully completed a simple comprehension test, the eye-tracking devices were calibrated, after which the subjects started the experiment. The experiment was programmed in zTree (Fischbacher 2007). At the end of the experiment, we collected some information on participants' demographic attributes and risk attitudes.[18] They were then privately paid their cumulated experimental earnings from one randomly selected game in cash (with a conversion rate of E$ 100 = AU$ 1) plus a show-up fee of AU$ 5. No subject was allowed to participate in more than one session. The average session lasted about 50 minutes, with average earnings of AU$ 23.86 (with a standard deviation of AU$ 9.95).

4.2 Behavioral Hypotheses

The best PBE (as well as all MPEs with a finite number of switches) predict players to play safe at all beliefs $p \leq p_1^*$, while efficiency would require that they play risky at all beliefs $p > p_n^*$, where $p_n^* < p_1^*$. Single players and players playing the best PBE should play risky at all beliefs $p > p_1^*$, i.e., in the average-payoff maximizing PBE, players on path adopt the same cut-off behavior as a single agent. In any MPE, by contrast, since at least one player is not playing a cut-off strategy, at least one player will play safe at some beliefs above p_1^*. Indeed, it is possible to derive a lower bound $p^\ddagger \in (p_1^*, p^m)$, where $p^m := \frac{s}{g}$ is a myopic player's cut-off belief, such that, for all beliefs in $(p_1^*, p^\ddagger)$, at least one player plays safe.[19] In

[16] Please see Appendix B for more details and screen shots.

[17] The instructions handed out to all participants can be found in Appendix C.

[18] The theoretical treatment of strategic-experimentation problems has so far focussed on risk-neutral players only. Given the small stakes at play in the experiment, we did not expect subjects' risk attitudes to have an impact on their behavior. Consistently with this prediction, our data do not allow us to establish any effect of risk aversion on subjects' behavior. Details are available from the authors upon request.

[19] Indeed, as KRC show (their Equation (6), p.49), it is a best response for player i to play safe if and only if his value function $u_i(p)$ satisfies $u_i(p) \leq s + K_{-i}(p)c(p)$, where $K_{-i}(p) := \sum_{j \neq i} k_j(p)$ is the number of players other than i who play risky at belief p, and $c(p) := s - pg$ is a player's myopic opportunity cost for playing

the following, we refer to the belief region $(p_1^*, p^\ddagger)$ as the *free-riding region*. By the same token, we can derive an upper bound $\bar{p}$ on the lowest belief at which risky is a dominant action.[20] Table 1 provides an overview of belief thresholds, together with their numerical values given our parameters.

Table 1: Belief Thresholds

Symbol	Interpretation	Value
p_0	Prior belief	0.5
p^m	Myopic cutoff	0.4
p_1^*	Single-agent cutoff	0.2326
p_2^*	Efficient cutoff for $n = 2$	0.1031
p_3^*	Efficient cutoff for $n = 3$	0.0535
$\bar{p}$	$[p_1^*, \bar{p}]$ is a superset of the *free-riding region*	0.3578 (0.3742)
$p^\ddagger$	$(p_1^*, p^\ddagger)$ is a subset of the *free-riding region*	0.3428 (0.3609)

The values for $\bar{p}, p^\ddagger$ are for $n = 2$ ($n = 3$).

As $p_0 = 0.5 > 0.4 = p^m$, players start out with a belief that makes playing risky the dominant action. If, in the strategic treatment, n players were uninterruptedly playing risky and there was no breakthrough, the belief would drop to p^m after $40.6/n$ seconds, to our upper bound in the game with $n = 2$ players ($n = 3$ players) $\bar{p}$ after $58.5/n$ ($51.5/n$) seconds, to our lower bound in the game with $n = 2$ players ($n = 3$ players) $p^\ddagger$ after $65.0/n$ ($57.0/n$) seconds, to p_1^* after $119.4/n$ seconds, to p_2^* after $216.4/n$ seconds, and to p_3^* after $287.4/n$ seconds. For the control treatment, the same times apply with $n = 1$.The bang-bang structure of the best PBE is highlighted in Figure 1 in Section 5.

4.2.1 Free-Riding

Let $\hat{T}_i$ be the time player i's risky arm is revealed to be good or the end of the game, whichever arrives first. In order to measure the prevalence of free-riding, we investigate the behavior of the *average experimentation intensity*, where, following KRC, we define the *experimentation intensity at instant t* as $\sum_{i=1}^n k_{i,t}$. Note that, in the control treatment, a player conforming to the theoretical prediction will always play risky until his belief hits p_1^*. The same holds true in the best PBE in the strategic setting, but conditionally on no success arriving, beliefs will decrease faster in the strategic setting, as player i's belief also decreases in response to player j's hapless experimentation. Since both effects go in the same direction, we hypothesize that average experimentation intensities are lower in the strategic treatment. To set the stage, we thus formulate the following

Prediction 1. *The average experimentation intensity* $\frac{\sum_{i=1}^n \int_0^{\hat{T}_i} k_{i,t}\, dt}{\sum_{i=1}^n \hat{T}_i}$ *is lower in the strategic treatment than in the control treatment.*

risky, given the belief p. An upper bound on a player's equilibrium value function u_i is given by V_{n,p_1^*}, the value function of all players playing risky on $(p_1^*, 1]$, and safe on $[0, p_1^*]$. Thus, a lower bound $p^\ddagger$ is given by the unique root $V_{n,p_1^*}(p^\ddagger) - s - (n-1)c(p^\ddagger) = 0$.

[20] For this, we use the fact that the single-agent value function V_1^* constitutes a lower bound on a player's equilibrium value function u_i, and find our upper bound $\bar{p}$ as the unique root $V_1^*(\bar{p}) - s - (n-1)c(\bar{p}) = 0$.

Thus, a lower average experimentation intensity in the strategic treatment need not be due to subjects' strategic free-riding, since beliefs decrease faster in the strategic treatment. Strategic equilibrium free-riding can manifest itself in two ways: (i) some players play safe while the belief is above the single-agent cutoff p_1^*; (ii) players stop experimenting at the single-agent cutoff p_1^* (while efficiency would require them to experiment until the belief hits p_n^*). Effect (i) is *not* predicted to occur in the best PBE, whereas it is predicted to occur in any MPE. Effect (ii), by contrast, is predicted to arise in any equilibrium, Markovian or not. We can test for Effect (i) by comparing average experimentation intensities in the *free-riding belief region* where at least one player plays safe in *any* MPE. Theory would predict this intensity to be 1 in the control treatment; in the strategic treatment, the best PBE would predict it to be 1 as well, whereas it would be strictly less than 1 in any MPE. We therefore interpret a significantly lower average experimentation intensity for the strategic treatment in this belief region as evidence both for strategic free-riding and against the best PBE. These considerations lead us to formulate the following

Hypothesis 1. *(a) The average experimentation intensity* $\frac{\sum_{i=1}^{n}\int_0^{\hat{T}_i} k_{i,t}\,dt}{\sum_{i=1}^{n}\hat{T}_i}$ *in the free-riding region* $(p_1^*, p^\ddagger)$ *is strictly lower in the strategic treatment than in the control treatment.*
(b) Moreover, it is no higher in the safe-dominant region $[0, p_1^*]$.

Our game is one of purely (positive) informational externalities; i.e., players always have the option of ignoring the additional information they get for free from their partner(s). Therefore, players should do better in the strategic treatment, which motivates our following

Hypothesis 2. *Players' average final payoffs are higher in the strategic treatment.*

4.2.2 Structural Properties of the Best PBE

Cut-off behavior consists in a player playing risky at the outset, and continuing to play risky until his risky arm is revealed to be good, the game ends, or he switches to the safe action, and continues to play safe until the game ends or his risky arm is revealed to be good. As explained above, KRC predict that subjects will use cut-off strategies in the control treatment; by the same token, HKR show that cut-off behavior prevails on path in the strategic setting also if the best PBE is played. This leads us to the following

Hypothesis 3. *There is no difference in the frequency of cut-off behavior between the two treatments.*

Even if players were using cut-off strategies, they would not be conforming to the theoretical predictions if they were applying different cut-offs. Indeed, theory predicts that neither in the single-agent problem of the control treatment nor in the best PBE should exactly one of the players plays risky at any time. This is in contrast to any of KRC's "simple" MPEs, which all feature a pioneer who is experimenting alone on some interval of beliefs. This motivates

Hypothesis 4. *The proportion of time before a first breakthrough in the group during which exactly one player plays risky is the same in the strategic treatment as in the control treatment.*

5 Experimental Results

This section is, in the main, devoted to testing our behavioral hypotheses of Subsection 4.2. Throughout this section, we conduct our analysis by averaging over the six games each subject played. Analysis of the individual games can be found in Appendix A. For each of the six games, we conducted four treatments (strategic and control treatment with $n = 2$ and $n = 3$), with ten groups each. We had simulated all the relevant parameters ahead of time, as explained in Section 4. These included separate processes for the games' duration, the quality of the risky arm and the timing of successes on the risky arm in case it was good. The duration of the games ranged from 32 seconds for Game 5 to 230 seconds for Game 4.

5.1 Average Experimentation Intensities

As we have argued in Subsection 4.2, we should expect average experimentation intensities to be lower in the strategic treatment. Recall that, in the strategic treatment, the experimentation intensity is calculated for each player until the time of a first breakthrough by *any* player in a group or the end of the game, whichever arrives first. In the control treatment, this measure is calculated until the time where the *individual* player observes a success or the game ends, whichever occurs first. Table 2 lists the observed mean experimentation intensities, using group averages across games for our four treatments.

Table 2: Average Experimentation Intensities

	Strategic Treatment		Control Treatment	
Group Size	Obs.	Experiment. Intensity	Obs.	Experiment. Intensity
$n = 2$	60	.594 [.186]	60	.818 [.212]
$n = 3$	60	.539 [.244]	60	.839 [.180]

Average [st. dev.] experimentation intensity using group averages.

In the strategic treatment, for groups of size $n = 2$ ($n = 3$), the *ex ante* expected experimentation intensity for the best PBE is 0.786 (0.712). For MPE, the *ex ante* expected experimentation intensity is between 0.739 and 0.750 (0.654 and 0.668).[21] Conditional on the random realizations of the stochastic processes in the experiment, the *average* over the game realizations for the best PBE is 0.609 (0.517), while, for MPE, it is 0.604 (0.481), for $n = 2$ ($n = 3$). By contrast, in the control treatment, the *ex ante* expected experimentation intensity would have been 0.903. Conditional on the random realizations of the stochastic processes, the *average* over the game realizations is 0.820 (0.853) for $n = 2$ ($n = 3$). Thus, the observed experimentation intensities are very much in line with the conditional predictions given the realizations of the random parameters, for either equilibrium concept.[22]

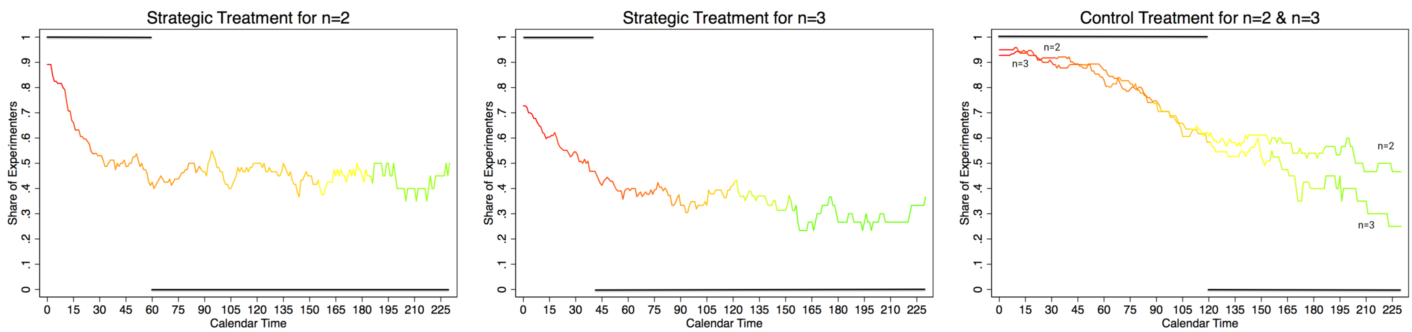
[21] Because of the multiplicity of MPE, it is not possible to give a point prediction of MPE experimentation intensities.

[22] Indeed, a one-sample t-test indicates that there is no evidence that the observed mean experimentation intensity is different from the predicted values (all p-values ≥ 0.4877), with the exception of the MPE prediction for $n = 3$, with a p-value of 0.0704.

We now revisit Prediction 1 and Hypotheses 1-4. To test our behavioral hypotheses from Subsection 4.2 and treatment differences non-parametrically, we apply two-sided Wilcoxon rank-sum (Mann-Whitney) tests, using group averages as independent observations. We begin with Prediction 1. As Table 2 reveals, the additional presence of one (two) perfectly positively correlated risky arm(s) leads to lower experimentation intensities. This is highly statistically significant in both settings with $n = 2$ and $n = 3$. The corresponding p-values in both cases are 0.0001.[23] This is in line with Prediction 1, a finding we summarize in the following

Remark 1. *The average experimentation intensity* $\frac{\sum_{i=1}^{n} \int_0^{\hat{T}_i} k_{i,t}\, dt}{\sum_{i=1}^{n} \hat{T}_i}$ *is lower in the strategic treatment. This result holds for both* $n = 2$ *and* $n = 3$.

In Figure 1, we illustrate the evolution of experimenting subjects divided by all subjects across all games and all subjects. More precisely, the share of experimenting subjects, before their risky arm was revealed to be good or the game ended, whichever happened first, is indicated at each unit of calendar time. The denominator, accounting for all subjects still playing, decreases as calendar time progresses and individual action choices gain in relative weight. This is captured by a color gradient, which highlights the evolution of subjects still actively playing (many = red to few = green). The theoretical predictions of both the optimal single-agent solution and the best PBE are included according to group size. These have a bang-bang structure, with a cut-off at the point in time at which the belief threshold p_1^* would be reached if players adhered to the theoretical prediction, for any n.



The share of experimenters by treatment across all games and subjects is shown. The color gradient displays the evolution of subjects still in the game (many = red to few = green). Optimal, and best-PBE, predictions are indicated in black color for all group sizes and treatments. For the control treatment, share of experimenters is highlighted for both n in one panel. Labels indicate the corresponding lines for groups of size $n = 2$ and $n = 3$.

Figure 1: Share of Experimenters by Treatment

[23] The Wilcoxon ranksum test treats group averages as independent observations. Yet, one might argue that players' action choices are not independent across subsequent games they play. As a robustness test, we additionally conduct a Wilcoxon test where we also average over all games for each group, thus yielding one independent data point across all games for each group of interacting subjects. The corresponding p-values for $n = 2$ ($n = 3$) are 0.0019 (0.0012). In Section 6.4, we complement the non-parametric analysis by reporting results from ordinary least-square regressions with random effects and clustering of standard errors by group. We find no effect of the number of games previously played on subjects' behavior, and results reported throughout the paper remain robust.

As is evident from the figure, players change their behaviors over time. While often playing risky at the beginning, players' use of the risky arm decreases as time passes and no success is observed. This suggests that our subjects adapted to the evolving information about their environment. The stark bang-bang structure of the theoretical predictions, however, is not borne out by the average experimentation intensities. Note that the theoretically predicted cut-off only applies "on path," i.e., it presupposes that everyone involved played risky with an intensity of 1 at all times before the cut-off. If this is not the case, the optimal "off-path" cut-off shifts to the right on the time axis. As we shall discuss in greater detail below, updated beliefs below p_1^* were reached in both the strategic and the control treatments only in two of the six games we simulated. As we shall also see below, subjects in the control treatment mostly followed cut-off strategies. As is apparent from the figure, the average proportion of risky play stays quite high for longer in the control treatment. This is consistent with theory, since subjects only have a single signal per unit of time to update their beliefs with in the control treatment. Meanwhile, they have two or three signals if their partner(s) also play(s) risky in the strategic treatment. Thus, information arrives faster, meaning that subjects become pessimistic faster (conditionally on no success being observed), in the strategic treatment. Subjects also alternate between risky and safe much more frequently and start to do so earlier. The figure also shows a remarkable similarity in subjects' behavior whether $n = 2$ or $n = 3$ in the control treatment, which is also consistent with theory; indeed, theory predicts subjects to behave like single agents, independently of group size n.

5.2 Free-Riding

As we have discussed in Subsection 4.2.1, the reduced exploration intensity in the strategic treatment, which we have documented in the previous subsection, may well be partially, or even completely, owed to the faster information accumulation in the strategic treatment. Yet, from a theoretical standpoint, we are more interested in the phenomenon of strategic free-riding, i.e., subjects' taking advantage of the information they receive for free from their partners in order to reduce their own exploration efforts. As we have discussed in Subsection 4.2.1, our setup allows us to identify Aspect (i) of strategic free-riding via the comparison of experimentation rates between the strategic and control treatments for beliefs in $(p_1^*, p^\ddagger)$, i.e., a belief region in which at least one player is predicted to play safe and risky each in any MPE; by contrast, all players are predicted to play risky in the best PBE and in the efficient solution, as well as in the single-agent optimum. We therefore interpret a lower experimentation intensity in the strategic treatment for this belief region as evidence of both strategic free-riding and against the best PBE. The following table summarizes average experimentation intensities in the belief region $(p_1^*, p^\ddagger)$.[24]

[24]We omit Games 5 and 6 from this table, since Player 2 has a success after 9 seconds on the risky arm in Game 6, and Game 5 lasted only 32 seconds, which implies that the *free-riding region* cannot be attained in the control treatment and only lasts for a few seconds in the strategic treatment, if it is attained at all. The other missing observation corresponds to one three-player group in the control treatment that has not reached the *free-riding region*.

Table 3: Average Experimentation Intensities in Free-Riding Region

	Strategic Treatment		Control Treatment	
Group Size	Obs.	Experiment. Intensity	Obs.	Experiment. Intensity
$n = 2$	40	.505 [.155]	40	.776 [.311]
$n = 3$	40	.510 [.220]	39	.779 [.090]

Average [st. dev.] experimentation intensity using group averages.

The average experimentation intensity in the *free-riding region* is substantially and significantly lower in the strategic treatment. Independently of group size, the p-values of the two-sided Wilcoxon ranksum test amount to 0.0001.

Result 1a. *Subjects are free-riding: average experimentation intensities are lower in the strategic treatment over the free-riding region* ($p_1^*, p^\ddagger$).

Result 1a, taken together with Remark 1, raises the question as to what extent subjects free-ride "correctly," i.e., at the "right" beliefs. In order to investigate this question, we analyze subjects' experimentation intensities in the belief region $(\bar{p}, p_0]$, where risky is a dominant action (which we subsequently label the *risky dominant region*). For $n = 2$, the average experimentation intensity is lower in the strategic treatment with 0.675 [0.222] than in the control treatment with 0.899 [0.161], where we report the standard deviation in square brackets. The same applies to our three-player groups, where the average experimentation intensity amounts to 0.632 [0.281] in the strategic treatment, while it is substantially higher in the control treatment with 0.932 [0.152]. These differences are highly statistically significant, with the p-values of the two-sided Wilcoxon ranksum test amounting to 0.0001 for both group sizes. While there is no theoretical rationale for the lower experimentation intensities in the *risky dominant region*, one may speculate that it may be due to subjects' aiming to reduce their experimentation intensities in the *free-riding region*, while not hitting this region precisely. Indeed, there is a theoretical rationale (namely, MPE) for different experimentation intensities across these two regions in the strategic treatment; there is no such rationale in the control treatment, where optimality would require an experimentation intensity of 1 in both belief regions.

In order to investigate this question further, we conduct a "difference-in-differences"-analysis, comparing the difference in intensities across belief regions and across treatments. As there is no *a priori* reason for imprecisions in belief updating or in the computation of the relevant thresholds to be more prevalent in the strategic treatment as compared to the control treatment, we interpret a bigger difference across belief regions in the strategic treatment as suggestive of MPE-style free-riding. For $n = 2$, this difference-in-differences is statistically significantly higher at the 5%-level in the strategic treatment, with a p-value of 0.0389. By contrast, no such evidence can be established for groups of size $n = 3$, where the effect is not statistically significant (p-value of 0.5118). Thus, there is stronger evidence that the free-riding we document is motivated by strategic rationales for the smaller group size $n = 2$. As we discuss in greater detail in Subsection 6.2, behavior was generally more reminiscent of MPE-play for the smaller group size $n = 2$.

To conclude our discussion of free-riding, we turn to the region $[0, p_1^*]$, where safe is a dominant action in both the strategic and control treatments. Indeed, the KRC model, which we have chosen for our experimental investigation on account of its tractability, exhibits no encouragement effect.[25] We can compute the average experimentation intensities in the region $[0, p_1^*]$, for Game 4 as well as for the two-player groups in Game 2.[26] Even in this region, the average experimentation intensity is lower in the strategic treatment: 0.511 [0.042] in the strategic treatment for Game 4 with $n = 2$ vs. 0.655 [0.237] in the control treatment; 0.325 [0.091] vs. 0.756 [0.220] in Game 4 for $n = 3$, and 0.511 [0.063] vs. 0.696 [0.251] in Game 2. Thus, mean group averages are lower in the strategic treatment compared to the control. For Game 2, the effect is not significant (p-value of 0.1149). For Game 4, the p-value is 0.0849 (0.0015) for $n = 2$ ($n = 3$). This leads us to state the following

Result 1b. *There is no encouragement effect in our data.*

Results 1a and 1b are fully in line with our Hypothesis 1. They also militate against our subjects' being motivated by social preferences.

5.3 Payoffs

Strategic interaction is predicted to arise among players as a result of (positive) informational externalities, i.e., the information produced by their partners allows players to make better decisions and hence to secure themselves higher payoffs. While certain courses of action are better than others from an *ex ante* expected point of view, the mapping from actions to payoffs is of course very stochastic in our setting, as it depends on the particular realizations of the random variables governing the length of the game, the quality of the risky arm and the timing of lump-sum arrivals from a good risky arm. Indeed, *conditionally* on a particular realization of the stochastic process, *ex ante* optimal behaviors may do very poorly, while *ex ante* very eccentric behaviors may well be optimal. Moreover, in our implementation, payoffs resulted from compound stochastic processes, implying a large *ex ante* variance in the mapping from behaviors to payoffs. We would thus caution against ascribing inferential value to payoff comparisons beyond what we do here, namely to compare payoffs between the strategic and control treatments, *for a given realization of the stochastic processes*.[27] For most of our inferences, though, we rely on our subjects' being ignorant of the realizations of the stochastic processes when they made their choices, which "thus filters out the noise" that prevails in the mapping from actions to payoffs.

Our test in this subsection is thus a simple one: Do subjects take advantage of the information they get for free from their partners in order to achieve higher payoffs in the

[25] The *encouragement effect* has been identified by Bolton and Harris (1999) and is not predicted to arise in the KRC setting. By virtue of this effect, players experiment more than if they were by themselves. They do so in the hope of producing public good news, which, in turn, makes their partners more optimistic. As their partners become more optimistic, they will be more inclined to experiment, thus providing some additional free-riding opportunities to the first player. This effect is absent in KRC, because here good news is conclusive: It resolves all uncertainty, so that, as soon as there is good news, players are not interested in free-riding any longer.

[26] These are the only settings in which this region is reached (and lasts for more than a few seconds) for both the strategic and the control treatments.

[27] Recall from Section 4 that we have kept the same realizations of the random processes for the strategic and control treatments.

strategic treatment? Theoretical *ex ante* expected per-capita payoffs are highest for the efficient solution, followed by the best PBE and then MPE, in the strategic treatment.[28] Any of these solution concepts implies, to varying degrees, players' taking advantage of the additional information they get from their partners, and thus leads to higher predicted payoffs than in the single-agent optimum. For groups of size $n = 2$ ($n = 3$), the *ex ante* expected per-capita payoff for the efficient solution is 1714.81 (1774.59), while, for the best PBE, it is 1699.00 (1734.90). For MPE, the *ex ante* expected per-capita payoff is between 1687.91 and 1690.68 (1714.49 and 1721.46).[29] For the single-agent optimum, the *ex ante* expected payoff is 1621.01. One can interpret the difference between the value of the efficient group solution and that of the single-agent optimum, 93.80 and 153.58 for groups of size $n = 2$ and $n = 3$, respectively, as measuring the size of the social dilemma we are analyzing. While entailing conspicuous differences in behavior, the impact on payoffs from the competing equilibrium concepts is small, roughly 10 (15) for groups of size $n = 2$ ($n = 3$). By contrast, even the lower bound for MPE payoffs entails an important payoff gain with respect to autarky, underlining the importance of the positive informational spillovers: 66.90 and 93.48 for groups of size $n = 2$ and $n = 3$, respectively.

Table 4: Average Final Payoffs

	Strategic Treatment				Control Treatment			
Group Size	Obs.	Final Payoffs	Min	Max	Obs.	Final Payoffs	Min	Max
$n = 2$	60	1235.50 [1235.11]	0.00	3945.00	60	1030.75 [1272.16]	0.00	3870.00
$n = 3$	60	1420.28 [1045.41]	0.00	3363.33	60	981.22 [904.08]	0.00	2860.00

Average [st. dev.] final payoffs using group averages.

Table 4 displays the average final payoffs using group averages across games for our four treatments. Average final payoffs are much higher in the strategic treatment than in the control treatment, for both group sizes. This is statistically significant: for $n = 2$ ($n = 3$), the corresponding p-values are 0.0674 (0.0001).[30] Thus, our subjects indeed take advantage of the positive informational externalities in the strategic treatment, lending support to Hypothesis 2.

Result 2. *For both group sizes, players' average final payoffs are higher in the strategic treatment.*

[28]This is not necessarily the case *given the particular realizations of the stochastic process*; please see Table A.3 in Appendix A.2.

[29]Because of the multiplicity of MPE, it is not possible to give a point prediction of MPE payoffs.

[30]To verify that our results are not driven by one particular game that may have unique features, we have computed our statistical tests each time excluding a different game. Differences in payoffs always remain statistically significant. This observation is confirmed by our ordinary least-square regressions with random effects controlling for learning effects (see Subsection 6.4), where results do not qualitatively change: we find a strong positive effect of the correlation structure—our strategic treatment—on payoffs. The same also holds true for groups of size $n = 2$.

5.4 Cut-Off Behavior

As we have pointed out above, optimality in the individual decision-making problem in our control treatment implies cut-off behavior, defined by a unique threshold belief above which it prescribes risky play, while prescribing safe play below it. The best PBE also features cut-off behavior on the path of play while KRC have shown that there does not exist an MPE in cut-off strategies. If subjects were trying to play the best PBE, therefore, we should observe roughly the same level of cut-off behavior in the strategic and the control treatments. The following table shows that the data emphatically reject this hypothesis, as cut-off behavior drops from roughly 80% in the control treatment to less than 33% in the strategic treatment. As it is not clear what it means for a group to engage in cut-off behavior, we report each individual subject's decisions.

Table 5: Average Frequency of Cut-Off Behavior

	Strategic Treatment		Control Treatment	
Group Size	Obs.	Total (Relative) Frequency	Obs.	Total (Relative) Frequency
$n = 2$	120	35 (.292)	120	100 (.833)
$n = 3$	180	59 (.328)	180	142 (.789)

Total number of cut-offs (number of cut-offs divided by total observations).

The difference between the treatments is statistically significant, yielding p-values of 0.0001 in both settings. When, in the strategic set-up, one excludes Games 5 and 6, which are characterized by either a short duration (Game 5 lasted only 32 seconds) or a resolution of uncertainty that occurs very early in the game (with Player 2 achieving a success after exploring for 9 seconds in Game 6), the total number of cut-off observations drops to 5 (23) out of 120 (180) overall observations for $n = 2$ ($n = 3$).

To complement our binary measure of cut-off behavior, we are also analyzing a more "continuous" measure of cut-off behavior in order to capture the distance of a subject's behavior to a cut-off strategy.[31] In particular, we measure the proportion of time in which a subject plays safe before ever playing risky, or plays risky after they had previously switched from risky to safe, before his risky arm is revealed to be good or the end of the game, whichever arrives first. We define 1 minus this proportion of time as our continuous cut-off measure, so that a score of 1 indicates perfect cut-off behavior. While this measure has some shortcomings (e.g., a subject starting with risky, then switching repeatedly back to the risky arm for short amounts of time after having switched to safe, would be classified as being close to cut-off behavior), we think that, in conjunction with our cruder binary measure, it can serve as a useful robustness test. In the strategic treatment, this measure amounts to 0.7511 (0.7737) and in the control treatment to 0.9590 (0.9284) for groups of size $n = 2$ ($n = 3$). This difference is highly statistically significant for both n, with p-values of 0.0001.[32]

[31] We are indebted to an anonymous referee for the suggestion.

[32] Our conclusion remains qualitatively unchanged if we instead use a more "lenient," less discriminating, measure where, after a switch from risky to safe, we focus only on a subject's second spell on the risky arm. Specifically, we measure the proportion of time in which a subject plays safe before playing risky plus the

Result 3. *For both group sizes, the frequency of cut-off behavior is higher in the control treatment, contradicting Hypothesis 3.*

5.5 Pioneers

In the control treatment as well as in the best PBE, players are predicted to play risky on $(p_1^*, \frac{1}{2}]$; i.e., conditionally on no success arriving, players should switch from risky to safe only once, and do so at the same time, at which their beliefs reach p_1^*. Thus, if subjects conformed to the best PBE, we should observe lonely pioneers for roughly the same proportion of time in both treatments.

Table 6: Proportion of Time with a Single Pioneer

	Strategic Treatment		Control Treatment	
Group Size	Obs.	Single Pioneer	Obs.	Single Pioneer
$n = 2$	60	.634 [.298]	60	.198 [.244]
$n = 3$	60	.497 [.338]	60	.080 [.168]

Average [st. dev.] proportion of time with a single pioneer in a group.

Table 6 shows the average proportion of time during which *exactly one* player is exploring before a first breakthrough by any player in his group. It is more than three times as large in the strategic treatment and the difference between treatments is highly statistically significant with p-values of 0.0001 for both $n = 2$ and $n = 3$. Thus, Hypothesis 4 is also emphatically rejected.

Result 4. *The proportion of time before a first breakthrough in the group during which exactly one player plays risky is higher in the strategic treatment, contradicting Hypothesis 4. This result holds for both* $n = 2$ *and* $n = 3$.

6 Discussion

In the previous section, we have seen that subjects' behavior differs starkly from the predictions of the best PBE. In this section, we provide additional results as well as robustness tests of the results presented in Section 5. We also discuss possible interpretations of subjects' behavior in light of some qualitative features of KRC's Markov Perfect Equilibria.

6.1 Switches of Action

As pointed out above, in the best PBE, as well as in the single-agent optimum, players are predicted to switch from risky to safe at most once. Meanwhile, the turn-taking behavior

proportion of time taken up by the subject's second spell on the risky arm, before his risky arm is revealed to be good or the end of the game, whichever arrives first, and define 1 minus this proportion as our alternative measure of cut-off behavior. In the strategic treatment, this measure is 0.8341 (0.7975) while in the control treatment it is 0.9644 (0.9403) for groups of size $n = 2$ ($n = 3$). The corresponding p-values are both 0.0001. Thus, our conclusion that subject behavior was closer to cut-off behavior in the control treatment seems quite robust to how we measure the "distance to cut-off behavior."

predicted by MPE implies that players should switch arms more often in the strategic treatment. Yet, learning also tends to be faster in the strategic setting, so that beliefs may more quickly reach the threshold at which the player will want to change his action.[33] Recall that for any number of role changes, there exists an MPE with that number of role changes, as KRC show. For a two-player game, this, e.g., implies that one of the players must switch actions at least twice, with the other one switching once, before p_1^* is reached.[34]

To control for the effect that, the longer the game goes on, the more time players have to switch actions, we define the *incidence of switches* as the number of a player's switches in a given game per unit of effective time, where *effective time* is understood as the time before the game ends or the player's risky arm is revealed to be good, whichever happens first.

Table 7: Average Number of Switches per Player

	Strategic Treatment		Control Treatment	
Group Size	Obs.	Switches per Player	Obs.	Switches per Player
$n = 2$	60	3.067 [2.450]	60	.792 [1.063]
$n = 3$	60	2.261 [2.040]	60	.778 [1.080]

Average [st. dev.] switches of players using group averages.

Table 7 displays the average number of switches per player across games for our four treatments.[35] The incidence of switches in the strategic treatment is much higher than in the control treatment for both $n = 2$ and $n = 3$ (both p-values of 0.0001).

Result 5. *For both group sizes, the incidence of switches is higher in the strategic treatment.*

In addition, we examine and test for the difference in timing when the first switch from risky to safe in a given game occurred. For both group sizes, the first switch from risky to safe in calendar time is realized statistically significantly earlier in the strategic treatment (all p-values of 0.0001). As we have mentioned above, information accumulation is potentially faster in the strategic treatment. On account of the conditionally independent

[33]Note that if players were to play the best PBE and the game happened to stop at a time such that p_1^* is only reached in the strategic treatment, we should observe exactly one switch per player in the strategic treatment and none in the control treatment. Therefore, a higher number of switches in the strategic treatment is not inconsistent with players' playing the best PBE. However, the magnitude of the effect, which we report here, cannot be accounted for by this explanation. While this effect would add to making switching more prevalent in the strategic treatment, a substantially higher number of switches in the strategic treatment would provide suggestive evidence that subjects may indeed have endeavored to take turns, as predicted by KRC's MPEs.

[34] It is optimal for players to continue to play risky after observing a success until the game ends; in the strategic treatment, it does not matter whether the success has been achieved by the player himself or his partner, while, in the control treatment, only a player's own successes are informative. Overall across both treatments, there were 65 (110) successes in the groups of size $n = 2$ ($n = 3$). Across all treatments and any n, only 6 subjects did not continuously explore until the end of the game after observing a success that resolves all uncertainty. Of these 6 players, 5 switched to safe for a few seconds and one subject reverted to playing safe after continuously playing risky for 120 seconds following his own success.

[35]While we run our hypothesis test with the average *incidence* of switches, we rather report the average *number* of switches in Table 7, as this may be easier to interpret. The number of switches is also statistically significantly higher in the strategic treatment for both n, with p-values of 0.0001.

Poisson processes, the information acquired within a given unit of time is proportional to the number of players currently playing risky. To account for the fact that the decrease in beliefs may be up to n times faster in the strategic treatment, we compare n times the calendar time of the first switch in the strategic treatment to the calendar time of the first switch in the control treatment. For group size $n = 2$ ($n = 3$), the former was 36.31 (57.13) on average in the strategic treatment vs. 76.55 (76.14) in the control treatment. This difference is statistically significant for $n = 2$ (p-value of 0.0007) and $n = 3$ (p-value of 0.0330). Thus, for both n we can conclude that, on average, the first switch in the strategic treatment occurred at a statistically significantly more optimistic belief than in the control treatment. This is further evidence that participants attempted to actively *free-ride* on the information generated by their partner(s). KRC's MPEs have the property that the first switch occurs at a belief strictly higher than the single-agent cut-off p_1^*. Taken together with the much higher frequency of cut-off play in the control treatment, this suggests that subjects' behavior may possibly be better predicted by MPE than by the best PBE. We explore this concept in greater depth in the following subsection.

6.2 Groups of $n = 2$ vs. $n = 3$

As Figure 1 illustrates, behavior in the control treatment is remarkably similar across two- and three-player groups, as subjects do not have the opportunity to free-ride on the information generated by others. Meanwhile, in the strategic treatment, the coordination required by MPE play is decidedly more involved than that which underlies the best PBE. This complexity increases with the number of players for the former, while it remains unchanged for the latter. Indeed, recall that the latter implies cut-off behavior on the path of play, while the former is characterized by role changes. Coordinating role changes is inherently more difficult in three-player groups. Therefore, one might expect that MPE-type behavior is more prevalent in groups of $n = 2$ players than in groups of size $n = 3$. Moreover, the size of the social dilemma is more than 50% larger in groups of size $n = 3$. Possible indicators of more MPE-like behavior in the strategic treatment would be less cut-off behavior, more switches and more single pioneers for the smaller group size $n = 2$.

Recall that our difference-in-differences analysis of experimentation intensities across belief regions and treatments (see Subsection 5.2) shows a statistically significantly larger difference between the *risky dominant* and *free-riding* regions in the strategic treatment only for groups of size $n = 2$. Moreover, the observed overall average experimentation intensity is furthest away from the MPE-prediction for groups of size $n = 3$, being significantly different from the observed value at the 10%-level (see Footnote 22). Our following result provides additional evidence that the more sophisticated forms of coordination required by MPE seem to be more prevalent for $n = 2$ than for $n = 3$.

Result 6. *The frequency of single pioneers is significantly higher in the strategic treatment for $n = 2$ than for $n = 3$. Furthermore, the incidence of switches is higher and cut-off behavior is less frequent in the strategic treatment for $n = 2$ than for $n = 3$. However, the latter two effects are not statistically significant.*

The p-value is 0.0252 for the proportion of time with a single pioneer. It is 0.2237 and 0.5096 for the average incidence of switches per player, and the average frequency of cut-off behavior, respectively. If we omit Games 5 and 6 (arguably outliers on account of their short length and the very early success, respectively), the difference in cut-off behavior is highly significant as well (p-value of 0.0101).[36] Thus, overall, our subjects are not behaving—or aiming to behave—as in the best PBE. Generally, our subjects' behavior seems qualitatively to be better described by MPE play, though the evidence for this conclusion is stronger for groups of size $n = 2$ than for $n = 3$.

6.3 Attention Paid To Partners' Experimentation Efforts

As a robustness test, we would like to ensure that the differences in behavior and payoffs between the strategic and control treatments, which we are observing, are indeed due to the positive informational externality theory predicts. To do so, we study directly how much heed subjects paid to the information provided by their partner(s). We employ eye-tracking data obtained by two (three) Tobii-TX300 eye trackers with a sampling rate of 300 Hz. The relative frequency of fixations corresponds to the relative importance of an information in the subject's decision-making process (Jacob and Karn 2003, Poole, Ball, and Phillips 2005). In our setting, eye fixations can thus provide information about the importance subjects assigned to the different payoff streams, which revealed both a player's actions and payoffs. While the use of this technology imposed subject constraints in the data-collection process, it allows us to gain additional insights into subjects' cognitive processes with the aim of better understanding subjects' behavior in the strategic treatment *relative* to the control treatment. If subjects were not making *any* use of the free information provided by their partners, then no statistically significant difference in observed attention should be detected. This in turn would invalidate any game-theoretical explanation of observed differences in behavior or payoffs between the strategic and control treatments, since theory predicts that the only source of strategic interaction in our game is the positive externality that arises because the information players produce is a public good.[37] We define a subject's fixation intensity as the total number of fixations on his own payoff stream, divided by the total number of all fixations (i.e., both on his own and on his partner's [partners'] payoff stream[s]) during a game before a breakthrough arrives or the game ends.

Table 8: Average Fixation Intensities

	Strategic Treatment		Control Treatment	
Group Size	Obs.	Fixation Intensity	Obs.	Fixation Intensity
$n = 2$	60	.614 [.087]	60	.865 [.090]
$n = 3$	60	.383 [.078]	60	.712 [.106]

Average [st. dev.] fixation intensity using group averages.

As Table 8 shows, the average fixation intensity is much lower in the strategic treatment.

[36] If we analyzed the *number*, rather than the *incidence* of switches, the difference would be significant at the 10%-level (p-value of 0.0771) for all six games, and even at the 1%-level for Games 1-4 only.

[37] Video recordings illustrating the use of the eye-tracking devices are available at the author's website: www.johanneshoelzemann.com. Heat maps spotlighting information search and attention behavior can be found in Appendix A.

This is highly statistically significant for both group sizes (both p-values are 0.0001 for $n = 2$ and $n = 3$). While a subject who is unsure about how to solve his decision problem might also be tempted to "copy" from his partner in the control treatment, the fact that players focus on each other much more in the strategic treatment very much suggests a strategic rationale for players' behavior, in that they are trying to learn about the quality of their own risky arms by observing their partners' exploration efforts. These results furthermore suggest that subjects do indeed understand the simple, non-strategic, nature of the control treatment.

6.4 OLS Estimations

As a further robustness test and to complement the non-parametric analysis in Section 5 and key elements discussed so far in this section, we ran ordinary least-square regressions with random effects controlling for learning effects. In particular, we regressed experimentation intensity, payoffs, cut-off strategy, switches of action, and fixation intensity on the treatment dummy *Correlation*, which is 0 for the control treatment and 1 for the strategic treatment. Recall that subjects played the six games in random order and any order of these games that was used for k ($k \in \{1, \cdots, 10\}$) groups in the strategic treatment was replicated for k groups in the control treatment. In order to verify that subjects treated the games they successively played as independent games rather than as parts of a larger super-game, we define a weighted learning function $\{g_o\} = \{\frac{1}{o}\}$ where o ($o \in \{1, \cdots, 6\}$) corresponds to the random order in which each subject was exposed to each game. All regressions control for trends over time using this weighted learning function. The results do not qualitatively change when we replace the learning function with a linear version such that $\{g_o\} = \{o\}$. To account for the fact that behavior within groups of two (three) participants is not independent, we treat each group as our units of statistically independent observations and cluster standard errors by group.

Table 9 lists the results from this analysis where Panel A shows the results for two-player groups and Panel B displays the results for $n = 3$. We find a strong negative effect of the correlation structure, our *strategic treatment*, on experimentation intensity across both belief regions, cut-off strategy, and fixation intensity. By contrast, we find a strong positive effect of the additional presence of one (two) perfectly positively correlated arm(s) on stage game payoffs and switches of action.[38]

Thus, our OLS estimations with random effects confirm all of our previous, non-parametric, results.[39] In particular, there is no evidence of super-game effects, as subjects' behavior does not change in response to the number of games they previously played.[40]

[38] We do not report estimates of the proportion of time with a single pioneer as the interpretation of the lonely pioneers is only sensible at the individual group-level.

[39] Our results remain qualitatively unchanged if we use our "continuous" cutoff measure (see p. 19) instead.

[40] We also ran our non-parametric analysis using only the last (first) games played by each group. While this implies the loss of a large amount of data, and hence statistical power, our qualitative conclusions remain unaltered, although a few of our effects are no longer statistically significant.

Table 9: OLS Estimations with Random Effects of Experimentation Intensity, Payoffs, Cut-off Strategy, Switches of Action, and Fixation Intensity.

	Experimentation Intensity			*Payoffs*	*Cut-off*	*Switches of*	*Fixation*
	ALL	*Risky Dom*	*Free-Riding*		*Strategy*	*Action*	*Intensity*
Panel A: $n = 2$							
Intercept	0.883^{***}	0.973^{***}	0.812^{***}	983.861^{***}	0.873^{***}	0.827^{***}	0.851^{***}
	(0.053)	(0.051)	(0.070)	(60.227)	(0.063)	(0.296)	(0.023)
Correlation	-0.223^{***}	-0.191^{***}	-0.299^{***}	204.750^{***}	-0.542^{***}	2.275^{***}	-0.251^{***}
	(.050)	(.050)	(0.066)	(55.838)	(0.058)	(0.378)	(0.024)
Learning	-0.148	-0.118	-0.016	106.566	-0.089	-0.081	0.033
	(0.119)	(0.115)	(0.141)	(103.356)	(0.103)	(0.659)	(0.035)
σ_ϵ	0.217	0.242	0.254	1474.627	0.411	1.946	0.103
σ_u	0.117	0.111	0.150	0	0.073	0.543	0.071
N	240	200	148	240	240	240	240
(Between) R-squared	0.420	0.319	0.375	0.116	0.705	0.602	0.716
Panel B: $n = 3$							
Intercept	0.903^{***}	1.001^{***}	0.763^{***}	963.333^{***}	0.878^{***}	0.738^{**}	0.717^{***}
	(0.058)	(0.064)	(0.082)	(52.382)	(0.082)	(0.317)	(0.030)
Correlation	-0.300^{***}	-0.300^{***}	-0.282^{***}	439.056^{***}	-0.461^{***}	1.483^{***}	-0.330^{***}
	(0.062)	(0.090)	(0.073)	(44.900)	(0.085)	(0.438)	(0.030)
Learning	-0.145	-0.158	0.065	40.658	-0.203	0.089	-0.011
	(0.114)	(0.149)	(0.126)	63.533	(0.147)	(0.508)	(0.049)
σ_ϵ	0.253	0.216	0.292	1449.951	0.399	1.622	0.112
σ_u	0.126	0.205	0.177	0	0.182	0.918	0.103
N	360	300	223	360	360	360	360
(Between) R-squared	0.491	0.336	0.283	0.404	0.500	0.311	0.693

For all estimations, robust standard errors are clustered at the group level and shown in brackets.
***Significant at the 1 percent level.

7 Conclusion

We have analyzed a problem of dynamic public-good provision, where the public good in question is information about an uncertain state of the world. In particular, a group of several agents was facing the same decision problem, in which the optimal course of action depended on an unknown state of the world, which, in the strategic treatment, was common to everyone in the group. Therefore, the information produced by one agent benefited the other group member(s) as well. This informational externality constituted the only strategic link across players. Information, and hence agents' contribution incentives, evolved as the game progressed. We compare subjects' behavior in this strategic treatment to the behavior of subjects in the *control treatment*, where each agent's *individual* state of the world was iid, and there were therefore no strategic links across group members.

We have shown that experimentation intensities are lower in the strategic treatment. In particular, this is the case in the *free-riding belief region*, which points to strategic free-riding. Moreover, subjects seem to attempt to coordinate in rather elaborate ways, as evidenced, *inter alia*, by the much lower incidence of cut-off behavior and the higher incidence of lonely pioneers in the strategic setting. Overall, this leads us to reject the hypothesis that subjects played according to the best PBE. Indeed, the best PBE would predict no free-riding in the *free-riding region*, cut-off play and no lonely pioneers. While this does of course not constitute conclusive evidence in favor of MPE, it bears noting that these behaviors are fully in line with the qualitative predictions of MPE.

Why subjects should refrain from engaging in the simple cut-off behavior prescribed by the welfare-optimal equilibrium seems somewhat of a puzzle. The control treatment shows that subjects were not, in principle, averse to playing cut-off strategies. We conjecture that, in the strategic setting, the idea of taking turns (Cason, Lau, and Mui 2013; Leo 2017), as evidenced by the greater prevalence of lonely pioneers and a greater number of switches, was attractive to subjects. Turn-taking is a feature of any of KRC's MPEs in this setting. Vespa and Wilson (2015, 2019) had already documented a tendency of a substantial fraction of subjects to adopt MPE behavior; in their setting, MPE play was simpler—in ours, it is not. Our investigation shows that, even in a setting like ours, where from a theoretical perspective one equilibrium is welfare-optimal and prescribes particularly simple behavior, making it an obvious candidate for a focal equilibrium, this equilibrium may well not describe subjects' behavior accurately. Our findings may thus counsel caution when supposing which, among a multitude of equilibria, may be considered focal by players. We commend the analysis of other dynamic games with an *a priori* "obvious" candidate for a focal equilibrium for future research. In particular, it would be interesting to analyze a game in which the welfare-optimal equilibrium was both particularly simple in structure *and* Markovian.

As a further robustness test, one could in principle show subjects the current updated belief on their screens, in order to separate the task of belief updating from that of determining the cut-offs. We have decided against doing so here, as we were concerned about nudging subjects toward certain behaviors, which would have made the interpretation of our results more difficult. It might also be interesting to test whether the encouragement effect can be shown in the laboratory for settings in which the theory would predict it to arise, such as, for instance, the Poisson setting with inconclusive breakthroughs à la Keller and Rady (2010), or the Brownian-motion setting of Bolton and Harris (1999). It would also be intriguing to try and test the impact of privately observed actions or payoffs in the laboratory. We commend these questions for future research.

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Bandits in the Lab
Online Appendix

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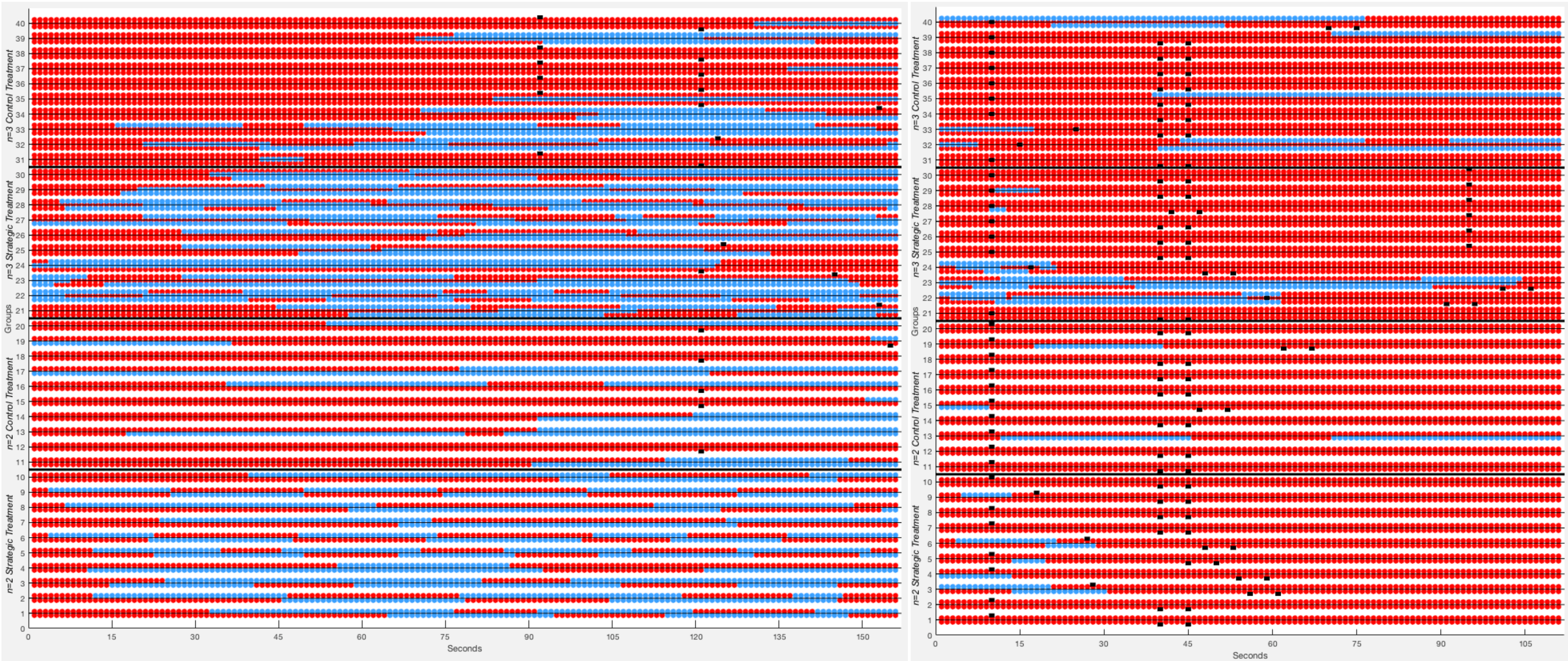
A Analysis of Individual Games

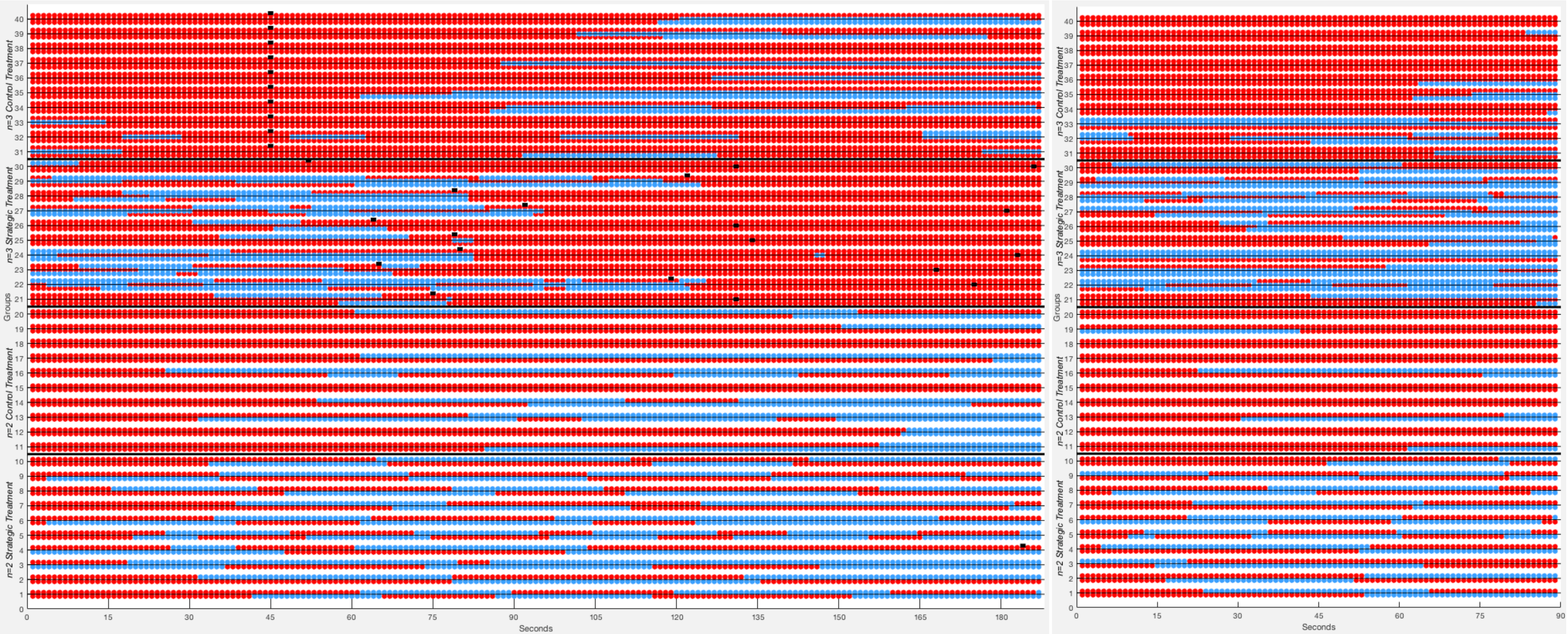
In Section 5 and 6 of the main text, we have presented our aggregate results for all six games. We now conduct a separate analysis of the several games, which differed in the realizations of the underlying random processes we simulated ahead of time, as Figures A.1-A.3 show. Indeed, insights that hold in all, or most, of these six games might be considered more robust than results that held only on average over the games.

Figures A.1, A.2 and A.3 display the evolution of players' action choices over all six games. Players' actions are described by dots, the width of which corresponds to one second of time. For each of the six games, we conducted four treatments with ten groups each, the parameters of which (i.e., their duration, the quality of the risky arm and the timing of successes on the risky arm in case it was good) we had simulated ahead of time, as explained in Section 4. As the figures show, the duration of the games ranged from 32 seconds for Game 5 to 230 seconds for Game 4. As is furthermore evident from the figures, players change their behaviors over time. While often playing risky at the beginning, players seem to grow less inclined to use the risky arm the longer it has unsuccessfully been used before. This shows that our subjects adapted to the evolving information about their environment.



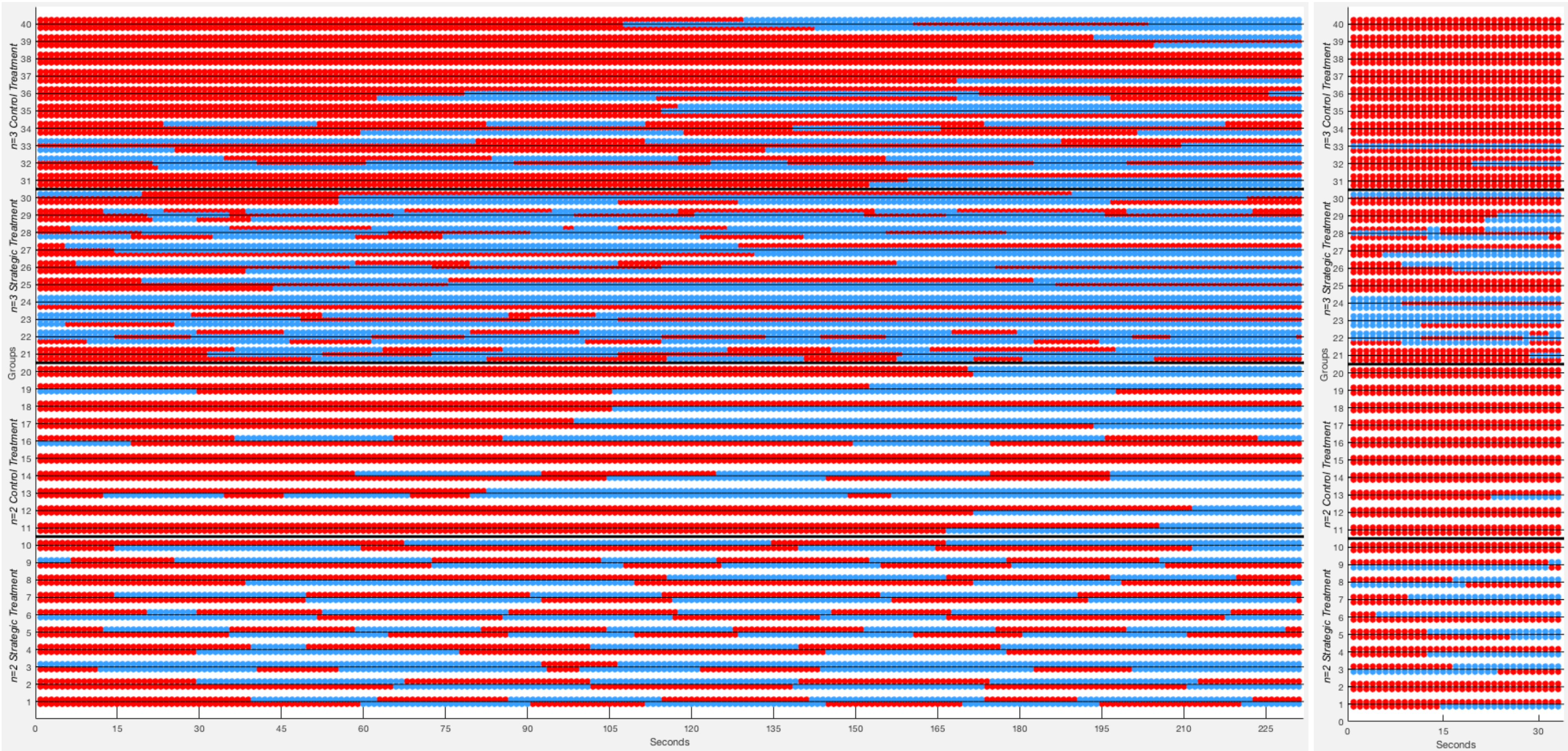
Games 1 and 6 are shown. Players' actions are described by dots, the width of which corresponds to one second of time. Groups 1-10 correspond to the strategic treatment for two-player groups; groups 11-20 are the corresponding control treatments. Groups 21-30 played the strategic treatment for three-player groups, while groups 31-40 were the corresponding control treatments. In each group, we refer to the lowermost player as 'player 1', while 'player 2' will denote the player right above, and 'player 3' is the uppermost player. The x-axis represents calendar time. A *red* dot indicates that a player is playing *risky* in a given second, while a *blue* dot indicates that the player is playing *safe*. A black square indicates the arrival of a lump sum on the risky arm.

Figure A.1: Action Choices by Players over Time, Games 1 & 6



Games 2 and 3 are shown. Players' actions are described by dots, the width of which corresponds to one second of time. Groups 1-10 correspond to the strategic treatment for two-player groups; groups 11-20 are the corresponding control treatments. Groups 21-30 played the strategic treatment for three-player groups, while groups 31-40 were the corresponding control treatments. In each group, we refer to the lowermost player as 'player 1', while 'player 2' will denote the player right above, and 'player 3' is the uppermost player. The x-axis represents calendar time. A *red* dot indicates that a player is playing *risky* in a given second, while a *blue* dot indicates that the player is playing *safe*. A black square indicates a the arrival of a lump sum on the risky arm.

Figure A.2: Action Choices by Players over Time, Games 2 & 3

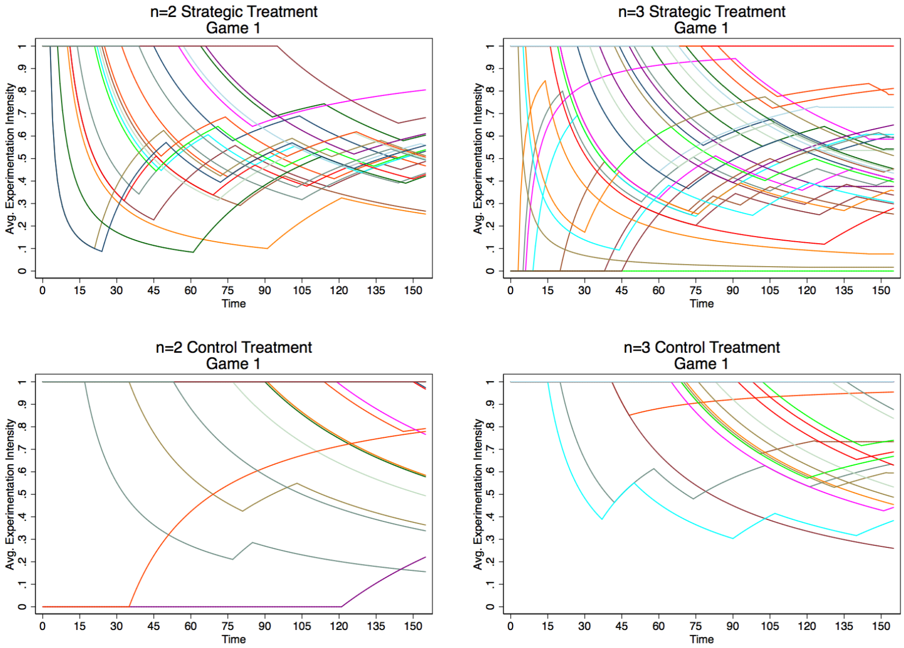


Games 4 and 5 are shown. Players' actions are described by dots, the width of which corresponds to one second of time. Groups 1-10 correspond to the strategic treatment for two-player groups; groups 11-20 are the corresponding control treatments. Groups 21-30 played the strategic treatment for three-player groups, while groups 31-40 were the corresponding control treatments. In each group, we refer to the lowermost player as 'player 1', while 'player 2' will denote the player right above, and 'player 3' is the uppermost player. The x-axis represents calendar time. A *red* dot indicates that a player is playing *risky* in a given second, while a *blue* dot indicates that the player is playing *safe*. There were no lump-sum arrivals in Games 4 and 5.

Figure A.3: Action Choices by Players over Time, Games 4 & 5

A.1 Experimentation Intensity

In order to illustrate subjects' dynamically evolving incentives for public-good provision, Figure A.4 displays the evolution of each player's cumulated experimentation intensity over time in Game 1.[41] In the strategic treatment, increasing and flat parts at level 1, of a player's curve correspond to periods in which the player actively provides information to the group by exploring the risky arm. By contrast, the player relies on his partner's experimentation efforts when the curve is decreasing or flat at level 0. The figure shows that, when players are still optimistic at the start of the game, they overwhelmingly tend to play risky. This is followed by a period in which subjects tended to alternate between safe and risky, with the safe action becoming more frequent toward the end. Behavior in the control treatment, however, provides a sharp contrast, as most curves are monotonically decreasing, indicating cut-off behavior.



Experimentation Intensity for each subject.

Figure A.4: Evolution of Cumulated Experimentation Intensity over Time by Player

Also at the individual game level, the additional presence of one (two) perfectly positively correlated arms leads to lower experimentation intensities in all games. When considering all belief regions of a game, this is statistically significant for Games 1-5, but not for Game 6, in both settings with $n = 2$ and $n = 3$. The corresponding p-values in the case of $n = 2$ are 0.0155, 0.0493, 0.0009, 0.0102, 0.0013, and 0.3748 for Games 1-6, respectively. In the setting with $n = 3$, the average experimentation intensity is also lower in the strategic treatment (p-values of 0.0019, 0.0081, 0.0011, 0.0007, 0.0013, and 1.0000 for Games 1 to 6, respectively). As Figure A.1 highlights, Game 6 features an early success by Player 2 after 9 seconds of exploration, as well as successes by Player 1 after 39 and 44 seconds of exploration, respectively.

[41] Corresponding figures for the other games look qualitatively similar.

We proceed with our analysis by conducting our parameter tests separately by belief region. As player 2 has a success after 9 seconds of using the risky arm, we omit Game 6 from these tables. We furthermore omit Game 5 from the tables for the *free-riding region*, as this game lasts only 32 seconds, implying that the *free-riding region* cannot be attained in the control treatment and only lasts for a few seconds in the strategic treatment, if it is attained at all. For Game 3 in the three-player set-up, the missing observation for the *free-riding region* corresponds to three individual players in one group in the control treatment that have not reached the *free-riding region* either on account of an early success or because they did not use the risky arm enough. Table A.1 summarizes our findings for each game separately by belief region.

Table A.1: Average Experimentation Intensity by Belief Regions and by Game

		$n = 2$				$n = 3$			
		Strategic Treatment		Control Treatment		Strategic Treatment		Control Treatment	
Game	Belief Region	Obs.	Exp. Intensity	Obs.	Exp. Intensity	Obs.	Exp. Intensity	Obs.	Exp. Intensity
1	All	10	.508 [.065]	10	.730 [.293]	10	.455 [.107]	10	.797 [.217]
2	—	10	.512 [.116]	10	.696 [.283]	10	.543 [.227]	10	.833 [.125]
3	—	10	.565 [.086]	10	.878 [.235]	10	.457 [.169]	10	.866 [.199]
4	—	10	.519 [.120]	10	.678 [.239]	10	.383 [.089]	10	.728 [.183]
5	—	10	.653 [.204]	10	.984 [.072]	10	.596 [.264]	10	.953 [.110]
6	—	10	.810 [.259]	10	.941 [.167]	10	.800 [.314]	10	.857 [.182]
1	Risky Dominant	10	.648 [.217]	10	.835 [.184]	10	.709 [.310]	10	.935 [.133]
2	—	10	.723 [.254]	10	.888 [.192]	10	.649 [.291]	10	.976 [.040]
3	—	10	.617 [.189]	10	.906 [.155]	10	.593 [.303]	10	.906 [.204]
4	—	10	.732 [.261]	10	.880 [.177]	10	.613 [.275]	10	.889 [.218]
5	—	10	.653 [.204]	10	.984 [.051]	10	.596 [.264]	10	.953 [.110]
1	Free-Riding	10	.503 [.171]	10	.726 [.365]	10	.537 [.230]	10	.760 [.273]
2	—	10	.445 [.114]	10	.752 [.350]	10	.549 [.261]	10	.674 [.299]
3	—	10	.589 [.184]	10	.895 [.225]	10	.482 [.204]	9	.884 [.168]
4	—	10	.484 [.128]	10	.732 [.301]	10	.471 [.204]	10	.807 [.189]

Average [st. dev.] experimentation intensity using group averages. For $n = 3$ in the control treatment, only players in nine groups entered the *free-riding region*.

Also, at the game level, the average experimentation intensity is substantially lower in the strategic treatment for both belief regions. In the *free-riding region* for groups of size $n = 2$, the corresponding p-values are 0.1679, 0.0176, 0.0089, and 0.0186 for Games 1-4, respectively. For groups of size $n = 3$, the same is true, with the exception of Game 2. This is most likely due to an early success by player 3 after only 44 seconds of exploration. The p-values are 0.0603, 0.2395, 0.0023, and 0.0023 for Games 1 - 4, respectively. As for the *risky dominant region*, experimentation intensities in the two-player groups are lower in the strategic treatment, which is statistically significant at least at the 5%-level in Games 1, 3, and 5; there is no significant difference for Games 2 and 4. The p-values of the two-sided

Wilcoxon ranksum test amount to 0.0249, 0.1364, 0.0044, 0.1180, and 0.0013 for Games 1 to 5, respectively. The difference is statistically significant at least at the 10%-level for groups of size $n = 3$, however, with p-values amounting to 0.0823, 0.0138, 0.0097, 0.0215, and 0.0013 for Games 1-5, respectively.

We further conduct our "difference-in-differences"-analysis by comparing the difference in intensities across belief regions and across treatments for Games 1-4. For $n = 2$, this difference-in-differences is higher in the strategic treatment in all four games, though not statistically significant. The corresponding p-values are 0.4490, 0.1255, 0.5366, and 0.3239 for Games 1 to 4, respectively. For groups of size $n = 3$, there is no statistical evidence of a difference in treatments with p-values of 0.9393, 0.1304, 0.7090, and 0.6477 for Games 1 to 4, respectively.

While we cannot establish statistical significance on the individual-game level in the difference-in-differences analysis, we can do so by directly testing for different experimentation intensities between the two belief regions. In particular, in the strategic treatment for two-player groups, the difference between the *risky dominant* and the *free-riding region* is statistically significant with p-values of 0.0340, 0.0152, and 0.0154 for Games 1, 2 and 4, respectively, but not for Game 3 where the p-value amounts to 0.7336. In the control treatment, where no difference between these two belief regions is predicted to arise, we document p-values of 0.6791, 0.4247, 0.8425, and 0.2937 for Games 1-4.

In the strategic treatment with $n = 3$, even though average experimentation intensities decrease when moving from the *risky dominant region* to the *free-riding region*, no such statistical evidence can be established. The p-values are 0.3603, 0.4710, 0.8189, and 0.4473 for Games 1-4, respectively. Thus, the comparison between the two belief regions does not yield any indication for MPE-type behavior for $n = 3$, whereas it does for $n = 2$. In the control treatment, where no difference between the regions is predicted to arise, we find no statistically significant differences for Games 1, 3, and 4 (p-values of 0.1592, 0.2576, and 0.2145). However, Game 2 is an outlier here, with behavior across regions exhibiting significant differences (the p-value is 0.0138).

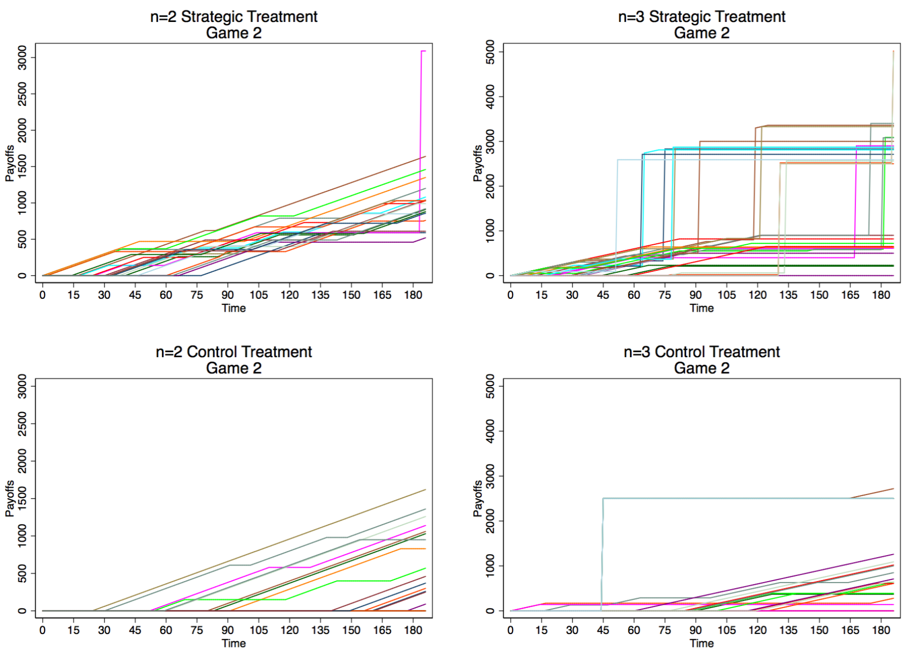
A.2 Payoffs

Table A.2 displays average final payoffs per group per game. With the exception of Game 1, average final payoffs are much higher in the strategic treatment than in the control treatment, for both group sizes. For $n = 2$ ($n = 3$), the p-values are 0.0233 (0.0004), 0.0007 (0.0015), 0.0081 (0.0007), and 0.0012 (0.0013) for Games 2-5, respectively. The average-payoff difference is not statistically significant with p-value of 0.1145 for $n = 2$ in Games 6; however, such statistical evidence can be established in the setting with $n = 3$, with a p-value of 0.0028. Game 1 is an outlier in that average final payoffs are statistically significantly higher *in the control treatment,* with p-values of 0.0342 (0.0820). Thus, also at the game-level, our subjects indeed take advantage of the positive informational externalities in the strategic treatment (with the exception of Game 1).

Table A.2: Average Final Payoffs by Game

	Strategic Treatment				Control Treatment			
Game	Obs.	Final Payoff	Min	Max	Obs.	Final Payoff	Min	Max
Panel A: $n = 2$								
1	10	817.50 [111.61]	670.00	1060.00	10	1176.50 [440.24]	500.00	1765.00
2	10	1092.00 [452.30]	755.00	2220.00	10	577.50 [446.27]	0.00	1210.00
3	10	407.00 [86.35]	230.00	535.00	10	109.50 [158.03]	0.00	405.00
4	10	1181.00 [279.65]	895.00	1910.00	10	761.00 [460.14]	0.00	1710.00
5	10	115.00 [68.39]	0.00	200.00	10	5.50 [17.39]	0.00	55.00
6	10	3800.50 [69.82]	3750.00	3945.00	10	3554.50 [677.30]	1630	3870.00
Panel B: $n = 3$								
1	10	1177.67 [365.25]	703.33	1743.33	10	1488.67 [411.23]	610.00	1910.00
2	10	2110.33 [433.68]	1370.00	2686.67	10	1161.00 [232.98]	833.33	1616.67
3	10	496.33 [153.39]	226.67	686.67	10	123.33 [180.88]	0.00	510.00
4	10	1465.33 [209.24]	1166.67	1790.00	10	641.67 [433.23]	0.00	1453.33
5	10	137.00 [88.71]	0.00	250.00	10	15.33 [35.28]	0.00	106.67
6	10	3135.00 [354.57]	2373.33	3363.33	10	2457.33 [433.21]	1276.67	2860.00

Average [st. dev.] final payoffs using group averages.



Payoffs over time for each subject.

Figure A.5: Evolution of Payoffs over Time by Player

Figure A.5 displays the evolution of each player's cumulated payoff over time for Game 2. Positive slopes correspond to periods during which a subject played safe; flat parts indicate hapless risky play, while jumps denote lump sums arrivals from the risky arm.

In Table A.3, we provide the theoretically expected payoffs *conditional* on the realizations of the stochastic processes, which we had simulated ahead of time.[42] Of course, *conditionally* on a particular realization of the stochastic process, *ex ante* optimal behaviors may do very poorly, while *ex ante* very eccentric behaviors may well be optimal.[43] Note, for instance, that, in Game 4, the equilibrium strategy, which gives up earlier, does better than the efficient solution. In fact, for groups of size $n = 2$, the best PBE does weakly better than the efficient solution for all six games. As differences in predicted payoffs are to a large extent driven by the timing of the big lump-sum payoffs from the risky arm, Table A.3 provides a cautionary tale against ascribing excessive inferential value to observed payoff differences; except for Result 2, which compares payoffs across treatments for *given realizations* of the stochastic processes, we have relied on differences in observed behavior for our inference. Indeed, as subjects did not know the realizations of the stochastic processes when choosing their actions, observed behavioral differences will "filter out" the considerable additional noise that stems from the—"very stochastic"—mapping of behavior into realized payoffs.

Table A.3: Average Predicted Payoffs

	Efficient		Best PBE		MPE		Single-Agent	
Game	$n = 2$	$n = 3$	$n = 2$	$n = 3$	$n = 2$	$n = 3$	$n = 2$	$n = 3$
1	470.00	1666.67	950.00	1150.00	955.00	1150.00	360.00	1073.33
2	780.00	2500.00	1260.00	1460.00	1265.00	1460.00	670.00	1280.00
3	0	0	280.00	480.00	280.00	480.00	0	0
4	1230.00	1340.00	1710.00	1900.00	1705.00	1900.00	1120.00	1100.00
5	0	0	0	0	5.00	70.00	0	0
6	3750.00	3333.33	3750.00	3333.33	3750.00	3333.33	3750.00	2500.00

A.3 Cut-Off Behavior

We now turn to the frequency of cut-off behavior. As we have seen in Result 3, cut-off behavior is much more frequent in the control treatment than in the strategic treatment for both group sizes. While it increases sharply in Games 5 and 6, as compared to Games 1-4, in the strategic treatments, it is still higher in the corresponding control treatments for either group size. In Game 5, this sharp increase is most likely due to the short duration of

[42] To get our MPE estimates, we assume each player hypothetically splitting each instant 50:50 between the two arms in the *free-riding region*, which, for the purpose of this table, we equate to the belief region $(p_1^*, \frac{p^\ddagger + \bar{p}}{2})$.

[43] Indeed, the equilibrium strategy in the matching-pennies game, for instance, while being an *ex ante* best response, will do rather poorly conditionally on a *particular realization* of the opponent's equilibrium strategy; by contrast, a pure strategy does strictly better than the equilibrium strategy given a particular realization of the opponent's equilibrium mixed strategy. Our game is no different in this respect. For example, in Game 4, the best course of action conditionally on the realizations of the random variables would have been to play safe throughout, even though *ex ante* "safe" is a dominated action at the start of the game. (Indeed, $p_0 > p^m$, so that even a myopic player should play risky.) By the same token, in Game 1, players should have switched to safe right after player 1 first obtained a success, had they known that the game ended before the second success would arrive.

that game. In Game 6, it is most likely driven by the resolution of uncertainty very early in the game, with Player 2 achieving a success after exploring for 9 seconds.

Table A.4: Frequency of Cut-Off Behavior by Game

	$n = 2$			$n = 3$		
		Strategic Treatment	Control Treatment		Strategic Treatment	Control Treatment
Game	Obs.	Tot. (Rel.) Freq.	Tot. (Rel.) Freq.	Obs.	Tot. (Rel.) Freq.	Tot. (Rel.) Freq.
1	20	0 (0)	15 (.75)	30	3 (.10)	21 (.70)
2	20	0 (0)	15 (.75)	30	3 (.10)	22 (.73)
3	20	5 (.25)	19 (.95)	30	11 (.37)	26 (.87)
4	20	0 (0)	14 (.70)	30	6 (.20)	19 (.63)
5	20	17 (.85)	20 (1)	30	17 (.57)	29 (.97)
6	20	13 (.65)	17 (.85)	30	19 (.63)	25 (.83)

Total number of cut-offs (number of cut-offs divided by total observations).
The number of observations refers to both strategic and control treatment.

We find the difference in the frequency of cut-off behavior between the two treatments to be highly statistically significant for Games 1-4, for both group sizes. All p-values are 0.0001 for Games 1-4, respectively, with the exception of Game 4 for $n = 3$ where the p-value amounts to 0.0007. In the last two games where we observe a sharp increase in cut-off behavior in the strategic treatment for the reasons outlined above, the corresponding p-values for $n = 2$ ($n = 3$) are 0.0754 (0.0003) and 0.1492 (0.0824) for Games 5 and 6, respectively.

When we use our "continuous" measure of cut-off behavior, differences across treatments are mostly highly statistically significant as well. Recall txhat this measure is defined as 1 minus the proportion of time in which a subject plays safe before ever playing risky, or plays risky after they had previously switched from risky to safe, before his risky arm is revealed to be good or the end of the game, whichever arrives first. For groups of size $n = 2$, the p-values are 0.0001 for Games 1-4, 0.0756 and 0.5675 for Games 5 and 6, respectively. For $n = 3$, the corresponding p-values are 0.0001, 0.0008, 0.0004, 0.0080, 0.0004, and 0.7674 for Games 1-6, respectively.

A.4 Pioneers

There is a range of beliefs containing $(p_1^*, p^\ddagger)$ such that safe and risky are mutually best responses in any Markov Perfect Equilibrium, so that there exists a range of beliefs in which just *one pioneer* should play risky in MPE while the other player(s) free-ride(s). By contrast, in the control treatment as well as in the best PBE, players are predicted to play risky on $(p_1^*, \frac{1}{2}]$. In this belief region, conditionally on no success arriving, players should switch from risky to safe only once, and do so at the same time, at which their beliefs reach p_1^*. At the game-level too, we confirm Result 4.

Table A.5: Proportion of Time with a Single Pioneer by Game

		$n = 2$			$n = 3$	
		Strategic Treatment	Control Treatment		Strategic Treatment	Control Treatment
Game	Obs.	Single Pioneer	Single Pioneer	Obs.	Single Pioneer	Single Pioneer
1	10	.724 [.156]	.284 [.258]	10	.670 [.178]	.097 [.156]
2	10	.708 [.176]	.315 [.254]	10	.425 [.352]	0 [0]
3	10	.745 [.156]	.187 [.253]	10	.563 [.348]	.136 [.256]
4	10	.757 [.175]	.294 [.214]	10	.741 [.171]	.249 [.198]
5	10	.581 [.360]	.029 [.092]	10	.361 [.304]	0 [0]
6	10	.288 [.399]	.078 [.246]	10	.219 [.369]	0 [0]

Average [st. dev.] proportion of time with a single pioneer in a group.
The number of observations refers to both strategic and control treatment.

As Table A.5 highlights, also at the individual game level, we can confirm for all games that the addition of one (two) perfectly positively correlated arm(s) leads to a much higher proportion of time where just one pioneer plays risky while the other remaining player(s) free-ride. This is highly statistically significant for all games in the three-player set-up and for Games 1-5, but not for Game 6, in the setting with $n = 2$. The corresponding p-values in the case of $n = 2$ are 0.0011, 0.0019, 0.0003, 0.0007, 0.0013, and 0.1494 for Games 1-6, respectively. In the setting with $n = 3$, the incidence of switches is also lower in the strategic treatment (p-values of 0.0002, 0.0006, 0.0026, 0.0003, 0.0019, and 0.0682 for Games 1 to 6, respectively). Recall that Game 6 is characterized by an early success for two players: after 9 seconds of exploration by Player 1 and after 39 and 44 seconds of exploration by Player 1.

A.5 Switches of Action

In any Markov Perfect Equilibrium, we should expect players to switch roles at least once. As theory predicts and Result 5 shows for the aggregate data, significantly more switches are observed in the strategic treatment than in the control treatment, for both group sizes. Recall that we have defined the incidence of switches as the number of a player's changes in action choice in a given game per unit of effective time.

Table A.6 displays the average number of switches per player across games for our four treatments. As in the main text, we perform our statistical tests on the average *incidence* (rather than the *number*) of switches, and find that the average incidence of switches in the strategic treatment is much higher than in the control treatment in all games (for $n = 2$ with p-values of 0.0001, 0.0003, 0.0001, 0.0002, 0.0019, and 0.1352 for Games 1-6, respectively; in the $n = 3$ setting with p-values are of 0.0040, 0.0005, 0.0073, 0.0336, 0.0018, and 0.3526 for Games 1-6, respectively). Here again, the early success in Game 6 reveals the risky arm to be good and thus resolves all uncertainty at the very beginning of the game.

Table A.6: Average Number of Switches per Player by Game

	$n = 2$			$n = 3$		
		Strategic Treatment	Control Treatment		Strategic Treatment	Control Treatment
Game	Obs.	Switches Per Pl.	Switches Per Pl.	Obs.	Switches Per Pl.	Switches Per Pl.
1	10	4.45 [1.74]	.90 [.66]	10	3.40 [1.77]	1.13 [1.23]
2	10	4.50 [1.87]	1.35 [1.13]	10	2.77 [1.65]	.97 [.81]
3	10	2.20 [1.03]	.30 [.42]	10	1.73 [1.14]	.47 [.69]
4	10	6.05 [1.57]	1.85 [1.56]	10	4.00 [2.82]	1.7 [1.63]
5	10	.60 [.39]	.05 [.16]	10	.70 [.73]	.03 [.11]
6	10	.60 [.74]	.30 [.54]	10	.97 [1.29]	.37 [.55]

Average [st. dev.] switches per player using group averages.
The number of observations refers to both strategic and control treatment.

A.6 Eye-Tracking Data by Game

Players in the strategic treatment focus much more intensively on their partners' actions and payoffs. Also at the individual game-level, our eye-tracking data further confirms that players were indeed paying attention to the additional information their partner(s) provided them, a necessary condition for free-riding. By contrast, in the corresponding control treatments, where the information generated by their partners is of no value as the risky arms are uncorrelated, subjects seemed to focus almost exclusively on their own stream of payoffs, thus confirming our theoretical prediction according to which a rational player should completely ignore a partner's actions and payoffs in the control treatments.

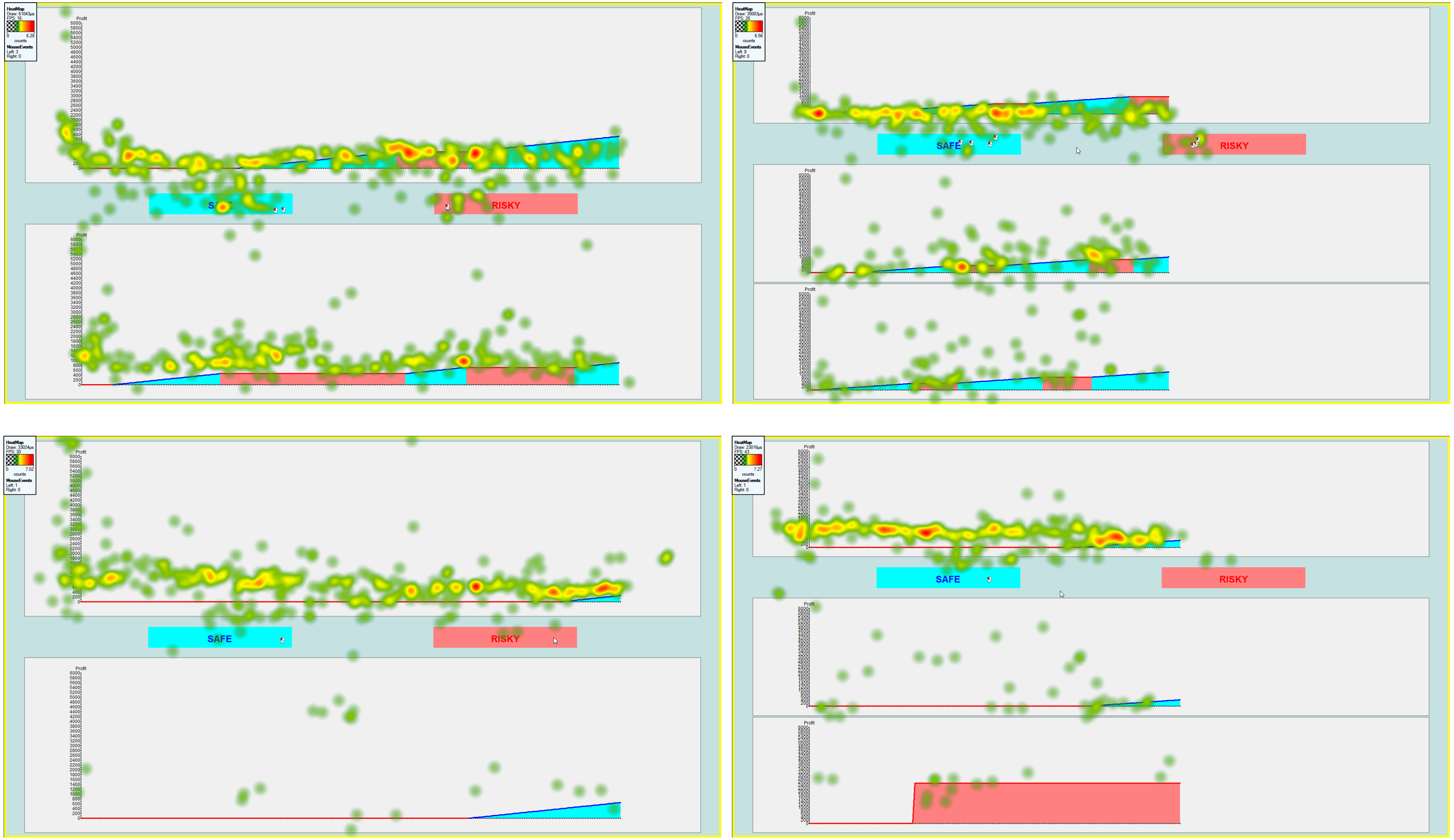
Table A.7: Average Fixation Intensity by Game

	$n = 2$			$n = 3$		
		Strategic Treatment	Control Treatment		Strategic Treatment	Control Treatment
Game	Obs.	Fixation Intensity	Fixation Intensity	Obs.	Fixation Intensity	Fixation Intensity
1	10	.620 [.066]	.870 [.046]	10	.384 [.080]	.710 [.091]
2	10	.620 [.099]	.882 [.085]	10	.365 [.069]	.709 [.119]
3	10	.600 [.050]	.874 [.105]	10	.392 [.079]	.762 [.065]
4	10	.615 [.047]	.875 [.116]	10	.389 [.094]	.700 [.091]
5	10	.633 [.116]	.876 [.105]	10	.383 [.089]	.745 [.129]
6	10	.594 [.125]	.814 [.073]	10	.382 [.070]	.646 [.111]

Average [st. dev.] fixation intensity using group averages.
The number of observations refers to both strategic and control treatment.

As Table A.7 highlights, the average fixation intensity using group averages is significantly lower in the strategic treatment, irrespective of the group size. This is highly statistically significant for all six games for both group sizes. For $n = 2$ ($n = 3$) the corresponding p-values are 0.0002 (0.0002), 0.0002 (0.0002), 0.0002 (0.0002), 0.0015 (0.0002), 0.0007 (0.0002), 0.0009 (0.0003) for Games 1-6, respectively.

Figure A.6 displays (non-representative) heatmaps to illustrate the different information acquisition behavior in our four treatments. The measure of interest is the total number of fixations. For each heatmap, the accumulated number of fixations is calculated for an entire game and the image corresponds to the last point in calendar time before the game ends. A color gradient is employed to display the areas that attained more fixations (low=green to high=red). As Figure A.6 illustrates, players not only switch actions more frequently in the strategic treatment but also focus much more intensively on their partners' actions and payoffs. This is in sharp contrast to the corresponding control treatment, where players seem to focus almost exclusively on their own streams of payoffs.



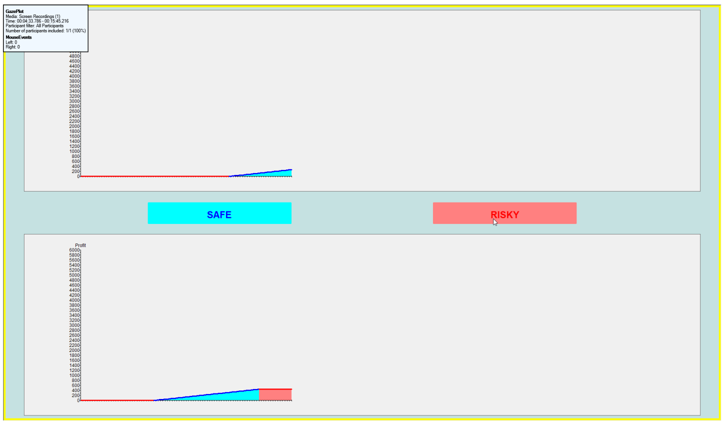
In the top-left corner, the strategic treatment with $n = 2$ is illustrated, with the corresponding control treatment represented just below. In the top-right corner, the strategic treatment with $n = 3$ is displayed, while the control treatment with $n = 3$ is shown at the bottom-right. All four heatmaps show the total number of fixations. The accumulated number of fixations is calculated for an entire game (Game 4 in the $n = 2$ set-up and Game 2 in the $n = 3$ set-up). Each fixation made has the same value and is indepentent of its duration. A color gradient is used to indicate the areas with more fixations (low=green to high=red).

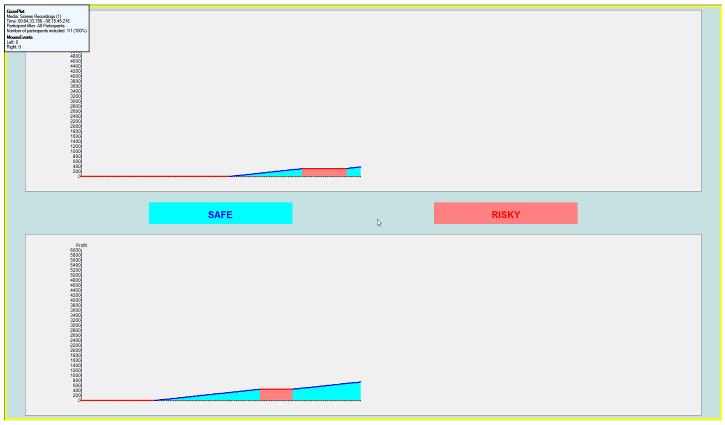
Figure A.6: Heatmaps of Four Treatments

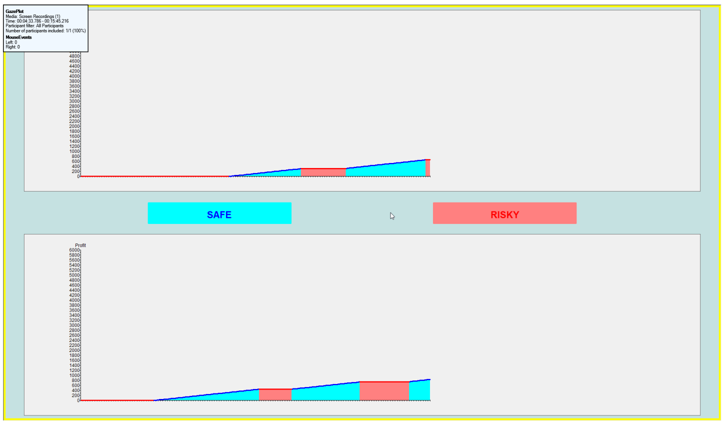
B Interfaces

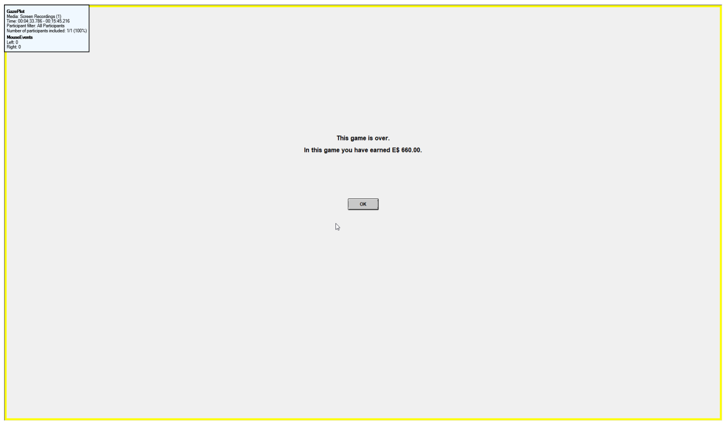
In this Appendix, we exhibit examples of the interfaces subjects saw during the game, showing the evolution of the screen over intervals of 30 seconds. In the top half (third) of his screen, a subject could see his own past actions and payoffs, while the bottom half (two thirds) of the screen showed his fellow group member's (or members') actions and payoffs. A blue (red) part of the payoff curve indicated that the player used the safe (risky) arm over the corresponding period. The x-axis represented calendar time, while the y-axis gave the player's cumulated total earnings up to each point in time. There was no prior indication of the point in time the game would end.

n = 2 Strategic Set-up: Example for Game 1

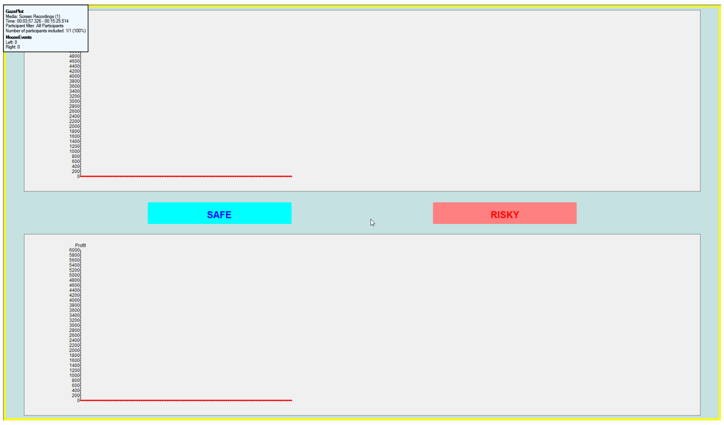

SAFE
RISKY

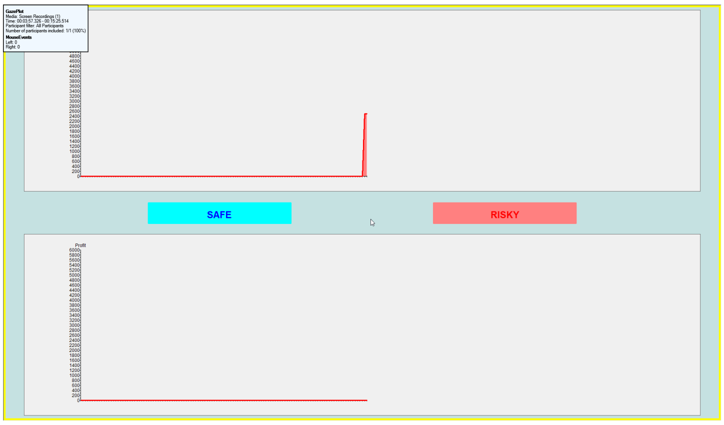

SAFE
RISKY

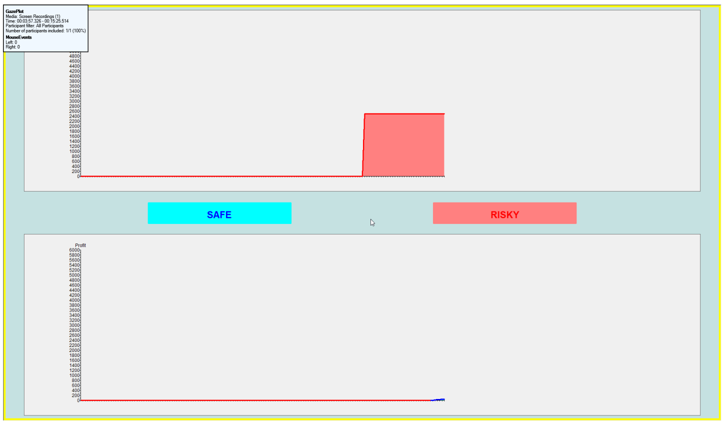

SAFE
RISKY

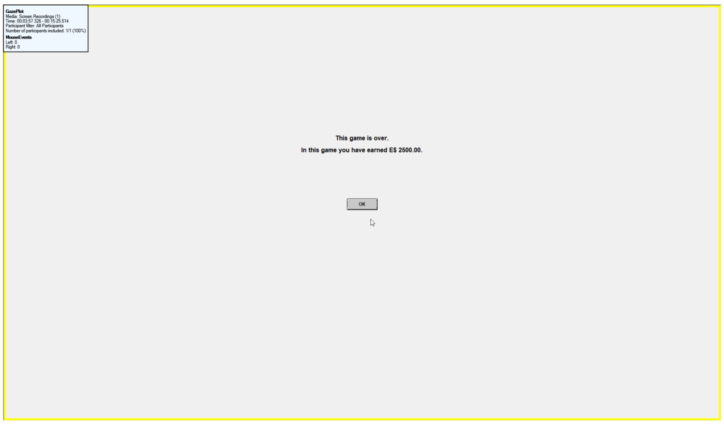

GazePlot
Media: Screen Recordings (1)
Time: 00:04:33.786 - 00:15:45.216
Participant filter: All Participants
Number of participants included: 1/1 (100%)
MouseEvents
Left: 0
Right: 0
This game is over.
In this game you have earned E$ 660.00.
OK

n = 2 Control Set-up: Example for Game 1

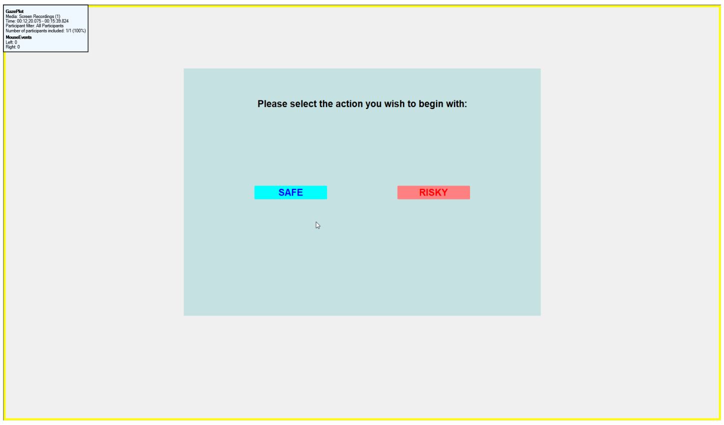

SAFE
RISKY
Profit

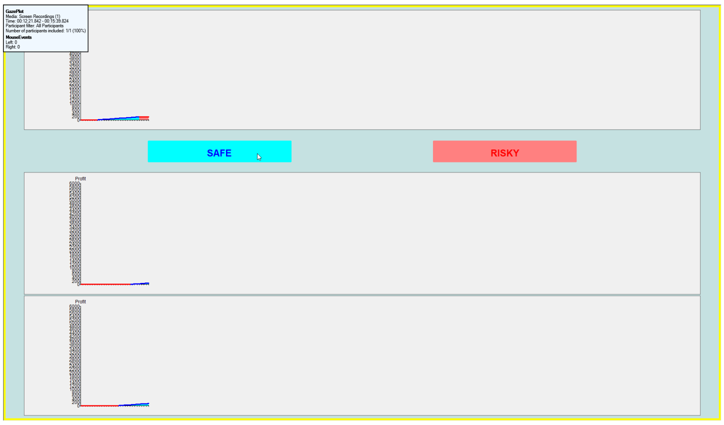

SAFE
RISKY
Profit

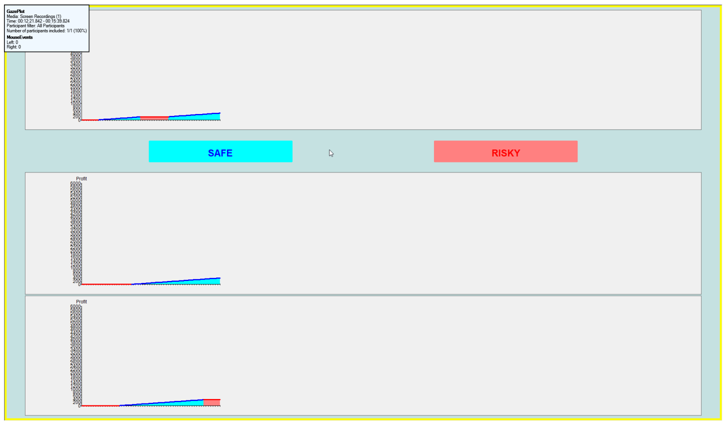

SAFE
RISKY
Profit


This game is over.
In this game you have earned E$ 2500.00.
OK

n = 3 Strategic Set-up: Example for Game 2

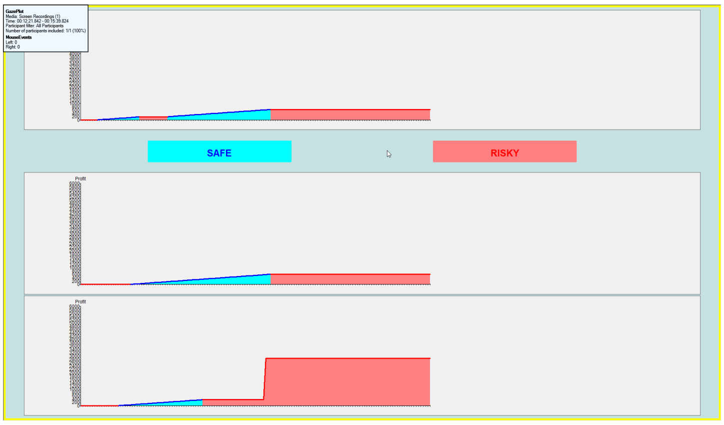


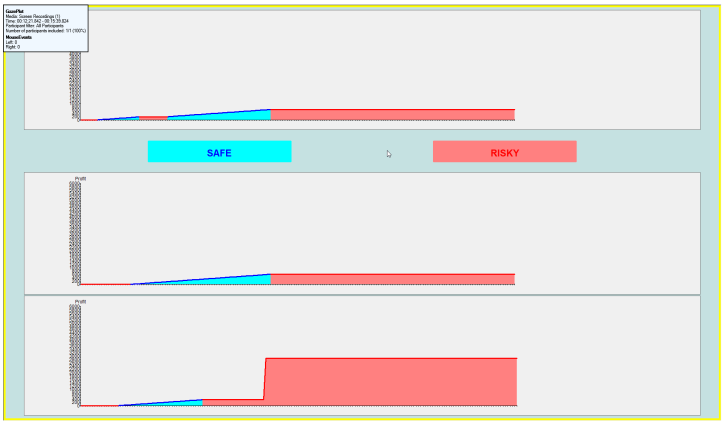


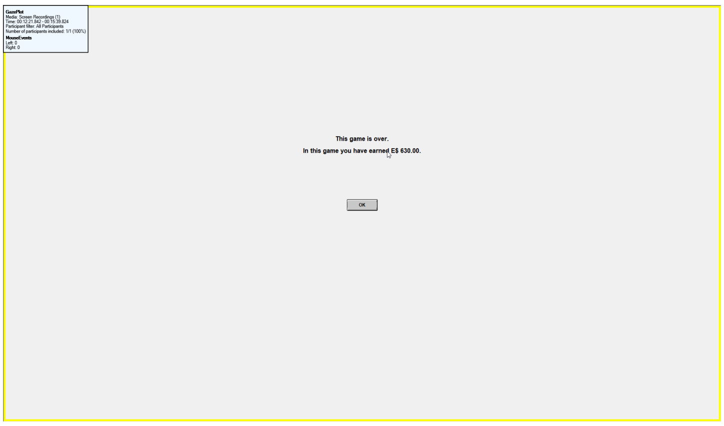



SAFE
RISKY

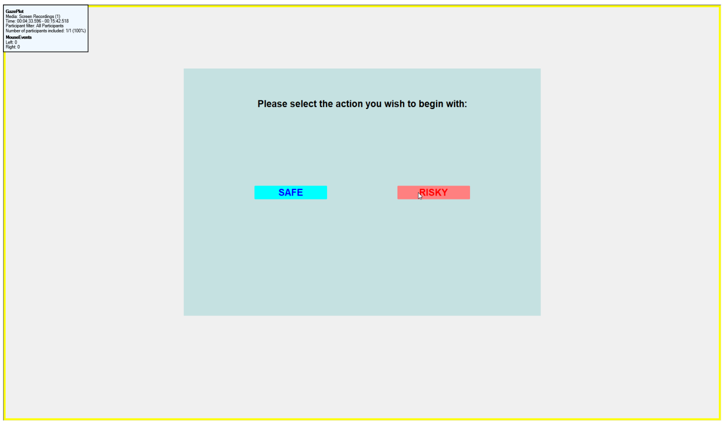

SAFE
RISKY

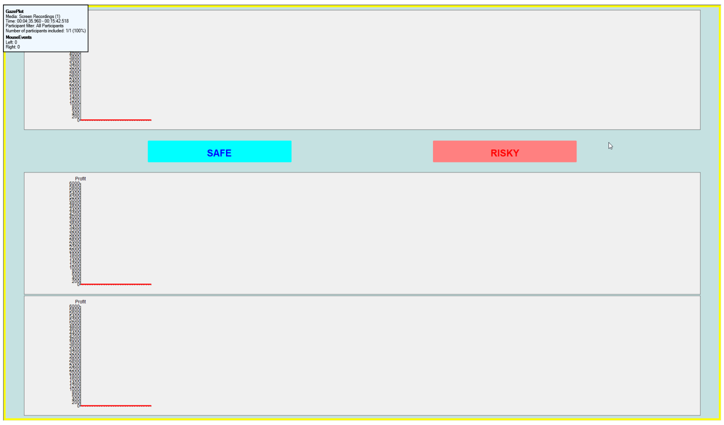

SAFE
RISKY

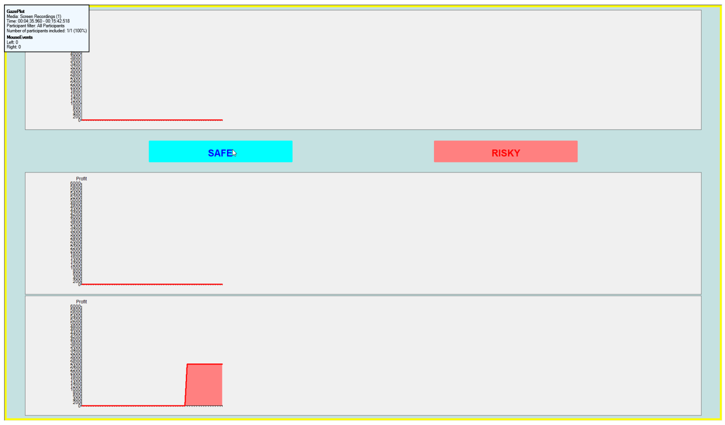

SAFE
RISKY

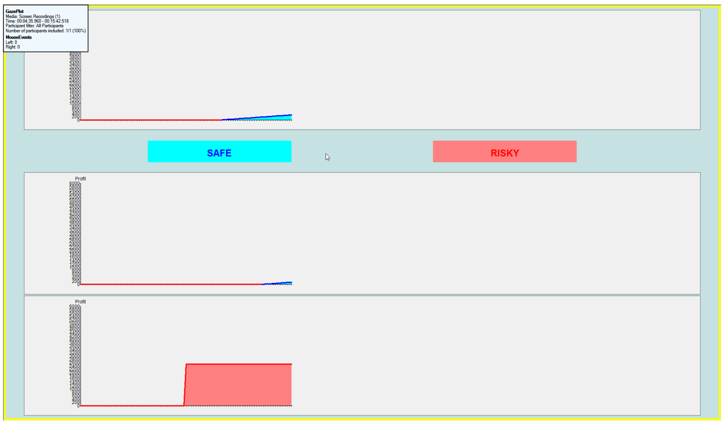

This game is over.
In this game you have earned E$ 630.00.
OK

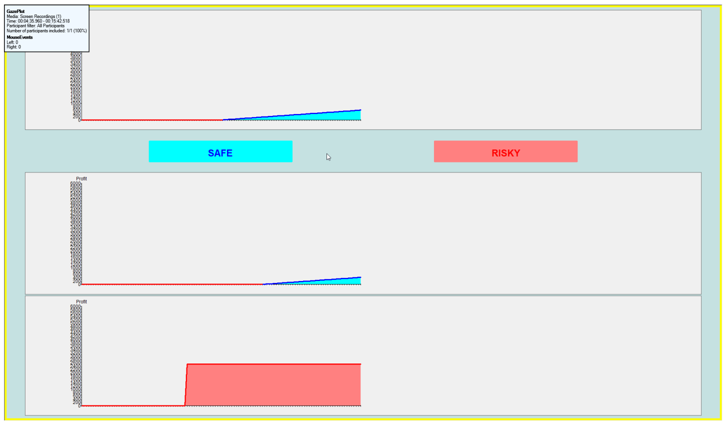
n = 3 Control Set-up: Example for Game 2

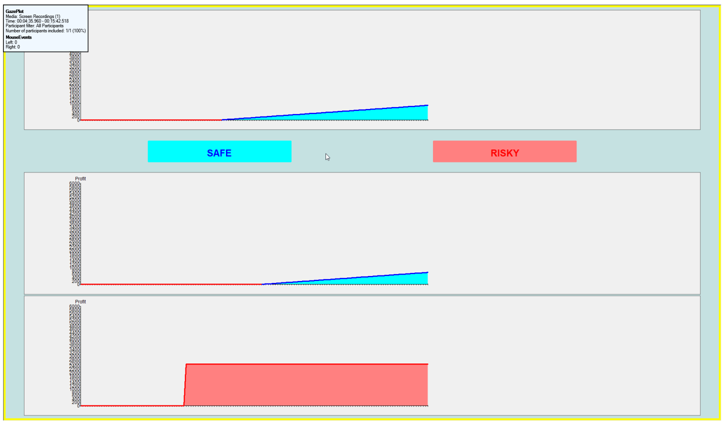


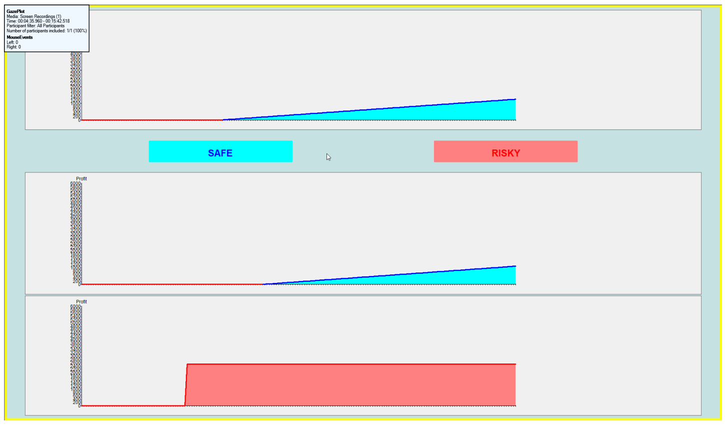


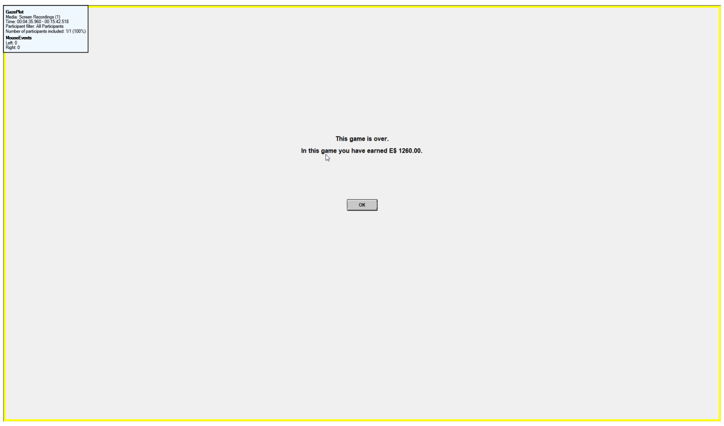



SAFE
RISKY


SAFE
RISKY


SAFE
RISKY


SAFE
RISKY


This game is over.
In this game you have earned E$ 1260.00.
OK

C Instructions

The order of the instructions is as follows:

1. $n = 2$: Strategic Treatment
2. $n = 2$: Control Treatment
3. $n = 3$: Strategic Treatment
4. $n = 3$: Control Treatment

Experiment Instructions

Ground Rules

Welcome to the experiment. Please read the instructions carefully. The earnings you make in this experiment will be paid to you, in cash, at the end of the session.

Your earnings will be determined by your choices and the choices of other participants.

Communication between participants is not allowed. Please use only the computer to input your decisions. Please do not start or end any programs, and do not change any settings.

How Groups are Organized

This experiment consists of six games in total. In the beginning of the first game, participants are randomly matched to pairs and the pairs stay the same in all six games. Therefore, in each game you will interact with the same participant.

How the Timing Works

Games will last on average ***120 seconds*** but may end at any time. The probability that the game ends is the same at each instant. Equivalently, the probability that the game ends during a given period of time depends only on the length of that period of time, and not on how long the game has already been going on. (Such processes are known as *exponential processes* in statistics.)

How the Game Works

In every game, you have to decide whether you want to play the "**safe**" or the "**risky**" option. You can switch between the two options at any time and as often as you like by clicking on the safe (Blue) or risky (Red) button on the screen.

Whenever you choose the **safe** option, your payoff will increase for sure at the rate ***E$ 10***. That means the **safe** option will give you a reward of ***E$ 10*** every second during which you use it.

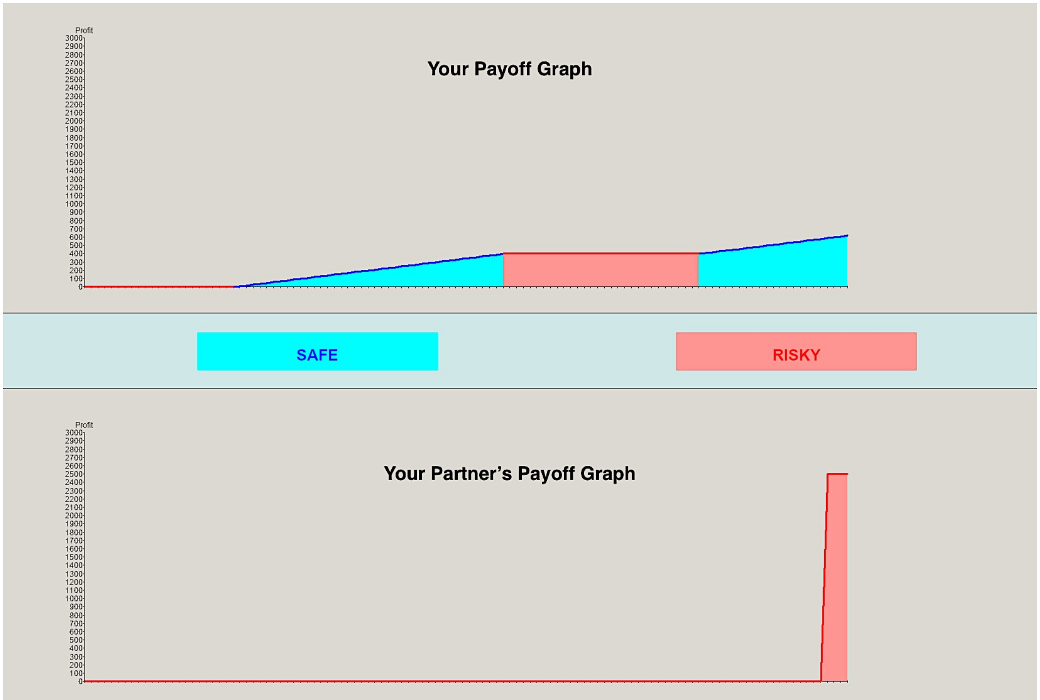
When you choose the **risky** option, however, what you will be getting depends on the quality of that risky option. The quality of the **risky** option is determined by the computer once and for all at the start of each game; it never changes during the course of the game. We have programmed the computer so that the risky option will be **good** or **bad** with equal probability in each of the six games. The quality of the risky option in later games is independent of its quality in previous games. That is, in each of your six games, with probability ½ your risky option will be **good**; with probability ½ it will be **bad**. The same is true for your partner. Note that your risky option and that of your partner's might or might not be of the same quality.

If your risky option is **good**, it may give you a reward of ***E$ 2500***, but it will only ever do so if you use it. A good risky option yields such a reward after using it on average for 100 seconds. The probability that you get this reward from a good risky option during a given period of time during which you use it depends only on the length of that period of time; it does not depend on anything else, e.g. on how long the game has already been going on. Note that a good risky option may give you more than one reward of *E$ 2500* per game.

If your risky option is **bad**, it will never give you any reward.

You can switch back and forth between the risky option and the safe option at will and as many times as you like. All that matters for your chance of getting the reward is (1) the quality of the risky arm as determined by the computer before the game starts and (2) the overall amount of time you choose to spend on it.

The following graphic illustrates what you are going to see on your screen during the game. The graphs will be updated every second.



- The **upper** diagram always shows **your** actions and payoffs.
- In this example, you have started playing the risky option (highlighted in Red), then you have switched to the safe option (highlighted in Blue), then you have switched back again to the risky option, etc.
- The **lower** diagram always shows **your partner's** actions and payoffs.
- In this example, your partner has started playing the risky option and continues to do so.
- Note that, in this example, your partner's risky option was good and gave him once a reward of ***E$ 2500***.

The parameters are chosen in such a way that, *if you knew* the risky option to be good, you would be best off by **always** choosing it. Yet, *if you knew* the risky option to be bad, you would be best off by **always** choosing the safe option. In short:

Good risky option > Safe option > **Bad** risky option

Your partner is solving the exact same problem as you and has read the exact same instructions.

Payment

In the experiment you will be making decisions that will earn you E$ (Experimental Dollars). At the end of the experiment, the E$ you earned will be converted into Australian Dollars at an exchange rate of E$ 100 = AU$ 1, and paid out in cash. This amount will be added to your show-up fee of AU$ 5.

After completing the experiment, the computer will randomly select one out of the six games (this will be the same game for all participants), and this game will then be used to determine your payoffs.

Experiment Instructions

Ground Rules

Welcome to the experiment. Please read the instructions carefully. The earnings you make in this experiment will be paid to you, in cash, at the end of the session.

Your earnings will be determined by your choices and the choices of other participants.

Communication between participants is not allowed. Please use only the computer to input your decisions. Please do not start or end any programs, and do not change any settings.

How Groups are Organized

This experiment consists of six games in total. In the beginning of the first game, participants are randomly matched to pairs and the pairs stay the same in all six games. Therefore, in each game you will interact with the same participant.

How the Timing Works

Games will last on average ***120 seconds*** but may end at any time. The probability that the game ends is the same at each instant. Equivalently, the probability that the game ends during a given period of time depends only on the length of that period of time, and not on how long the game has already been going on. (Such processes are known as *exponential processes* in statistics.)

How the Game Works

In every game, you have to decide whether you want to play the "**safe**" or the "**risky**" option. You can switch between the two options at any time and as often as you like by clicking on the safe (Blue) or risky (Red) button on the screen.

Whenever you choose the **safe** option, your payoff will increase for sure at the rate ***E$ 10***. That means the **safe** option will give you a reward of ***E$ 10*** every second during which you use it.

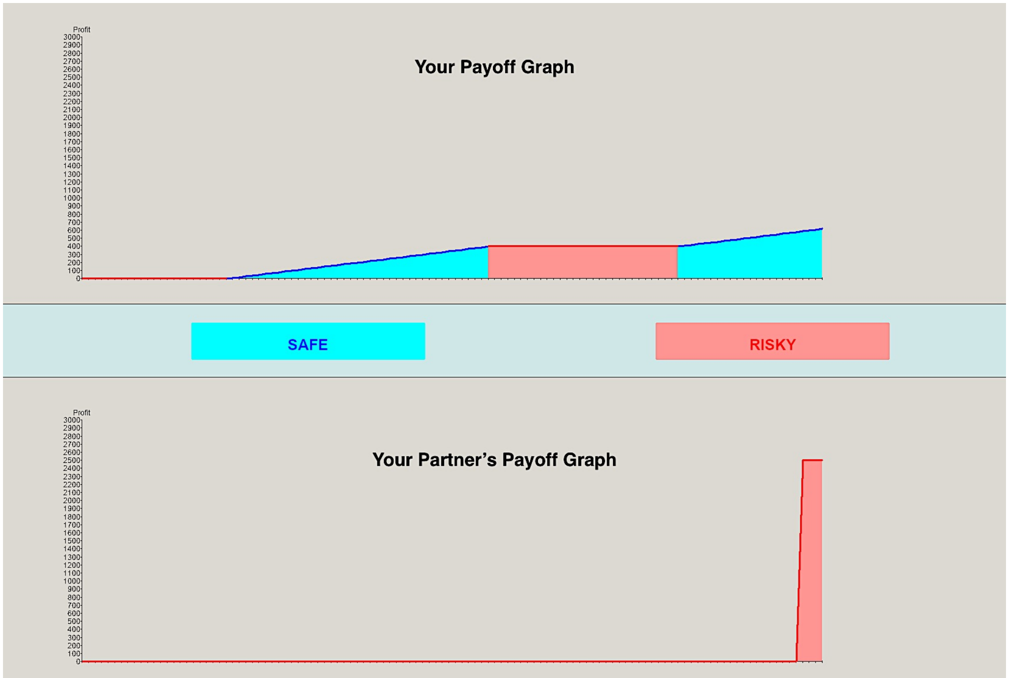
When you choose the **risky** option, however, what you will be getting depends on the quality of that risky option. The quality of the **risky** option is determined by the computer once and for all at the start of each game; it never changes during the course of the game. We have programmed the computer so that the risky option will be **good** or **bad** with equal probability in each of the six games. The quality of the risky option in later games is independent of its quality in previous games. That is, in each of your six games, with probability ½ your (and your partner's!) risky option will be **good**; with probability ½ they will be **bad**. Note that your risky option and that of your partner's will always be of the **same** quality.

If your risky option is **good**, it may give you a reward of ***E$ 2500***, but it will only ever do so if you use it. A good risky option yields such a reward after using it on average for 100 seconds. The probability that you get this reward from a good risky option during a given period of time during which you use it depends only on the length of that period of time; it does not depend on anything else, e.g. on how long the game has already been going on. Note that a good risky option may give you more than one reward of *E$ 2500* per game.

If your risky option is **bad**, it will never give you any reward.

You can switch back and forth between the risky option and the safe option at will and as many times as you like. All that matters for your chance of getting the reward is (1) the quality of the risky arm as determined by the computer before the game starts and (2) the overall amount of time you choose to spend on it.

The following graphic illustrates what you are going to see on your screen during the game. The graphs will be updated every second.



- The **upper** diagram always shows **your** actions and payoffs.
- In this example, you have started playing the risky option (highlighted in Red), then you have switched to the safe option (highlighted in Blue), then you have switched back again to the risky option, etc.
- The **lower** diagram always shows **your partner's** actions and payoffs.
- In this example, your partner has started playing the risky option and continues to do so.
- Note that, in this example, your partner's risky option was good and gave him once a reward of ***E$ 2500***. This means that your risky option was good too.

The parameters are chosen in such a way that, *if you knew* the risky option to be good, you would be best off by **always** choosing it. Yet, *if you knew* the risky option to be bad, you would be best off by **always** choosing the safe option. In short:

Good risky option > Safe option > **Bad** risky option

Your partner is solving the exact same problem as you and has read the exact same instructions. Note that by observing the behaviour of his risky option (provided he uses it) you can learn something about your risky option as well.

Payment

In the experiment you will be making decisions that will earn you E$ (Experimental Dollars). At the end of the experiment, the E$ you earned will be converted into Australian Dollars at an exchange rate of E$ 100 = AU$ 1, and paid out in cash. This amount will be added to your show-up fee of AU$ 5.

After completing the experiment, the computer will randomly select one out of the six games (this will be the same game for all participants), and this game will then be used to determine your payoffs.

Experiment Instructions

Ground Rules

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Your earnings will be determined by your choices and the choices of other participants.

Communication between participants is not allowed. Please use only the computer to input your decisions. Please do not start or end any programs, and do not change any settings.

How Groups are Organized

This experiment consists of six games in total. In the beginning of the first game, participants are randomly matched to groups of three players and the groups stay the same in all six games. Therefore, in each game you will interact with the same participants.

How the Timing Works

Games will last on average ***120 seconds*** but may end at any time. The probability that the game ends is the same at each instant. Equivalently, the probability that the game ends during a given period of time depends only on the length of that period of time, and not on how long the game has already been going on. (Such processes are known as *exponential processes* in statistics.)

How the Game Works

In every game, you have to decide whether you want to play the "**safe**" or the "**risky**" option. You can switch between the two options at any time and as often as you like by clicking on the safe (Blue) or risky (Red) button on the screen.

Whenever you choose the **safe** option, your payoff will increase for sure at the rate ***E$ 10***. That means the **safe** option will give you a reward of ***E$ 10*** every second during which you use it.

When you choose the **risky** option, however, what you will be getting depends on the quality of that risky option. The quality of the **risky** option is determined by the computer once and for all at the start of each game; it never changes during the course of the game. We have programmed the computer so that the risky option will be **good** or **bad** with equal probability in each of the six games. The quality of the risky option in later games is independent of its quality in previous games. That is, in each of your six games, with probability ½ your risky option will be **good**; with probability ½ it will be **bad**. The same is true for your partners. Note that your risky option and that of your partners' might or might not be of the same quality.

If your risky option is **good**, it may give you a reward of ***E$ 2500***, but it will only ever do so if you use it. A good risky option yields such a reward after using it on average for 100 seconds. The probability that you get this reward from a good risky option during a given period of time during which you use it depends only on the length of that period of time. It does not depend on anything else, e.g. on how long the game has already been going on. Note that a good risky option may give you more than one reward of *E$ 2500* per game.

If your risky option is **bad**, it will never give you any reward.

You can switch back and forth between the risky option and the safe option at will and as many times as you like. All that matters for your chance of getting the reward is (1) the quality of the risky arm as determined by the computer before the game starts and (2) the overall amount of time you choose to spend on it.

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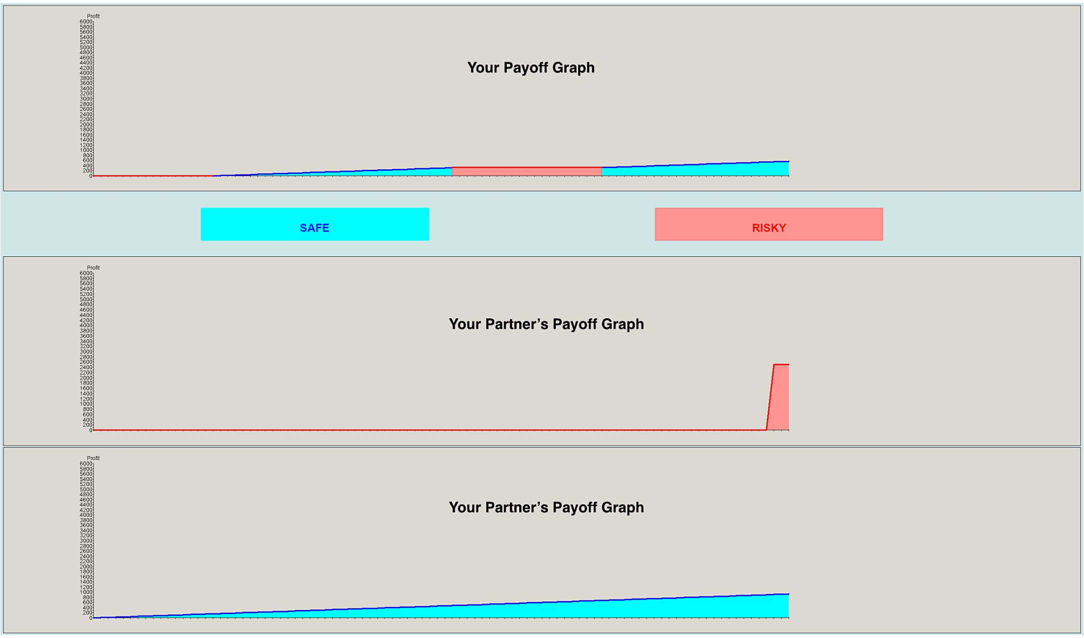

- The **upper** diagram always shows **your** actions and payoffs.
- In this example, you have started playing the risky option (highlighted in Red), then you have switched to the safe option (highlighted in Blue), then you have switched back again to the risky option, etc.
- The **lower** diagram always shows **your partner's** actions and payoffs.
- In this example, one of your partners has started playing the risky option and continues to do so. The other partner has started and continues playing the safe option.
- Note that, in this example, at least one of your partner's risky option was good and gave him once a reward of ***E$ 2500***.

The parameters are chosen in such a way that, *if you knew* the risky option to be good, you would be best off by **always** choosing it. Yet, *if you knew* the risky option to be bad, you would be best off by **always** choosing the safe option. In short:

Good risky option > Safe option > **Bad** risky option

Your partners are solving the exact same problem as you and have read the exact same instructions.

Payment

In the experiment you will be making decisions that will earn you E$ (Experimental Dollars). At the end of the experiment, the E$ you earned will be converted into Australian Dollars at an exchange rate of E$ 100 = AU$ 1, and paid out in cash. This amount will be added to your show-up fee of AU$ 5.

After completing the experiment, the computer will randomly select one out of the six games (this will be the same game for all participants), and this game will then be used to determine your payoffs.

Experiment Instructions

Ground Rules

Welcome to the experiment. Please read the instructions carefully. The earnings you make in this experiment will be paid to you, in cash, at the end of the session.

Your earnings will be determined by your choices and the choices of other participants.

Communication between participants is not allowed. Please use only the computer to input your decisions. Please do not start or end any programs, and do not change any settings.

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This experiment consists of six games in total. In the beginning of the first game, participants are randomly matched to groups of three players and the groups stay the same in all six games. Therefore, in each game you will interact with the same participants.

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How the Game Works

In every game, you have to decide whether you want to play the "**safe**" or the "**risky**" option. You can switch between the two options at any time and as often as you like by clicking on the safe (Blue) or risky (Red) button on the screen.

Whenever you choose the **safe** option, your payoff will increase for sure at the rate ***E$ 10***. That means the **safe** option will give you a reward of ***E$ 10*** every second during which you use it.

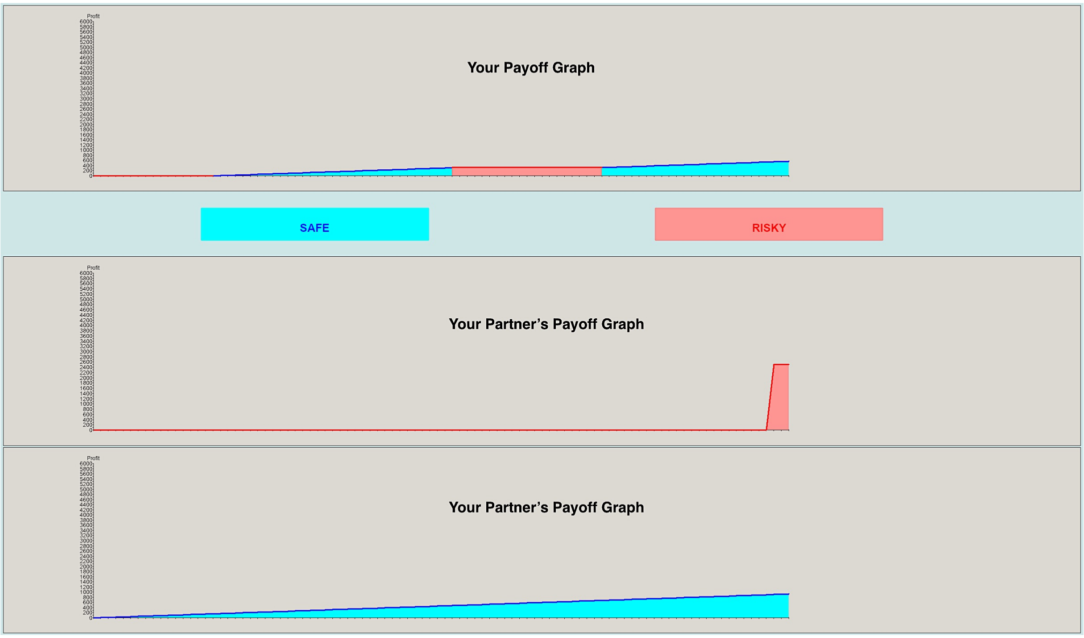
When you choose the **risky** option, however, what you will be getting depends on the quality of that risky option. The quality of the **risky** option is determined by the computer once and for all at the start of each game; it never changes during the course of the game. We have programmed the computer so that the risky option will be **good** or **bad** with equal probability in each of the six games. The quality of the risky option in later games is independent of its quality in previous games. That is, in each of your six games, with probability ½ your (and your partners'!) risky option will be **good**; with probability ½ they will be **bad**. Note that your and your partners' risky option will always be of the **same** quality.

If your risky option is **good**, it may give you a reward of ***E$ 2500***, but it will only ever do so if you use it. A good risky option yields such a reward after using it on average for 100 seconds. The probability that you get this reward from a good risky option during a given period of time during which you use it depends only on the length of that period of time. It does not depend on anything else, e.g. on how long the game has already been going on. Note that a good risky option may give you more than one reward of *E$ 2500* per game.

If your risky option is **bad**, it will never give you any reward.

You can switch back and forth between the risky option and the safe option at will and as many times as you like. All that matters for your chance of getting the reward is (1) the quality of the risky arm as determined by the computer before the game starts and (2) the overall amount of time you choose to spend on it.

The following graphic illustrates what you are going to see on your screen during the game. The graphs will be updated every second.



- The **upper** diagram always shows **your** actions and payoffs.
- In this example, you have started playing the risky option (highlighted in Red), then you have switched to the safe option (highlighted in Blue), then you have switched back again to the risky option, etc.
- The **lower** diagram always shows **your partners'** actions and payoffs.
- In this example, one of your partners has started playing the risky option and continues to do so. The other partner has started and continues playing the safe option.
- Note that, in this example, your partner's risky option was good and gave him once a reward of ***E$ 2500***. This means that your risky option was good too.

The parameters are chosen in such a way that, *if you knew* the risky option to be good, you would be best off by **always** choosing it. Yet, *if you knew* the risky option to be bad, you would be best off by **always** choosing the safe option. In short:

Good risky option > Safe option > **Bad** risky option

Your partners are solving the exact same problem as you and have read the exact same instructions. Note that by observing the behaviour of their risky option (provided they use it) you can learn something about your risky option as well.

Payment

In the experiment you will be making decisions that will earn you E$ (Experimental Dollars). At the end of the experiment, the E$ you earned will be converted into Australian Dollars at an exchange rate of E$ 100 = AU$ 1, and paid out in cash. This amount will be added to your show-up fee of AU$ 5.

After completing the experiment, the computer will randomly select one out of the six games (this will be the same game for all participants), and this game will then be used to determine your payoffs.