

# Bandits in the Lab<sup>\*</sup>

Johannes Hoelzemann<sup>†</sup>      Nicolas Klein<sup>‡</sup>

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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. We find strong support for the prediction of 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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<sup>†</sup>University of Toronto. Mailing address: University of Toronto, Department of Economics, Max Gluskin House, 150 St. George Street; Toronto, ON M5S 2E9, Canada. e-mail: j.hoelzemann@utoronto.ca.

<sup>‡</sup>Université de Montréal and CIREQ. Mailing address: Université de Montréal, Département de Sciences Économiques, C.P. 6128 succursale Centre-ville; Montréal, H3C 3J7, Canada. e-mail: kleinnic@yahoo.com.

# 1 Introduction

Economists have long been concerned with social dilemmas related to the production of public goods. Indeed, 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).<sup>1</sup>

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 good in question is information. By contrast, these situations are the object of the so-called strategic multi-armed bandit problems, which have received a lot of attention in the recent theoretical literature. In this paper, we propose, to the best of our knowledge, the first experimental investigation of subjects' willingness to contribute to a *dynamically evolving* public good.

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,<sup>2</sup> 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 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

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<sup>1</sup>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).

<sup>2</sup>We are indebted to an anonymous referee for this example.

especially has since been widely used to analyze a wide array of applications, such as, for instance, R&D races.<sup>3</sup>

Thus, the object of this paper is to analyze subjects' behavior when they are asked to contribute to a *dynamically evolving* public good, such as information. 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 (2019) (subsequently: HKR) have shown, the welfare-optimal equilibrium<sup>4</sup> 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<sup>5</sup> (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 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

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<sup>3</sup>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.

<sup>4</sup>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  $\Delta$ , there exists a PBE (which happens to be strongly symmetric) with payoffs that, as  $\Delta \rightarrow 0$ , 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.

<sup>5</sup>A cut-off strategy is defined by a unique threshold belief above which it prescribes risky play, while prescribing safe play below it.

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: the average experimentation intensity is lower, and subjects' payoffs are higher, in the strategic treatment. Secondly, we find evidence that, in the strategic treatment, subjects are adopting more sophisticated behaviors 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. Moreover, there is a larger proportion of time during which exactly one player is playing risky in the strategic treatment. 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. Indeed, subjects should pay attention to their partners' experimentation, as it gives them an additional signal for *their own* decision problem. Eye-tracking technology strongly indicates that subjects indeed understood the value of monitoring their partners' experimentation.

We interpret our findings as showing that, while understanding the informational externality, subjects were *not* behaving, or aiming to behave, as in the best PBE. Incidentally, the behaviors we document are consistent with the qualitative predictions of *any* of KRC's (infinitely many) MPEs, which feature turn-taking 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 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.<sup>6</sup>

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 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

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<sup>6</sup>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.

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 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.

Rabanal, Chernulich, Horowitz, Rud, and Sharifova (2019) 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 that 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. Battaglini, Nunnari, and Palfrey (2016) investigate a sequential stage game of contributions to a durable public good in the laboratory. They find that subjects' qualitative behavior is by and large consistent with the predictions of the Markov perfect equilibrium, although they find some evidence of non-Markovian history dependencies. Oprea, Charness, and Friedman (2014) study experimentally a standard public-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.

### 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  $\tau$  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  $i \in \{1, \dots, n\}$  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  $k_i : [0, 1] \rightarrow \{0, 1\}$ ,  $p \mapsto k_i(p)$ , where a player's action after any history can be written as a time-invariant function of the common belief at that history.<sup>7</sup> 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, which highlights the incentives for free-riding that arise due to the nature of the informational social dilemma. 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.<sup>8</sup> HKR extend the 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  $r$ .<sup>9</sup> With one

<sup>7</sup>In KRC, Markov strategies are actually defined as functions  $k_i : [0, 1] \rightarrow [0, 1]$ . 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.

<sup>8</sup>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 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.

<sup>9</sup>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.



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 mutually best responses, before entering the region where safe is dominant. The realizations of all random processes were simulated ahead of time.<sup>10</sup> 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.<sup>11</sup> Participants' interfaces (see Appendix B) were updated every second.<sup>12</sup>

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 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  $k$  ( $k \in \{1, \dots, 10\}$ ) 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.<sup>13</sup>

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.<sup>14</sup> After the subjects had successfully completed a simple compre-

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<sup>10</sup>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.

<sup>11</sup>Details are available from the authors upon request.

<sup>12</sup>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.

<sup>13</sup>Please see Appendix B for more details and screen shots.

<sup>14</sup>The instructions handed out to all participants can be found in Appendix C.

hension 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.<sup>15</sup> 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

One of the main theoretical predictions is that players use the risky arm less in a strategic setting than in a situation in which they are single players. This is because players *free-ride* on the information their partners are producing. Indeed, the best PBE (as well as all MPEs with a finite number of switches) predict players to play safe at all beliefs  $p \leq p_1^*$  in all these instances, 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.<sup>16</sup> By the same token, we can derive an upper bound  $\bar{p}$  on the lowest belief at which risky is a dominant action.<sup>17</sup>

With our numerical parameters,  $p^m = 0.4$ ,  $\bar{p} \approx 0.3578$  ( $\bar{p} \approx 0.3742$ ) if  $n = 2$  ( $n = 3$ ),  $p^\ddagger \approx 0.3428$  ( $p^\ddagger \approx 0.3609$ ) if  $n = 2$  ( $n = 3$ ),  $p_1^* \approx 0.2326$ ,  $p_2^* \approx 0.1031$ , and  $p_3^* \approx 0.0535$ . 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.

<sup>15</sup>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.

<sup>16</sup>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 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  $V_{n,p_1^*}(p^\ddagger) - s - (n-1)c(p^\ddagger) = 0$ .

<sup>17</sup>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  $V_1^*(\bar{p}) - s - (n-1)c(\bar{p}) = 0$ .

### 4.2.1 Free-Riding

Let  $\hat{T}$  be the time of a first breakthrough 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 the faster decrease in beliefs in the absence of a breakthrough leads to a greater proportion of time spent playing safe, we should expect a lower experimentation intensity in the strategic treatment. As the best PBE maximizes overall experimentation among all equilibria, we should expect the effect to be the stronger if a different equilibrium were played. We thus formulate the following

**Hypothesis 1.** *Subjects free-ride, i.e., the average experimentation intensity  $\frac{\int_0^{\hat{T}} \sum_{i=1}^n k_{i,t} dt}{n\hat{T}}$  is lower in the strategic treatment than in the control treatment.*

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 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 (control and strategic 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 Eye-Tracking Data

Before analyzing our hypotheses concerning free-riding and the structural properties of the best PBE, we investigate to what extent subjects took the strategic nature of the interaction into account in their decision making in the strategic treatment. To do so, we study directly how much heed they 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 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.<sup>18</sup>

Table 1: Average Fixation Intensity

Group Size	Strategic Treatment		Control Treatment	
	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.

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 part-

<sup>18</sup>Video recordings illustrating the use of the eye-tracking devices are available at the author's website: [www.johanneshoelzemann.com](http://www.johanneshoelzemann.com). Heat maps spotlighting information search and attention behavior can be found in Appendix A.

ner’s [partners’] payoff stream[s]) during a game before a breakthrough arrives or the game ends. To test for treatment differences non-parametrically, we apply two-sided Wilcoxon rank-sum (Mann-Whitney) tests, using group averages as independent observations. As Table 1 shows, the average fixation intensity is significantly lower in the strategic treatment. 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 did indeed understand the simple, non-strategic, nature of the control treatment.

## 5.2 Average Experimentation Intensities

One of the main qualitative predictions of the theoretical analysis is that players will tend to free-ride on the experimentation provided by their partners. Table 2 lists the mean experimentation intensity observed, using group averages across games for our four treatments.

Table 2: Average Experimentation Intensity

Group Size	Obs.	Strategic Treatment	Obs.	Control Treatment
		Experiment. Intensity		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.

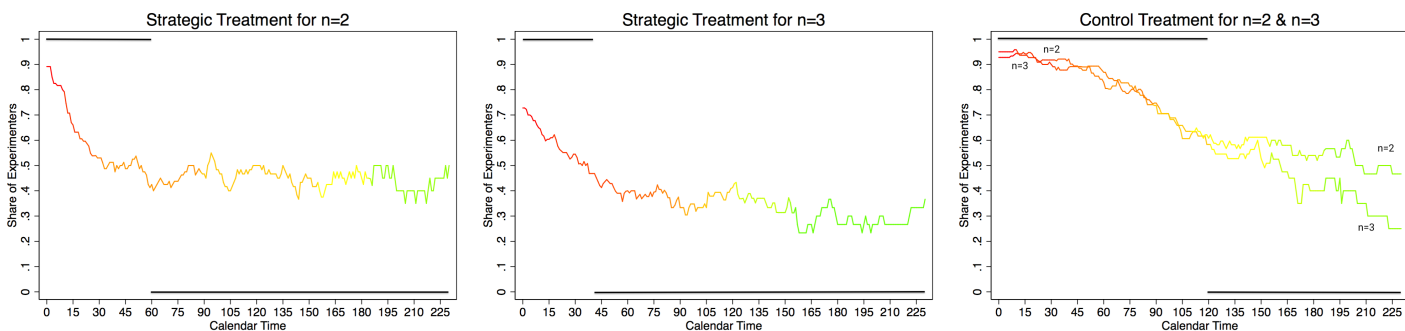
As Table 2 reveals, the additional presence of one (two) perfectly positively correlated arms leads to lower experimentation intensities in all games. This is highly statistically significant in both settings with  $n = 2$  and  $n = 3$ . The corresponding  $p$ -values in both cases are 0.0001.<sup>19</sup>

Under Hypothesis 1, players will use the risky arm less in the strategic treatment. The data provides support for this hypothesis.

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

<sup>19</sup>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.

In Figure 1, we illustrate the evolution of experimenting subjects divided by all subjects across all games and all subjects. The share of experimenting subjects before a first breakthrough or the end of a game, whichever arrived 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

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 intensity. 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 in the strategic treatment, by contrast, 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 the theory; indeed, theory predicts subjects to behave like single agents, independently of group size  $n$ .

### 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. Thus, players' payoffs should be higher on average in the strategic treatment.

Table 3: Average Final Payoffs

Group Size	Strategic Treatment				Control Treatment			
	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 3 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.<sup>20</sup> This is statistically significant. For  $n = 2$  ( $n = 3$ ), the corresponding  $p$ -values are 0.0674 (0.0001). 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.*

### 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.

<sup>20</sup>For groups of size  $n = 2$  ( $n = 3$ ), the *ex ante* expected per-capita payoff for the efficient solution would have been 1714.81 (1731.90), while, for the best PBE, it would have been 1699.00 (1710.20). For the optimal single-agent solution, it is 1621.01.

Table 4: Average Frequency of Cut-Off Behavior

Group Size	Strategic Treatment		Control Treatment	
	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).

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

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  $n = 2$  ( $n = 3$ ).

## 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 5 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.

Table 5: Proportion of Time with a Single Pioneer

Group Size	Strategic Treatment		Control Treatment	
	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.

**Result 4.** *The proportion of time before a first breakthrough during which exactly one player plays risky is higher in the strategic treatment. 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 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.<sup>21</sup> 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.<sup>22</sup>

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 6: Average Number of Switches per Player

Group Size	Strategic Treatment		Control Treatment	
	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.

<sup>21</sup>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 below, 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.

<sup>22</sup>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. We refer the interested reader to Appendix A.

Table 6 displays the average number of switches per player across games for our four treatments.<sup>23</sup> 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.*

Further, 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 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 both  $n = 2$  ( $p$ -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.<sup>24</sup> 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 rather than the best PBE. We explore this concept in greater depth in the following subsection.

## 6.2 Average Experimentation Intensities by Belief Region

While Results 1 to 5 document a tendency toward free-riding in the strategic treatment, it does not explore potential structural regularities in subjects' behavior. We now delve into the question as to whether MPE may help us interpret certain prevalent subject behaviors. Conditionally on no success arriving, players' beliefs will tend to decrease more quickly in the strategic setting, implying that more time will be spent at more pessimistic beliefs. To ensure that Result 1 is not solely due to this effect and that free-riding occurs at the "right" beliefs, we conduct our parameter tests separately by belief region. Specifically, we consider the belief regions  $[\bar{p}, \frac{1}{2}]$ , where risky is a dominant action, and  $(p_1^*, p^\ddagger)$ , where risky and safe are mutually best responses in MPE.<sup>25</sup> In the control treatment or if players

<sup>23</sup>While we run our hypothesis test with the average *incidence* of switches, we rather report the average *number* of switches in Table 6, as this may be easier to interpret.

<sup>24</sup>The same conclusion holds if we consider the first switch in a given game, whether it occurred from risky to safe or from safe to risky. In the strategic treatment, the calendar time of the first switch is significantly smaller than in the corresponding control treatment for both  $n = 2$  and  $n = 3$ , with  $p$ -values of 0.0001. Comparing again  $n$  times the calendar time of the first switch in the strategic treatment to the calendar time of the first switch in the control treatment, we find that the former was 35.46 (48.36) on average in the strategic treatment for  $n = 2$  ( $n = 3$ ) vs. 67.15 (68.36) in the control treatment. This difference is statistically significant for both  $n = 2$  ( $p$ -value of 0.0070) and  $n = 3$  ( $p$ -value of 0.0435).

<sup>25</sup>Besides the beliefs  $(\frac{1}{2}, 1]$ , which can never be reached in the absence of a success, the complementary set of these beliefs thus consists of the region  $[0, p_1^*]$ , where safe is a dominant action, and the (small) interval

were behaving according to the best PBE in the strategic setting, by contrast, all players should play risky in both regions. Table 7 summarizes our findings by belief region.

Table 7: Average Experimentation Intensity by Belief Region

Group Size	Belief Region	Strategic Treatment		Control Treatment	
		Obs.	Experiment. Intensity	Obs.	Experiment. Intensity
$n = 2$	R dominant	50	.675 [.222]	50	.899 [.161]
$n = 2$	Mutually BR	40	.505 [.155]	40	.776 [.311]
$n = 3$	R dominant	50	.632 [.281]	50	.932 [.152]
$n = 3$	Mutually BR	40	.510 [.220]	39	.779 [.090]

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

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 “mutually BR” region, as this game lasts only 32 seconds. This implies that this belief 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 Games 1-4, the missing observation for the “mutually BR” region corresponds to one three-player group in the control treatment that has not reached this specific belief region.

The comparison of the strategic treatment with the control treatment shows that the average experimentation intensity is substantially lower in the strategic treatment, for *both* belief regions. The effect is statistically significant at the 1%-level for both belief regions. Independently of group size, the  $p$ -values of the two-sided Wilcoxon ranksum test amounting to 0.0001. While this indicates that players free-ride on the experimentation efforts of their partner(s), the free-riding that occurs in the “R dominant” region cannot be explained by the theory.

We now test whether players behave differently by belief region within a given regime. Recall that they should play risky in both regions in the control treatment. In the strategic treatment, however, while subjects should always play risky in the “R dominant” region, we should observe less risky play in the “Mutually BR” region, if they play MPE, and no difference if they play the best PBE. Our analysis shows that, in the strategic treatment for groups of  $n = 2$ , the average experimentation intensity is statistically significantly higher in the “R dominant”-region, with a  $p$ -value of 0.0001. In the corresponding control treatment, the difference is not statistically significant ( $p$ -value of 0.1088). In the strategic treatment with  $n = 3$ , while players tend to use the risky arm less in the “mutually BR” region than in the “R dominant” region, statistically no such effect can be established ( $p$ -value of 0.1296). In the control treatment, however, the difference between the belief regions is significant ( $p$ -value of 0.0005), for which there is no theoretical rationale.

In order to investigate to what extent the differences in behavior across belief regions are accounted for by strategic aspects, we also test for a “difference-in-differences,” com-

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of beliefs [ $p^\ddagger$ ,  $\bar{p}$ ], which we have not assigned to either region. Indeed, as we explain in Section 4, we rely on conservative bounds in defining the “R dominant” and “Mutually BR” regions.

paring the difference in intensities across belief regions and across treatments. Since only MPE would predict there to be a difference between these belief regions in the strategic treatment, we interpret a bigger difference across belief regions in the strategic treatment as suggestive of MPE-type behavior. Indeed, there is no *a priori* reason for imprecisions in belief updating or in the computation of the relevant thresholds, which could also account for observed differences in behavior across the two belief regions, to be more prevalent in the strategic treatment as compared to the control treatment. For  $n = 2$ , this difference-in-differences is statistically significantly higher in the strategic treatment, with a  $p$ -value of 0.0054. By contrast, no such evidence can be established for groups of size  $n = 3$ , where the effect is not statistically significant ( $p$ -value of 0.8615).

The KRC model, which we have chosen for our experimental investigation on account of its tractability, exhibits no encouragement effect.<sup>26</sup> We can compute the average experimentation intensities in the region where safe is a dominant action,  $[0, p_1^*]$ , for Game 4 as well as for the two-player groups in Game 2.<sup>27</sup> 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, where we report the standard deviation in square brackets. 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  $n = 2$  ( $n = 3$ ). Thus, there is no encouragement effect in our data. By the same token, this result, together with the prevalence of free-riding, militates against our subjects' being motivated by social preferences.

Thus, especially for groups of size  $n = 2$ , behavior seems somewhat in line with the qualitative predictions of KRC's "simple MPEs." In the following subsection, we further investigate the impact of group size.

### 6.3 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. This is inherently the knottier the more players there are. Therefore, one might expect that MPE-type behavior was more

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<sup>26</sup>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.

<sup>27</sup>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.

prevalent in groups of  $n = 2$  players than 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  $n = 2$ .

Our difference-in-differences analysis across belief regions and treatments (see Sub-section 6.2) showed a statistically significantly larger difference in the strategic treatment only for groups of size  $n = 2$ . Our following result provides further evidence that the more sophisticated forms of coordination required by MPE are seemingly 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).<sup>28</sup>

## 6.4 Robustness of Results

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, fixation intensity, cut-off strategy, and switches of action 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  $k$  ( $k \in \{1, \dots, 10\}$ ) 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  $o$  ( $o \in \{1, \dots, 6\}$ ) 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 8 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, fixation intensity and cut-off strategy. By contrast, we find a strong positive

<sup>28</sup>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).

effect of the additional presence of one (two) perfectly positively correlated arms on stage game payoffs and switches of action.<sup>29</sup>

Table 8: OLS Estimations with Random Effects of Experimentation Intensity, Payoffs, Fixation Intensity, and Switches of Action

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

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

\*\*\* Significant at the 1 percent level.

Thus, our OLS estimations with random effects confirm all of our previous, non-parametric, results. In particular, there is no evidence of supergame effects, as subjects' behavior does not change in response to the number of games they previously played.<sup>30</sup>

<sup>29</sup>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.

<sup>30</sup>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.

## 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, evolve as the game progresses. 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.

In a first step, we have exhibited strong evidence for strategic free-riding, as experimentation intensities are lower, and payoffs higher, in the strategic setting. Our eye-tracking data furthermore suggest that, in the strategic setting, subjects were paying keen attention to their partners' exploration efforts. These findings suggest that subjects fully understood the nature of the strategic interaction. 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 switches in the strategic setting. This contrasts with the very simple structure of on-path play in the best PBE.

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 check, 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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