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Abstract

People who compete alone may entertain different psychological motivations from those who compete for a team. Using a real-effort experiment, we examine the behavioural consequences of these psychological motivations, absent strategic interdependence and uncertainty among team members. We exploit a dynamic pairwise team contest in which strategic uncertainties among team members play a minimised role in individual rational behaviour; and we create strategically-equivalent individual contests to isolate the pure psychological effects of team situation on individual competitive behaviour. We find that behaviour in individual contests and in sterile team contests follows a psychological momentum effect in which leaders work harder than trailers. In contrast, in team contests enriched with intra-team communication, behaviour follows a neutral effect. We discuss the implications of our results for theoretical modelling of contests and practical implications for the optimal design of team incentive schemes and personnel management.

Keywords: individual versus team behaviour, real-effort experiment, pairwise team contest, best-of-three team contest, communication, psychological momentum effect

JEL Classification: C33, C72, D79, C91, C92

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1. Introduction

Competitions between teams or groups permeate a wide variety of social, economic and political situations, influencing human adaptation of preferences, norms, and biases and creating social phenomena such as discrimination, terrorism, ethnic genocide, and interstate war (Fiske 2002; Bowles 2006; Bowles 2009). Sports teams compete for trophies, political parties compete to ensure the majority seats in parliaments, universities compete in rankings to attract more students and grants, and private firms compete to boost revenues. Compared to being alone, playing on a team introduces opportunities of using different and more complex strategies in competition. For example, players might choose to strategically free-ride on other team members (Olson 1965); they might try to outguess each other, picking up the slack left by others only if it is necessary for team's success (Rapoport and Bornstein 1987); and they might choose to cooperate in anticipation of reciprocity from team members (Axelrod 1984).

The economics and psychology literatures have explored the effects of team situation on individual's competitive behaviour in myriads of ways. Studies on social categorisation and group identity mainly look at influences on individuals' other-regarding preferences and biases towards own teammates and opposing players (Sherif et al. 1961; Tajfel and Turner 1979; Charness, Rigotti, and Rustichini 2007; Y. Chen and Li 2009). Sherif et al.'s work is the first to study team contest in a field experiment with two teams of boys engaging in a series of competitions. This line of research was continued in social psychology literature by Amnon Rapoport, Gary Bornstein and their colleagues who study the effects of inter-group competition on intra-group cooperation (e.g., Rapoport and Bornstein 1987; Bornstein 1992; Bornstein and Ben-Yossef 1994; Bornstein 2003; Kugler and Bornstein 2013). More recently economists have started using game-theoretic analysis to study the effect of team incentives on individuals' strategic behaviour and social welfare (e.g., Nalbantian and Schotter 1997; Abbink et al. 2010; H. Chen and Lim 2013).[1] Yet an important question remains unanswered: how do people's pure

[1] Most studies in the literature model the collective action situation as a prisoners' dilemma or a public goods game (Nalbantian and Schotter 1997; Gunnthorsdottir and Rapoport 2006; West et al. 2006; Tan and Bolle 2007; Puurtinen and Mappes 2009; Sutter and Strassmair 2009; Burton-Chellew, Ross-Gillespie, and West 2010; Kugler, Rapoport, and Pazy 2010; Reuben and Tyran 2010; Sääksvuori, Mappes, and Puurtinen 2011; Burton-Chellew and West 2012; Leibbrandt and Sääksvuori 2012; Böhm and Rockenbach 2013; Chen and Lim 2013; Egas et al. 2013; Markussen, Reuben, and Tyran 2014; Puurtinen, Heap, and Mappes 2015; Hargreaves Heap et al. 2015), whereas a few others used coordination games (Bornstein, Gneezy, and Nagel 2002; Cason, Sheremeta, and Zhang 2012). Another related strand of literature models the intergroup conflict as rent seeking contests between groups. For example, economists have tested in laboratory experiments how different group impact functions such as perfect-substitutes, weakest-link and best-shot (Sheremeta 2011), how various intragroup enforcement strategies such as peer punishment (Abbink et al. 2010; Abbink et al. 2012) and leadership (Eisenkopf 2014) and how heterogeneity such as group size (Ahn, Isaac, and Salmon 2011), group power differentials (Bhattacharya 2016), and ethnic identity (Chowdhury, Jeon, and Ramalingam 2016) affected individual investment to their own group. While our

psychological motivations to exert costly effort differ when competing alone and when competing for a team? This question matters because it sets an important benchmark for us to understand individual behaviour in teams which often introduce more complex strategic incentives and uncertainties. In this paper, using a real-effort experiment we aim to answer this question and examine the behavioural consequences of these psychological motivations.

Our experimental framework is based on a purposefully selected sequential best-of-three team contest, in which six symmetric players compete in three-member teams for a prize, which is awarded to each member of the winning team. The contest comprises three pairwise battles, which are played out sequentially, and each battle is between two players, one from each team. Henceforth, we refer to the paired players in the first battle as "first movers", pairs in the second battle as "second movers", and pairs in the third battle as "third movers". In each battle, the two players exert effort independently after they learn the outcomes of previous battles. The first team to win two out of three battles wins the contest. Section 2 shows that a rational second mover's behaviour exhibits "strategic neutrality": being in a leading or trailing position after the first battle has no bearing on second mover's effort. The sequential best-of-three team contest was first theoretically introduced by Fu, Lu, and Pan (2015) and then tested experimentally using a real-effort task by Fu, Ke, and Tan (2015) and empirically using data from squash tournaments by Huang (2016). Both tests found that individual behaviour closely follows the theory prediction.

The theoretical property of strategic neutrality provides an ideal environment to disentangle the pure psychological effects of being on a team from the strategic effects of uncertainty about other team members' behaviour. It is because second movers know what has happened in the first battle and what to expect in the third battle, but neither of the outcomes in these two battles affects their equilibrium effort. In other words, in a sequential best-of-three contest, second movers face minimal strategic uncertainty about their teammates' behaviour compared to other games that have been studied in the literature such as public goods games (Tan and Bolle 2007; Leibbrandt and Sääksvuori 2012; Markussen, Reuben, and Tyran 2014), coordination games (Bornstein, Gneezy, and Nagel 2002; Cason, Sheremeta, and Zhang 2012) or Tullock contests (Abbink et al. 2010; Ahn, Isaac, and Salmon 2011). While strategic interdependence in these games captures important features of intra-group dynamics, such frameworks do not allow for a clear-cut test of the *pure* psychological influences of team

study and those cited above focus on individual *decentralised* decision making in teams, another important strand of economics literature on group decision making studies how teams make *centralised* decisions as opposed to individuals (e.g., Charness and Sutter 2012).

situation on individual competitive behaviour, *devoid* of any interactions with strategic incentives. Thus, our experimental design exploits this theoretical property by constructing strategically-equivalent individual contests to isolate the pure psychological effects of team situation on individual competitive behaviour.

Our research proceeds by running two sets of experiments. First, we conduct a more strenuous test of strategic neutrality in sequential best-of-three team contests in a relatively sterile team situation. We conjecture that the previously observed strategically neutral behaviour in sequential best-of-three team contests could be due to either the absence of explicit effort costs or the presence of a rich context of team play, which might have allowed the "do your best" rule of thumb to figure prominently. Both in Fu, Ke & Tan (2015) and Huang (2016), players have no direct costs of effort that could deter them from exerting the maximum amount of effort to win the contest. This is a common criticism of most real-effort experiments where subjects choose to compete or invest in risky games so as not to sit idly in the lab (Falk and Fehr 2003). At the same time, the analysis of sport contests renders it difficult to observe non-neutral strategic responses to previous histories during tournaments possibly for the fear of being ranked down or ostracised by athletes' teammates and coaches. In contrast, we implement a sequential best-of-three team contest in a sterile team situation with a novel real-effort task that bears explicit monetary costs of effort. The results show second movers in the leading position exert higher effort than in the trailing position, a psychological momentum effect which is reminiscent of the "success breeds success" in folk psychology (e.g., Crust, Nesti, and College 2006).

Secondly, we ask whether the absence of team situation and a richer context of team situation will affect second movers' behaviour conditional on their strategic positions. Although, given our experimental construction, there is no strategic reason for the varied team situation to make a difference, players may be motivated by psychological reasons to behave differently when competing on teams as opposed to competing alone. Individuals may have other regarding preferences towards their teammates (Abbink et al. 2012; Bowles 2006) and feel guilty if they do not meet their teammates' expectations (Battigalli and Dufwenberg 2007). Therefore, they may not only think harder about winning strategies but also resort more often to pro-social rules of thumb (e.g. dropping-out or slacking off can be construed as shirking which is less socially appropriate). As a follow up to our sterile best-of-three team contest, we designed three additional experiments to test the conjecture that the absence of team situation might lead to more pronounced psychological momentum effects, whereas richer team situations might induce strategically neutral behaviour by cultivating stronger senses of responsibility and instilling "do your best" rule of thumb. We manipulated the absence of team situation by constructing structurally-equivalent individual contests which mimicked the underlying strategic incentives for second movers in teams; and we varied the richness of team situation by allowing team members to communicate with each other before each round of the contest. With such a design, we held the fundamental economic incentives faced by second movers (and thus the equilibrium predictions) constant across the experiments while varying the absence and richness of team situation.

The results from follow up experiments show that in the strategically-equivalent individual contest there is even stronger evidence of psychological momentum effects than in the sterile team situation, whereas in the enriched team situation, second movers behave in accord with the predictions of strategic neutrality. We also find that the differences in effort levels between leading and trailing positions is mainly explained by differences in dropout behaviour. Hence, the psychological momentum effect appears to primarily reflect individuals' decisions to drop out from competition conditional on their strategic positions. We test the robustness of our findings to various specifications of effort and test learning effects between the first and second halves of the experiments. We present an extensive discussion about the potential mechanisms behind our results and conclude with practical implications in Section 5.

2. Theoretical Framework

We consider a simple model of best-of-three team contest with symmetric players and complete information.[2] In the best-of-three contest, three battles occur sequentially. In each battle, one player from each team plays against an opponent from the rival team and the side exerting greater effort wins the battle. The team that wins two out of three battles wins the contest. We denote a player's effort, $e_{i(t)}, i = A, B;\ t = 1, 2, 3,$ where i is the team to which the player belongs and t the order of her battle. The marginal cost of effort is normalised to 1. The winning team receives a prize of V for each member while the losing team gets v; $V > v$.

A key observation of the best-of-three structure is that in each battle, the two players always face the same level of incentive to win. This is the case for the second movers, irrespective of their being on the leading or trailing team after the first battle. To see this, first note that if the third battle were to occur, from the perspective of the second players each side would win with a probability of 50%. The second mover on the leading team reasons that if she wins, she receives the prize V immediately; if she loses, the third battle occurs and her expected

[2] This section is adapted from Fu, Lu, and Pan (2015) and Fu, Ke, and Tan (2015).

payoff is $V/2 + v/2$. Thus, the prize incentive for her to win the battle is $V - V/2 - v/2 = V/2 - v/2$. On the other hand, the opposing second mover on the trailing team reasons that if she wins, the third battle occurs and the expected payoff is $V/2 + v/2$; if she loses, she receives v with certainty. Thus, the prize incentive for her to win the battle is also $V/2 + v/2 - v = V/2 - v/2$.

Since the second movers on the leading and trailing teams face the same prize incentive, it is well known that in an all-pay auction with two symmetric risk-neutral players, both players will play the same mixed strategy, namely, $e_{i(2)} \sim U[0, V/2 - v/2], i = A, B$. Therefore, *the second movers' rational behaviour is independent of their team being in a leading or trailing position.*

This theoretical property, called the "strategic neutrality", can be more readily understood by contrasting to the strategic momentum effect in a best-of-three contest between two *individuals* (Klumpp and Polborn 2006; Konrad and Kovenock 2009). If two individuals have to repeatedly fight in three battles till one side has won two battles, both the leader and trailer after the first battle need to take into account the potential effort cost to be incurred in the third battle. Both players' expected payoff from the third battle becomes $V/2 + v/2 - (V - v)/4 = V/4 + 3v/4$ because the expected effort cost in the third battle is $(V - v)/4$. Thus, the prize incentive for a leader in the second battle is $V - (V/4 + 3v/4) = 3V/4 - 3v/4$, whereas that for a trailer is $(V/4 + 3v/4) - v = V/4 - v/4$. Therefore, a leader faces a higher prize incentive than a trailer to win the second battle in the best-of-three individual contest and hence the leader's equilibrium effort is higher than the trailer's.

Fu, Ke, and Tan (2015) is the first experimental paper that compares the best-of-three individual and team contests. They find that, in contrast to the theoretically predicted strategic momentum effect in individual contests, trailers exert more effort than leaders, which they explain with a joy of winning hypothesis. Second movers' behaviour in teams, on the other hand, demonstrates strategic neutrality, namely both trailers and leaders exert the same amount of effort. Our experiment also compares individual contests to team contests but in a different spirit. Recall that the key reason behind the different equilibrium dynamic effects between individual and team best-of-three contests is the differing incentives behind the two contest structures. We control this difference in the experiment by designing an individual contest that mimics the strategic environment second movers face in the best-of-three team contest. By doing so, we are able to assess the effects of team situation on individual behaviour in strategically equivalent contests, holding constant the underlying economic incentives.

We hypothesise that team situation (or its absence) in and of itself may create psychological incentives that affect individuals' behaviour in contests. For example, behaviour in our individual contests might take the form of psychological effects as in the individual contest of Fu, et al. (2015) even in the absence of the relevant economic incentives. Or it might exhibit a psychological momentum effect reminiscent of "success breeds success", although in our individual contests the previous success is either exogenous or determined by pure luck.

3. Experimental Design

All of our experiments have the same two-part structure. The first part, which was the same across all experiments, consisted of three rounds incentivised by a piece-rate. The first part was primarily meant to familiarise subjects with the real effort work task, which was also used in the second part.

We used the ball-catching task as our real-effort task (Gächter, Huang, and Sefton 2016). Subjects had a fixed amount of time to catch balls that fall randomly from the top of the screen by using mouse clicks to move a tray at the bottom of the screen.[3] The *number of clicks* is interpreted as the *effort* in a round.[4] The ball-catching task permits a level of control over the effort cost function by attaching financial costs to mouse clicks and thus to effort levels. Therefore, subjects who work on the ball-catching task have to engage in an explicit trade-off between the benefits of higher probability of winning and the costs of higher effort. Previous experiments using the ball-catching task have shown that the effort (the number of clicks) does respond both qualitatively and quantitatively to various incentives such as piece rates, team incentives, and tournaments (Gächter, Huang, and Sefton 2016). Furthermore, since the relationship between catches and clicks, albeit noisy, follows an estimable production function with diminishing returns to inputs (see Figure B1 in the Appendix B), the ball-catching task also allows us to make quantitative predictions on subjects' effort provision.[5] Our version of the task lasted only one minute and thus allowed us to repeatedly measure the behaviour of each

[3] The "random" falling pattern is set according to the same seed number used to generate the random numbers by computer. It means all subjects face exactly the same task in every round.
[4] Given that most previous literature use task performance as a noisy measure of effort, in Section 4.3, we use the number of catches as an alternative measure of effort and the results are qualitatively similar whether we use clicks or catches as our dependent variable.
[5] Gächter, Huang, and Sefton (2016) showed in various experiments the point predictions are indeed borne out and are consistent with the corresponding induced value experiments. This suggests that while some subjects may make their clicks more carefully than others, it is no more than the fact that some subjects in induced value experiments may make more accurate calculations than others. Heterogeneous ability (physical or cognitive) or non-monetary costs and benefits always exist to some degree. The key point is that clicks as effort are costly and it is the fact that the ball-catching task satisfies the precepts of non-satiation, salience and especially dominance (Smith, 1982) that provides the necessary control of the experimental environment.

subject. The task thus combines the advantages of induced-effort tasks giving us control over monetary effort costs and of real-effort tasks providing arguably stronger realism.

In all of our experiments we varied the second part of the experiment. We ran the first set of experiments to test for strategic neutrality in a best-of-three team contest in a sterile team environment. We ran the second set of experiments to test for the absence and richness of team situation affecting second movers' behaviour in equivalent contests with the same economic incentives. We describe and analyse the second part of each experiment separately and draw careful conclusions from comparing the experiments to each other given the differences in timing and locations between the two sets of experiments.

3.1 Benchmark Pairwise Team Contest

The first and benchmark experiment—*TEAM*—mimicked the theoretical best-of-three team contest with symmetric risk-neutral players. In each of the 12 paid rounds, subjects competed in three-member teams by working sequentially on the ball-catching task. To minimise the possibility of reputation and other peer effects due to easy identification of other subjects' past behaviour, both the team composition and the matching of two competing teams in a contest were randomised before every round at the session level. After the matching was completed, each subject in a team was assigned the role of either First, Second or Third Mover. Subjects did not know others' identities or performance histories at any point during the session.

In each battle, the side that caught more balls at the end of the allotted time won the battle and the first team to win two battles would receive the winner prize V while the losing team would receive the loser prize v. With the session-level randomisation, we created ex-ante symmetric team competition, which is crucial assumption to test the theory. The randomization both within team and between teams at each period helped fix the effective prize spread for second movers at a constant level (i.e., $(V - v)/2$), since ex-ante each third mover had the same probability of winning the third battle. By keeping the valuation of winning for all second movers constant, we ensured that each second mover faced the same level of economic incentives. Importantly, because the prediction of strategic neutrality relies on the assumption of risk neutrality, we fixed the incentives between leading and trailing second movers at a constant level. It ensured that failure of risk neutrality could not systematically explain any differences in second movers' behaviour conditional on being in leading or trailing positions and across our follow-up experiments.

Within a contest, the feedback structure was kept as simple as possible insofar as the theory permitted: each of the six subjects in a team competition received feedback only on the outcomes of previous battles, which remained visible when they were working on the ball catching task in the next battle, but not on the actual number of caught balls by previous movers. We chose such minimal feedback because if team members could observe each other's actual performance, it might give rise to additional strategies such as dropping out of their own battles after being disappointed by other team members' poor performance or peer effects given the observability of effort choices. Lastly, the third battle would not occur if one team had already won the first two battles. Along with the randomisation, this last feature was designed to minimise second movers' uncertainty about third movers' actions because, unlike the first battle which is indecisive and the second battle which is ex-post unfair, the third battle, if necessary, was both decisive and fair for both third movers, and therefore not likely to cause uncertainty about its expected outcome.[6] The setup of TEAM thus served as an ideal environment for the test of the prediction of strategic neutrality in a sterile team environment. We ran six sessions of TEAM with 30 subjects in each, at the University of Nottingham in November 2014 with an average payment of £11.2.

3.2 Individual Contests

Given the results of TEAM, we ran two follow-up experiments to assess the effects of the absence of team situation on individuals' competitive effort. In *INDIVIDUAL* and *INDIVIDUAL_Bo3* experiments, we deprived second movers of the team situation while retaining the basic economic incentives. Specifically, the individual contest in these two experiments mimicked the second battle in a best-of-three contest of TEAM. Recall that in each second battle, one player was on the leading team and the other was on the trailing team. The player on the leading team was in a position where if she won the battle her team won the contest, whereas if she lost the contest outcome was essentially determined by a fair coin toss because, from the perspective of the second movers, the two third movers were ex-ante symmetric. However, the opposing player on the trailing team was in a position where if she lost the battle her team lost the contest, while if she won, the contest outcome was also equivalent to a fair coin toss.

Given the structure of the second battles, in INDIVIDUAL we assigned one player the *Red Type* which corresponded to a second mover on a leading team, and the other player the *Blue Type* which corresponded to a second mover on a trailing team. Accordingly, the rule of

[6] Note that in equilibrium there is no *strategic* uncertainty about the expected outcome of the third battle. Even if subjects may entertain some psychological uncertainties, given the session level randomizations, perceiving the third battle as a 50-50 chance is the most natural and focal assumption. Supporting this view, ex-post analysis of the data shows that third movers' effort levels (as well as winning probabilities) do not depend on second movers' strategic positions (Table B8 in the Appendix B), implying that second movers should at least learn to realise the outcome of the third battle does not depend on previous battle histories.

winning became as follows: if the Red Type worked harder than the Blue Type, she would win the contest; if the Blue Type worked harder than the Red Type, the contest outcome would be determined by a fair coin toss; and if there was a tie, which happened with zero probability in theory with continuous strategy space, the contest outcome would again be determined by a fair coin toss. Therefore, we retained the basic economic incentives in the second battle of a best-of-three team contest, while converting the second battle to a strategically equivalent (asymmetric and unfair) individual contest.

INDIVIDUAL_Bo3 was similar to INDIVIDUAL but with the key difference that we explicitly mentioned in the instructions that the battles would proceed in a best-of-three structure, and that the outcome of the first and third battles would be determined by two separate fair coin tosses. Subjects received feedback after the first fair coin toss, whether they won or lost the first battle, and then only competed in the second battle; the third battle, in which the result was determined by another fair coin toss, only followed in the case of a tie after the first two battles. Importantly, we did not frame the first and third battles as the ones between two computer players because this reference to other players, even played by a computer, might trigger the cue for behaving as if for a team, thus preventing a clean separation between competing in teams and competing as individuals.

As in TEAM, the contest in INDIVIDUAL and INDIVIDUAL_Bo3 was repeated for 12 rounds. In every round, both the matching and the type assignment were randomised at the session level. We ran one session of INDIVIDUAL and one session of INDIVIDUAL_Bo3 with 30 subjects in each, at the University of Surrey in May of 2016 with an average payment of £11.7.

3.3. Enriched Pairwise Team Contest

In *TEAMCHAT* we enriched the psychological motivations of the benchmark TEAM contest by allowing intra-team communication via text chat before each round of the contest. Past research has found that intra-team communication helps develop parochial altruism and promote cooperation but at the cost of fiercer and less efficient inter-team competition (Sutter and Strassmair 2009; Cason, Sheremeta, and Zhang 2012; Leibbrandt and Sääksvuori 2012). Likewise, we also expect that intra-team communication helps foster stronger accountability towards own team and therefore increase effort without changing the strategic incentives of the contest structure. All other respects of TEAMCHAT were kept the same as TEAM except that the team contest was repeated for only 10 rounds because we intended to keep the length of the session and therefore the monetary incentive per unit of time similar to that in TEAM. We ran three sessions of TEAMCHAT with 30 subjects in each at the University of Surrey in May of 2016 with an average payment of £12.0.

3.4 Parameterisation

The parameters of the experiment were as follows. In the first part, the first round was not paid and the next three rounds were paid by a piece rate, in which each caught ball was worth 20 tokens while each click cost 10 tokens. In the second part, a winner in INDIVIDUAL or each member from a winning team in both TEAM and TEAMCHAT was awarded a winner prize of 1200 tokens; a loser or each member from a losing team received a loser prize of 400 tokens. In both parts, the cost of each mouse click that moved the tray, that is the marginal cost of effort, was 10 tokens and this was kept constant across all experiments. Under this condition, we emphasise that a subject's optimal strategy is never to click as much as possible (also see footnote 11). Subjects' earnings were the sum of their payoffs in both parts and were converted to British Pounds at the rate of 1000 tokens equal to £1 at the end of the session. Given these parameters, the predicted average effort levels are 20 clicks for first and second movers and 40 for third movers, although in theory all players should play according to mixed strategies in equilibrium because of the all-pay mechanism of the battles.[7]

The software was programmed in z-tree (Fischbacher 2007). Upon arriving at the lab, each participant was randomly allotted a computer booth by the experimenter. The instructions for the second part were distributed after subjects completed the first part. A post-experimental survey, including questions about demographics, general risk and competitive attitudes concluded the session. The characteristics of subjects in each treatment, for which we control in our parametric analysis, are summarized in Table B1 of the Appendix B. A typical session lasted around 1.5 hours. Full experimental instructions are reproduced in the Appendix A.

4. Results

We first examine average effort compared to the static equilibrium predictions in Section 4.1, and then provide evidence on the effects of strategic position on second movers' effort levels across our experiments in Section 4.2.[8] To simplify the exposition of the results, we refer to the

[7] To derive these predictions, note that the valuation of winning in the third battle is 800. In the mixed strategy Nash equilibrium, the average effort cost is half of the valuation of winning and thus the average effort is 40 clicks for ex-ante symmetric third movers. For the ex-ante symmetric first and second movers, the valuation of winning is always 400 and thus their average predicted efforts are 20 clicks.

[8] Throughout the analysis, we excluded one subject in TEAMCHAT because one subject dropped out of the session due to health reasons and the consequent actions of this subject was controlled by a dummy subject to allow the session to proceed. The dummy subject's action was not to click in any round. Our results are robust to excluding the whole session to which the dummy subject belongs from our analysis.

players in INDIVIDUAL and INDIVIDUAL_Bo3 also as second movers. In Section 4.3 we test the robustness of our results by looking at learning effects across the first and second halves of the experiments, and using the number of catches as an alternative measure of effort.

4.1. Descriptive Statistics of Effort

We present the results on average effort, detailed tabulation of which is collected in Table B2 in the Appendix B. Given the all-pay mechanism of contests, the theory predicts a mixed strategy of second mover's effort on the range between 0 and 40 clicks, and therefore the average effort in equilibrium is 20 clicks. We find over-clicking of second mover's clicks in all of our experiments: the average effort is 25.80, 27.34, 24.49 and 30.03 in TEAM, INIDIVIDUAL, INIDIVIDUAL_Bo3 and TEAMCHAT, respectively (p<0.020, t-tests).[9] The effort in INIDIVIDUAL is not significantly different from either INDIVIDUAL_Bo3 (p=0.429, ranksum), TEAM (p=0.339, rank-sum) or TEAMCHAT (p=0.239, rank-sum). In contrast, the effort is significantly higher in TEAMCHAT than in TEAM (p=0.002, ranksum) and INDIVIDUAL_Bo3 (p=0.000, rank-sum).

For TEAM and TEAMCHAT, we also analyse first and third movers' efforts. The average first mover's clicks, which is 25.67 in TEAM and 29.49 in TEAMCHAT, is significantly higher than the predicted average of 20 clicks (p<0.000, t-tests). Effort differs significantly between TEAM and TEAMCHAT (p=0.026, rank-sum). However, the average third mover's clicks, which is 31.94 in TEAM and 34.23 in TEAMCHAT, is significantly lower than the predicted average of 40 clicks (p<0.000, t-tests). Effort also differs significantly between TEAM and TEAMCHAT (p=0.085, rank-sum).

These comparative results are only suggestive as we should be careful in interpreting the differences in effort levels across the experiments given the differences in timings and locations between the experiments. To address this issue, we control for possible differences in subject pools between the experiments by using propensity score matching (PSM) technique given subjects' observable characteristics (gender, age, nationality, risk and competitive attitudes) and estimate average treatment effects on treated. This analysis shows significant differences in average clicks of second movers between TEAMCHAT and the other three experiments (INDIVIDUAL_Bo3 p=0.000; INDIVIDUAL p=0.012; and TEAM p=0.000). Differences in first, second and third mover clicks between the other experiments are not significant. PSM analysis is consistent with the raw data analysis and supports higher average

[9] All reported test statistics are clustered at the subject level. This means the unit of observation is a subject's average effort for a specific role across all rounds.

second mover effort levels when competing in a richer team situation compared to competing in a more sterile team situation and competing individually.

While in all experiments there are significant deviations from game theory predictions on average effort levels, the directional prediction for third mover's clicks holds: third mover's clicks is significantly higher than both first mover's and second mover's clicks in both TEAM (3rd vs. 1st, p=0.000; 3rd vs. 2nd, p=0.000, rank-sum) and TEAMCHAT (3rd vs. 1st, p=0.007; 3rd vs. 2nd, p=0.035, rank-sum).[10] This finding implies that in both team situations, subjects correctly understand their strategic positions and the associated economic incentives.

4.2. Second Mover Effort

Turning to the competitive behaviour of second movers, within each experiment we calculate each subject's average clicks across those rounds when they played as second movers, both when they were in the leading and the trailing positions. Figure 1 shows the results (detailed tabulation is presented in Table B3 in the Appendix B). Since this exercise results in matched data for each subject, we use Wilcoxon signed-rank tests to examine dynamic effects on second mover's clicks.

The results from the signed-rank tests show that second mover exerted higher efforts when they were in the leading positions than when they were in the trailing positions in the individual contests but not in the team contests. The difference in second movers' effort levels between leading and trailing positions is largest in INDIVIDUAL (av. diff.= 6.9, N=30, p=0.016), followed by INDIVIDUAL_Bo3 (av. diff.= 5.8, N=30, p=0.13), TEAM (av. diff. = 52.4, N=140, p=0.182) and TEAMCHAT (av. diff.= 1.2, N=64, p=0.519). Performing signed-rank tests comes at the cost of losing some unmatched data where some subjects were only either on leading teams or on losing team, but not both. The conclusion, however, is unchanged if we do rank-sum tests: the difference in effort levels between leading and trailing second movers remains significant in INDIVIDUAL (p=0.053) and INDIVIDUAL_Bo3 (p=0.032), but insignificant in TEAM (p=0.344) or TEAMCHAT (p=0.487).

We perform a random effects regression analysis of second mover's clicks by regressing the second mover's clicks on the binary variable—*Lead*—which takes the value of 1 in the round if this second mover is on a leading team and 0 if on a trailing team. The model controls experience dummies with the first experience providing the omitted category; the experience

[10] The effort levels for all movers, however, are well below the number of clicks they could have made if they were really "working as hard as possible". In an experiment reported in Gächter, Huang, and Sefton (2016) using the ball-catching task, subjects were awarded by catching balls but bore no costs of clicking. The average effort is close to 60 clicks and the highest is around 90 clicks.

variable only accumulates in those rounds where a subject has actually worked on the ball-catching task including the current round and it is equal to the round variable if the subject has worked in all rounds. The model further controls observable subject characteristics given the potential differences across the experiments and session fixed effects because of the session-level randomisation we employed. Table 1 reports the coefficient estimates on the Lead variable.

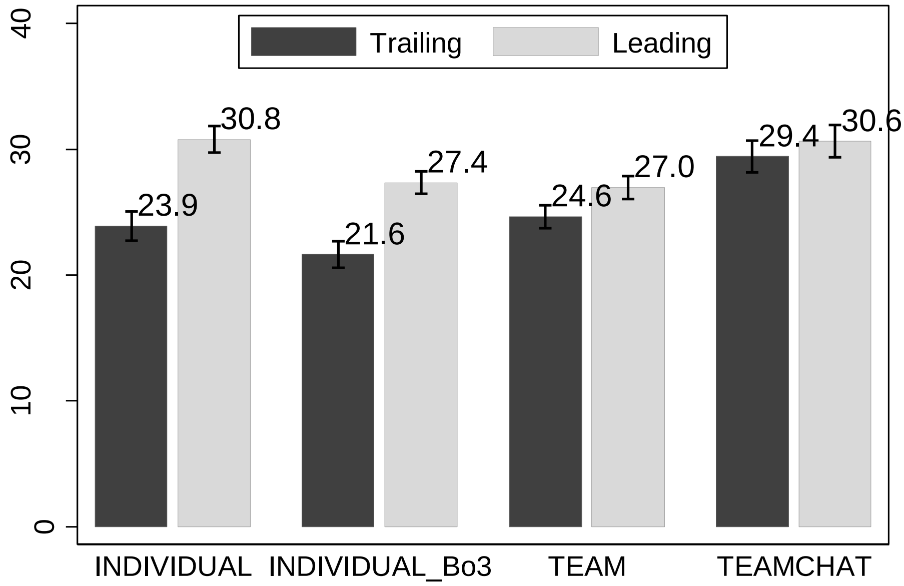

Figure 1: Average Second Mover's Clicks. *The error bars are ±SEM.*

Consistent with the non-parametric tests, the estimate shows that in INDIVIDUAL and INDIVIDUAL_Bo3 leaders make on average 6 and 3.9 more clicks than trailers, which is statistically significant at the 1% level. In TEAM, second movers on leading teams are estimated to make on average 2.4 more clicks than those on trailing teams. Contrary to the non-parametric test, the difference is statistically significant at the 5% level. In TEAMCHAT, second movers on leading teams are estimated to make on average only 0.3 more clicks than those on trailing teams, which does not differ significantly from zero.[11] Given these results, we reject strategic neutrality in three out of four experiments with equivalent underlying strategic incentives. Our results can be explained by a psychological momentum effect where an advantageous leading position of an individual results in higher competitive effort (i.e. success

[11] Table 1 uses the full sample including those second movers who have been only on either leading or trailing teams, but not both. All of the results, however, are robust to using a subsample of second movers who have been both on leading and trailing teams in different rounds. Note that this subsample is exactly the same as the full sample in INDIVIDUAL and INDIVIDUAL_Bo3. The estimate (s.e.) of Lead is 2.587 (0.978), N=623 in TEAM; and 0.280 (1.554), N=235 in TEAMCHAT.

breeds success). The effect could arise from leaders gaining more confidence, higher morale or greater eagerness to conclude the contest in the second battle.

We also examine pairwise comparisons between experiments by including the experiment dummy and its interaction with the Lead variable in the random effects regression. The estimates on the interaction terms show that the leading effect in INDIVIDUAL differs significantly from that in either TEAM (p=0.015) or TEAMCHAT (p=0.003), but is not significantly different between TEAM and TEAMCHAT (p=0.248). Again, we have to be cautious when comparing our experiments across each other and drawing conclusions given the differences in timings and locations of the experiments. We conduct analogous PSM analysis as in the previous subsection to control for differences in observable characteristics of subjects and compare the leading effects between the experiments. We find very similar results which are reported in Table B4 of the Appendix B.

Table 1: Random Effects Regressions of Second Mover's Clicks

	(1) INDIVIDUAL	(2) INDIVIDUAL_Bo3	(3) TEAM	(4) TEAMCHAT
Lead	6.006***	3.941***	2.366**	0.338
	(1.179)	(0.991)	(0.924)	(1.491)
σ_ω	10.749	10.004	13.711	10.443
σ_u	10.847	9.007	10.896	11.090
Obs.	360	360	720	294
Hausman test for random	$\chi^2(12)$=0.16	$\chi^2(12)$=0.56	$\chi^2(12)$=7.50	$\chi^2(12)$=12.8
vs. fixed effects	p=1.000	p=1.000	p=0.823	p=0.232

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. ** p<0.05 *** p<0.01

To investigate the factors that underlie the average leading effects, first note that second movers sometimes dropped out (i.e. no clicks were made), implying that the average leading effects may conceal heterogeneous effects (see Figure 2). Dropping-out or quitting behaviours are not uncommon in tournament-style situations and have previously been observed both in lab experiments (e.g., Schotter and Weigelt 1992; Müller and Schotter 2010) and in field experiments (e.g., Fershtman and Gneezy 2011). From the theoretical viewpoint, if in an all-pay auction two players' valuations of winning differ, the player with lower valuation may optimally choose to drop out because her mixed-strategy equilibrium bid has a mass on zero, whereas the player with higher valuation never chooses to drop out.[12] This implies that among those who exert strictly positive effort, the average efforts of leaders and trailers should be the same. Thus, the leading effect might be primarily driven by heterogeneous dynamic effects of dropping-out.

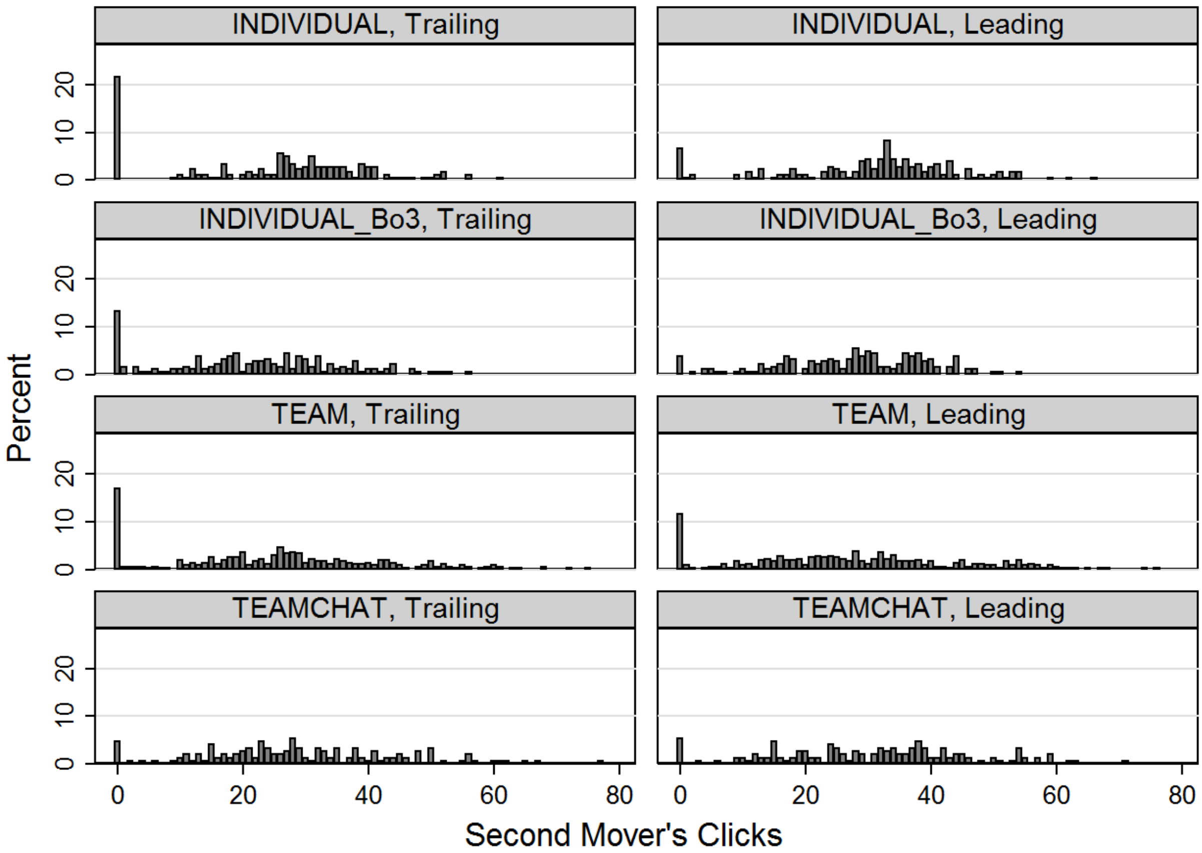

Figure 2: Distributions of Second Mover's Clicks

When comparing the proportions of dropping-out between leaders and trailers, we use the bootstrap method to calculate the standard error for the difference in proportions to account for the possibility that some subjects may drop out disproportionally more often than others. Table 2 presents the results, showing that leaders are indeed much less likely to drop out than trailers in INDIVIDUAL (6.7% vs. 21.5%) and INDIVIDUAL_Bo3 (3.9% vs. 13.2%). The difference is smaller but still statistically significant in TEAM (11.7% vs. 17.1%), and is close

[12] Formally, in an all-pay auction, if a leader's valuation of winning is v_L and a trailer's valuation of winning is v_T, $v_L > v_T$, then in the mix-strategy equilibrium, the leader randomises between 0 and v_T; the trailer's strategy has a mass at 0 and he randomises between 0 and v_T for the rest of time.

to zero and not statistically significant in TEAMCHAT (5.5% vs. 4.7%). Additional confirmation comes from random effects regression analysis where we use the binary variable of dropout as the independent variable while retaining the same set of explanatory variables from Table 1. The coefficient estimates on Lead, which are reported in Table B5 in the Appendix B, lead to the same conclusion as do the parametric test statistics. The estimated effects of being on leading team on the probability of dropping-out are 14.9% in INDIVIDUAL, 6.1% in INDIVIDUAL_Bo3, 6.7% in TEAM, and -1.9% in TEAMCHAT, close to the actual effects calculated from the raw data.

Table 2: Second Mover's Dropout rates

Experiment	Trailing	Leading	Difference	SE	P-value
INDIVIDUAL	21.5%	6.7%	14.8%	0.061	0.015
INDIVIDUAL_Bo3	13.2%	3.9%	9.3%	0.030	0.002
TEAM	17.1%	11.7%	5.4%	0.028	0.057
TEAMCHAT	4.7%	5.5%	-0.8%	0.034	0.798

Note: Dropout rates are calculated by pooling over trailing and leading teams. Standard errors are bootstrapped allowing clustering at the subject level. P-values are from two-tailed tests.

Can the heterogeneous dynamic effects on dropping-out explain the psychological momentum effects of being in leading position on second mover's effort? In Figure 3 and Table 3, we re-produce Figure 1 and re-estimate Table 1 excluding the observations where second movers drop out. Wilcoxon signed-rank tests suggest that leaders do not make significantly more clicks than trailers in either INDIVIDUAL (N=29, av. diff.=2.5 clicks, p=0.157), TEAM (N=116, av. diff.=0.8 clicks, p=0.840), or TEAMCHAT (N=63, av. diff.=1.5 clicks, p=0.524). Only in INDIVIDUAL_Bo3 do we observe leaders make 3.5 more click than trailers which is statistically significant (N=29, p=0.033). The coefficient estimates of Lead are consistent with the non-parametric tests: only in the INDIVIDUAL_Bo3 does being on a leading position have a significant effect on second mover's clicks. Equally important, we find that when we remove dropouts the *average* second mover's clicks no longer differ significantly across the INDIVIDUAL, TEAM and TEAMCHAT experiments (31.8, 30.77 and 31.05 clicks, joint χ^2 test p-value = 0.897) while effort in INDIVIDUAL_Bo3 remains significantly lower compared to the other experiments. Hence, in three out of the four experiments, differing psychological momentum effects across the experiments are largely driven by heterogeneous dynamic effects on dropping-out behaviour; so does the significant difference in average second mover's clicks between TEAM and TEAMCHAT presented in Section 4.1. This provides some evidence that the differences across our experiments presented themselves through varying dropping out behaviour and when removing the drop-outs, the experiments bear comparable effort levels.

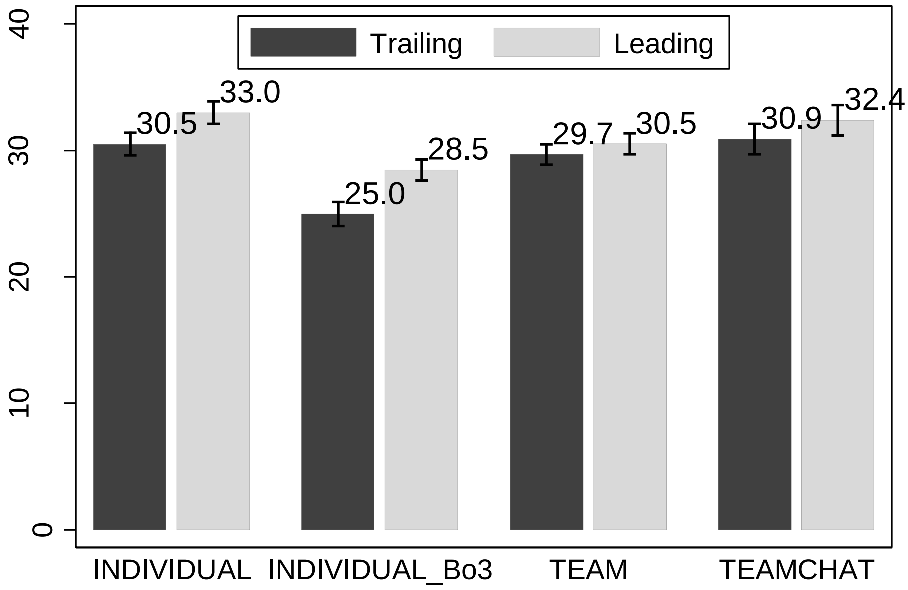

Figure 3: Average Second Mover's Clicks (No Dropout). *The error bars are ±SEM.*

Table 3: Random Effects Regressions of Second Mover's Clicks (No Dropout)

	(1) INDIVIDUAL	(2) INDIVIDUAL_Bo3	(3) TEAM	(4) TEAMCHAT
Lead	1.303	2.286***	0.425	1.185
	(0.879)	(0.851)	(0.712)	(1.360)
σ_ω	9.692	9.217	13.016	11.114
σ_u	7.291	7.353	7.460	9.419
Obs.	309	329	617	279
Hausman test for random	$\chi^2(12)$=0.37	$\chi^2(12)$=1.35	$\chi^2(12)$=3.56	$\chi^2(12)$=3.39
vs. fixed effects	*p*=1.000	*p*=0.999	*p*=0.990	*p*=0.971

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables.

4.3. Robustness Checks

Given that in our experiments the contests are repeated for multiple rounds, is there any evidence that subjects learn to behave in accord with the theoretical prediction of strategic neutrality? Figure B2 in the Appendix B provides a breakdown of Figure 1 experience by experience for each experiment. It shows the average second movers' clicks on leading teams are almost always higher than those on trailing teams in both INDIVIDUAL and INDIVIDUAL_Bo3. However, we find no clear pattern in either TEAM or TEAMCHAT. Table 4 re-estimates the model presented in Table 1 separately for the first and second halves of a session. Contrary to the learning hypothesis, the estimates on Lead show that the leading effects remain statistically significant for INDIVIDUAL, INDIVIDUAL_Bo3 and TEAM, and remain insignificant for TEAMCHAT throughout a session. In fact, if there is any learning effect, it appears that the leading effect in each of the former three experiments is strengthened rather than weakened.

We also look at the dropping-out pattern more closely by displaying the proportion of dropping-out experience by experience for every experiment in Figure B3 and re-estimating the same model as in Table 4 separately for the first and second halves of a session. Table B6 reports the estimates. Again, contrary to the learning hypothesis, we find that on average trailers tend to drop out even *more often* in later rounds than leaders in both INDIVIDUAL and INDIVIDUAL_Bo3, although both trailers and leaders seem to be increasingly more likely to drop out in later rounds. This trend could have been explained by boredom or fatigue or by an income effect due to winning in earlier rounds. However, the dropout rate does not change over the course of the experiment either in TEAM or in TEAMCHAT, where subjects had to wait for their team members sitting idly for longer.

Table 4: Random Effects Regressions of Second Mover's Clicks in the First and Second Halves of All Rounds

	INDIVIDUAL		INDIVIDUAL_Bo3		TEAM		TEAMCHAT	
Round	1-6	7-12	1-6	7-12	1-6	7-12	1-5	6-10
Lead	5.613***	6.432***	2.889***	5.854***	2.634**	3.030**	-0.663	-0.988
	(1.540)	(1.726)	(1.121)	(1.613)	(1.320)	(1.403)	(2.086)	(2.424)
σ_ω	9.522	12.280	10.842	9.853	13.160	15.760	10.152	11.317
σ_u	9.748	11.040	6.934	10.017	10.334	10.694	9.717	11.280
Obs.	180	180	180	180	360	360	148	146

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. ** p<0.05 *** p<0.01

Given that most previous experimental studies typically used observed performance or output as a noisy measure of effort, we replicate our previous analysis by using the number of catches as an alternative measure of effort. Figure 4 plots the average second mover's catches and Table B7 in the Appendix B presents the results of a panel data regression analysis. The results are qualitatively similar to the ones reported above using the number of clicks as effort. Individuals catch significantly fewer balls when they are in trailing compared to leading positions in INDIVIDUAL, INDIVIDUAL_Bo3 and TEAM. In TEAMCHAT, the number of caught balls are identical between leading and trailing positions. The leading effect on catches is significantly weaker in TEAM than in INDIVIDUAL at the 5% level, and there is no significant difference between TEAM and TEAMCHAT.

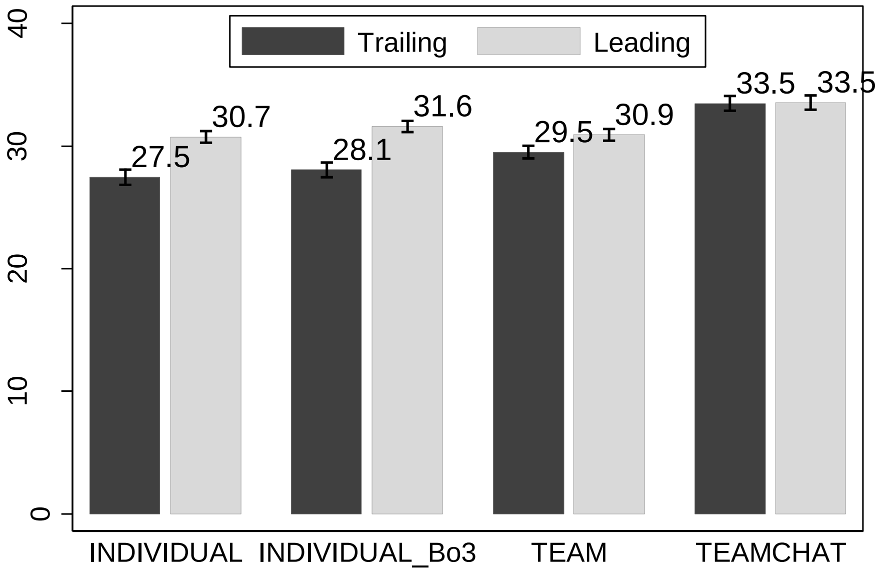

Figure 4: Average Second Mover's Catches as an Alternative Measure of Effort.

The error bars are ±SEM.

5. Discussion

In this paper, we study the effects of team situation on individual competitive effort levels in contests. Past research in economics and psychology literatures on how team situations affect individual behaviour has not met the challenge of disentangling the pure psychological effect of team situation from the strategic uncertainty about other team members' behaviour. We attempt to take the first step to fill this gap by exploiting a best-of-three team contest, in which strategic uncertainty plays little role in explaining individual behaviour. Our results show that the frame of fighting alone or fighting for a team can in and of itself have significant effects on individual competitive behaviour, a psychological possibility yet underappreciated in the economic modelling of contests.

First, we test whether individual behaviour in teams follows the prediction of strategic neutrality. By creating a relatively sterile team situation with explicit monetary costs of effort, we find that second movers in trailing positions drop out of competition more often than second movers in leading positions. Contrary to the strategic neutrality hypothesis, our results can be supported by a psychological momentum effect that discourages second movers from competing once their team has lost the first battle. Secondly, by comparing the two follow-up experimental contests—two structurally-equivalent individual contests—to the benchmark sterile team contest, we observe even stronger psychological momentum effects, whereby leaders compete harder than trailers in the absence of team situation. Thirdly, in the enriched team situation, the effect of first battle outcomes on second movers' behaviour is neither statistically nor economically significant: individuals in enriched teams behave seemingly in accord with strategic neutrality.

Our results have important implications for the theoretical modelling of contests. We find that players get discouraged by their trailing position when they compete as individuals and as members of sterile teams. This contradicts the game theory prediction that the dynamic asymmetries in players' standing should not affect their effort levels when trailers and leaders have the same valuations of winning the contest as is the case in our experiment (Konrad and Kovenock 2009; Fu, Lu, and Pan 2015). Our results highlight the importance of psychological motives in contests: advantageous start fuels higher performance whereas disadvantageous start can be highly demotivating both in individual and sterile team competition. This contrasts with the findings of Mago, Sheremeta, and Yates (2013) who reject psychological momentum effects in favour of strategic momentum effects when explaining behaviour in individual multi-battle contests. Further, the psychology momentum effect works in the opposite direction of the psychological effect in which leaders slack off while laggards work harder in individual multi-battle contests of Fu, Ke, and Tan (2015) and Berger and Pope (2011). We note the disparate findings may partly arise from the fact that our individual contests only consist of a single effort-exerting battle and that leading and trailing positions are created by exogenous manipulations, whereas in Berger and Pope (2011), Mago et al. (2013) and Fu et al. (2015) the strategic positions are endogenously determined by efforts in earlier battles. Thus, one possible explanation is that leaders may think they can afford slacking-off with their earlier advantage but only if the advantage is earned by hard work rather than endowed by nature. Likewise, a similar effect working in the opposite direction may apply to trailers. Note that such an effect may depend on the degree of the advantage (Berger and Pope 2011).

In contrast, in teams where we enrich the team situation by intra-team communication, players' strategic positions do not affect their effort. So why do we fail to observe the psychological momentum effect in this team situation? How does communication foster more competitive behaviour and less dropping out? One explanation is guilt aversion. The theory predicts that people respond to others' expectations by behaving accordingly and are unwilling to let others down.[13] The neutrality result that we observe in enriched teams can be explained by guilt-averse players who refuse to let down their team members with whom they have discussed winning strategies, have made promises to each other and have cheered each other to win. The content analysis of the chat messages (presented in Appendix C) indeed shows that subjects often expressed the desire and also encouraged others to work as hard as possible. These messages might have created expectations within teams not to drop out regardless of previous battle outcomes. Such expectations not only contributed directly to the higher average effort in enriched teams (especially for second movers on trailing teams), but might also have worked indirectly by pre-emptying subjects from being influenced by the psychological effects of being in leading or trailing positions when playing individually. Our result highlights the importance of the psychological motives within teams in raising team output. In this regard, our findings complement the earlier finding by Chen and Lim (2013) who in a simultaneous team contest showed that socialisation among teammates promotes effort through their concern about teammates' welfare. Similarly, Chen and Lim developed a behavioural model based on the concept of guilt aversion to explain their findings.

Our results also have important practical implications for designing contests in organisations. Individuals and teams in disadvantageous positions caused, for example, by stereotype threat or by irrelevant and often noisy interim feedback may be more likely to drop out of competition. There is ample evidence of how gender and ethnicity gaps in choosing competitive STEM courses emerge because of socially stereotyped positions students find themselves in during their academic development (Niederle and Vesterlund 2010; Riegle-Crumb, Moore, and Ramos-Wada 2011). Similarly, Baker and Horton (2003) show that the psychological atmosphere created by the belief of East African advantage in running can have

[13] The tests of the theory have found mixed success (Chang et al. 2011; Ellingsen et al. 2010; Dufwenberg, Gächter, and Hennig-Schmidt 2011; Battigalli, Charness, and Dufwenberg 2013). More recently, Khalmetski (2016) develop a new method by exogenously manipulating others' expectations and his finding lends support to the theory.

significant consequences on actual performance. In organisational settings, the interaction of employees' relative performance standing and feedback they receive can relate to how feedback affects subsequent performance (Smither, London, and Reilly 2005; Kuhnen and Tymula 2012). Our results indicate the relevance of such contextual asymmetries to individual decision making in contests may be diminished or even eliminated in teams enriched with a strong sense of team membership. Our findings lay basis for advising leaders in organisations to selectively assign roles to team members, for example assigning players with stronger team accountability to intermediary rounds to ensure that contextual and strategic uncertainties play a minimal role in players' motivations to exert effort.

It is worth noting that our results provide a lower bound for possible effects of team situations on individual strategic behaviour in real world settings. Our manipulations of team presence and team richness were minimal. Sterile teams were formed only on the basis of sharing the same fate in terms of financial incentives. Enriching the team situation was also minimally achieved, where teams chat at the beginning of contests; except the one-minute communication before the contests, there is no other interaction between the team members. Yet such minimal manipulations successfully decrease the psychological momentum effects and give rise to a more strategically neutral behaviour. In real world settings where team formation and development are naturally stronger, strategic neutrality in real world team situations may be more robust than in lab environments, *even though the underlying logic could be fundamentally psychological*.

To expand on this point, we note that the chat messages show that our subjects almost never explicitly discuss dynamic strategies (see Appendix C). Therefore, conscious information sharing of optimal strategies among team members cannot explain the observed strategic neutrality. This, however, does not necessarily mean that subjects do not think about dynamic strategies. It may be "politically incorrect" to even chat about contingent strategies on what happened in other battles, which may create distrust or lower team morale among team members. Alas, the absence of such discussion suggests that although people behave more like *homo economicus* in enriched teams, they might not be thinking hyper-rationally as game theorists assume. Team members may have tacitly developed shared understanding about the dynamic independence of each battle and their undivided responsibility for the consequence of each battle. As we have also shown, the behavioural pattern is not due simply to the best-of-three structure but instead to the presence of team situation, and is plausibly fuelled by trailers' guilt aversion or stronger senses of responsibility towards teammates as discussed earlier. This may have inadvertently led to the observed neutrality result in team contests with communication. Normatively, this interpretation of our findings highlights that we need to caution against equating the consistent empirical evidence to the success of standard economic theory and its underlying (game-theoretic) assumptions (Fu, Ke, and Tan 2015; Huang 2016), and too quickly disposing of psychological mechanisms as determinants of competitive behaviour.

Admittedly, our experiment is not a direct test of the underlying psychological motives of leaders' and trailers' behaviour, which request further examinations. In a best-of-three team contest, a trailer is both in a position where she can cost her team a defeat with a loss and in a position where she can draw her team even with a win. Thus, we cannot distinguish whether it is such a "clinch battle" that makes the trailer feel more accountable to her team or due simply to the fact that she *is* a trailer. A clearer separation of these two explanations can be achieved in a best-of-five or a best-of-seven team contest. For example, in a best-of-five, consider a second mover whose team is down by 0:1 and a fourth mover whose team is down by 1:2. In both cases, their teams are behind by exactly one point, but only the latter player is in a clinch battle. Therefore, switching from an individual to a team situation could make a greater difference for the fourth mover than for the second mover if the clinch battle contributes to trailers' stronger sense of accountability to their teams. Further research should explore the differences between individual and team competition in such longer best-of contests.

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Appendix (Intended for Online Supplementary Materials)

A. Experimental Instructions

[Same for all experiments]

Instructions

Welcome to the experiment. Please read these instructions carefully. For participating in this experiment you will receive a £3 show-up fee. In addition you can earn money by completing tasks in two parts of the experiment. You will receive separate instructions before the start of each part.

During the experiment, your earnings are calculated in tokens. At the end of the experiment, every 1000 tokens will be converted to £1 in cash and your cash payment will be the sum of your earnings from both parts, in addition to the show-up fee.

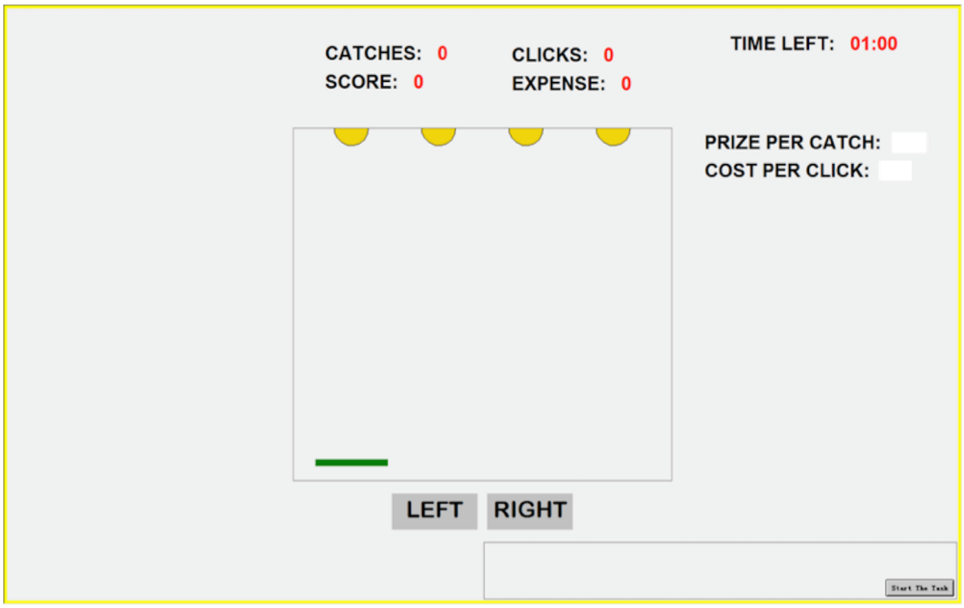
Before we start the experiment, please read and sign the CONSENT FORM on your desks that you are willing to participate in this experiment and consent to the use of your data.

If you have a question, please raise your hand and someone will come to your desk to answer it.

Instructions for Part 1

In this part, you will be asked to work on a computerized ball-catching task for 4 periods. The first period serves as a practice period for you to familiarize yourself with the ball-catching task. The next three periods will be for real and your earnings in this part will be the sum of your earnings in these three paying periods.

Each period lasts one minute. In each period, there will a task box in the middle of the task screen like the one shown below:



Once you click on the “Start the Task” button, the timer will start and balls will fall randomly from the top of the task box. You can move the tray at the bottom of the task box to catch the balls by using the mouse to click on the LEFT or RIGHT buttons. To catch a ball, your tray must be below the ball before it touches the bottom of the tray. When the ball touches the tray your catches increase by one.

You will receive a prize of 20 tokens for each ball you catch and incur a cost of 10 tokens for each mouse click you make. In each period, the number of balls you have caught so far (displayed as CATCHES) and the number of clicks you have made so far (CLICKS) are shown right above the task box. Also shown above the task box are SCORE, which is

CATCHES multiplied by the prize per catch, and EXPENSE, which is CLICKS multiplied by the cost per click.

At the end of the period your earnings in tokens for the period will be your SCORE minus your EXPENSE.

When you are ready, please press the “Start the Task” button at the lower right corner on the task screen.

Instructions for Part 2

[INDIVIDUAL]

In this part, there are 12 periods. In each period, you will be randomly matched with another participant in this room. The random matching is completed by the computer and has nothing to do with your decisions in previous parts of the experiment.

The whole matching process will remain anonymous throughout the entire experiment. You will not be told the identities of your matched participants. Also note that the matching will be re-done randomly in each period. It is very unlikely that you will be matched with the same participant twice.

Your Task in Each Period

In each period, you will compete with the other participant for a winner prize of *1200 tokens* and a loser prize of *400 tokens*. One of you will be assigned the *Red Type* and the other will be assigned the *Blue Type*. You will be told which one of you is assigned the Red Type and Blue Type before the start of each period.

You and your matched participant will independently work on the ball-catching task. The rule for winning the winner prize is as follows.

- If the Red Type catches more balls than the Blue Type at the end of the task, the Red Type will receive the winner prize of 1200 tokens and the Blue Type will receive the loser prize of 400 tokens.
- If the Blue Type catches more balls than the Red Type, then the computer will flip a fair coin and determine which one of you receives 1200 tokens and 400 tokens.
- If both you and your matched participant catch the same number of balls, the computer will flip a fair coin and determine which one of you receives 1200 tokens and 400 tokens.

Each mouse click on the LEFT or RIGHT buttons incurs a cost of *10 tokens*. The number of balls caught so far (displayed as CATCHES) and the number of clicks made so far (CLICKS) are shown on your screens right above the task box. Also shown above the task box is EXPENSE, which is CLICKS multiplied by the cost per click.

Your earnings in each period will be (winner or loser) prize minus your EXPENSE.

Your Earnings in Part 2

Your earnings in this part will be the sum of your earnings from all 12 periods.

[TEAM]

In this part, there are 12 periods. In each period, you will be randomly matched with two other participants in this room to form a team. The random matching is completed by the computer and has nothing to do with your decisions in previous parts of the experiment.

Your team will be randomly matched with another team consisting of three other participants in the room. The random matching of two teams is also completed by the computer and has nothing to do with any of the decisions in previous parts of the experiment.

The whole matching process will remain anonymous throughout the entire experiment. You will not be told the identities of either your team members or the members of the other matched team. Also note that both the matching with two other team members and the matching between two teams will be re-done randomly in each period. It is very unlikely that you will be matched with the same team members and the same other team members twice.

Your Task in Each Period

In each period, your team will compete in a best-of-three contest with the other team for a winner prize of *1200 tokens* for each member of the winning team and a loser prize of *400 tokens* for each member of the losing team.

The competition consists of up to three stages. You will participate only in one of three stages. The computer will randomly determine your participation order in the competition. You will be told whether you are the First Mover, the Second Mover, or the Third Mover before the start of each period. In the first stage two First Movers, one from each team, will compete. In the second stage two Second Movers will compete and in the third stage, if necessary, two Third Movers will compete. The winning team in each period will be the one that wins two out of three stages. The rule for winning each stage is as follows.

During the first stage, two First Movers will simultaneously work on the ball-catching task. The team whose First Mover catches more balls at the end of the task will win the first stage. If the two First Movers catch the same number of balls, the computer will randomly select the winner of the stage. Each mouse click on the LEFT or RIGHT buttons incurs a cost of *10 tokens* to the First Mover who makes the click. For each First Mover, the number of balls caught so far (displayed as CATCHES) and the number of clicks made so far (CLICKS) are shown right above the task box on the First Mover's screen. Also shown above the task box is EXPENSE, which is CLICKS multiplied by the cost per click. While the First Movers are working on the task, the other team members should wait quietly and patiently.

At the end of the first stage, all team members of both teams will be informed of which team won the first stage.

The second stage proceeds in the same fashion as the first stage. The Second Movers will participate in this stage while the other team members should wait quietly and patiently. The team whose Second Mover catches more balls at the end of the task will win the second stage. Each Second Mover will also incur an EXPENSE herself by clicking. At the end of the second stage, a similar summary screen will show which team won the second stage.

If one team has won both stages, the competition ends and each member from the winning team will receive the winner prize of 1200 tokens and each member from the losing team will receive the loser prize of 400 tokens. If each team has won one of the two stages, the Third Movers will compete in the third stage following the same competition rule for the first two stages. The team whose Third Mover catches more balls at the end of the task will be the winning team. At the end of the third stage, a similar summary screen as in the first two stages will be shown.

Your earnings in each period will be (winner or loser) prize minus your EXPENSE. If the third stage is not necessary, the Third Mover earnings will be simply the (winner or loser) prize.

Your Earnings in Part 2

Your earnings in this part will be the sum of your earnings from all 12 periods.

[TEAMCHAT]

In this part, there are 10 periods. In each period, you will be randomly matched with two other participants in this room to form a team. The random matching is completed by the computer and has nothing to do with your decisions in previous parts of the experiment.

Your team will be randomly matched with another team consisting of three other participants in the room. The random matching of two teams is also completed by the computer and has nothing to do with any of the decisions in previous parts of the experiment.

The whole matching process will remain anonymous throughout the entire experiment. You will not be told the identities of either your team members or the members of the other matched team. Also note that both the matching with two other team members and the matching between two teams will be re-done randomly in each period. It is very unlikely that you will be matched with the same team members and the same other team members twice.

Your Task in Each Period

In each period, your team will compete in a best-of-three contest with the other team for a winner prize of *1200 tokens* for each member of the winning team and a loser prize of *400 tokens* for each member of the losing team.

The competition consists of up to three stages. You will participate only in one of three stages. The computer will randomly determine your participation order in the competition. You will be told whether you are the First Mover, the Second Mover, or the Third Mover before the start of each period. In the first stage, two First Movers, one from each team, will compete. In the second stage, two Second Movers will compete and in the third stage, if necessary, two Third Movers will compete. The winning team in each period will be the one that wins two out of three stages.

Before the start of each period, you will be asked to communicate with your team members via a text chat box on the screen for 60 seconds. You can discuss anything you like, including what you think is the best approach to win the competition, what you plan to do, or what you would like others to do. However, there are three important restrictions on the types of messages that you may send.

- You may not send a message that attempts to identify you to other team members. Thus, you may not use your real name, nicknames, or self-descriptions of any kind ("Tom Smith here", "I'm the guy in the red shirt sitting near the door", "It's me, Sandy, from French class", or even "As a woman [Latino, Asian, English, etc.], I think...").
- There must be no use of abusive language, and threats or promises pertaining to anything that is to occur after the experiment ends.
- All of the communication must be in English.

The experimenter will screen your messages. If your message is found to violate any of the rules, you may be excluded from the payment in this experiment.

After the communication, the contest will begin. The rule for winning each stage in the contest is as follows.

During the first stage, two First Movers will simultaneously work on the ball-catching task. The team whose First Mover catches more balls at the end of the task will win the first stage. If the two First Movers catch the same number of balls, the computer will randomly select the winner of the stage. Each mouse click on the LEFT or RIGHT buttons incurs a cost of *10 tokens* to the First Mover who makes the click. For each First Mover, the number of balls caught so far (displayed as CATCHES) and the number of clicks made so far (CLICKS) are shown right above the task box on the First Mover's screen. Also shown above the task box is EXPENSE, which is CLICKS multiplied by the cost per click. While the First Movers are working on the task, the other team members should wait quietly and patiently.

At the end of the first stage, all team members of both teams will be informed of which team won the first stage.

The second stage proceeds in the same fashion as the first stage. The Second Movers will participate in this stage while the other team members should wait quietly and patiently. The team whose Second Mover catches more balls at the end of the task will win the second stage. Each Second Mover will also incur an EXPENSE by clicking. At the end of the second stage, a similar summary screen will show which team won the second stage.

If one team has won both stages, the competition ends and each member from the winning team will receive the winner prize of 1200 tokens and each member from the losing team will receive the loser prize of 400 tokens. If each team has won one of the two stages, the Third Movers will compete in the third stage following the same competition rule for the first two stages. The team whose Third Mover catches more balls at the end of the task will be the winning team. At the end of the third stage, a similar summary screen as in the first two stages will be shown.

Your earnings in each period will be (winner or loser) prize minus your EXPENSE. If the third stage is not necessary, the Third Mover earnings will be simply the (winner or loser) prize.

Your Earnings in Part 2

Your earnings in this part will be the sum of your earnings from all 10 periods.

[INDIVIDUAL_Bo3]

In this part, there are 12 periods. In each period, you will be randomly matched with another participant in this room. The random matching is completed by the computer and has nothing to do with your decisions in previous parts of the experiment.

The whole matching process will remain anonymous throughout the entire experiment. You will not be told the identities of your matched participants. Also note that the matching will be re-done randomly in each period. It is very unlikely that you will be matched with the same participant twice.

Your Task in Each Period

In each period, you will compete in a best-of-three contest with the other participant for a winner prize of *1200 tokens* and a loser prize of *400 tokens*.

The competition consists of up to three stages. In the first stage, the computer will randomly determine whether you win or lose the first stage. At the end of the first stage, you will be informed if you won or lost the first stage.

In the second stage, you and your matched participant will independently work on the ball-catching task. The participant who catches more balls at the end of the task will win the second stage. If you catch the same number of balls as your matched participant, the computer will randomly select the winner of the stage. Each mouse click on the LEFT or RIGHT buttons incurs a cost of *10 tokens* to you. The number of balls caught so far (displayed as CATCHES) and the number of clicks made so far (CLICKS) will be shown right above the task box on your screen. Also shown above the task box will be EXPENSE, which is CLICKS multiplied by the cost per click. At the end of the second stage, you will be informed if you won or lost the second stage.

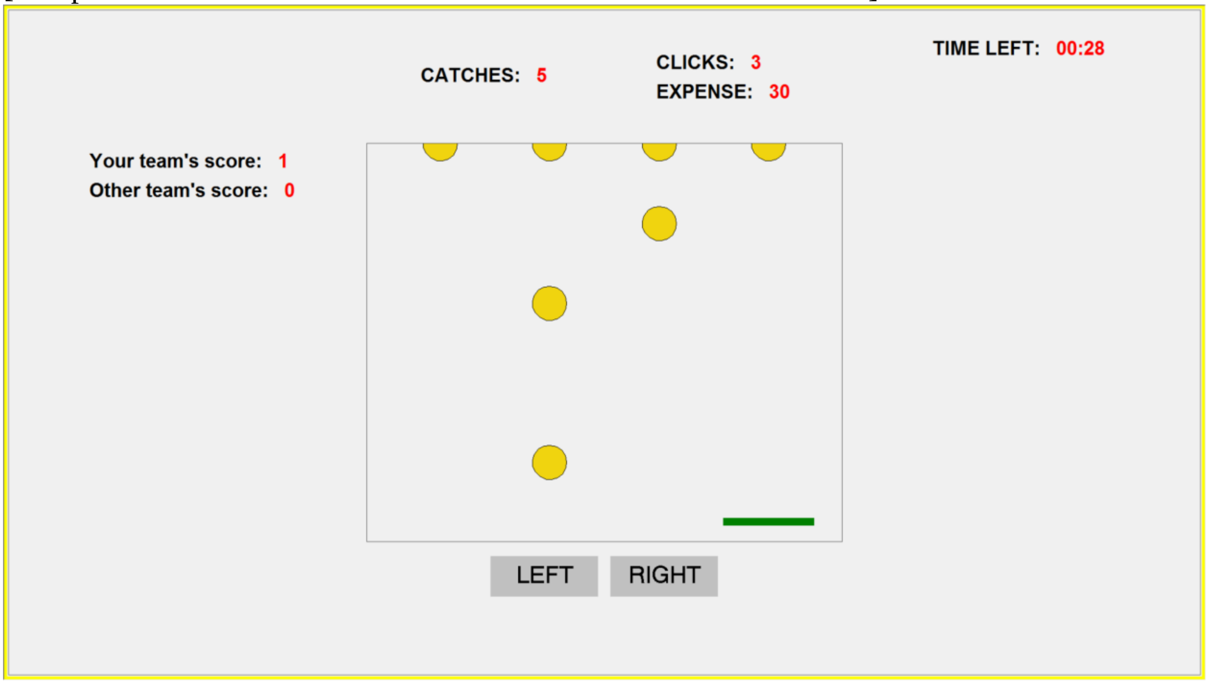
If one participant has won both stages, the competition will end and the winning participant will receive the winner prize of 1200 tokens and the losing participant will receive the loser prize of 400 tokens. If each participant has won one of the two stages, the computer will randomly determine whether you win or lose the third stage. The participant who wins in the third stage will receive the winning prize and the participant who loses in the third stage will receive the loser prize.

Your earnings in each period will be (winner or loser) prize minus your EXPENSE.

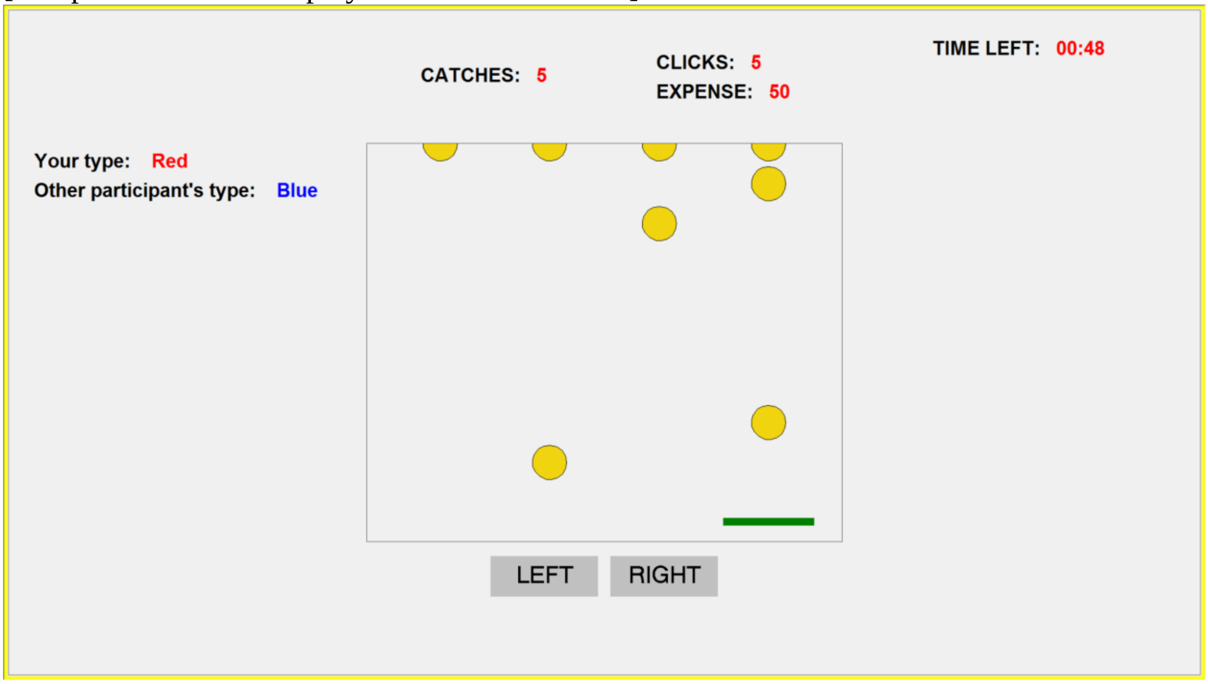
Your Earnings in Part 2

Your earnings in this part will be the sum of your earnings from all 12 periods.

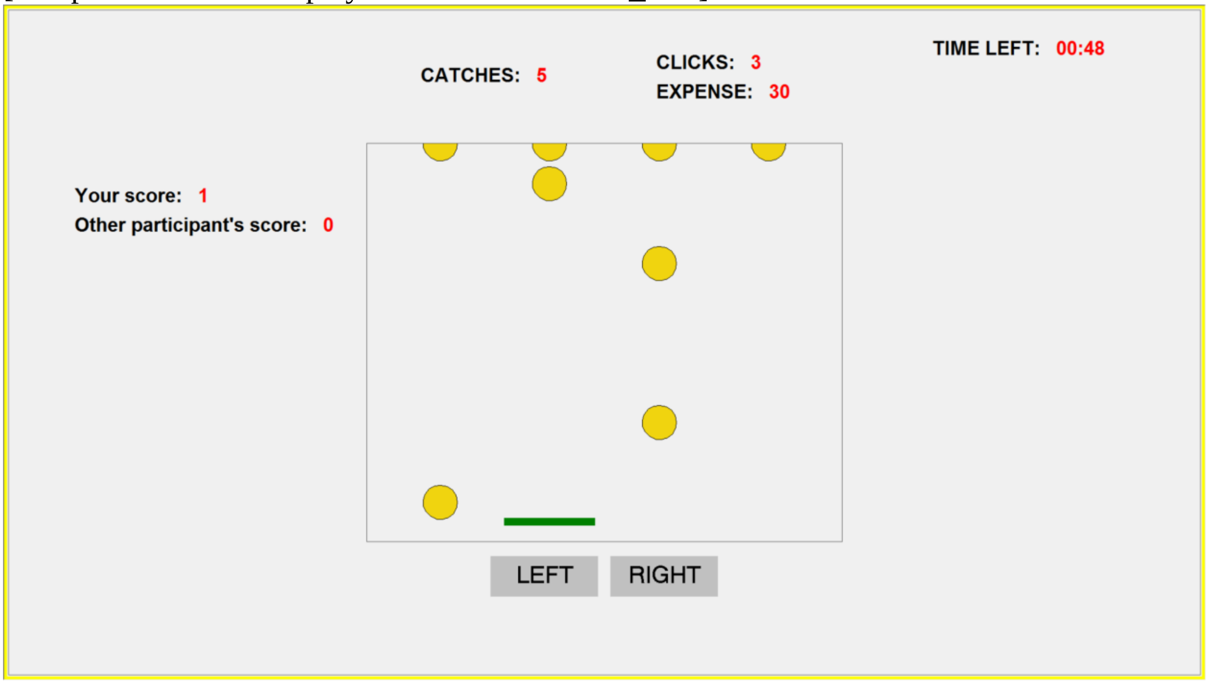
[Sample screenshot for second movers in TEAM and TEAMCHAT]



[Sample screenshot for players in INDIVIDUAL]



[Sample screenshot for players in INDIVIDUAL_Bo3]



B. Additional Tables and Figures

Table B1: Characteristics of Subjects across the Experiments

	TEAM	INDIVIDUAL	INDIVIDUAL_Bo3	TEAMCHAT
Female	59%	57%	70%	55%
Age (Median)	21	21	21	21
British	48%	27%	53%	49%
Risk Attitudes (Median)	6	6	7	6
Competitiveness (Median)	7	6	5.5	7
N	180	30	30	89

Notes: 0: not at all, 10: very (competitiveness and risk attitudes measures)

Table B2: Descriptive Statistics for All Players

Experiment	N	Clicks				Catches			
		Mean	SD	Min	Max	Mean	SD	Min	Max
INDIVIDUAL									
All	360	27.34	15.27	0	66	29.10	7.60	6	43
INDIVIDUAL_Bo3									
All	360	24.49	13.26	0	56	29.84	7.43	6	46
TEAM									
1st Mover	720	25.67	17.02	0	73	29.73	8.09	7	50
2nd Mover	720	25.80	17.15	0	76	30.31	8.64	7	49
3rd Mover	360	31.94	17.07	0	83	33.94	7.24	8	48
TEAMCHAT									
1st Mover	298	29.49	14.63	0	70	32.39	6.10	10	46
2nd Mover	294	30.03	15.49	0	77	33.19	7.11	8	45
3rd Mover	158	34.23	15.15	0	69	36.44	6.04	10	50

Table B3: Descriptive Statistics for Second Movers

Experiment	N	Clicks				Catches			
		Mean	SD	Min	Max	Mean	SD	Min	Max
INDIVIDUAL									
Leading	180	30.78	14.02	0	66	30.74	6.41	8	43
Trailing	180	23.89	15.72	0	61	27.45	8.34	6	43
INDIVIDUAL_Bo3									
Leading	180	27.35	11.88	0	54	31.61	6.12	8	43
Trailing	180	21.63	13.97	0	56	28.07	8.19	6	46
TEAM									
Leading	360	26.95	17.15	0	76	30.86	8.37	7	49
Trailing	360	24.65	17.09	0	75	29.75	8.88	7	47
TEAMCHAT									
Leading	147	30.63	15.54	0	71	33.34	6.89	8	45
Trailing	147	29.43	15.46	0	77	33.04	7.35	8	45

Table B4: Random Effects Regressions of Second Mover's Clicks. Pairwise Comparisons of Leading Effects Using Propensity Score Matching Technique.

Benchmark	TEAM	TEAM	TEAM	TEAMCHAT	TEAMCHAT	INDIVIDUAL
Lead	2.209**	2.634***	2.408**	0.373	0.463	7.016***
	(0.991)	(0.911)	(0.935)	(1.281)	(1.413)	(1.184)
Lead×INDIVIDUAL	3.481**			9.143***		
	(1.558)			(2.206)		
Lead×IND_Bo3		1.411			3.471**	-2.024
		(1.492)			(1.784)	(1.703)
Lea×TEAMCHAT			-2.252			
			(1.822)			
σ_ω	10.81	10.24	10.94	10.75	9.76	10.32
σ_u	13.50	13.25	12.81	9.64	10.24	8.88
Obs.	976	975	965	467	609	636

Note: Subjects with propensity scores in the common support for pairwise treatment comparisons are used in the analysis (Angrist and Pischke, 2009). All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. ** p<0.05 *** p<0.01

Table B5: Random Effects Regressions of Second Mover's Dropping Out

	INDIVIDUAL	INDIVIDUAL_Bo3	TEAM	TEAMCHAT
Lead	-0.149***	-0.061***	-0.067***	0.019
	(0.031)	(0.022)	(0.022)	(0.024)
σ_ω	0.194	0.157	0.231	0.111
σ_u	0.284	0.201	0.259	0.181
Obs.	360	360	720	294
Hausman test for random vs. fixed effects	$\chi^2(12)$=0.00 p=1.000	$\chi^2(12)$=0.17 p=1.000	$\chi^2(12)$=16.98 p=0.150	$\chi^2(12)$=3.38 p=0.971

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. *** p<0.01

Table B6: Random Effects Regressions of Second Mover's Dropping Out in the First and Second Halves of All Rounds

	INDIVDUAL		INDIVIDUAL_Bo3		TEAM		TEAMCHAT	
Rounds	1-6	7-12	1-6	7-12	1-6	7-12	1-5	6-10
Lead	-0.106***	-0.205***	-0.003	-0.109***	-0.082***	-0.069**	-0.002	0.060
	(0.034)	(0.049)	(0.021)	(0.040)	(0.029)	(0.036)	(0.019)	(0.039)
σ_ω	0.109	0.280	0.153	0.152	0.199	0.271	0.000	0.199
σ_u	0.222	0.310	0.127	0.251	0.243	0.275	0.117	0.184
Obs.	180	180	180	180	360	360	148	146

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. ** p<0.05 *** p<0.01

Table B7: Random Effects Regressions of Second Mover's Catches

	INDIVIDUAL	INDIVIDUAL_Bo3	TEAM	TEAMCHAT
Lead	3.153***	2.558***	1.479***	0.324
	(0.646)	(0.592)	(0.535)	(0.744)
σ_ω	4.516	4.272	5.907	4.139
σ_u	5.955	5.396	6.384	5.639
Obs.	360	360	720	294
Hausman test for random	$\chi^2(12)$=0.00	$\chi^2(12)$=0.45	$\chi^2(12)$=17.71	$\chi^2(12)$=8.07
vs. fixed effects	*p*=1.000	*p*=1.000	*p*=0.125	*p*=0.622

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. *** p<0.01

Table B8: Random Effects Regressions of Third Movers' Clicks and Winning

	TEAM		TEAMCHAT	
	Clicks	Win	Clicks	Win
Lead	0.877	0.051	-1.692	0.075
	(1.516)	(0.055)	(2.005)	(0.082)
σ_ω	12.133	0.187	8.363	0.248
σ_u	11.408	0.479	9.515	0.438
Obs.	362	362	158	158

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, session fixed effects and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables.

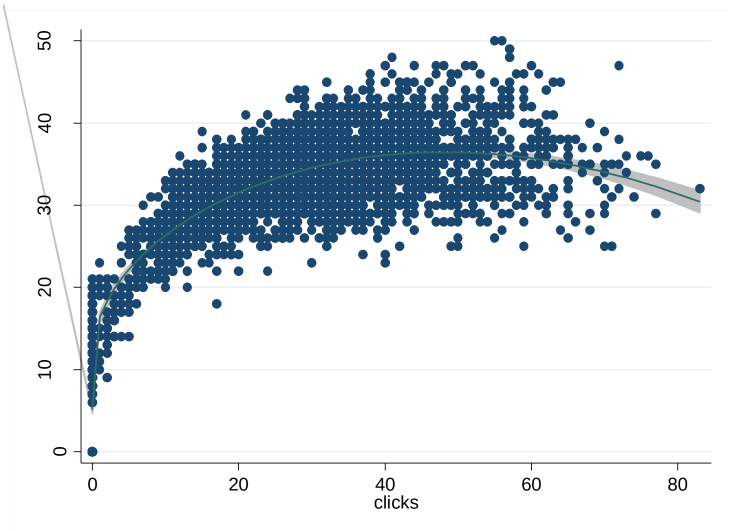


Figure B1: The Relationship Between Clicks and Catches and the Estimated Production Function from Part 1

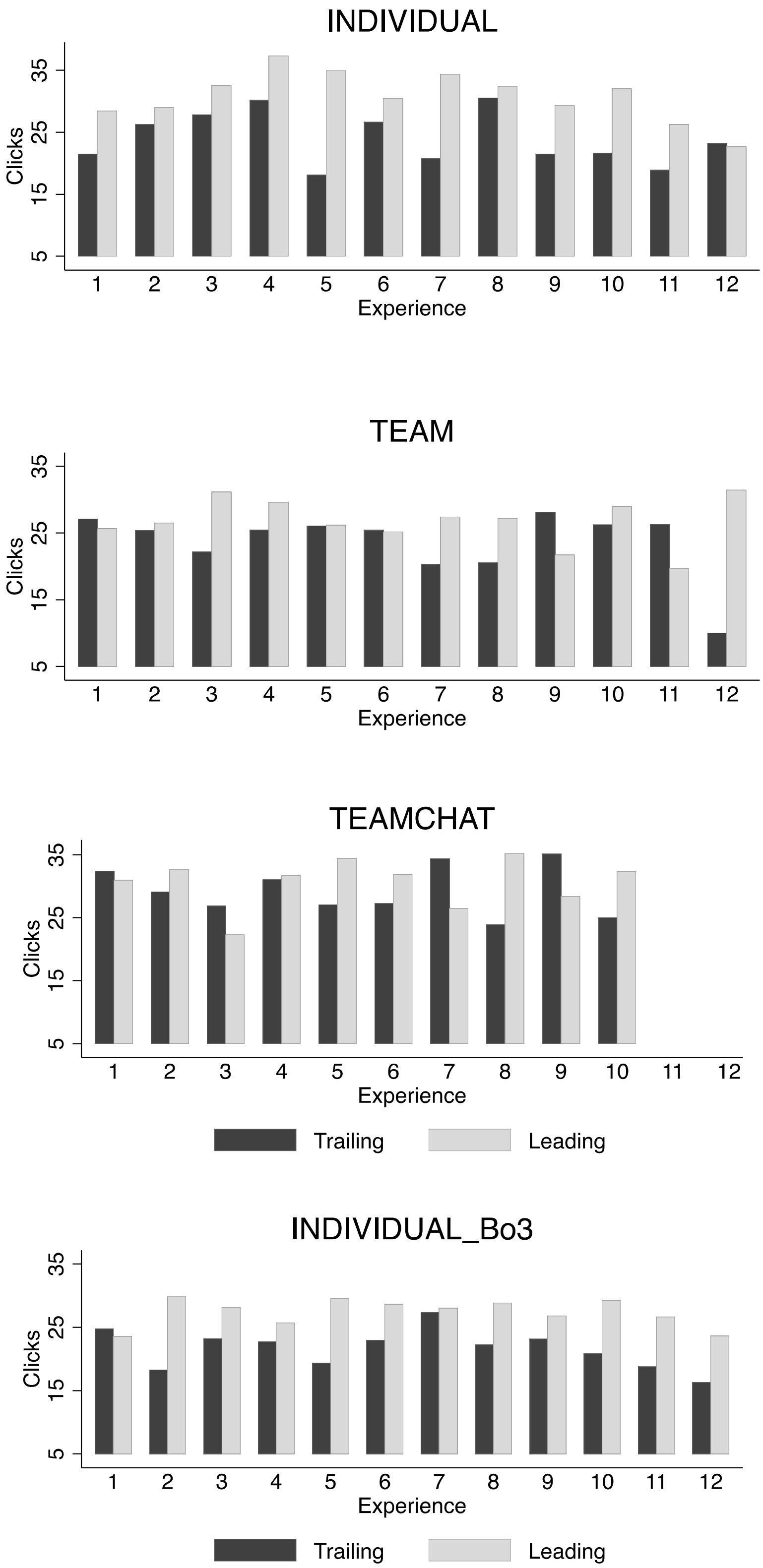


Figure B2: Average Second Mover's Clicks by Experience

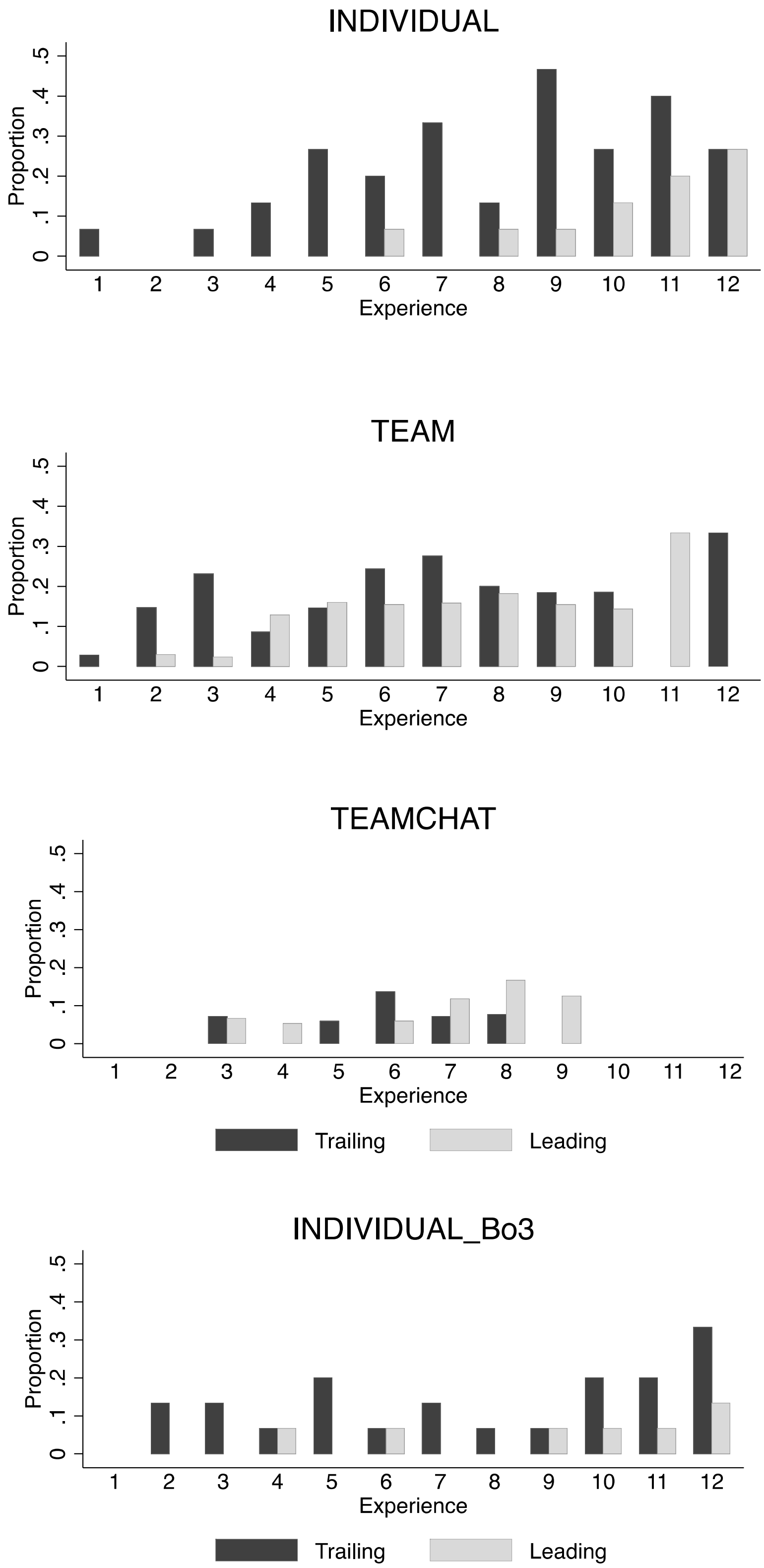


Figure B3: Proportion of Second Mover's Dropping-out by Experience

C. Content Analysis of the Chat Data

We developed a coding system for different types of messages based on reading through parts of the conversations to establish empirically relevant categories of argumentation. Two research assistants were independently trained to code the messages in each round, assigning a tick for each of the categories that showed up in the communication stage for each team member.

The messages could be categorised into four main categories:

- Messages of cheering characters are labelled as *Cheer* (e.g. "We are the dream team", "we are awesome", "good luck!"),
- Messages with a promise to one's team members are labelled as *Promise* (e.g. "I will try my best!", or a response of "OK/ Agreed/ I will" to the statement by a team member, "catch many balls", "try to win", "do your best" etc.),
- Messages urging team members to catch as many balls as possible and disregard the cost of clicks are labelled as *MaxCatches* (e.g. "move as much to catch as much, you win by catching as many balls as possible, I have won 2 games so far with this tactic", "You need to get all the balls - don't worry about the clicks")
- Messages advising team members to try to minimise the expense are labelled as *MinClicks* (e.g. "don't ever move from the first to the last, unless you see two balls coming, only ever move within two spaces", "don't overclick, you will lose tokens if you do").

All other messages were not categorised as they did not deal with the game or the outcome of the game.[14] There were just two instances of messages about strategic effects between battles: third movers urged the first and second movers to do their best so that the outcome did not depend on the third battle, to which the first and second movers responded with "OK" and "will try my best" and these were categorised as Promise. There were no messages pertaining to the strategic situation of second movers or discussion of strategic neutrality. We hence discount the possibility of conscious information sharing and thus learning of the dynamically neutral rational strategy out of team discussion.

The level of agreement between the two coders was assessed by computing the Cohen's kappa coefficient.[15] We find a "Moderate" to "Substantial" agreement in all four categories of

[14] Examples include discussing yesterday's football match, whether they like the experiment or not, or greeting each other.

[15] Cohen's kappa coefficient (k) is a statistical measure of inter-coder agreement used to assess the agreement between two independent coders. $k = \frac{Pr(a)-Pr(e)}{1-Pr(e)}$ where *Pr(a)* is the probability of agreement between coders and

messages with the Cohen's Kappa coefficient always greater than 0.50. In our analysis, we use only those messages that both coders agreed on the category. Table C1 reports the level of agreement between the coders per each message category. In this table, we also calculate the proportion of subjects sending each message category. This is the number of times players sent a message of certain category divided by the total number of times players could have sent a message, which is 890 (89 subjects across the ten rounds). For example, if we only had one player sending a cheering message to her team members in all 10 rounds, this would count as 10/890 for the proportion of Cheer. We find that the most frequent message is of cheering nature and the least frequent message is about minimising clicking: 38.6% Cheer; 16.2% Promise; 30.7% MaxCatches; and 12.0% MinClicks. Pairwise comparisons of proportion of messages sent show significant differences at the 10% level, except we cannot reject the hypothesis that Cheer and MaxCatches messages were sent equally frequently.

Table C1: Observed frequency of categories in chats

	Proportion	Cheer	Promise	MaxCatches
Cheer	0.317***			
Promise	0.114**	(0.000)		
MaxCatches	0.230***	(0.129)	(0.000)	
MinClicks	0.089***	(0.000)	(0.0712)	(0.000)

Note: P-values from paired sample sign tests are in parentheses. * Cohen's Kappa coefficient between 0.3 and 0.4. ** Cohen's Kappa coefficient between 0.4 and 0.6. *** Cohen's Kappa coefficient above 0.6.

In Table C2, we analyse whether the messages exchanged within a team affect second mover's effort and how the type of the message interacts with second mover being on a leading or trailing team. We use the same set of independent variables as in regressions reported in the main text plus dummies for each message category and a variable for the number of message lines exchanged within a second mover's team as a measure of team bonding. Column (1) shows that messages of MaxCatches motivate second movers to make 5 more clicks compared to those whose team did not exchange a MaxCatches message. None of the other categorised messages has a significant effect on second mover's clicks. Column (2) additionally controls interactions of category dummies and whether a second mover is on a leading or trailing team.

Pr(e) is the probability that the agreement is reached by chance. If the coders are in complete agreement, then *k*=1. If there is no agreement among the coders, other than what would be expected by chance, then *k*=0. Kappa values between 0.41 and 0.60 are considered a "Moderate" agreement, and those above 0.60 indicate a "Substantial" agreement (Landis and Koch 1977).

We find that in response to MaxCatches messages, second movers on a trailing team click significantly more often than those on a leading team. Moreover, in response to cheering messages, second movers on a leading team click significantly more often than those on a trailing team. Hence, it appears that trailers are more responsive to MaxCatches, whereas leaders are encouraged by Cheer.

Table C2: Second Mover's Clicks and Messages

	(1)	(2)
Lead	-0.251	-0.814
	(1.504)	(4.276)
Cheer	-2.707	-3.526
	(1.719)	(2.475)
Promise	0.419	-2.591
	(1.609)	(2.177)
MaxCatches	4.954***	8.313***
	(1.860)	(2.569)
MinClicks	-2.375	-3.430
	(1.725)	(2.328)
NumberMessage	0.340	0.345
	(0.227)	(0.229)
Lead×Cheer		1.488
		(3.397)
Lead×Promise		5.946**
		(3.034)
Lead×MaxCatches		-6.009*
		(3.580)
Lead×MinClicks		1.641
		(2.974)
σ_ω	9.814	10.088
σ_u	10.873	10.791
Obs.	293	293

Note: All regressions further include self-reported risk and competitive attitudes, gender, age, nationality, experience dummies, and intercept. Standard errors are in parentheses. σ_ω denotes the squared root of the variation due to the persistent unobserved individual characteristics. σ_u represents the squared root of the variation due to the transitory unobservables. * p<0.10 ** p<0.05 *** p<0.01