Do non-strategic players really exist? Evidence from experimental games involving step reasoning*

José de Sousa† Guillaume Hollard‡ Antoine Terracol§

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Abstract

It has long been observed that players in experimental games differ in their strategic ability. In particular, some players seem to lack any strategic ability whatsoever. These non-strategic players have not however been analyzed per se to date. Using a controlled experiment, we find that half of our subjects act non-strategically, i.e. they do not react to significant changes in the environment. We explore why these subjects perform so poorly. Our design allows us to rule out a number of widespread explanations such as lack of attention, misconception or insufficient incentives. Using reaction time information, we find that these subjects do actually pay attention to relevant changes in the environment, but fail to process this information in an appropriate manner. This inability to act strategically is a robust finding in the sense that it transfers across games. Last, bearing in mind that our subjects are chess players recruited from an international tournament, we ask why their strategic chess-playing ability does not transfer to laboratory games.

JEL classification: D81, D03, C91.

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†Université de Paris Sud, ADIS and Centre d'Économie de la Sorbonne, Université Paris 1 Panthéon-Sorbonne.

‡Paris School of Economics and CNRS.

§Paris School of Economics and Centre d'Économie de la Sorbonne, Université Paris 1 Panthéon-Sorbonne.

1 Introduction

Game theory has had a major influence on economics and social science. Countless articles, in any of the fields of economics, appeal to game theory as a way of predicting the behavior resulting from strategic interactions. For instance, Nash equilibrium is widely used to predict behavior in non-cooperative games. However, Nash equilibrium is often found to be a poor predictor of player behavior in one-shot games played in the lab. The beauty-contest game[1] in particular is a striking example of massive deviations from this equilibrium.

One puzzling feature is that a noticeable fraction of players behave in a rather non-strategic way. More precisely, some players exhibit behaviors that are difficult to account for using reasonable beliefs. As a consequence, non-strategic players typically lose money as they do not respond to the monetary incentives provided in lab experiments. Folk explanations suggest that they do not pay attention to the instructions or are unable to think in a strategic way. However, to the best of our knowledge, these folk explanations have not to date been tested. Non-strategic players are thus often disregarded as representing noise or errors. We consequently know only little about an important category of players who are found in many lab and field experiments (Ostling *et al.*, 2011; Brown *et al.*, 2012).

To better understand non-strategic players, we here design an experimental protocol that consists of three phases. The data from each of these three phases in turn gradually paint a portrait of non-strategic players by providing new evidence and eliminating some commonly-made assumptions (e.g. subjects do not pay attention or are not able to act strategically in general, or the stakes are too low)

A preliminary challenge is to identify non-strategic players in a non-controversial way. We here propose a simple method of doing so. We use the phase 1 data, a series of 10 beauty-contest games, to construct a simple criterion that controls for beliefs. We then test whether this criterion has any out-of-sample predictive power in explaining the data from phases 2 and 3, which include a different game. We find a considerable proportion of non-strategic players, almost one half of the sample. Moreover, phases 2 and 3 confirm that players who were classified as non-strategic in phase 1 continue to act non-strategically in subsequent games.

Our design allows us to rule out a number of explanations which can explain the considerable proportion of non-strategic players found in our experiment. Using reaction time data, we show that non-strategic subjects *do* exert effort and spent time thinking about the games; they also *do* pay attention to relevant changes in the environment but *fail* to take these changes into account.

[1]The beauty-contest or guessing game is fairly simple, as described by Nagel (1995): a large number of players have to state simultaneously a number in the interval [0, 100]. The winner is the person whose chosen number is closest to the mean of all of the numbers chosen multiplied by a common knowledge positive parameter p. For $0 \leq p < 1$, there is a unique Nash equilibrium in which all of the players announce a value of zero.

Even when the stakes are raised, non-strategic players are unable to process information in a relevant manner. Phase 2 allowed us to test the robustness of this finding. Even when all possible precautions have been taken to ensure that they understand the rules, the players continue to act randomly. Furthermore, since our subjects are chess players recruited during an international tournament,[2] we can safely rule out the possibility that non-strategic subjects are simply unable to act strategically in general or are suffering from deficits in cognitive ability. Indeed, the Elo ranking – which measures the ability to play chess – does not have any significant influence on the likelihood of being classified as non-strategic. Last, phase 3 allowed us to check that possible misconceptions about the reality of the cash payment did not play an important role.

Our results overall suggest that the existence of non-strategic players in one-shot games is a robust feature. Rather than disregarding non-strategic players as noise, we believe that they should be considered as one of the main empirical regularities found in situations in which economic agents are confronted with new situations.

The remainder of the paper is organized as follows. Section 2 presents the experimental design, and Section 3 discusses the phase 1 results, which allow us to draw a first sketch of non-strategic players. Section 4 then exploits phase 2 data to test the robustness of our preliminary conclusions and render our portrait of non-strategic players more detailed. Section 5 exploits phase 3 data to complete our portrait. Last, Section 6 concludes.

2 Experimental design

We recruited 270 chess-players during a major international tournament held in Paris in 2010. Subjects were approached while they were at the tournament (but not playing). They were then allocated to an adjacent room that serves as an experimental lab. The experiment was computerized. All players read the instructions on the screen; these were also read aloud by the experimenter. Subjects were allowed to ask questions.

Our experiment consisted of three phases:

Phase 1. Subjects were asked to play a series of 10 beauty-contest games. They had to choose a number as close as possible to $m\times$mean (where mean indicates the mean of the answer of all players). The parameter m took on two values: $m = 2/3$ and $m = 4/3$.

Each game was played against five types of opponents, labeled as A, B, C, D and Random.

[2]Chess players were used in this experiment as we thought that they should exhibit some minimal ability to play games, i.e. they should be able to figure out that they are facing an opponent. We are fully aware that chess players may not be very different from usual lab subjects, even if this point is subject to controversy (see the discussion in Levitt *et al.* (2009) and Palacios-Huerta and Volij (2009), as well as the evidence from a beauty-contest game in Buhren and Frank (2010)).

The letters indicates the Elo-Ranking of the opponent who were thus explicitly identify as chess-players.[3] "Random" indicates that the subject is facing a random device that will select a strategy using a uniform probability distribution over the strategy space. Subjects played 10 different games: one game against each type of opponent (A, B, C, D or Random) for each value of $m \in \{2/3, 4/3\}$. The order of the ten games was randomized. Subjects received no feedback during the 10 games.

All treatments were identical except that half of the subjects played the beauty contest against *two* opponents of the same level (i.e. A, B, C, D or Random), while the other half played against *one* opponent only. This difference matters as the two-player version of the game has a dominant strategy, while the three-player version does not. In addition, as the payment rule is the same (10 points for each of the ten games, to be shared among the winners), there is a difference in expected earnings: those who play the three-player version of the game earn 33.33 points on average, while those playing the two-player version of the game earn 50 points on average.

Phase 2: After playing their ten beauty contests, subjects start a new game, the 11-20 game (described below). They played this game only once, against another chess player whose level was not specified. We added questions to ensure that subjects had understood the rules before starting the game.

After completing the eleven games (the ten beauty contest plus the 11-20 game), the screen displayed the numbers chosen by players in the 10 beauty-contest games and in the 11-20 game. Subjects were given the opportunity to observe the consequences of their actions. Each action was associated with a number of points and these points were in turn converted into Euros according to a previously announced exchange rate of .2€ per point. Subjects then proceeded in turn to another room where they individually and anonymously received their payments in cash.

Phase 3: *After* receiving their cash payment, subjects were offered the chance to take part in an additional beauty-contest game (with $m = 2/3$) involving all of the participants in the experiment. Subjects were informed that the name of the winners would be publicly announced at the end of the chess tournament. The tournament lasted for 10 days, so players had to wait up to 10 days before receiving their payment were they to win this last game. The two best players (i.e. the two closest to the winning number) both received a cash payment of 150€. These results were publicly announced immediately after the official announcement of the results of the chess tournament. As our subjects were not the usual lab subjects, we were

[3]The letters correspond to the following ranking: A=Elo $\geq$ 2150, B=2150 $>$ Elo $\geq$ 1800, C=1800 $>$ Elo $\geq$ 1500, D=Elo $<$ 1500.

worried that they might not believe that they would really be paid. We thus proposed this additional game *after* they had received their first cash payment, so as to make our promise of a further cash payment credible. Note that at this stage players had also received some feedback regarding the results of their actions in the first two phases.

2.1 Theoretical predictions

We use two games in this paper. The first, the beauty-contest game, is well-known to experimentalists and its theoretical and empirical properties have been well-described. We thus restate the main results. The second game used was recently introduced by Arad and Rubinstein (2012): we will thus present this game in more detail.

2.1.1 The beauty-contest game

The beauty-contest game has been widely used in game theory to capture the notion of step reasoning (see Buhren *et al.*, 2009 for a historical account). Each player i in this game chooses a number x_i between 0 and 100. The goal is to choose the x_i that is the closest to the target of $m * (\sum_{i=1}^{n} x_i)/n$, where m can take on different values and n designates the number of players. The player whose x_i is the closest to the target wins a fixed prize, while the other players receive nothing.

For $m < 1$, the unique equilibrium in the beauty-contest game is where all players choose to play 0.[4] We will also consider a version of the game with $m > 1$. In this case, the focal equilibrium is that where all players choose 100.[5]

One interesting feature of the beauty contest is that there is a (weakly) dominant strategy when there are only two players: this strategy is to play 0 when $m < 1$ and 100 when $m > 1$. However, with three or more players there is no longer any dominant strategy.

In the popular case where $m = 2/3$, the mean value chosen in the literature is around 35, which is far removed from the equilibrium prediction. Almost no subjects are found to play the equilibrium strategy in one-shot games. A variety of different subject pools have played this game, including chess players, with results remaining fairly stable across groups regarding the small numbers who play the equilibrium.

[4]Note that all players playing 1 may also be an equilibrium if the strategy space is restricted to integers. It is also important to specify that in the case of a tie, either players share the prize or the prize is randomly allocated to one player (in our case, we broke potential ties randomly). If all players receive the entire prize in the case of a tie, additional equilibria may exist

[5]When there are three or more players, there also exists an unstable equilibrium in which all players play 0. This equilibrium no longer exists when there are only two players. See Lopez (2001) for more details on the equilibrium set for integer games.

2.1.2 The 11-20 game

The 11-20 money-request game was recently introduced by Arad and Rubinstein (2012) and presented to subjects as follows:

> "You and another student are playing a game in which each player requests an amount of money. The amount must be an integer between 11 and 20 shekels. Each player will receive the amount he requests. A player will receive an additional 20 shekels if he asks for exactly one shekel less than the other player. What amount of money would you request?"

This game is different from similar games, like the traveler's dilemma, in that to win a prize you have to play *exactly* one step lower than the other player. Given the structure of the game, there is no Nash equilibrium in pure strategy. Assuming both players to be expected gain maximizers, there is a unique symmetric mixed-strategy Nash equilibrium.[6] The symmetric equilibrium distribution puts a zero probability on strategies 11 to 14, probability 1/4 on strategies 15 and 16, and probabilities 4/20; 3/20; 2/20 and 1/20 on strategies 17, 18, 19 and 20, respectively. This equilibrium distribution is not at all obvious to identify, and depends on the assumptions made regarding players' utility functions.

To the best of our knowledge, this game has only been played with students. Arad and Rubinstein found that even students who are trained in game theory do not play as theory predicts. However, their student results do provide a benchmark for the behavior of subjects who are expected to be amongst the most strategic.

3 A first portrait of non-strategic players

Using data from phase 1, we first provide a criterion to classify players as strategic or not. By "non-strategic", we mean that their behavior cannot be rationalized by plausible beliefs. A natural question is the degree to which non-strategic players look like random players. Random players, also known as level-0 players in reference to level-k models [7], are defined as players who simply pick a strategy at random out of the strategy space. We thus explore whether non-strategic players behave like random players. Last, we test whether the most common assumptions put forward to explain the existence of non-strategic players can explain our results. We end up rejecting these. Extrapolating from the collected evidence, we propose a first portrait of non-strategic players.

[6] There are four other asymmetric mixed strategy equilibria.

[7] These models were first presented in Stahl and Wilson (1994 and 1995) and Nagel (1995) and have given rise to numerous publications including Camerer, Ho and Chong (2004), Crawford and Iriberri (2007) and Crawford, Costa-Gomes and Iriberri (2010)

3.1 Looking for non-strategic players

The series of 10 beauty-contest games, i.e., phase 1 of our protocol, allows us to propose a very minimal rationality requirement which strategic players should satisfy. When confronted with a device that chooses randomly, as in our "random" condition, playing *lower* when m=4/3 than when m=2/3 is difficult to rationalize with any set of beliefs. This criterion has the advantage of better capturing the notion of a non-strategic player than the identification of those who use dominated strategies. We here extend this criterion to games played against humans, and it seems reasonable to assume that strategic players will play differently as m varies while they are facing the same opponents. We thus count the number of times each individual played lower with m=4/3 than with m=2/3, when facing the same type of opponent.[8] This identification of non-strategic players allows us to control for beliefs without appealing to any complex belief-elicitation mechanism, which would not be convenient given that our experiment was designed to last no longer than 20 minutes.

As there are five pairs of games against the same opponents (opponents of level A, B, C and D, and Random), there are five observations per player, which we can use to calculate a "Pairwise Rationalizable Actions Index" (PRAI). This allows us to split players into six categories: an index value of 0 indicates that the subjects systematically play a lower number when m=4/3 than when m=2/3; a value of 5 indicates that no such violation of consistency occurred. Table 1 shows the distribution of players according to this index.

Table 1: Classification of players according to PRAI

Index value	N	Frequency
0	15	5.6
1	21	7.8
2	26	9.6
3	61	22.6
4	46	17.0
5	101	37.4
Total	270	100.0

We use this index to identify a subgroup of non-strategic players and then carry out various tests to investigate this particular group in more detail. In what follows, we consider two groups of players: those with a PRAI value of 0 to 3 and those with values of 4 or 5. For ease of exposition, the first group will be referred to as "non-strategic" and the second as "strategic". This is a slight abuse of language since thus far we can only claim that non-strategic subjects

[8]For the sake of completeness, it is possible that some players hold very extreme beliefs about their opponents in the three-player version of the game which allow them to rationalize playing lower in the 4/3 than in the 2/3 version of the game. This is very unlikely to occur but however remains theoretically possible.

sometimes use strategies that can not be rationalized, i.e. their behavior cannot be though of as being a best-response to their subjective beliefs. But, as the next sections will explain, there are good reasons to think that these subjects are non-strategic in a broader sense.

As any classification is debatable, it is worth asking whether alternative cut-off points of the PRAI index lead to similar results. Appendix B shows the distribution of actions according to each value of the PRAI index. The behavior of categories 0 to 3 appears to be similar, while that of categories 4 and 5 is notably different, suggesting that our classification is meaningful.

We obviously do not pretend to have identified all non-strategic players or actions using our PRAI. Our empirical strategy is to identify a subgroup of players who fail in some respect to act strategically and then carry out various tests to investigate the behavior of this particular sub-population in more detail.

3.2 Evidence on strategic and non-strategic players

The beauty-contest game has been played many times, with various kinds of subject pools. The mean value for the 2/3 version of the game is often found to lie between 35 and 38, with a standard deviation ranging from 20 to 25. Our subjects play slightly higher than the usual lab subjects (students) at close to 42. We obtain similar results in the 4/3 version of the game, with chess-players playing slightly lower. Our results are not particularly high: Camerer *et al.* (2004) discuss experiments in which more extreme values are sometimes observed (e.g. they report a mean value of 54 in the 2/3 version of the game) and Agranov *et al.* (2012a) find similar figures, especially when players have a limited time to think about the game (30 seconds).

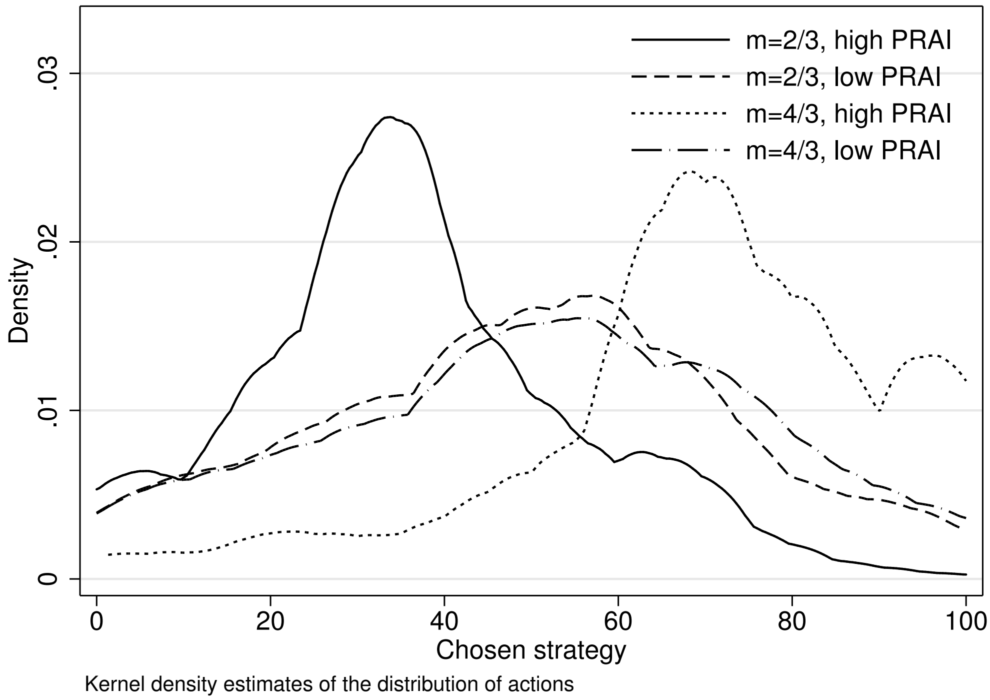
We split our sample into two subgroups based on the index described above. We first compare the behavior of non-strategic and strategic players as the key parameter of the game changes from 2/3 to 4/3. One group, our non-strategic players, does not react as m varies. Non-strategic players, on average, play something close to the salient value of 50, whatever the value of m (48.98 when $m = 2/3$ vs. 51.25 when $m = 4/3$). In sharp contrast, strategic players react in the expected direction as m changes. They play on average 36.12 when $m = 2/3$ and 70.09 when $m = 4/3$.[9] Their average behavior is roughly[10] in line with that of players who best-respond to opponents who select their strategy using a random distribution with a mean of 50.

[9]Descriptive statistics on the choices of strategic and non-strategic players can be found in Tables 8 and 9 in Appendix D. Fixed-effects regressions of choices on a dummy for $m = 4/3$, controlling for opponent type and period, yield an estimated coefficient of 2.19 (p-value=0.072) for non-strategic players and of 33.91 (p-value$<10^{-3}$) for strategic players. The difference in the explanatory power is also striking, with a within R-squared of 0.018 for non-strategic players and 0.528 for strategic players.

[10]More precisely, by roughly we mean that strategic players behave as if they were playing a best-response against level-0 players, but they make an often-observed mistake: they fail to take into account the fact that their own choice is included in the calculation of the mean. A value of 33 is a best response to players playing 50 only in games which involve a large number of players.

In that sense they resemble the description of level-1 players. Figure 1 shows these differences between the two groups. That the two groups are not the same is no surprise given the way in which they were constructed. One potential issue is that our classification may have created an artificial difference amongst an otherwise homogeneous population. As further evidence will make clear, the two groups do indeed differ on various dimensions that cannot be influenced by the way the PRAI is constructed (e.g. reaction times in phase 2).[11] The differences between strategic and non-strategic players are thus informative.

Figure 1: Strategies chosen by low and high PRAI players



Had we identified non-strategic players as those using dominated strategies, we would have missed out a considerable percentage of non-strategic players (56 out of our 123 non strategic players did not play dominated strategies). Using information from a series of 10 games allows us to collate more information on each player and leads to a more accurate classification. As a consequence, the fraction of non-strategic players increased. Agranov *et al.* (2012b) in a very similar setting also found that about half of their subjects, do not react when their environment changes. The fraction of non-strategic players found is also very much in line with the estimated fraction of level-0 players found from the estimation of Camerer *et al.* (2004)'s cognitive-hierarchy model. The corresponding estimations are presented in Appendix B.

We next examine whether non-strategic players behave like level-0 players. Level-0 players

[11] We also address this issue by providing simulations, presented in Appendix A, that show that the distribution of our sample according to the PRAI index would have been different had all players simply picked strategies at random.

are assumed to pick a strategy randomly from the strategy space without any further consideration of the rules of the game or their opponents' strategy. It is possible that all the players currently classified as non-strategic have adopted a deterministic strategy that they use in all ten games. As shown in Table 2, we can rule out this possibility. The two panels in this table show the correlation coefficients between the five choices for each value of m. The choices are numbered in the order in which they are played. The coefficients for strategic subjects (Table 2, top panel) range from 0.649 to 0.791, while those for non-strategic players (Table 2, bottom panel) are much lower and range from 0.154 to 0.372. Moreover, for non-strategic players, the correlation between choices falls as the time between choices rises, which is not the case for strategic players. Overall, non-strategic players seem to pick strategies (almost) randomly, while strategic players appear to be much more consistent across games.

Table 2:
Correlations of choices over time: strategic vs non-strategic players

	Choice 1	Choice 2	Choice 3	Choice 4	Choice 5
Strategic players					
	Choice 1	Choice 2	Choice 3	Choice 4	Choice 5
Choice 1	1.000				
Choice 2	0.696	1.000			
Choice 3	0.705	0.790	1.000		
Choice 4	0.648	0.721	0.745	1.000	
Choice 5	0.666	0.740	0.744	0.782	1.000
Non-strategic players					
	Choice 1	Choice 2	Choice 3	Choice 4	Choice 5
Choice 1	1.000				
Choice 2	0.358	1.000			
Choice 3	0.214	0.280	1.000		
Choice 4	0.217	0.153	0.283	1.000	
Choice 5	0.277	0.199	0.241	0.371	1.000

Interpretation: The correlation coefficient between the values chosen the first and second time the subjects played games with same value of m is 0.694.

We may well wonder whether acting in a non-strategic way affects earnings. As can be seen from Table 3, earnings increase in an almost monotonic fashion with the value of the PRAI index. Players with an index value of 0 make only half as much as do those with an index value of 5 (3.6 vs 7.2). As expected, acting non-strategically entails considerable financial losses. Non-strategic players do spend time thinking and do exert effort (see below) but are literally leaving money on the table by not acting strategically.

As a result, we can rule out the possibility that many subjects are using heuristics which we do not understand but which are nonetheless effective.[12] Were some strange but effective

[12]For example, recent evidence suggests that a fair proportion of subjects in the guessing game - a game

Table 3: Earnings in Euros by PRAI level

Index value	Earnings Mean	Std. Dev.	p-value
0	3.6	1.9	
1	4.7	1.7	0.108
2	5.1	2.0	0.451
3	5.5	1.8	0.294
4	5.4	2.2	0.677
5	7.2	2.7	0.000

The p-values reflect the t-test of a difference in earnings relative to the previous index level

strategies to have been used by more than a small fraction of players, we would have seen less significant differences in earnings across groups.

3.3 Testing common assumptions for the existence of non-strategic players

The reason why some players perform so poorly in experiments is not currently well-understood. In what follows we test the most common assumptions that have been advanced in the literature. Broadly speaking the most common of these is that non-strategic players do not exert effort or pay attention. This could be because they think that the stakes are too low or because they are simply unable to perform the task required. In what follows, we consider these assumptions in the light of the specific features of our protocol.

3.3.1 Do non-strategic players simply not pay attention?

It is difficult to know whether non-strategic are paying attention or exerting effort. Our data do however offer some insights into this question. We recorded the reaction time in the beauty-contest games: these offer a guide to the cognitive effort exerted by subjects. The first surprising result is that our non-strategic players seem to spend *more* time thinking about the problem than do the other players: the former spend on average 28.87 seconds on each decision, as compared to 25.38 seconds for the latter. The p-value of the t-test on this difference at the individual level is 0.059.[13]

This difference in reaction times also holds for the 11-20 game in phase 2, as level-0 players also spend more time thinking (196.83 seconds vs 175.1; p-value=0.029). The fact that level-0 players reflect longer than do more strategic subjects suggests that the difference between

similar to the beauty contest - do use some kind of rule that is not described in standard game theory but which however makes sense, as in Fragiadakis *et al.* (2012).

[13]The recorded time for the first decision includes the time taken to read the instructions. We thus do not have a meaningful reaction measure for the first decision.

strategic and non-strategic players lies in the way subjects process strategic information, rather than in the effort or attention they put into the game.

The perhaps most surprising results refer to reaction times as the parameter m changes. The tendency across games is for subjects to play faster and faster. However, at some point they are confronted with a change in the parameter m. Recall that the order of the ten games is randomized. Some players will for instance play three games in a row in which $m = 2/3$ and then play the fourth with $m = 4/3$. When subjects are confronted with this kind of change, they need to adapt to a new environment and so increase their reaction time compared to the previous game. Intuition suggests that non-strategic players will not be affected by these changes, as their strategy does not vary with m. However, we actually find that non-strategic players do also react to changes in the value of m. A fixed-effects regression of the reaction time on a dummy for the first change and its interaction with our strategic/non-strategic classification, controlling for period, shows that being faced with the first change in m increases reaction time by 5.39 seconds (p-value=0.066) for strategic players, and that this increase is not significantly higher for non-strategic players (the interaction term attracts a coefficient of 1.16 with a p-value of 0.802).[14] This evidence strongly suggests that non-strategic players are aware that something has changed, but that they fail to take this change into account.

3.3.2 Are the stakes too low for subjects to provide any effort?

One possible confound here is that the stakes are too low, so that the expected gains are too slight to make it worth exerting any effort. Our experiment lasted about 20 minutes and subjects were already on site (mostly chatting or hanging around). They received on average about \$16 (11€) for these 20 minutes. Given that subjects did not pay any transport costs, the earnings correspond to an hourly wage of \$48 (33€) which is not different from usual earnings in the lab and perhaps even somewhat higher. We may however continue to wonder about the importance of stakes.

Our design also allows us to test whether expected earnings have any effect. We vary the number of players but keep the winner's reward constant. As such, players in the two-player version of the game have a higher expected payoff (50 points, i.e. 10€) than those in the three-player version (33.3 points, i.e. 6.66€). We thus calculate our index separately for these two populations. We found no significant differences between the distribution of players according to our index, with the proportion of non-strategic players being the same: we classify 45.52% of subjects as non-strategic in the two-player version of the game, versus 45.59% in the three-

[14] Also note that the period in which the first change takes place is the same for both player types (2.53 vs 2.62, p-value=0.43).

player version. The non-strategic percentage is thus not sensitive to a 50% rise in expected payoffs. Were stakes to explain the existence of non-strategic players, their fraction would have risen significantly.

We can therefore rule out the possibility that non-strategic players deliberately ignored the incentives as they considered them to be too low.

3.3.3 Are non-strategic players simply unable to think strategically?

The reason behind our recruitment of chess players during an international tournament was exactly to rule out the possibility that subjects could not think strategically. Whatever ability is required to play chess, players by definition have to think about what their opponent will do. We are therefore sure that our subjects, including those who we classify as non-strategic, are actually able to think strategically. Perhaps surprisingly, the Elo ranking plays only a limited role, if any: the average Elo ranking is very similar across our index. Non-strategic players have a mean Elo ranking of 1768 (with a standard deviation of 30), with corresponding figures for strategic players of 1814 (27). This difference is not significant (p=0.25).

3.4 Lessons from phase 1: A first portrait of non-strategic players

Evidence from phase 1 data allows us to paint a first portrait of non-strategic players. The common explanations (lack of attention or effort, limited cognitive ability, insufficient stakes) do not appear sufficient to explain our data. Non-strategic players seem to do their best to play the games (they spend time thinking about them and understand when the game changes). At this point, our best guess is that non-strategic players do perceive the necessary information, but fail to process it in an appropriate way. In what follows, we will further test this assumption.

4 Phase 2 data: robustness checks

The previous section, which covered the first phase, offered a number of insights into the behavior of non-strategic players. Phase 2 of our experiment consists of a single play of the "11-20" game described above. This phase 2 data will allow us to (1) have more control over the instruction phase and (2) test whether our classification from phase 1 is robust across games (i.e. can we make out-of-sample predictions?). We address these two issues in separate subsections.

Table 4: Number of attempts required to successfully answer four questions, by PRAI level

Level	Mean no. of attempts	Median no. of attempts	SD
high PRAI	4.74	4	1.09
Low PRAI	5.67	6	1.40

4.1 Are the instructions producing non-strategic players?

Poor instructions may lead a significant fraction of subjects to misunderstand the rules of the game. In phase 1 we used standard instructions: explanations were both displayed on the screen and read aloud by the experimenter, and subjects had the opportunity to ask questions. In that respect, our instructions are standard. However, we may want to have more control over whether subjects pay attention to the instructions.

The 11-20 game was selected because the instructions are short (only a few lines on the screen) and simple to understand. We further added four comprehension questions, to which subjects had to provide correct answers in order to be allowed to proceed to the game. In particular, subjects were asked to state the payoff of each player in different situations. This allows us to safely rule out the possibility that players did not understand the rules of the game, and thus to limit any misconceptions.

Table 4 shows that non-strategic players require significantly more attempts to provide these four correct answers. Non-strategic players made something like two mistakes (their median number of attempts is six) while most strategic players answered the four questions correctly (with a median value of four). This result reinforces our interpretation of a structural difference between strategic and non-strategic players. Non-strategic players have more trouble learning the rules of a simple game than do other subjects. As such, non-strategic players seem to be slow learners.

One intriguing fact is that the existence of non-strategic players probably has something to do with the instruction phase. Costa-Gomes and Crawford (2006) used very long instructions and found almost no level-0 players in their sample. On the contrary, Georganas *et al.* (2010) used shorter instructions and found a substantial fraction of level-0 players. Our experiment uses minimal instructions in phase 1, but much more detailed instructions for phase 2, including some quiz questions to detect subjects who did not understand the rules. However, despite our efforts to train subjects, non-strategic players still act randomly (as will be explained below). This raises subtle questions about the way in which subjects should be instructed. Existing evidence (e.g. Chou *et al.*, 2009) suggests that instructions do matter but their impact is still not perfectly understood.

4.2 Is our classification robust across games?

The stability of players' strategic levels across games is a fairly open question. For instance, should we expect a level-2 player in one game to behave as such in a subsequent game? Burnham *et al.* (2009), found that players with a low IQ are much more likely to play dominated strategies in the beauty-contest game and to be classified as level-0, suggesting that being a level-0 player might be a relatively stable individual characteristic across games. However, recent evidence in Brañas-Garza *et al.* (2012) has shown that the cognitive-reflexion test is associated with an identifiable pattern in the beauty-contest game, while another, the Raven test, is not. Other authors, such as Georganas *et al.* (2010), have found only little consistency across games. It is rather unclear whether this absence of any empirical regularity reflects the true absence of stability across games. It could also be the case that the way in which players are assigned to a level is debatable, or that the definition of the levels is problematic. In particular, different definitions of what is level-0 lead to different definitions of higher types, and it is not always easy to assess the stability of levels across games.[15]

To test whether our classification from the beauty-contest games has predictive power for behavior in the 11-20 game, we consider the behavior of each group separately. Figure 2 depicts the empirical cumulative distribution function (CDF) for each of our six PRAI levels, as well as the equilibrium CDF. It is clear from the figure that, although players at all levels fail to play the equilibrium strategy, they are closer to doing so as their index level increases. To test this more formally, we run a probit regression where the dependent variable is 1 if the subject chose an action which is not a mixing strategy in the Nash equilibrium. The results are displayed in Table 5, and indeed show that the probability of playing such an action falls with our index.

Table 5: Probit regression of out-of-equilibrium actions

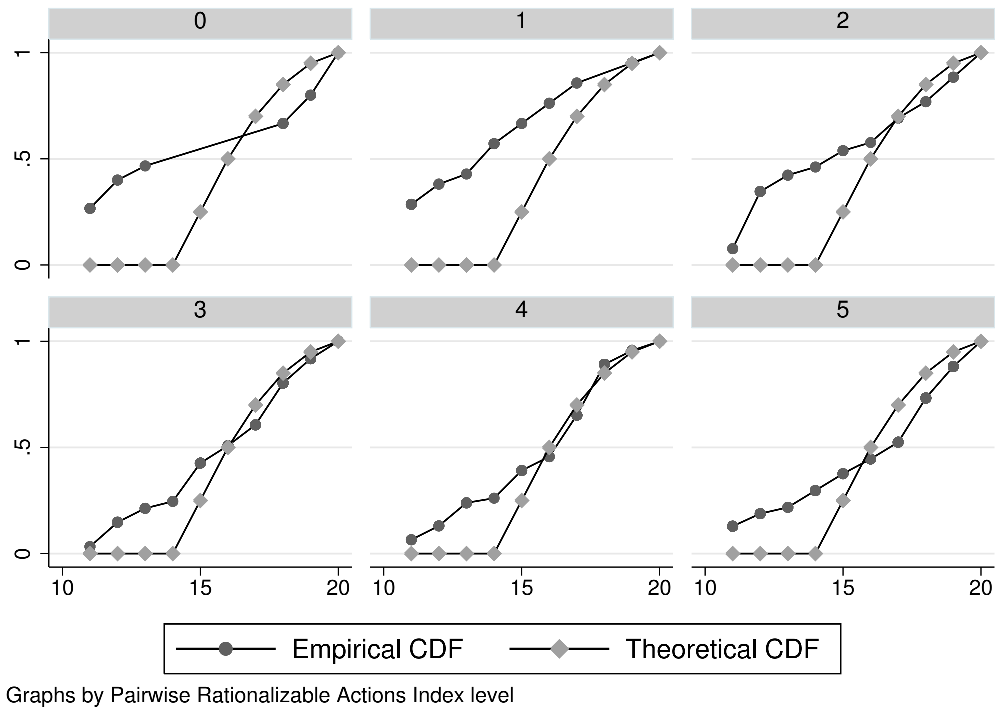
Variable	**Coefficient**	**(Std. Err.)**
PRAI level	-0.125*	(0.052)
Intercept	-0.021	(0.195)
N	270	
Log-likelihood	-167.473	
$\chi^2_{(1)}$	5.671	

Significance levels : † = 10%, ∗ = 5%, ∗∗ = 1%

It is also the case that the cumulative distribution of the strategy chosen by low-level players is not distinct from the uniform distribution, while it is so for the high values (i.e. 4 or

[15] We can here refer to an ongoing project by Bhui and Camerer (2011) which proves that the simple correlation across two games played by the same player may be too demanding a test for stability across games. They suggest rather the use of something like Cronbach's α.

Figure 2: Cumulative density functions in the 11-20 game



5). More precisely, running a Chi-squared test against the discrete uniform distribution over $\{11, 12, \ldots, 20\}$, we find a p-value of 0.22 for low-level players and 0.0003 for high-level players.

To the best of our knowledge, there are not many existing results confirming that strategic sophistication can be used to make out-of-sample predictions, especially when the games are different. Two remarks are however in order. First, we do not make sharp predictions, but only predict that non-strategic players will still behave "randomly", although using a different distribution. Second, our predictions are based on the outcomes of ten games. In that sense, we use a lot of more information compared to predictions based on only one game, as in Georganeas *et al.* (2010).

4.3 Lessons from phase 2: A clearer portrait of non-strategic players

Adding evidence from phase 2 allows us to provide more detail. Non-strategic players seem to have difficulty in learning in an abstract environment. Even carefully-designed instructions are not enough to generate more strategic behavior from the players who were classified as non-strategic in phase 1.

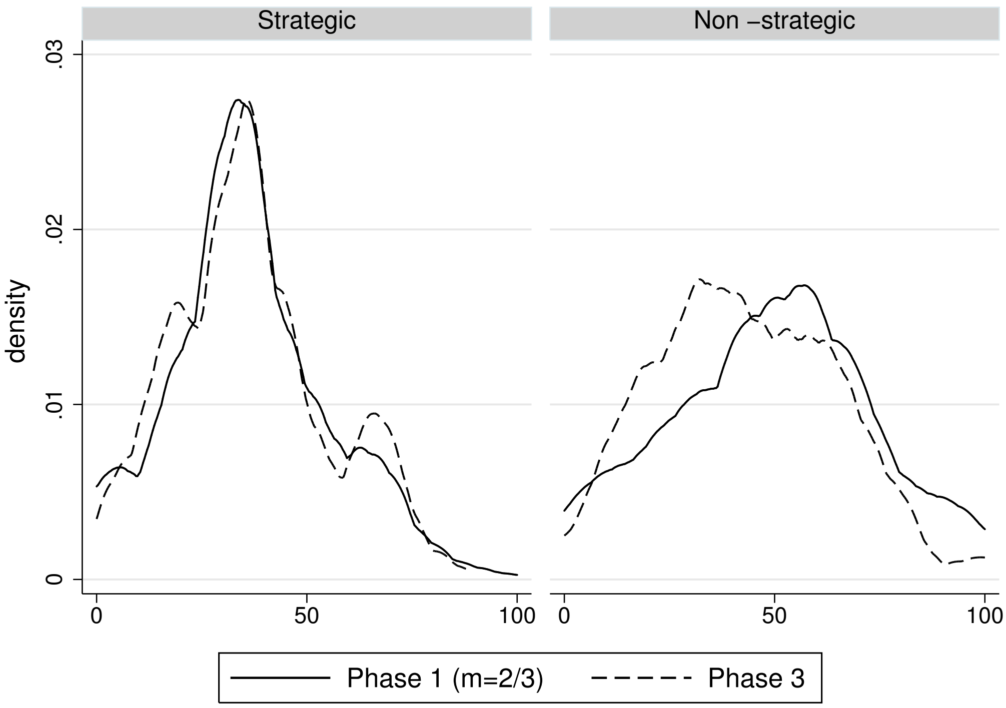
5 Phase 3: increasing stakes and feedback

Phase 3 was intended to make sure that subjects believe that the cash payment will be implemented. One last game was thus proposed after they had received their payment. To reinforce the idea that this was a serious proposition, we also informed them that results would be publicly announced at the same time as the results of the chess tournament. Furthermore, we raised the stakes to about $200 (150€). Subjects thus faced a situation in which they had a small chance of winning a large prize. Experimental results have shown that subjects generally overestimate such small probabilities.

As good practice in experimental economics recommends that subjects can check that their payment corresponds to the announced rules, they had the opportunity at the end of stage 2 to observe the consequences of their actions. However, they did not know at this point that they would be offered the opportunity to play one last game. The effect of feedback is thus not well controlled for.

The overall effect was limited (Figure 3) as subjects play 39.4 on average here, compared to an average of 42 in phase 1 when $m = 2/3$. The graphs show that strategic players used very similar strategies to those that they used in phase 1. Non-strategic subjects nevertheless improved a bit. So even if it is not possible to attribute this slight improvement to one particular feature of the game, we can claim that none of the introduced changes had a noticeable impact. We here compare games that are very similar, but which however have some notable differences (e.g. the number of players is not the same and the stakes are different). However, the two groups are still significantly different (a Student test yields a p-value of 0.0125, and a Kolmogorov-Smirnov test a p-value of 0.004). As a conclusion, we can claim that there is no simple trick that would allow us to greatly enhance the degree of sophistication of our subjects.

Figure 3: Actions in Phase 1 ($m = 2/3$) and Phase 3



6 Conclusion

The evidence we have presented here has shown that the existence of non-strategic players is a robust finding. Our design allows us to rule out a number of explanations which can explain the considerable proportion of non-strategic players found in our experiment. Non-strategic players do exert effort and spend time thinking about the games; they also detect relevant changes in their strategic environment. Phase 1 suggests that non-strategic players have trouble in processing information in an appropriate manner. Phase 2 allowed us to test the robustness of this finding. Even when confronted with a simple game (i.e. easy to learn and understand), they continue to act randomly. Phase 3 allowed us to check that possible misconceptions about the reality of the cash payment did not play an important role.

The distinction between strategic and non-strategic players remains similar across games: our classification thus has some out-of-sample predictive power. As non-strategic players resemble level-0 players, this suggests that adequately identifying non-strategic subjects allows us to establish a form of stability of strategic levels across experimental games. However, the distinction between strategic and non-strategic players does not correspond to field behaviour. Even strong chess players may end-up being classified as non-strategic in our experiment. This suggests that strategic ability is not an individual characteristic. Our favorite explanation is that some players may have a hard time learning new games in an abstract way, e.g. without receiving any feedback. In classic lab experiments, subjects have to learn within the space of a few minutes how to behave in new, and fairly complex, situations. Non-strategic players may well improve greatly when given the chance to learn via feedback. Chess players are a striking example of agents who have successfully learned strategic behavior over the long run but show no particular strategic skill when facing a new situation. This distinction may contribute to a better understanding of the often-observed gap between field and lab experiments (Levitt *et al.* 2010).

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Appendix

Appendix A: Simulating our Pairwise Rationalizable Actions Index (PRAI) for a homogeneous population

In this Appendix, we run simulations to assess whether the observed distribution of PRAI could have arisen by chance from a homogeneous population. We assume that the population is homogeneous and only composed of random players randomly drawing their actions from the joint empirical distribution of actions. More specifically, each run of the simulation creates 270 individuals. For each individual we draw 5 pair of actions. Each pair is drawn from the empirical joint distribution of pairs of actions against each type of opponent (i.e. A, B, C, D or Random). We thus end up with 5 pairs of actions for each simulated individual. We then calculate the proportion of simulated players falling into each level of our index. We use 9999 runs of the simulation and report the mean value, 1st and 99th percentile of the simulated proportions in Table 6.

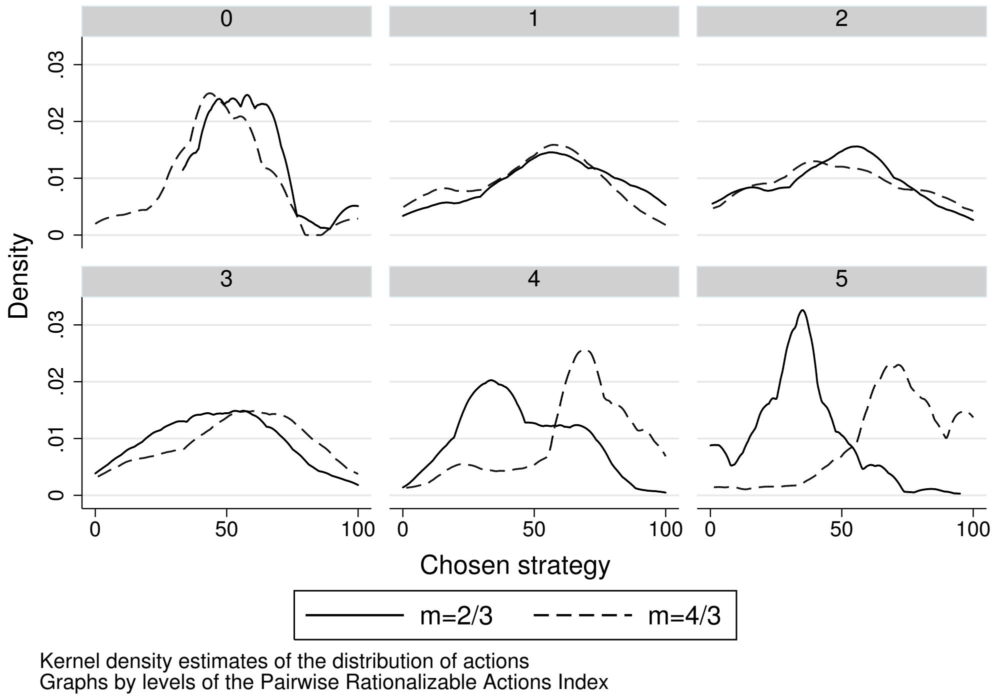
Table 6: Simulated versus actual proportions

	Simulated proportions			
Index	1st percentile	Mean	99th percentile	Observed proportion
0	0	0.2	1.1	5.6
1	0.7	2.8	5.6	7.8
2	8.5	13.2	18.1	9.6
3	24.4	30.9	37.8	22.6
4	29.3	36.0	42.6	17.0
5	11.9	16.8	22.2	37.4
Total	-	100.0	-	100.0

As shown in Table 6, the proportions differ substantially if we consider the mean value of each of our 9999 draws. Even if we concentrate on the most unlikely scenarios, the proportion of level 5 exceeds 22.2% in under 1% of cases, which is far from the observed proportion of 37.4%. In our view, this is in line with the fact that our most strategic players are not just random players who happened to draw good strategies by chance. Note that our simulations use the *joint empirical distribution* of pairs of actions; i.e. the scenario most likely to give rise to a simulated distribution very similar to ours.

Appendix B:

Figure 4: Distribution of actions by PRAI level



Appendix C: Estimations of the (Poisson) Cognitive-Hierarchy model

The cognitive hierarchy model is commonly used to estimate the distribution of players across levels. In what follows, we estimate the Camerer, Ho and Chong (2004) Cognitive-Hierarchy model where players are distributed among k levels of a cognitive ladder according to a Poisson distribution with parameter τ. Level-0 players play randomly on the strategy space, and level-k, $k \geq 1$ assume that they are the only player at this level, and that the other players are distributed on the levels below according to a normalized Poisson distribution. Players then best-reply to their beliefs over the distribution of players. If $\overline{X}_i$ is player i's belief about the mean action taken by the $N-1$ other players in the game, then his best reply can be shown to be $\frac{N-1}{N-m} m \overline{X}_i$.

We follow Camerer *et al.* (2004) and estimate τ via a method of moments estimator as shown in Table 7. The first two columns list the estimated Poisson parameter and the associated standard error. We estimate the model on various sets of games. The third column shows the implied proportion of level-0 players.

Table 7: Estimation of the cognitive hierarchy model

Sample	$\hat{\tau}$	Std. Err	% level-0
All	.32	.03	72
$N = 2$	.29	.03	74
$N = 3$	.39	.04	68
$N = 2$; $m = 2/3$	.40	.08	67
$N = 2$; $m = 4/3$	.27	.04	76
$N = 3$; $m = 2/3$	.42	.08	66
$N = 3$; $m = 4/3$	.38	.06	68

Standard errors obtained by block-bootstrapping the estimates.

The estimation using the complete sample (i.e. pooling all of the data) yields the prediction that 72% of players are at level-0. We obtain similar results when we apply the model to restricted samples of games. The cognitive-hierarchy model thus predicts a very considerable fraction of level-0 players, with at least two-thirds of players being classified as such.

Appendix D

Table 8: Means and standard deviations in the ten beauty-contest games for non-strategic players

		$m = 2/3$		$m = 4/3$	
Other players	Obs	Mean	Std. Dev.	Mean	Std. Dev.
A	123	51.6	24.3	50.1	26.6
B	123	51.6	25.1	50.7	25.5
C	123	48.6	24.7	54.7	24.3
D	123	48.3	24.8	47.4	25.8
Rand.	123	44.9	23.2	53.4	25.3
Overall	615	48.98	24.47	51.25	25.54

Table 9: Means and standard deviations in the ten beauty-contest games for strategic players

		$m = 2/3$		$m = 4/3$	
Other players	Obs	Mean	Std. Dev.	Mean	Std. Dev.
A	147	34.8	20.5	71.0	23.7
B	147	35.0	16.9	72.3	21.3
C	147	35.9	18.6	71.4	21.3
D	147	38.1	20.9	68.7	21.7
Rand.	147	36.8	18.1	67.0	21.5
Overall	735	36.12	19.04	70.09	21.95