Referee Report on
Social norms or low-cost heuristics: An experimental investigation of imitative behaviour
for the e-journal *Economics*

Given the title of the paper, the authors must have been disappointed with their findings. As they say in their abstract "our results do not provide strong evidence for imitative behavior". I am rather surprised that they did not change their title as a consequence.

Be that as it may, they carried out an interesting and unusual experiment. Subjects were asked to perform a cognitively demanding task – that of deciding which of 16 figures was closest to a given, target, figure. Their design was 2 by 2 – one dimension being the cognitive difficulty of the task (either 'high' or 'low'), and the other being information given about a particular suggested figure (either 'one of the most likely' or 'one of the most likely *and* chosen by a majority in a previous run of the experiment'); these latter are referred to as 'default' and 'majority.

One problem is that, once subjects click on this default or majority card, they are stuck with it and can no longer go on exploring the other 16 cards. Obviously a lot of exploration was carried out by the majority of the subjects, and therefore I am not surprised by the numbers in Table 5: out of 154 subjects only 8 chose the default/majority card. Although these numbers increase as we go through 'low-default', 'high-default', 'low-majority' and 'high-majority', they are far too small to do any serious statistical testing, despite the authors' valiant attempts to do so in the first paragraph on page 17. This implies that one hypothesis advanced in the paper cannot be reliably investigated. I cannot help feeling that the way that this was set up was not too clever: subjects were told, in addition to the information that the majority card "represents one of the 8 best cards appearing covered on the screen" (which was also the case in the default treatment), they are told that the default card "has been chosen in a previous experimental session by the majority of participants". Although I do not have the Instructions for that treatment to hand, if this is so, then it seems a very weak incentive for subjects. If it was meant to induce social imitation, it is difficult to imagine subjects feeling any kind of social connection with subjects "in a previous experimental session". I cannot help feeling that this why they did not observe much social imitation.

So this leaves us with a comparison of the low and high treatments. Here one the results are in line with what one might expect: payoff is lower in the high cognitive difficulty treatments. Yet at the same time (perhaps this is part of the same hypothesis) time spent is lower. There are minor differences between the numbers of uncovered cards and in the timing of decisions (though I would regard a significance level of 9.6% – foot of page 18 – to be pushing it a bit) but they are hardly dramatic.

I wonder if the subjects were given pen and paper to record their thoughts about the figures as they worked through them? I somehow feel not. Then they have to remember what they found and what they thought.

Given the nature of the experiment, I would have thought that one key interest would have been in the *process* by which subjects came to their decisions. Unfortunately it is difficult to see how the data recorded (presumably the figures clicked on and their timing) can tell us anything about that. Indeed the whole design of the experiment seems to inevitably imply that one can infer nothing about what they were thinking about as they searched for the best figure. True one can infer something rather superficial about the effect of changing the cognitive difficulty, but that is all.

I think that subjects only played the game once. This is a pity as practice and repetition may have changed behaviour.

I am unsure about the belief elicitation task and about its validity. I quote from the Instructions: "After you have made your choice … the computer will ask you to specify the amount (*number?*) of squares for which you think the DEFAULT card (that you cannot visualize) differs with respect to the TARGET card. In case your answer is correct, you will receive an additional 1 euro to your final earning." It seems from the reported statistics that 45 of the 154 subjects got the answer right. I find this amazing, unless I have misunderstood what was going on. Perhaps something has been lost in translation? Subjects cannot see the default card and have absolutely no idea how many squares are in the default card or where they are. There is not even any information about the possible number of squares, as different figures have, according to the Instructions, "a different number of squares". How can 45 out of 154 get it spot on? I would have thought that the chances were close to zero. If I had been a subject I would have thought that, and would have guessed at random. But even if I had some information, the incentives are weak – one euro for a spot-on guess and zero otherwise. I need to know more.

At the end of the day, it is difficult to decide what to recommend. I like the task posed to the subjects and the software. But I think that the way that social imitation was meant to be induced is remarkably weak – and undermines the whole purpose of the experiment. Changing the cognitive difficulty is also a clever idea, but the results are rather superficial – in that they tell us that subjects find more difficult the task that is cognitively difficult, but not *why* nor *what they do about it*. I would recommend publishing it as an interesting experiment, but without insightful results.