Review of "The option to leave: Conditional dissociation in the evolution of cooperation"

Ayad Noureddine, Pierard Florian, Rodrigues Vitoria Joël and Vercauteren Drubbel Alizée

General review

The following paragraphs reflect a general appreciation of the work done by Issa A., Hollevoet Y. and Laurels F. Please note that the points are treated in no particular order.

We would like to start by congratulating the work of the group. They followed the guidelines and produced a document that reflected their teamwork. But, as an exercise of constructive critique, we would like to point out some general aspects that were discussed by our group members.

Firstly, we will address the form and the presentation. The quality of the plots was good, meaning that the size and resolution allowed an easy visual analysis. This being said, it would have been of interest to add a more exhaustive figure description as to allow a certain "autonomy" of the figures, i.e. that they can be globally understood without a deeper reading of the article. Also, at times the choice of colours made the interpretation difficult, especially when similar shades were used for strategies of interest. Lastly, even though we understand the difficulty of implementing the figures inside the main article body, we would have appreciated at least some of the figures next to the relevant text parts. Of course, this would have fragmented the article, but would have greatly helped the reader understand your points.

Secondly, the general redaction quality of the article. Overall the style was of good quality, allowing the reader to follow most of the arguments and explanations. We did spot some spelling mistakes here and there, most being minor. But some did change the meaning of the sentences:

- **Methodology:** should have been "T (temptation) if the one individual defects and the other individual **cooperates**". (remark: you could have used a payoff matrix, which would have helped visualise the different type of situations)
- **Discussion:***"… avoid getting the sucker payoff S in the Prisoners Dilemma but the reward S instead."* probably meant to say *"… avoid getting the sucker payoff S in the Prisoners Dilemma but the reward **R** instead."*

We would also like to insist on the fact that the introduction seemed rather short. A bit more context and some specific details about the cited articles would have been good. This would have helped to better situate the originality of your work and the global question that feeds this domain of game theory (i.e. how can cooperation emerge in certain systems, namely those that favour defectors). Some of the key points are addressed in the discussion, but should have been mentioned in the introduction. Also, it would have helped the reader to grasp you principal scientific question by formulating it clearly in the last part of the introduction. For example, there is no hypothesis mentioned about the effect of life expectancy. Furthermore, mentioning the article from where you replicated the results would have been of interest, notably to compare yours with the original authors'.

Finally, we will end this review with some positive notes. We appreciated the clear and well-structured methods section, which explained step by step the model. After reading it, we felt rather confident about being able to reproduce the results of this article. The model used in this study

shines by its simplicity and allows for a reader, even with basic knowledge in game theory, to grasp the basic approach. From what we could see, the results from the original article where reproduce adequately, combined with overall plots of good quality.

3 negative points (complementary to the general review)

1) It would have been of interest to add in the abstract one of the main conclusions, e.g. the effect of a prolonged life expectancy on cooperation. Furthermore, the last sentence "We finally find out that conditional dissociation is the key to improve cooperation" seems a bit too absolute. A more cautious formulation would have been more appropriate, since the results of this paper are not sufficient as to stipulate that conditional dissociation is "the key", but more one of the possible factors that could explain the emergence of cooperation in a population.

2) In continuity with the previous point we would have loved a "no conditional dissociation" control run. Indeed, you show that with a conditional dissociation and an adapted mortality rate a cooperative system emerges. But, the question remains if, by using strategies without the leave option, there are cases were cooperation emerges. A source, having explored this question, would have sufficed to put the reader at ease.

3) Unfortunately you didn't really describe a good application of your model in the real life. You should have pointed out some concrete systems where your conclusions remain valid and can bring some interesting results and perspectives for the prediction of real situations.

3 positive points

1) Clear methodology. The type of game and the definitions of the strategies are given. The steps for each iteration of the algorithm are given, allowing thus the reproduction of the results.
2) The basic topic and aim of this work is quite interesting. The emergence of cooperation in systems that seem to favour defectors is indeed of particular interest. Especially to draw parallels with natural evolution and further understanding how cooperation emerged, as observed in many species. The model is simple and understandable even with basic knowledge in game theory. In this case, the influence of the α parameter (life expectancy) is clearly put forward and shows the benefit of a longer individual life expectancy on the emergence of cooperation strategies.
3) The presentation of the results is concise and straight to the point. The in text description of the results fits what can be seen on the graphical plots. The results of the original paper were successfully reproduced.

Questions of interest

1) In your model you used a constant mean life expectancy for each individual (α). In other words at each turn an individual had a probability of dying β = 1/α. In your opinion, would it be of interest to take into account the given fitness of a strategy when computing the deaths in a generation?
   Let's for example take a very rudimentary addition to the model:
   - Define the average fitness of a strategy as being

$$\bar{f}_x = \frac{average\ payoff\ of\ player\ using\ strategy\ X}{\sum_i^N average\ payoff\ of\ player\ using\ strategy\ i}$$

- Take this into account when calculating the death probability of a player using the strategy x. For example:

$$\beta = \frac{1}{\alpha * \bar{f}_x}$$

Would this improve the model? Or would this change too much the basic assumptions of the model?

2) You chose a mutation rate of μ = 0.05. Was this arbitrary or semi-empirical (e.g. based on other articles, inspired by biological observations etc.)?
   What would probably happen if you increase or decrease the value, given the same set of parameters you tested in the article?
3) If we understood well, when an individual dies, it is replace by a new individual that will adopt the strategy that reflects the current population's gene distribution relatively to its efficiency. So a death and rebirth process can be associated to a learning process to see which strategy is the most efficient for the entire population. So we are not convinced by the explanation for the emergence of a defector system (at low life expectancy). Isn't this due to the fact that we have more individuals that are learning (because more individuals are "reborn") and due to the fact that their lifespan is shorter?
4) Compared to the original paper, what addition to the model/discussion/results did you make?