"Beating your Neighbor to the Berry Patch"

Review

In this paper, the author proposes an elegant and interesting scenario of resource competition among foraging animals and analyzes that interaction using evolutionary game theory and a lab experiment. The resource "ripens" over time so individuals receive higher payoff if they wait to extract the resource but waiting risks other individuals extracting the resource first. Individuals also incur an opportunity costs by visiting and extracting the resource; in other words there is some alternative resource of constant value c that individuals can choose if they don't visit the more valuable but riskier resource. The author suggests a few nice examples from the anthropological literature where this game might be relevant and its intuitiveness suggests it might be found in many more examples. The evolutionary game theory analysis suggests that individuals evolve a mixed strategy where they choose a value of the resource $> c$ and pick values closer to c with higher probability. The model has some interesting non-intuitive results; for example, as the group size grows, the resource is visited with a lower probability and those who do visit get more from it. Using a simulation. the author shows that the population evolves towards the mixed strategy resource extraction value when the opportunity cost or group size is high enough and cycles around this value otherwise. The author's analysis suggests that the mixed strategy is not stable but the simulations suggest otherwise. The experimental data have a rough fit to the mixed strategy. Generally, I enjoyed this paper and found the problem interesting, the analysis elegant, and the exposition clear. Below, I will detail the places where there may be some gaps and where the paper can be improved.

The author presents a very nice analysis as to why there shouldn't be a pure evolutionarily stable strategy (ESS). They also show nicely that the support of the mixed-strategy should be the interval $(c, 1)$. In determining the mixed strategy on page 6 after eq 8, the author cites Maynard Smith for the result that pure strategy playing against the mixed strategy ESS I obtains the same payoff as I playing itself. In fact, the author should cite Theorem 1 from Bishop & Cannings (1978, JTB), which is what Maynard Smith cites. Moreover, Theorem 3 from Bishop & Cannings says that mixed strategy ESSs cannot completely overlap in their support, which implies that the mixed-strategy I in fact must be the unique mixed strategy since it's support is $(c, 1)$.

This uniqueness result has implications for section 3.3 where the author looks for the stability of the mixed strategy equilibrium. It suggests that the mixed strategy, if stable, is the only one that can exist, which would not be surprising given the simulation results. The stability analysis in 3.3 focuses on what condition 14, which is effectively a second-order condition in mutant frequency (rare mutants at frequency ϵ interact with probability ϵ^2). Looking more closely at the Bishop and Cannings result, it doesn't address the second order condition, so the results in section 3.3 should be compatible. However, given the simulation results, it is very suggestive that condition 15 determines the stability of the mixed strategy against both pure and mixed strategies. For example, this scenario could be like the rock-paper-scissors game (see Hofbauer and Sigmund 1998 or Sigmund 2010 books) where the mixed strategy is sometimes stable and when not stable leads to a heteroclinic cycle. The results in A.2 however suggest that I is not only unstable but is in fact a minimum and is always invadable by a nearby mixed strategy. I couldn't find nothing wrong with the analysis and I don't know what to suggest here, but my guess is that the mixed strategy isn't always a minimum.

The experimental data were suggestive that the mixed strategy is predictive but having groups of different sizes would help determine more robustly how much student players replicated the model predictions. I'm not an experimentalist, but I suspect the author should also include additional experimental information (such as how subjects are recruited, any demographic information, etc) and any necessary IRB information.

Specific Comments

- Equation above equation 12: you could just say that $\Pi(1, I^K) = Q^K = c/U$.
- Page 8: "dynamically stable". Define this.
- Figs 4 and 5. The stars aren't very useful since cycling is occurring in Fig 4 and they clearly won't fit there (though the time average obviously fits better).
- End of page 8: "Plugging equation 15 into equation 12 shows". Just cite equation 15?
- Page 11: Isn't $Q_K = Q^K$? This might be mentioned.
- Table 1. v_K from the simulation doesn't fit the theory for any group size very well so I wonder if there is a reason its doesn't match (e.g., how its calculated from the simulation data?). Also, for D, does "final frequency distribution" (page 11) mean the last generation simulated? If so, then obviously the cycling when it occurs will lead to large D even when a time averaged value might lead to smaller D.
- Page 14: "definition of I implies that $\Pi(J, I^K) = \Pi(I, I^K)$." Yes but this goes to the heart of where Bishop and Cannings Theorems 2 says that a mixed ESS can't be completely contained in another the support of another ESS.
- Page 16. The second line of the equations following "Integrating by parts produces..." has an integral symbol missing the lower bound c.