COMMENTS FROM ASSOCIATE EDITOR: Dr Bret Elderd

Associate Editor

Comments to the Author:

I would like to thank the authors for submitting an interesting manuscript examining the potential biocontrol benefits of generalist predators in rice fields. Like both of the reviewers, I felt that the authors needed to have additional data sources (preferably beyond their survey data) to support their rather strong conclusions in the manuscript. The study does a nice job of showing that generalist predators will shift their diets based on the frequency of prey in a rice field and that diet composition changes as the prey base changes over time. However, there is no indication that the generalist predators have any effect on the prey population or on rice yield. Both of these metrics would seem to be of primary importance when considering whether or not a particular group of predators are an important part of any biocontrol effort. For instance, if you spray insecticide on the conventional farms that also knock back predator populations, predators could have no effect on the prey population even though the surviving generalist predators are tracking the herbivores in the systems. As one of the reviewers points out, what about predator abundance? Thus, the current analysis seems to be the first step in answering the questions asked.

Besides the above and the comments by the reviewers that should be taken into consideration. I also have a couple of additional comments. 1) In their use of the k-means clustering algorithm, how did the authors decide on k = 3? Was this solely based on the Dominik et al. 2018 study cited in the manuscript? Why not explicitly test whether or not k = 3 best fits the data? Maybe, the rice fields used in this study are different in some way from those used in other studies or maybe they are the same. Either way, a relatively quick analysis of the data could alleviate this concern. 2) In the study design, the authors pick paired farms -- one conventional and one organic. In their analysis, the paired sampling pattern is never taken into account. Shouldn't it be included in the analysis? 3) For the analysis of the ''Predators' trophic niches'', the authors give a mean distance-to-centroid measurement to come to a rather strong conclusion about trophic niche breadth. The means seem rather close together and there is no indication of what the variability about the mean may be. The variability should be calculated and included in the manuscript. Then, the appropriate conclusions should be drawn from the mean and the variability.

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REVIEWERS' COMMENTS

(If no comments are included please see note below)

Reviewer: 1

REVIEWER REPORT FOR THE AUTHORS

This manuscript presents a study of prey composition of (unspecified) generalist predators at organic and conventional rice farms using stable isotope analyses at different crop stages. With their analyses the authors can estimate the proportion of rice herbivores, other herbivores and detrivores that the predators have feed upon. In itself this is interesting information and a step towards better understanding biological control of rice pests. However, as shown already by the title, the paper claims to have investigated biocontrol efficacy which is not correct.. The abundance of predators was not taken into account and there was no independent measure of impact of the predators on pest populations. What was studied is how the proportion of prey in the guts of generalist predators varies with crop stage, relative abundance of different prey types and with type of management. The manuscript must be rewritten to reflect this.

Specific comments:

1) Title. See above. Please change to reflect what is studied, i.e predator diets.

2) In general I find the introduction unbalanced. It makes it sound like 1) the value of generalist predators is unclear, but that 2) stable isotope analyses will clarify their role. I would like to see a more balanced discussion of what is actually known about generalist predators and how stable isotopes can help. What are the pros and cons with this method compared with others? There are lots of other studies who have used stable isotope, including comparing organic and conventional systems (e.g. Birkhofer et al 2011 Agricultural and Forest Entomol). What did they find, and what could they not explain with that method?

3) Line 27-28. I suggest changing to ‘… and conventional farms shifted trophic niches over the crop cycle and consumed increasing proportions of rice herbivores…’

4) Line 65-66 Why specifically late in the season? A key advantage in some systems is that they can be abundant early in the season before the pests have arrived. This is for example the case for aphids in spring-sown barley (e.g. Östman et al. 2003 Ecol Econ).

5) Line 86-88. Why not?

6) Line 88-90. I certainly see a place for studies of stable isotopes. It can give an idea of long term broader feeding patterns, but I see it more as a complement to exclusion studies or detection of DNA in the guts. So I would rather say you are providing complementing information compared to other approaches.

7) Line 93. Change to ‘…over the crop cycle.’

8) Line 107-108. Could you provide some more information about how the organic and conventional farms were selected and how organic differs from conventional management in the area? How did you ensure that they mainly differed in terms of farming system? How were they located in the landscape, how far from each other were they located? Size of farms/field etc? How long had the organic farms been organic? In what way did the organic farms differ from the conventional ones? How much were the conventional farms sprayed for example?

9) Line 196 – 200. Please explicitly explain what this shift in diet position means.

10) Line 221. Again, you are not investigating biocontrol efficacy in this paper. You’re the feeding niches of predators. To assess biocontrol efficacy you need to know how many predators are available in comparison to prey and you need an independent estimate of pest population reduction.

11) Line 228-232. It is confusing to write ‘consumed higher proportions of rice herbivores’ etc. This makes it sound like you studied how large proportion of the population of rice herbivores that was predated, but this is not the case. You only studied how large proportion of the predator diet that comprised of rice herbivores.

12) Line 244-246. I disagree with this. It could simply be a consequence of increasing populations of rice pests over time.

13) Line 253-254. Here you suddenly mention that you have measures of relative abundance of different prey types. I guess this is from the sweep net samples. It is very important that you have these data and I think you should bring this up in the results section and not only in the supplement.

14) Line 256-257. I don’t follow this argument.

15) Line 294-296. This could simply be a reflection of higher proportional abundances of rice pests on conventional farms as seen in Appendix S1: Figure S2.

16) Line 309-311. Please refer to the other similar studies here. How do the results of their studies compare to yours?

17) Line 318-324. Thank you for highlighting this. The density of different predator species is critical for biological control. You studied prey choice of predators, not their biological control efficacy.

18) Line 334. Again I disagree. You show that conception increased with increasing rice herbivore abundances. There is no evidence of increased biocontrol effect.

Reviewer: 2

REVIEWER REPORT FOR THE AUTHORS

Hsu et al present an interesting evaluation of trophic position, using stable isotope analysis, among a community of generalist predators in rice. A clear strength of the work is that it was conducted in production rice fields under two systems, organic and conventional. Farms were paired between farming systems to control, to a degree, for the influence of surrounding landscapes. The key “take home message” from the work was that predators in conventional fields exhibited isotopic signatures consistent with a higher proportion of rice-herbivore prey than were consumed by predators in organic fields, and that all predators consumed more pest through the cropping cycle. The methods (sample storage, R packages used, etc) appear appropriate, and the study was conducted in a globally-important crop under realistic, field conditions. While the work has some clear strengths, I also had a few specific concerns:

1) Worries about the role of generalists in biological control are not limited to concern that they will feed on detritus-feeding rather than herbivorous prey, but also that they will feed on other predators. Of course, feeding as a “higher order” predator can also change isotopic signatures. That being said, since the trend is moving to lower d15N and d13C values later in the season, it implies that they are feeding on lower trophic levels as the cropping cycle unfolds. Nonetheless, I worry that failing to consider intraguild predation when assessing trophic position weakens what the authors can say about the ecology of this system.

2) Looking at Figure 2 in combination with Supplemental figure S1, it looks like quite a few predators are falling below all the prey groups in both d15N and d13C. This raises the possibility that there might be another source not being accounted for in this system, and that the only reason rice herbivores are being calculated as a high % of the estimated diet is that it's simply the closest out of the listed sources. This is especially relevant because MixSIAR doesn't have an option to allow for an unknown source unlike a similar package, SIAR.

3) The number (N) of the predators involved in the calculations was unclear. Of course, the variability of the trophic niche would be impacted by how many individuals it's based on. The authors could correct this by presenting these numbers.

4) As the authors note (e.g., lines 318-322), consistent feeding on herbivores does not necessarily mean that predators are contributing to pest control. Including this caveat in the Discussion is important, but I would look carefully at the repeated implications throughout the manuscript that biocontrol is being altered – perhaps these should be toned down. Looking across all of the figures, the differences between the organic and conventional cropping systems seem relatively modest. This heightened my curiosity whether the patterns were sufficiently strong to lead to ecologically-meaningful differences in predator effects in the field.

5) It doesn’t seem fair to say that “quantitative studies regarding the trophic dynamics of generalist predators in agro-ecosystems are lacking”. In fact, the Settle et al. (1996) study cited in this paper is a classic example of predators switching between detritus- versus plant-feeding prey to improve biocontrol of rice pests. Many other studies look at impacts of non-pest prey on biocontrol, or on how intraguild predation interrupts biocontrol. Throughout, I would suggest that the authors work to make a more nuanced case about where their findings fit in the broad literature looking at generalist predator as biocontrol agents.

6) The authors are lumping some fairly broad groups of predators into single trophic groups, which might pool species with very different feeding positions (e.g., Steffan et al. 2015, Biological Control 91:34-41). In general, the paper would be strengthened with a second, complementary approach beyond stable istotpes to verify that the trophic interactions implied by the isotopes reflect, for example, feeding interactions that are directly observed in these same fields, or impacts on prey seen with experimental predator manipulations.

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