**Response to Reviewers’ Comments**

**Date**: April 22, 2023

**Manuscript Number**: AGEE36058

**Title of Article**: A predator in need is a predator indeed: generalist arthropod predators function as pest specialists at the late growth stage of rice

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**Associate Editor's Comments to the Author(s):**  
  
**Comment 1** > Thanks for your submission of the manuscript AGEE36058 titled ‘A predator in need is a predator indeed: generalist arthropod predators function as pest specialists at the late growth stage of rice’. This manuscript has been reviewed by two independent experts and their comments are copied below. As you will see, reviewers find the topic interesting however they find too much to criticize in relation to your manuscript which compromises its suitability for publication in AGEE. In particular, objectives and hypotheses are unclear. You should insist on the novelty of your study. The unbalanced design is questionable. More importantly, the method you used appears inappropriate: pest consumers were pooled at a taxonomic level rather too coarse (order) adequate isotopic signature resolution. The absence of intraguild predation is also highly intriguing and questions the significance of the results (especially how efficient spiders are in pest suppression). About the format, I agree with Reviewer#1 that Introduction and Discussion are excessively long. For all these reasons, I've resolved to decline the work with the option to resubmit. Should you ultimately choose to resubmit to AGEE, the expectation is that the commentary would be carefully and thoroughly accounted for in the revised manuscript.

**Response** >

**Reviewer's Responses to Questions**

1. Are the objectives and the rationale of the study clearly stated? Please provide suggestions to the author(s) on how to improve the clarity of the objectives and rationale of the study. Please number each suggestion so that author(s) can more easily respond.

Reviewer #1: Yes

Reviewer #2:

1. Yes, the objectives stated but are entangles with the overall study goal; these need to be disentangled.
2. The objectives imply work on generalist arthropods whereas only 2 groups of these (spiders and ladybeetles) were examined; most of the other generalist predators in rice-field systems (ants, ground beetles, earwigs, crickets, predatory bugs) were not part of the study.
3. No hypothesis is stated, and while this might not be absolutely necessary, it would be useful to add a small bit about what the study's expectations generally were, for the conclusions to be viewed by the general readership from the perspective of whether or not such expectations were met/achieved.

2. If applicable, is the application/theory/method/study reported in sufficient detail to allow for its replicability and/or reproducibility? Please provide suggestions to the author(s) on how to improve the replicability/reproducibility of their study. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Mark as appropriate with an X:  
Yes [X] No [] N/A []

Reviewer #2: Mark as appropriate with an X:  
Yes [] No [X] N/A []

Provide further comments here:

1. The sample sizes (number of farms) were unbalanced (3, 7 and 7 in the 1st, 2nd and 3rd years, respectively). There is need to clarify how the impact of this was addressed so as not to invalidate analyses methods applied.
2. The sweep-netting strategy needs clarification (whether only ridge-side plants were swept, as the authors state, or a more representative method covering adequate sections of rice-field-plots were sampled). They speak of sweeping while walking along ridges.
3. Need to add if field choice was randomized, and if so, how this was achieved for sampling independence.
4. Sampling frequency not stated.
5. 1-km buffer around farms to characterize forest cover effects on predation, is rather too far to have any impact on arthropod assemblages on the farms. the role of forest cover could well have been eliminated from the study.

3. If applicable, are statistical analyses, controls, sampling mechanism, and statistical reporting (e.g., P-values, CIs, effect sizes) appropriate and well described? Please clearly indicate if the manuscript requires additional peer review by a statistician. Kindly provide suggestions to the author(s) on how to improve the statistical analyses, controls, sampling mechanism, or statistical reporting. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Mark as appropriate with an X:  
Yes [X] No [] N/A []

Reviewer #2: Mark as appropriate with an X:  
Yes [] No [X] N/A []

Provide further comments here:  
1. Pest consumers were pooled at a taxonomic level rather too coarse (order) adequate isotopic signature resolution. Family level would have been much more acceptable.

1. Mention is made of beta regression (GLM or GLMM) but not mention of probability distribution of link function employed in such modelling.
2. Results of isotopic analyses of food proportions in predators diets using MixSIAR should be presented in form of median with credible intervals rather than means and standard errors.

4. Could the manuscript benefit from additional tables or figures, or from improving or removing (some of the) existing ones? Please provide specific suggestions for improvements, removals, or additions of figures or tables. Please number each suggestion so that author(s) can more easily respond.

Reviewer #1: No

Reviewer #2:

1. There should be at least one table summarizing median and credible intervals of pest proportions in predator diets.
2. Figures from the MixSIAR results should be presented in form of posterior density plots of medians, not linear graphs of means.
3. Therefore tables and figures presenting results in form of means and standard errors should be removed.

5. If applicable, are the interpretation of results and study conclusions supported by the data? Please provide suggestions (if needed) to the author(s) on how to improve, tone down, or expand the study interpretations/conclusions. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Mark as appropriate with an X:  
Yes [] No [X] N/A []

Reviewer #2: Mark as appropriate with an X:  
Yes [X] No [] N/A []

Provide further comments here:  
1. The results as currently presented are based on means and standard errors of MixSIAR models. These carry a high level of inaccuracy because MixSIAR model estimates of food source proportions are typically skewed, making means very vulnerable to effects of statistical tails. Medians are the most resilient to such impacts, but include credible intervals.

1. The authors state that generalist predators qualify as specialists because they have high proportions of pest food in their diets towards crop maturity, without providing justifiable evidence.
2. Throughout the article, there is need to emphasize that the study was based on two groups of generalist predators only - (spiders and ladybeetles), and not make an overarching generalization for all generalist predators.

6. Have the authors clearly emphasized the strengths of their study/theory/methods/argument? Please provide suggestions to the author(s) on how to better emphasize the strengths of their study. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Yes

Reviewer #2: In general yes

7. Have the authors clearly stated the limitations of their study/theory/methods/argument? Please list the limitations that the author(s) need to add or emphasize. Please number each limitation so that author(s) can more easily respond.

Reviewer #1: No

Reviewer #2: In the limitations/caveats subsection, the authors state that they are reluctant to imply actual pest suppression because they did not complement isotopic analyses (SIA) with direct field observation. The SIA technique is meant to more or less eliminate the need for direct field observation, or at least render it subliminal. Here it’s better to say that the study did not incorporate assessment of crop damage rates or yield gap data to support implications of pest suppression.

8. Does the manuscript structure, flow or writing need improving (e.g., the addition of subheadings, shortening of text, reorganization of sections, or moving details from one section to another)? Please provide suggestions to the author(s) on how to improve the manuscript structure and flow. Please number each suggestion so that author(s) can more easily respond.

Reviewer #1: Yes

Reviewer #2: Generally no, except for the need to clearly dis-engage or distinguish the study's mail aim/goal from objectives and hypothesis, putting all these in a stand-alone paragraph under Introduction section

**Response** > I think we can cut the information in the last paragraph of the introduction section (as suggested by reviewer 1) and make our study aims clearer.

9. Could the manuscript benefit from language editing?

Reviewer #1: No

Reviewer #2: No

**Reviewer 1's Comments to the Author(s):**

**Comment 1** > In this paper the authors investigated the role of generalist and specialist predators in the rice ecosystem. They employed stable isotope approach to test three hypotheses. They found predation on pests increased over season, was similar across years, and was higher in conventional than organic farming. They conclude that generalist predators have potential to produce stable top-down effect. The study investigates an important topic, the design was adequate and the results are sound but not in current version.

I have two major concerns. At first, the role of generalist spider predators has been known for a long time. So in this respect the study does not bring anything novel. But the quantification of rice pest suppression is new. Yet, this is the main problem of the study. The isotopic analysis is not the best to be used to quantify trophic interactions as it lacks the precision. In particular, the absence of intraguild predation is intriguing. The estimates of predation on pests are thus biased to an unknown degree. This is a serious limit of the study. Unfortunately, the authors acknowledge this limit only in the very last part of Discussion, so the reader is impressed by reading the paper how efficient spiders are in pest suppression. The authors should either provide evidence that intraguild prey was scarce and therefore unlikely to form a significant proportion of the diet or state this limit in the beginning of the study.

In addition I find both Introduction and Discussion excessively long. For example, the first paragraph of Introduction could be omitted as it is too general.

**Response** >

Thanks for the comments on our manuscript. Below we address the two concerns raised by the reviewer:

Although stable isotope analysis may not be as precise as molecular methods, it does reveal time-integrated dietary patterns of consumers, which is suitable for the purpose of our study. In fact, stable isotope analysis has long been applied in trophic ecology and relevant methodologies have been developed over the past two decades (Quinby et al. 2020). Various studies have made good use of it to quantify predator-prey trophic interactions (e.g., Blumenthal et al. 2012, Manlick et al. 2019, Carbonell Ellgutter et al. 2020), and we feel that our stable isotope approach can provide useful information that helps improve our understandings of this field.

We acknowledge that intraguild predation (IGP) is a limitation to our study, and currently it is not possible to quantify IGP using stable isotope mixing models. However, as pointed out in the potential caveats, we argue that IGP may not be a major concern in our study system because rice plants grow as dense clumps and form a complex structure that could reduce IGP. [I think we can briefly mention this limitation earlier in the discussion section and refer readers to the potential caveats for more details.]

[Regarding the length of the introduction and discussion section, I think we can still keep the original information there but reduce the contents to improve the overall readability.]

*References:*

Quinby, B. M., Creighton, J. C., & Flaherty, E. A. (2020). Stable isotope ecology in insects: a review. Ecological Entomology, 45(6), 1231-1246.

Blumenthal, S. A., Chritz, K. L., Rothman, J. M., & Cerling, T. E. (2012). Detecting intraannual dietary variability in wild mountain gorillas by stable isotope analysis of feces. Proceedings of the National Academy of Sciences, 109(52), 21277-21282.

Manlick, P. J., Petersen, S. M., Moriarty, K. M., & Pauli, J. N. (2019). Stable isotopes reveal limited Eltonian niche conservatism across carnivore populations. Functional Ecology, 33(2), 335-345.

Carbonell Ellgutter, J. A., Ehrich, D., Killengreen, S. T., Ims, R. A., & Unnsteinsdóttir, E. R. (2020). Dietary variation in Icelandic arctic fox (Vulpes lagopus) over a period of 30 years assessed through stable isotopes. Oecologia, 192, 403-414.

**Specific comments**

**Comment 2** > Line 18: Rather than biocontrol use Conservation control.

**Response** > I think we can use “Conservation biocontrol” instead.

**Comment 3** > Lines 86-90: To test the hypothesis of consistency is trivial unless there is a reason why generalists as opportunists would switch to a different prey some years.

**Response** > The opportunistic feeding nature of generalists means that they could potentially shift their diets depending on the abiotic and biotic factors, which can vary across years. Therefore, consistency in pest consumption would be a critical feature for generalist predators to serve as effective and stable biocontrol agents.

**Comment 4** > Lines 125-128: Repetition of the former text. Omit it.

**Response** > Yes, I agree that this part is repetitive of the earlier aims. We can simply remove it or merge it into the previous text.

**Comment 5** > Line 145: I am surprised to read that the conventional farms used only one application of insecticide per season. Is it really true?

**Response** > I am not sure about this. We probably need to double-check the documents.

**Comment 6** > Lines 143, 145: Add information when the insecticides were applied.

**Response** > I am not sure about this. We probably need to double-check the documents.

**Comment 7** > Line 147: How often sweeping was done? Once per growing stage?

**Response** > Yes, the sweeping was conducted once per crop stage.

**Comment 8** > Line 150: To what taxonomic level identification was performed? And how was it identified?

**Response** > The level of identification depends on the taxa. Most of the rice herbivore species were identified to genus level, whereas tourist herbivores and detritivores were mainly identified to family level (also see Appendix Table S1 for a list of major arthropod families/genera in the each study year). The majority of arthropod samples was identified using insect pest handbooks in Taiwan and Southeast Asia; some samples (mostly diptera species) were sent to a taxonomy lab at the Entomology Department at National Taiwan University for identification.

**Comment 9** > Line 159: I wonder why there were only 352 predators but 828 prey samples if the study plots were dominated by a single pest species?

**Response** > The arthropod communities during the early crop season were generally more diverse and tended to be dominated by one or a few pest herbivores at late crop stages. Among the 828 prey isotope capsule samples, 66.4% (550 out of 828) were rice herbivores, of which 73.6% (405 out of 550) were planthoppers (Delphacidae/*Nilaparvata*) and leafhoppers (Cicadellidae/*Nephotettix*)(Note that a single species can have multiple isotope capsule samples, each of which may contain multiple individuals in order to meet the minimum weight requirement for stable isotope analysis.)

**Comment 10** > Lines 167-170: Omit definition of guilds.

**Response** > I agree that we can probably remove this part.

**Comment 11** > Line 189: If samples at seedling stage were later omitted why do you earlier say that you collected data at four growing stages?

**Response** > We did collect arthropod samples at four crop stages during the first rice season in each study year. Even though the isotope capsule samples at the seedling stage were omitted from our stable isotope mixing model analysis, we still included the data on arthropod abundances in Fig. 3.

**Comment 12** > Line 203: How did you estimate the proportion of herbivores? This must have been done by the model. Explain.

**Response** > The data analyses in this study consisted of two stages. First, we ran stable isotope mixing models to estimate the proportions of three prey sources (rice herbivores, tourist herbivores, detritivores) consumed in predators’ diet. Next, we extracted the proportion estimates of rice herbivore consumption and fit beta regression models to examine the relationship between pest consumption by predators and various biotic and abiotic factors.

**Comment 13** > Lines 264-267: This belongs to Discussion.

**Response** > I think this part still belongs to the results section but we can move some information to the discussion section.

**Comment 14** > Lines 269-270: I am surprised to read the absence of association between proportion of herbivores in the predator body and their abundance in the field. If spiders are generalists then there should be such association. This makes the obtained results suspicious.

**Response** > We did provide an explanation for why there was a lack of such association in the last paragraph of section 4.3. “*Factors associated with pest consumption by predators*”. Briefly, there did exist a simple bivariate correlation between the relative abundance of rice herbivores and the proportion of pest consumed by predators. However, the relative abundance of rice herbivores increased over crop stages. When both the relative abundance and crop stage were included in our multiple beta regression models, most of the variations may have been explained by crop stage (which was indeed significant), leading to a non-significant result for the relative abundance.

**Comment 15** > Lines 274-294: This is just repetition of results. Omit it.

**Response** > I think a summary of key results of our study is still necessary. However, we should be able to trim down this paragraph.

**Comment 16** > Lines 331-344: This is trivial. Omit it.

**Response** > The consistency in pest consumption by predators over years is actually one of the most important findings in our study and we feel that it is worth talking about it in a separate section.

**Comment 17** > Lines 357-367: This is trivial. Omit it.

**Response** > Since we tested the effects of crop stage in our study, it would be logical to discuss it. Although the finding of increasing pest consumption toward later crop states may be expected, we disagree that it is trival as it does provide some implications for agricultural management.

**Reviewer 2's Comments to the Author(s):**

**Comment 1** > The study examined rates of consumption of a range of arthropod herbivores (pests) in rice-fields of Taiwan by spiders and lady beetles, by using stable isotopes mixing models MixSIAR) to estimate proportional contributions of these pests to predators' diels. They additionally evaluated the influence of some habitat variables in contributing to such pest consumption trends, comparing organic versus conventional farming systems over three consecutive years.

This is a very significant study, with potential to contribute immensely to the still narrow range of literature on the general subject of new techniques and strategies for quantifying pest biocontrol in cereal crops. It is also very timely, given the increasing value attachment to to non-chemical measures for mitigating crop damage by arthropod pests. The element of crop growth stage as a factor in determining pest consumption levels is especially interesting as it is rarely examined yet arthropod assemblage patterns, and presumably their trophic interactions do change with time.

While the study is generally well designed, it has a number of methodological gaps that would need to be addressed so as to put the results into proper perspective for conclusions to be justifiably drawn. These include wide generalizations about the range of predators examined (while only two taxonomic groups were involved), and combining pest consumers at taxonomic levels too course for isotopic analyses. Habitat variables' roles and how they were characterised are also not adequately described while samples (farm numbers) were not consistently balanced to justify temporal-scale comparisons. In addition, results of MixSIAR modelling that form the basis of the discussion and conclusions drawn are presented in form of means and standard errors while the conventional practice is to present medians and credible intervals. There are some inconsistencies (or gaps) in some non-MixSIAR analyses of data, particularly with regression, and a few non-clarifications on the study's objectives.

**Response** >

**Specific comments**

*Title*

**Comment 2** > The current title implies that all generalist predators were studied, which is not the case - only spiders and lady beetles were considered  
Suggested change: Spiders and lady-beetles consume higher proportions of rice pests at late growth stages regardless of farming system.  
This is because there were only 2 predator groups examined here: spiders and lady beetles  
**Response** >

*Abstract*

**Comment 3** > 1. This is generally well summarised, but elements of it and some wordings/sentences will change after some of the results-presentation suggestions are addressed.

**Response** >

**Comment 4** > 2. Also the claim of predators being specialist towards crop maturity is unsupported by the results

**Response** >

*Introduction*

**Comment 5** > 1. The objectives stated but are entangles with the overall study goal; these need to be disentangled

**Response** >

**Comment 6** > 2. The objectives imply work on generalist arthropods whereas only 2 groups of these (spiders and ladybeetles) were examined; most of the other generalist predators in rice-field systems (ants, ground beetles, earwigs, crickets, predatory bugs) were not part of the study

**Response** >

**Comment 7** > 3. No hypothesis is stated, and while this might not be absolutely necessary, it would be useful to add a small bit about what the study's expectations generally were, for the conclusions to be viewed by the general readership from the perspective of whether or not such expectations were met/achieved

**Response** >

**Comment 8** > 4. The role of 'years', in my opinion would not make a significant value since the rice agronomic practices do not change much from year to year in the study area (Taiwan). Therefore the crop stages are enough as a time-based parameter.

**Response** >

**Comment 9** > 5. There is need to clearly distinguish among overall goal/aim(s), specific objectives, and the study's expectations or working hypotheses. As at now, they seem to be all mixed up towards the end of the introduction section.

**Response** >

*Methods*

**Comment 10** > 1. Three farms in year one and 7 farms each in year 2 and 3 amounts to unbalanced sample size

**Response** >

**Comment 11** > 2. The authors need to more fully and clearly describe how mist-netting as conducted: it's not enough to say this was conducted while walking along ridges, because this implies sampling only insects along he field edges, rendering the collected samples unrepresentative

**Response** >

**Comment 12** > 3. Sweeping for canopy insets also implies that insects on other aerial parts of rice were ignored: not all pests are to be found on rice canopies (under-representation or under-sampling)

**Response** >

**Comment 13** > 4. Also how plots were selected for sampling including distance separation between sampled plots, and how this was independent of arthropod movement patterns (to eliminate pseud-replication) will be desirable

**Response** >

**Comment 14** > 5. It would have been useful if the study assessed the role of some surrounding vegetation on field margins (e.g., on ridges and levees) as a food source alternative to rice, and how it compares to rice as a contributor to pest diets. This is because it could be that the reason predators consume more rice prey in late stages is that at this time, drier conditions and no water support little growth of alternative plants such as on ridges and levees, making herbivores move from there and be more abundant in rice. Consequently, predator-prey interactions are enhanced on rice, in which case the key explanatory factor here is therefore the watering regime rather than the crop stage. In the absence of this data/results, the authors need to adequately describe the structure of this kind of vegetation, including its persistence across the grow-out period and water availability, for there to be a clear picture as to why it might not have affected predator or pest dispersal or consumption rate results presented

**Response** >

**Comment 15** > 6. The role of habitat variables in driving predation rates has been treated very subliminally: not included in the objectives, not thoroughly described in Methods as to how the habitat structure (forest cover, other vegetation on field margins etc) were measured. Mention is made of forest cover, but no details as to how this was measured either, other that GoogleEarth was used for estimates. How close within the 1-km buffer were the forests to the rice-fields, for there to be an expectation that arthropods they might influence predation rates on farms? A 1-km buffer around farms is quite wide and though a few mobile arthropods may disperse from there into farms (certainly not spiders and lady-beetles!!!), it will definitely not affect pest-predator trophic interactions

**Response** >

*Analyses*

**Comment 16** > 1. Combining/pooling pest consumers at order level seems very course for isotopic signature resolution. In stable isotopic analyses, pooling up consumers' food sources into similar guilds is usually a generally acceptable and realistic strategy which helps in saving time, effort and costs without risk of losing signature resolution for food source signatures in predators, for instance. However, this should usually be done at as low a taxonomic level as possible, say genus or family. Pooling food sources at order level seems like a rather wide stretch, not least because at that level, despite guild-commonality, feeding systems are so variant as to erode isotopic signature resolution roles of component groups in contributing to consumer diets. In your case, grouping grasshoppers with beetles is rather strange, especially as you said earlier that former has no trophic link to rice. Putting hemiptera and lepidoptera is equally awkward since some hemiptera are generalist predators or omnivores. Finally, omitting crickets as part of detritivores is curious

**Response** >

**Comment 17** > 2. Mention is made of beta regression but not probability distribution or link function applied, or whether this was conducted using GLM or GLMM modelling

**Response** >

**Comment 18** > 3. ANOVA procedure is stated to have been applied to analyse some data that were supposedly analysed using beta regression. Why would such repetition be necessary?

**Response** >

**Comment 19** > 4. From the unbalanced sample size 3, 7 and 7 farms) across the three years, assessment of the role of years would have requires a statement as to how such imbalance was addressed before analyses were performed in beta regression, e.g., incorporating a Kenward-Roger approximation with an autoregressive data structure, so as to reduce the impact of data heterokedascicity

**Response** >

**Comment 20** > 5. It is also not clear whether the interactive influences of the various explanatory factors were performed on mean, medial or other values of food-source proportions from MixSIAR, of from other datasets. This should be clarified

**Response** >

*Results*

**Comment 21** > 1. The results of the isotopic mixing models for pest contributions to predator diels should be presented in form of Medians accompanied corresponding by credible intervals, instead of Means and standard errors as the authors have done. This is because normally, the mean is more relevant when aspiring for a definite hypothesis test with normally-distributed (or transformed data) to reduce skewing and so we can test to get a p=value 0.05 etc.) because mean is very sensitive or vulnerable to errors arising from long statistical-distribution tails. By contrast, with the Bayesian MixSIAR modelling, food proportion estimates, are almost always skewed so a measure of means is even more vulnerable to such impacts of tails (skewing), and the better option is therefore to use the median, which has the least sensitivity to skewed tails (compared to means or modes). But one has to provide the credible intervals to accompany the medians, just as you need SD (not just SE as the authors have done here) to accompany means in direct hypothesis tests. Therefore, the authors need to go back to their MixSIAR model results isotopic results and construct results based on medians. For details on this, see various articles by Brian Stock and Semmens.

**Response** >

**Comment 22** > 2. The results of the medians should consequently be presented in at least 1 table, and also the figures should preferably be presented in form of posterior median density plots (usually generated automatically by MixSIAR) rather than the linear graphs presented here by the authors

**Response** >

**Comment 23** > 3. After that results change, the patterns and trends in food source contributions to consumer diets will change a lot, the authors will the need to re-write results section

**Response** >

**Comment 24** > 4. There is not justifiable case for implying that because proportions of pests in generalist predators diets are relatively higher towards crop maturity, these predators then become specialists. You would have to quantify each of the component herbivores individually to see if they are consuming only one type at that stage (diet specialization/monophagy)

**Response** >

**Comment 25** > 5. The role of habitat variables in driving predation rates have not been clearly treated in results, so it is not easy to see how they influenced observed predation patterns. They could well be eliminated from the paper

**Response** >

*Discussion*

**Comment 26** > 6. I have largely hesitated to review this section due to the changes that will likely results from the presentation of results of Median instead of Mean proportional contributions of pests to predator diets because after that results change, the patterns and trends in food source contributions to consumer diets will change a lot, the authors will the need to re-write discussion and conclusion sections

**Response** >