16-Jul-2020

Dear Dr. Ho:

Thank you very much for submitting your manuscript "Pest consumption by generalist arthropod predators increases with crop stage in both organic and conventional farms" # EAP20-0276 to Ecological Applications.  Both reviewers found the research questions to be very interesting. The seasonal shift in predator diets is very clear from the isotopic analyses and is an important result. However, I must agree with the reviewers that interpretations are greatly limited by the absence of abundance estimates for predators and prey. The lack of information on predator taxonomy and biology was a further limitation (e.g., making it difficult to assess the likelihood of intraguild predation). Based on the reviews, we will not be able to accept this manuscript for publication in the journal.

The reviews are copied below, and we hope they will help you should you decide to revise the manuscript for submission elsewhere.  Thank you again for thinking of the Journals of the Ecological Society.  We will look forward to further contributions from you and your colleagues.

Sincerely,

Dr. Matthew Ayres

Subject Matter Editor, Ecological Applications

Matthew.P.Ayres@Dartmouth.Edu, [Matt.Ayres@dartmouth.edu](mailto:Matt.Ayres@dartmouth.edu)

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Although your manuscript was not accepted for publication in Ecological Applications, we believe your manuscript is a strong candidate for potential publication in Ecosphere, a peer-reviewed, open-access, interdisciplinary journal providing rapid publication of high-quality research by the Ecological Society of America (ESA). Ecosphere has a similar standard for high-quality science as all of the ESA journals, but with a much quicker publication time, a broader scope, and fewer constraints on page lengths than the traditional journals.

I am willing to consider a version of your manuscript following major revision that addresses the concerns of the reviewers. Please choose the Agroecosystems track during submission. You should also send a cover letter indicating your response to the review comments and the changes you have made in the manuscript. If you disagree with a reviewer's point, please explain why.

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Dr. Debra Peters

Editor-in-Chief, Ecosphere

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Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

General comments

Hsu and colleagues use stable istotope analyses to examine how the trophic positions of generalist predators vary temporally with crop stage, and across conventional and organic rice production systems. This study’s primary value is that it slightly improves temporal coverage (3 timepoints) of recent efforts to quantify pest consumption by generalist predators in crop systems using more-costly molecular gut content analyses (Krey et al. 2017, Roubinet et al. 2017,2018, Mabin et al. 2020).  From a single season of field data, the authors found that predators’ diet composition changes over time by increasing the proportion of rice herbivores consumed and that generalist predators more frequently consumed rice herbivores in the late season than did predators on organic farms.  However, the paper lacks any report or analysis of background densities of predators and herbivores in each system, which is key in contextualizing the temporal patterns of prey consumption described.  Adding these conspicuously missing data seems key. In fact, the most abundant, key pest taxa are not even noted anywhere in the document (I’m assuming spiders and lady beetles were they key predators, but that should also be noted explicitly).  Several important details about methods and analyses are also missing. Finally, I suggest abbreviating some of the less-relevant sections of the introduction (general importance of generalist predators, weaknesses of controlled experiments) and incorporate a stronger synthesis of related work that clarifies the novelty of this study relative to other recent papers on prey consumption by generalist predators in conventional and organic agroecosystems. Just a few key references are pasted below.

> (WO) We did note the key pest and predator taxa in our document. Not sure what he means by “less-relevant” and what is “related

References to consider

Roubinet, E., Birkhofer, K., Malsher, G., Staudacher, K., Ekbom, B., Traugott, M., & Jonsson, M. (2017). Diet of generalist predators reflects effects of cropping period and farming system on extra‐and intraguild prey. Ecological Applications, 27(4), 1167-1177.

Roubinet, E., Jonsson, T., Malsher, G., Staudacher, K., Traugott, M., Ekbom, B., & Jonsson, M. (2018). High redundancy as well as complementary prey choice characterize generalist predator food webs in agroecosystems. Scientific reports, 8(1), 1-10.

Mabin, M. D., Welty, C., & Gardiner, M. M. (2020). Predator richness predicts pest suppression within organic and conventional summer squash (Cucurbita pepo L. Cucurbitales: Cucurbitaceae). Agriculture, Ecosystems & Environment, 287, 106689.

Krey, K. L., Blubaugh, C. K., Chapman, E. G., Lynch, C. A., Snyder, G. B., Jensen, A. S., ... & Snyder, W. E. (2017). Generalist predators consume spider mites despite the presence of alternative prey. Biological Control, 115, 157-164.

Positive Comments

•       The writing was strong, and the paper had really clear and well-executed transitions in to help the reader follow their logical path, even for a reviewer like me who does not have extensive experience with rice agriculture (esp. line 71-72)

•       Conducting complementary multivariate analyses of both the differences among centroids and differences in dispersion was a good idea and lent strength to the conclusions the authors drew (line 164-167)

•       Extremely clear and easy-to-follow explanation of the implications of the statistical results (e.g., line 215-216)

Major Concerns

•       125-126: It’s never noted how many samples were run per site and per timepoint. Please clarify.  Including a table that includes which taxa and how many individuals were analyzed for each site will clarify much.  At the very least, make sure to include your N for each farm system and timepoint.

> (WO) I recall we have a table on this in the supplementary document

•       Line 138-139: The authors’ argument that substituting trophic guilds for individual species would be appropriate for community analyses needs justification. Predator hunting mode can affect prey suppression and individual predator species can have different implications for the structure and functioning of the rest of the predator community. If this does not hold true for rice agroecosystems, then a citation is needed along with a fuller discussion of why a species-composition approach was forgone.

> (WO) In a parallel study (my thesis), we show that farming system had no significant effect on species composition. Can we use that as a justification?

•       Line 139-141: Were all predators either ladybeetles or spiders? If not, in what guild were other predatory species included? Were any predators actually omnivores and would this affect your results if there were?

> (WO) Actually, we can analyze/examine this by checking the variance in 13C (and 15N) of our predators. Ie. See if predator morphpecies differ in their isotopic values.

•       Line 179-180: Was an interaction between farm and crop stage included in these analyses? Many of the claims made by this paper implicitly rely upon such a term (i.e., that the effect of crop stage depends upon farm type) so this term is needed if it is not currently included. An interaction is included in a later suite of analyses (line 194-195) but it is unclear if one is included in the diet composition analysis

> (WO) We mentioned why interaction term/s were dropped(Eg. Line 198). I don’t think we actually claimed that effect of crop stage depend upon farm type…

•       Lines 194-196: It’s not clear what the independent variable is in your model.  Please clarify for readers unfamiliar with stable isotope analyses what you mean by “herbivore consumption” throughout the paper, how that value was generated, and what the units are.

> (WO) This is a good points actually, we should be more clear of what “herbivore consumption is”. But, consumption in our case is in % , in which case, it is unitless

•       Lines 202- 205: I don’t see the results from this model in the results section.

> (WO) Is it necessary for it to be in the main text? We do have it in SUPP

•       Line 223: Which are the key predators in the system?  Which are the most abundant herbivores in the system?  Please report all these things.  It’s not clear which taxa contribute to the effects reported throughout the paper.

> (WO) I think the premise/assumption of our study is that we can and want omit detailed taxonomic information and focus on trophic niche

•       Lines 267-268: This claim of a strong effect of predators on pests isn’t supported by the data.  Such a claim would require some sort of an experimental predator exclusion treatment. Even correlative evidence of this pattern is not offered here, no report of predator or herbivore densities are provided in this study. These claims need to be tempered throughout.

•       Line 299-301: A citation is needed to justify the claim that previous work has suggested that generalist predators “provide more effective biocontrol services in the field over time.”

> (WO) Agreed! I think we can also clarify what “over time” means.. I’m not quite sure what that entails myself. Is it over the course of the crop season, or temporal stability (within season, across years?)

•       318-319: This result is not particularly meaningful to me without information about the densities of both herbivores and predators in each system. If the conventional system had pest outbreaks, then of course the rate of consumption of herbivores in that system would be expected to be higher.  I’m not sure that truly highlights the unappreciated role of generalist predators.

> (WO) I agree with this statement because we’re essentially using “assumed” correlation to imply causation. Is it possible to at least say that pest population is “NOT” limited by predators? Although we still need density data to make that statement.

•       Figure 2: Given that an interaction term was not included for these analyses, the grouping of points by both farm type and crop stage makes interpreting the graph along the lines presented in the results difficult. This figure should be modified to be two panels, one with only two ellipses to show analyzed differences between the farm types (but lumped across crop stage) and a second with three ellipses to show tested differences among crop stages (but lumped across the two farm types).

> (WO) Good suggestion!

•       Figure 2: The colors used for crop stage are not likely to be visible to readers who are red-green colorblind. This colorblindness simulator (<https://www.color-blindness.com/coblis-color-blindness-simulator/>) accepts files and can be used to test whether your color palette will be visible to people with colorblindness. Alternatively, Colorbrewer (<https://colorbrewer2.org/#type=sequential&scheme=BuGn&n=3>) has a checkbox in the middle left for “colorblind safe” that can be used to find hexadecimal codes (R accepts these in all plotting packages of which I am aware) for palettes that are created to be colorblind safe.

•       Fig. 3 &4: please provide more information about units on the y axis. Proportion of what?  For whom?  In the caption, you might provide readers with a bit of detail about how you use stable isotopes to infer the proportion of a predators’ diet composed by different groups of arthropods.

> (WO) Herbivore consumption is in % (percentage of source in mixture) which is unitless.

Minor Comments

•       Line 76-79: In the phrase “…temporal variations in species composition…” it is unclear whether predator or herbivore species are exhibiting this temporal variation

> (WO) In the particular sentence, we were referring to the predator. Instead of species composition, we can say temporal variation in abundance?

•       Line 87-90: When indicating the lack of clarity on the impact of organic farming on predator efficacy, positive and nonsignificant results are not (in this reviewer’s opinion) sufficient to indicate that this is a gap in knowledge. I think a much simpler claim would be that organic farming sometimes has an effect on predators and thus organic farms need to be included in your study.

•       Lines 91-94: Missing reference

•       94-95: Missing reference

•       Line 108-109: For the claim that stable isotope analysis is “a common method,” a citation should be provided

•       Line 118: If the quality of the irrigation water has the potential to affect your results, include a table summarizing that information by farm in the supplemental information.

•       Line 123-124: Include to which taxonomic level the majority of insects were identified. Given that the paper invokes the importance of species composition at various points, this information seems important

> (WO) If the reviewer has a problem with us using the word “species composition” without providing fine taxonomic labels, we can just omit this term and say “guild composition” instead? But I think this is extremely minor…

•       Line 126-128: Does pooling multiple individuals into the same capsule for isotope analysis have the potential to change results? What was the average number of individuals per sample? If generally one sample was formed of a single individual that average should approach one and would be good to include.

•       Line 139-141: Include parenthetically the taxonomic name of the groups you consider “ladybugs” and “spiders.” This is more important for spiders as it will give an indication of whether spiders were of a single family or many.

•       Line 282-284: Did the abundance of predators change with the increasing abundance of  rice herbivores (line 279-281) or just the relative consumption rate of rice herbivores?

> (WO) Based on our data, it should be just the relative consumption of rice herbivores and not predator abundance.

•       Line 321: A rhetorical question is a weak transition to these mechanisms. Please rephrase this to more directly state that you have identified two mechanisms.

> (WO) I agree with this, simply cut to the point and state our proposed explanations is fine here.

•       Line 323-325: Don’t you technically have the data to examine both density and diversity of the arthropods in your systems?  Proposing mechanisms you can actually test with your data seems strange. I urge the authors to directly report these results instead of speculating about it here. They are quite important for contextualizing your claims of higher predation rates of rice pests in the conventional system.

> (WO) This is true. I’m not sure if we have the “taxonomic” detail of our stable isotope samples, but we can fit a correlation between prey density and estimated consumption proportion.

•       Lines 335-341: This point seems not particularly relevant and I suggest cutting it. Instead, a more serious caveat to consider could be how elevated trophic signatures of fungus and bacteria feeding decomposers might confound your results over time, as the availability of fungus and bacteria fluctuate.  (see ref below)

> (WO) I disagree to the point that spatial dynamics are irrelevant. But I do think that we did not make clear enough of a point. In reality, our sampling sites are connected in space, not to one another but also to adjacent habitats. It is possible that the stable isotope signatures of predators reflect what they have consumed outside of our local community. However, this is less likely since consumption patterns highly reflect the trophic abundance patterns over the cropping season in our sites.

Steffan, S. A., & Dharampal, P. S. (2019). Undead food-webs: integrating microbes into the food-chain. Food Webs, 18, e00111.

•       344: Yes indeed. Please report the density and diversity of predators in your fields.

•       Line 552: Give the range of individual predators per sample.

•       Figure 4: Why did you switch from ± standard error in Figure 3 to ± confidence intervals in Figure 4? It would be better if these were consistent. Also, include what the error bars are in the y-axis label (as you do in Figure 3).

•       Table S1: Add a column indicating to which order families belong.

Reviewer: 2

Comments to the Author

This is an innovative, highly interesting study using stable isotopes to explore pest consumption by generalist predators in rice crops. The focus is on seasonal changes in diet composition, and on differences between organic and conventional farming systems. Many of the central results such as the shift from detritivore to pest dominance both in availability and in consumption are expected, but have not been quantified in any comparable way before and are thus highly novel. The study is well designed and overall well presented. However, I have three main concerns that must be addressed before it can be published:

First, it is essential to provide information on management differences that may explain differences in predator-prey dynamics: fertilizer type and amount and plant protection (insecticides in particular). Differences between conventional and organic farming systems are usually due to these management factors, including indirect effects such as higher weed density and diversity in organic fields due to the absence of herbicides. To interpret the differences in diet composition reported in this study, it is important to know about possible underlying management differences.

Second, there are several conclusions about biocontrol, that cannot be drawn based on diet composition alone. We need information not only on the composition but also on the densities of predators and pests in both management systems across the season. See explanations below.

Third, the biocontrol potential of generalist predators can be limited by intraguild predation. This should at least be discussed. Is it possible to estimate intraguild predation (as a fourth prey category) with the current dataset? Bot ladybeetles and (hunting, not web building) spiders are known for high rates of intraguild predation.

L119 “Synthetic”, not “Synthesized”

L150 was intraguild predation considered in mixing models?

> (WO) This is currently impossible under the current framework. If predators were included as their own source, it would be 100%. A possibility for this would be to include priors in the Bayesian model and or other predictors of intraguild predation but these are unavailable to us.

L254-255 (and elsewhere, including in Abstract): It is indeed surprising that predators in conventional farms consumed higher proportions of rice herbivores than in organic farms at tillering, while the relative availability of rice herbivores was higher in organic than in conventional farms during that period. To conclude about biocontrol potential, it is nevertheless essential to report also (a) the level of pest infestation and ideally crop damage in both farming systems. As a minimum, the absolute number or biomass of rice herbivores and other prey groups per sampling effort should be given. (b) Similarly, the density of predators is essential, which is often (but not here?) higher in organic than in conventional crops. If predator density was multiple times higher in organic than in conventional farms, then the higher proportion of rice herbivores consumed in conventional farms may still mean lower instead of higher biocontrol potential. Thus, any conclusion about biocontrol potential is only valid if predator and prey densities are also reported. You already touch on this limitation in the potential caveats section, but you must keep it in mind throughout the manuscript and avoid overconclusion.

> (WO) I agree that “higher biocontrol potential” is rather misleading. “Potential" suggests that better CAN be done (ie. Room for improvement) and not that it does better already.

L268: A high per capita consumption of rice pests is no guarantee for successful biocontrol. There are countless examples of even specialist enemies that consume exclusively (100%) certain pest species but are not effective biocontrol agents. Also the increase of effect during the crop season (L271) is only true if total consumption (depending on the predator densities) is increasing faster than prey density.

L279-281: Don’t mix increases of relative and absolute abundances. An increase in the relative abundance does not automatically reflect an increase in absolute abundance; it may also reflect a decrease of other prey.

When speaking of per capita effects (L268, L292, 341), please be clear if you mean per capita prey or per capita predator. Based on the results shown so far, it can only be per capita predator. However, a high per capita predator consumption of rice pests has no strong implications for rice pest population regulation. Per capita can be misleading because it often refers to prey mortality rate, so please be clear and avoid overconclusion.

> (WO) I’m beginning to feel hesitant about calling consumption proportion as a “per capita effect”. Yes, it is indeed an estimate of the average predator individual (and what it eats), but it’s not clear what this “effect” has on the prey. In theory, stable isotopes is measure of the composition of predator biomass which is different from effect on prey population (the usual case). Moreover, because our estimate is in “proportions” it is even less likely to draw any conclusions on predator effects on prey population. Imagine we have 2 predators, one of them 2gs the other 6gs. Both consumed prey of equal proportions (eg, 20, 25, 55%). We might falsely conclude that these 2 predators have the same per capita effect. I’m not sure if there is workaround this situation… we should definitely try not to over-interpret.

L292, 341: You are showing diet composition, not effects on of predators on prey. Thus, the word “effects” must be replaced.

> (WO) Same as above.

L320: As explained above, you cannot conclude about pest management based on the diet proportion alone.

I think Figures S1, S2 and S3 are important enough to be moved to the main manuscript. Instead, Figure 1 could be moved to supplemental material. Figure S2 (relative prey availability) is particularly interesting in comparison with Figure 3, so maybe you want to combine all four panels in one figure. Figure 4 is partly redundant with Figure 3 (all means are already displayed there). I think Fig. 4 is illustrative, but not essential and the pairwise differences can also be described in the text. Thus, if space is limiting, this figure could be moved to the supplemental material.

L359: The biocontrol value depends on the densities of predators and rice pests, and (absolute) consumption rates. Diet composition alone does not allow to conclude about biocontrol value.

> (WO) I agree that Figure 4 is essentially a subset of Figure 3 and can be moved to supplementary material.

L360 “per capita pest consumption” is misleading, it can be interpreted as pest mortality rate which you have not determined.

Attachments area