**Response to Reviewers’ Comments**

**Date**: December 4, 2022

**Manuscript Number**: JZO-08-22-P-196

**Title of Article**: An experimental framework for quantifying the degree of intraguild predation in omnivorous food webs in the field

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Dear Dr. Richelle Tanner,

Thank you for inviting me to submit a revised version of the manuscript. I greatly appreciate the valuable comments and feedback from the reviewers. I have incorporated most of the suggestions and the revision has substantially improved the manuscript. In particular, I have made the following major changes:

* Corrected the citation format issue and added several recent articles to the manuscript to better reflect the current status of IGP research.
* Introduced the use of molecular gut content analysis for studying IGP and its potential limitation in the fourth paragraph of the *Introduction* section.
* Discussed the issue of ontogenetic shifts in prey nitrogen isotope signatures and how to address it via stage-specific IGP estimation in the fourth paragraph of the *Applications* section.
* Discussed the issue of mesopredator feeding on alternative prey and how to address it by calibrating the δ15N of top predator in the fifth paragraph of the *Applications* section.
* Discussed the issue of top predator feeding on non-focal prey and suggested several methods to address it in the sixth paragraph of the *Applications* section.
* Discussed the potential application of compound-specific nitrogen isotope analysis of amino acids in the proposed framework in the seventh paragraph of the *Applications* section.

Please also see the following section for my detailed point-by-point responses. All line numbers pertaining to the changes refer to the revised manuscript.

Sincerely,

Gen-Chang Hsu

Department of Life Science, National Taiwan University

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**Dear Dr Sun,  
  
We have received the reports from our advisors on your manuscript, 'Increased academic performance and prolonged career duration among principal investigators in ecology and evolutionary biology in Taiwan', which you submitted to Scientometrics.  
  
Based on the advice received, I feel that your manuscript could be reconsidered for publication should you be prepared to incorporate major revisions.  
When preparing your revised manuscript, you are asked to carefully consider the reviewer comments which are below, and submit a list of responses to the comments.  
  
If one or more reviewers have uploaded files related to their reviews, these files can be found online.  
  
In order to submit your revised manuscript, please access the Editorial Manager website.  
  
Your username is: sjs243  
If you forgot your password, you can click the 'Send Login Details' link on the EM Login page at**[**https://www.editorialmanager.com/scim/**](https://www.editorialmanager.com/scim/) **We look forward to receiving your revised manuscript by 22 Jan 2023  
  
Thank you very much.  
  
With kind regards,  
Lin Zhang  
Editor in Chief  
Scientometrics  
  
COMMENTS FOR THE AUTHOR:  
  
Reviewer #1: 1. This article collects various types of open data and investigates the academic job market in Taiwan, which is an important topic deserving further future studies.  
  
2. However, there are some questions or suggestions as follows:  
(1) About data collection  
A. In line 123 to 126, this research includes 145 PIs who had an updated CV. In other words, researchers who hadn't updated CV were excluded and those PIs' academic careers whether represent specific patterns or not. Hence, what's the representativeness of these 145 PIs in this study?  
B. Besides the institutional/departmental websites, and ORCID, other open data or database could be further consideration, such as Web of Science, Scopus, or Academic Research Service Portal Researcher Query of National Science and Technology Council.  
(2)About literatures  
A. This study includes those variables such as year of recruitment, gender, PhD university origin, PhD university ranking, year of promotion and so on (shown as Table 1). What's the theoretical basis of relationship between those variables?  
B. if this article supplied the section of literatures review, readers would more understand the existing related researches of this topic even the theoretical basis.  
3. According to the results and discussions, what's the concrete suggestions to higher education policy, recruitment of university's teaching and research staffs, or PhD students who aim to academic careers?  
  
  
  
Reviewer #2: The authors examine "how academic performance as well as duration before recruitment as a new principal inverstigator (PI) and promotion to full professor changed over time, and how PhD university origin, PhD university ranking, and gender affected the career success". The manuscript has potential to make a contribution to the literature. However, the manuscript has some problems which makes me recommend major revision. Hopefully my questions and comments can help the authors to improve the manuscript.  
  
Title  
  
The title does not really convey what the study is about.  
  
Abstract  
  
The first sentence in the abstract "Academic job markets have become increasingly challenging worldwide, yet it remains poorly characterized how competitively-successful candidates should be and what the underlying determinants of their success are" seem unsubstantiated. There are numerous studies that have examined determinants of academic success (see. e.g., Hirsch, 2007; Danell, 2011; Acuna et al., 2012; Havemann and Larsen, 2015; Bornmann and Williams, 2017a; Lindahl, 2018). My recommendation is that the authors include a more extensive literature review on previous research in the field and provide a more accurate and nuanced summary of the state of this research.  
  
Materials and Methods  
  
Measurement of academic performance  
  
The data collection with the Publish or Perish software for the h-index need to be described and presented much more and in greater detail. There is not enough detail to be able to review the data collection or the data for calculating the h-index. As a reader I'm not sure how the authors collected the publications for the authors. Did they conduct searches through Publish or Perish at the publication level or the author level? How was the search queries formulated, i.e., did the authors conduct searches on the basis of publication titles, persistent identifiers, etc? My recommendation is (1) that the authors provide the search queries in the manuscript or as an appendix and (2) that they provide much more detail about the data collection procedure and what they have done including how they handle the CV data etcetera.  
  
I cannot see how many documents that are included in the final dataset? This should be included in the manuscript.  
  
What do the authors mean with "regardless of authorship for" in the sentence on page 7 row 1-3?  
  
The authors use the h-index to measure research performance. The h-index is not a normalized indicator of research performance, i.e., it do not adjust for, e.g., research area, publication year, and publication type, and do not live up to best practice in scientometric research. See e.g., Waltman (2016) for a review of citation indicators. To use non-normalized bibliometric indicators as measures of research performance can lead to severe biases in the analyses. The h-index has been heavily criticized in the scientometric literature (Bornmann, & Daniel, 2007; Bornmann, & Daniel, 2009) and it is not recommended to use to measure research performance at the individual level (Waltman, & Van Eck,2012). My recommendation is that the authors change their dependent variable to a normalized bibliometric indicator that is in accordance with best practice in scientometric research or provide good arguments for why the use of h-index should be used in this case. Another potential  
solution is to use a variation of the h-index that adjust for the problems with the h-index and fit the context of the authors study (see e.g., Alonso et al., 2009, for a review of h-index and its variant).  
  
The authors need to discuss the pros and cons of using google scholar. Why use Google Scholar instead of a citation database, e.g., Scopus or the citation indices accessible through Web or Science? Harzing state that Web of Science and Scopus have higher accuracy so why not use them (see**[**https://harzing.com/resources/publish-or-perish/manual/using/query-results/accuracy)?**](https://harzing.com/resources/publish-or-perish/manual/using/query-results/accuracy)?)**How might the use of Google Scholar affect the results? The authors should provide a discussion in the manuscript where the pros and cons of using Google Scholar become transparent for the reader.  
  
Statistical analyses  
  
CV data usually comes with allot of missing values. However, I cannot find anything about missing values in the text. Is there no missing values in the data? If there are missing values a wonder how have the authors handled the missingness.  
  
Regarding the LMMs the authors are referencing R-packages which is fine. But I recommend the authors to also provide relevant references for the actual methods they use.  
  
What do the authors mean with the following sentence: "Non-significant interactions (p > 0.05) were dropped from our final model results. Did the authors first try all possible interactions for each model and then in the final models they only included the significant interactions. Or did the authors include all interactions in the final model but only show the significant interaction in the results (i.e., Table 1)?  
  
The authors write that they log-transformed the dependent variables "to meet the assumption of normality". (page 9, row190-191). Did the authors test the assumption of normality on the transformed variables?  
  
Results  
  
The authors should provide descriptive statistics for their data and variables. Either in the results section or in the Materials and methods section. This is important so that the reader can get an overview of the data and its properties.  
  
The authors should be clearer about how they use p-values and how they interpret them, especially since their data is not a random sample. For example, the authors write that "PhD university origin, ranking, and gender had no effect on the duration either before recruitment or before promotion (page 10, row 49-54). In Table 1 I can observe that, e.g., the coefficient for the "PhD university origin" is 3.48 which indicates a positive effect and that the p-value is 0.06. I take it that the chosen significance level in the manuscript is 0.05. 0.06 is not that much higher than 0.05. From my perspective I would say that there is a positive effect but that the p-value indicates that there are some uncertainty and that interpretation should therefore be done with some caution. There are two issues here. First, how does the authors define and use p-values in the manuscript? This is not clear. Second, the sample is not a random sample so it seems a bit strange to be super strict  
about the p-values and e.g., conclude that there is an effect if the p-value is 0.04999 and conclude that there is no effect of the p-value is 0.05, regardless of the size of the coefficient, taking sample size into consideration, etc. The use of p-values does in either case not live up to the required assumptions for making real inference due to the non-random sample. Overall (i.e., this is a recommendation for all the results and not just for the example I provided regarding the "PhD university origin"). The sample size is small (N = 145) and a larger sample size would likely produce significant results.  My suggestion for the authors is to adopt a less dichotomous and more nuanced strategy for interpreting the results of their analyses, e.g., using confidence intervals for determining uncertainty together with the p-values, and not dogmatically approve or disprove of an effect just by looking at the p-values.  
  
Can the authors complement the analyses with effect sizes so that it become easier for the reader to understand the size of the effects?  
  
Can the authors transform back the coefficients so that it becomes easier to interpret the actual effects.  
  
I believe that there is to little information in Table 1 regarding the models and outcomes. As a reader it is difficult to properly assess the results of the analysis. Standard errors and confidence intervals should be included. Some kind of model of fit measure should be included.  
  
As I understand it Table 1 present 6 models. This should be more clearly presented in the table, i.e., that each dependent variable in the first column denotes a specific model.  
  
References  
  
Acuna, D. E., Allesina, S., & Kording, K. P. (2012). Predicting scientific success: Daniel E. Acuna, Stefano Allesina and Konrad P. Kording present a formula to estimate the future h-index of life scientists. Nature, 489(7415), 201-202.**[**https://doi.org/10.1038/489201a**](https://doi.org/10.1038/489201a) **Alonso, S., Cabrerizo, F. J., Herrera-Viedma, E., & Herrera, F. (2009). h-Index: A review focused in its variants, computation and standardization for different scientific fields. Journal of informetrics, 3(4), 273-289.  
Bornmann, L., & Daniel, H. D. (2007). What do we know about the h index?. Journal of the American Society for Information Science and technology, 58(9), 1381-1385.  
Bornmann, L., & Daniel, H. D. (2009). The state of h index research: Is the h index the ideal way to measure research performance?. EMBO reports, 10(1), 2-6.  
Bornmann, L., & Williams, R. (2017). Can the journal impact factor be used as a criterion for the selection of junior researchers? A large-scale empirical study based on ResearcherID data. Journal of Informetrics, 11(3), 788-799.**[**https://doi.org/10.1016/j.joi.2017.06.001**](https://doi.org/10.1016/j.joi.2017.06.001) **Danell, R. (2011). Can the Quality of Scientific Work Be Predicted Using Information on the Author's Track Record? Journal of the American Society for Information Science and Technology, 62(1), 50-60.**[**https://doi.org/10.1002/asi.21454**](https://doi.org/10.1002/asi.21454) **Havemann, F., & Larsen, B. (2015). Bibliometric indicators of young authors in astrophysics: Can later stars be predicted? Scientometrics, 102(2), 1413-1434.**[**https://doi.org/10.1007/s11192-014-1476-3**](https://doi.org/10.1007/s11192-014-1476-3) **Hirsch, J. E. (2007). Does the h index have predictive power? Proceedings of the National Academy of Sciences, 104(49), 19193-19198.**[**https://doi.org/10.1073/pnas.0707962104**](https://doi.org/10.1073/pnas.0707962104) **Lindahl, J. (2018). Predicting research excellence at the individual level: The importance of publication rate, top journal publications, and top 10% publications in the case of early career mathematicians. Journal of Informetrics, 12(2), 518-533.  
Waltman, L., & Van Eck, N. J. (2012). The inconsistency of the h‐index. Journal of the American Society for Information Science and Technology, 63(2), 406-415.**

**Associate Editor's Comments to the Author:**  
  
**Comment 1** > Both reviewers are positive about this manuscript and make important recommendations that will improve its scope and strength. The author might wish to consider a revision of their title, especially in line with the discussions and implications suggested by reviewer 2 (i.e. is their approach only relevant to a three-species food web?). The author might also wish to explore advanced approaches of stable isotope trophic ecology beyond bulk isotopes, such as compound-specific stable isotopes (e.g. essential amino acids) - might this add value to the interpretations that can be made from complicated food webs where not all dietary items are known or sampled? (again, in line with some of reviewer 2's comments)

**Response** > I would like to thank the two reviewers for their positive attitude towards this manuscript and their constructive comments on the potential limitations of the proposed framework. I have carefully considered the concerns raised by the reviewers and made several changes accordingly (particularly regarding top predator and mesopredator consumption on alternative prey and cannibalism in top predator). I have also modified the title (removed “three-species”) since the revised manuscript now extends beyond only three focal species and considers a broader suite of trophic interactions in omnivorous food webs.

I would also like to thank the editor for bringing up the advanced isotope technique of compound-specific nitrogen isotope analysis of amino acids (CSIA-AA). I have now briefly discussed the potential application of CSIA-AA in the proposed framework and pointed to several references on this new technique in the seventh paragraph of the “*Applications*” section (Line 204-218):

*“Recent advances in compound-specific isotope analysis of amino acids (CSIA-AA) provide a promising tool for studying trophic interactions (see Ishikawa [2018] and McMahon & McCarthy [2016] for more details on the use of CSIA-AA in trophic ecology). A major advantage of CSIA-AA over bulk stable isotope analysis is that it can estimate trophic positions (TP) of consumers even when some of their prey items are unknown to researchers. A potential application of CSIA-AA that may aid in the proposed framework is to analyze and compare the TP of field-collected and lab-reared top predator. Theoretically, if top predator consumes more non-focal prey items in the field, its TP will deviate more from (presumably be lower than) that of the lab-reared top predator because it incorporates more lower-level biomass into the tissue. In this regard, the amount of deviation can allow researchers to gauge the actual degree of IGP in the field relative to the one estimated via the controlled feeding trial (e.g., a larger deviation in TP may indicate a lower actual degree of IGP in the field compared to the IGP estimated via the feeding trial). Additional measures can be taken to account for the effects of non-focal prey on IGP interactions (see previous paragraph for details).”*

Although CSIA-AA is relatively new to ecologists, and relevant theories, analytical models, and methodological details (e.g., trophic discrimination factors for 15N of different amino acids) are currently in development, I agree that it certainly provides a promising tool for better studying IGP and complex food web dynamics.

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

**Comment 1** > Intraguild predation (IGP) is very common in arthropods. However, due to too many uncertain environmental factors and technical methods, it is difficult to quantify IGP. In this study, the control feeding experiment and stable isotope analysis of field samples were combined to evaluate the degree of IGP in a three-species omnivorous food web (top predator + mesopredator + shared prey). The experimental design of this study is reasonable, and the stable isotope analysis technology used is relatively mature and reliable, which can solve the problem of quantifying IGP to a certain extent.

**Response** > Thanks for the positive comments on my manuscript.

Enumerated Concerns:

**Comment 1** > Manuscripts should quote more literatures published in the past five years, while the current cited literatures have been published for a long time, which does not reflect the cutting-edge research.

**Response** > I have added several recent articles to the manuscript as described below:

* Wang, S., Brose, U. & Gravel, D. (2019). Intraguild predation enhances biodiversity and functioning in complex food webs. Ecology, 100, e02616. (Line 46)

This modeling work shows that IGP could enhance biodiversity and ecosystem functioning (e.g., total biomass or primary production).

* Hagler, J. R., Casey, M. T. & Machtley, S. A. (2020). A procedure for pinpointing cannibalism, intraguild predation, and life stage-specific feeding events. BioControl, 65, 297-304. (Line 66)

This article discusses the use of molecular methods and immunological techniques to study IGP.

* Snyder, G. B., Smith, O. M., Chapman, E. G., Crossley, M. S., Crowder, D. W., Fu, Z., Harwood, J. D., Jensen, A. S., Krey, K. L. & Lynch, C. A. (2022). Alternative prey mediate intraguild predation in the open field. Pest Management Science, 78, 3939-3946. (Line 68)

This study applies molecular gut content analysis to estimate the probability of IGP on the mesopredator *Geocoris sp*. by the top predator *Nabis sp*.

* Saqib, H. S. A., Liang, P., You, M. & Gurr, G. M. (2021). Molecular gut content analysis indicates the inter‐and intra‐guild predation patterns of spiders in conventionally managed vegetable fields. Ecology and evolution, 11, 9543-9552. (Line 74)

This is a recent example study that quantifies the strength of IGP based on the proportions of prey DNA reads in the top predator’s gut contents.

* Macías-Hernández, N., Athey, K., Tonzo, V., Wangensteen, O. S., Arnedo, M. & Harwood, J. D. (2018). Molecular gut content analysis of different spider body parts. Plos One, 13, e0196589. (Line 76)

This study shows that the detectability of prey DNA sequences varies among different parts of the spider’s gastrointestinal tract.

* Michalko, R., Birkhofer, K. & Pekár, S. (2022). Interaction between hunting strategy, habitat type and stratum drive intraguild predation and cannibalism. Oikos, 2022, e08662. (Line 160)

This study examines how habitat type (vegetation vs. ground) affect IGP interactions among spider mesopredators and top predators.

**Comment 2** > Please check the description format of references in and after the text. The format is inaccurate in some places.

**Response** > Thanks for pointing out the citation format mistakes. I have corrected the citation style template in my EndNote library and made the following changes to the in-text citations (“original” to “revised”):

* “GagnonHeimpel & Brodeur 2011” to “Gagnon et al., 2011” (Line 44 and 66)
* “PolisMyers & Holt 1989” to “Polis et al., 1989” (Line 44, 46, and 166)
* “UiterwaalDell & DeLong 2019” to “Uiterwaal et al., 2019” (Line 54)
* “WiseMoldenhauer & Halaj 2006” to “Wise et al., 2006” (Line 56)
* “RickersLangel and Scheu (2006)” to “Rickers et al. (2006)” (Line 57)
* “CautAngulo & Courchamp 2009” to “Caut et al., 2009” (Line 63)
* “SchneiderScheu & Brose 2012” to “Schneider et al., 2012” (Line 81)
* “QuinbyCreighton & Flaherty 2020” to “Quinby et al., 2020” (Line 101)

**Comment 3** > In the sentence of collecting samples in the field (L 132 − 134), it is necessary to clarify the type and spatial location of the sampling habitat, or explain the environmental factors in the collection area, so as to eliminate the impact of other different environmental factors as far as possible and improve the accuracy of evaluating the degree of IGP.

**Response** > This is a critical point for accurately quantifying the degree of IGP in the field. The predator and prey samples should be taken under homogeneous conditions to minimize the effects of various environmental factors, and also at an appropriate spatial scale relevant to the research goal. I have revised the fifth paragraph in *The proposed experimental framework* section to incorporate this information (Line 123-128):

*“Finally, field samples of the top predator and shared prey are collected under homogeneous site conditions (e.g., similar ambient temperature and vegetation structure) to minimize the potential confounding effects of abiotic and biotic factors. The spatial scale at which the samples are taken should pertain to the research goal (e.g., various locations within a farm to quantify farm-level IGP, or various plots within a one-hundred-hectare grassland to quantify community-level IGP).”*

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

**Comment 1** > The present manuscript propose a method to evaluate the intraguild predation in trophic webs by using the N15 isotopic content of top predators, meso-predators and a shared prey aiming to construct an IGP curve based on controlled feeding trials. The author proposes to use this IGP curve to estimate the degree of intraguild predation in the field. IGP is a problematic issue in trophic web studies when a large number of taxa are included. The author´s proposal is an interesting point of view aiming to facilitate the management of field data but I believe it could be useful for simpler trophic interactions than the example proposed in the manuscript (although is asimple tri-trophic web, spiders have very complex feeding preferences). I am aware of the difficulty of studying arthropod food webs in the field and the advantages and disadvantages of using stable isotopes. I would like to expose several considerations about the proposal in general:

The main problem of the IGP to study trophic interactions in very generalist predators, such as spiders, is the high number of potential prey available in the field (including cannibalism) that could be determinant to establish correct levels of N15 and unapproachable in an experimental trial. In fact, although these generalist taxa belong to agricultural systems (simplified systems), the variety of prey can be very high. The author considers this limitation and recommends collecting large enough field samples of top predator to reflect the overall IGP patterns. However, other limitation comes from those cases where phytophagous insects show high levels of N15. For example, in Lepidoptera, the metamorphosis metabolism results in adult individuals with N15 levels comparable to those of predators (see Tibbets et al. 2008). Predating on such phytophagous insects could lead to a high enrichment in the N15 content of top predators not produced by the IGP. Therefore, I consider that this procedure could be useful for other less complex food webs in which IGP is present but the number of potential prey for top and meso-predators is less varied (Acari or Neuroptera for instance). In line with the same subject, in lines 168-172 the author indicates that it is possible to adjust the N15 signature of top predators that feed on non-focal prey. What is the way to do this calibration without knowing the N15 of these other preys? Please specify the way to calibrate N15 and add references.

As the author rightly points out, this is a promising proposal but one that needs to be refined in the future. Therefore, in order to be able to carry out the necessary experiments to fine-tune this protocol, it would be necessary to set out in more detail its limitations.

Tibbets TM, Wheeless LA, Del Rio CM, 2008. Isotopic enrichment without change in diet: An ontogenetic shift in δ15N during insect metamorphosis. Funct. Ecol. 22, 109–113.

**Response** > Thanks for pointing out several critical points regarding the implementation of the proposed framework. I appreciate the suggestions and have revised the manuscript accordingly where appropriate. The details are provided in the following paragraphs.

A major concern of the proposed framework is that top predator can feed on alternative non-focal prey items in the field, which may interfere with the interpretation of the δ15N values of top predator. In fact, as stated in the third paragraph of the *Applications* section, the framework is best suited to relatively simple food webs with strong interactions among the focal species, in agreement with the reviewer’s opinion that the framework is useful for less complex food webs in which IGP is present but the number of potential prey for top predator and mesopredator is less varied.

For more complex webs, researchers can first identify the potential non-focal prey in top predator’s diet via field observations or molecular gut content analysis, collect field samples of these prey items and analyze their nitrogen isotope signatures, and adjust the δ15N of top predator by subtracting the δ15N difference between non-focal and the focal shared prey. Alternatively, researchers can directly include these non-focal prey items in the feeding trials to account for their effects on IGP interactions and thus the δ15N of top predator. Finally, collecting large field samples of top predator can help better capture the overall IGP patterns at the population level (see the sixth paragraph of the *Applications* section for more details).

As the reviewer mentioned, some top predator species (e.g., spiders) may engage in cannibalism, which can alter their δ15N values in the field. In this case, I suggest rearing multiple top predator individuals in the same experimental arena to allow for cannibalism events in the feeding trial. This will better reflect predator-predator interactions in the field and account for their effects on the δ15N of top predator (also see my response to Reviewer 2’s comment 5).

Another potential limitation the reviewer pointed out is the nitrogen isotope enrichment from larvae to adults in phytophagous insects due metamorphosis metabolism. Consuming these 15N-enriched prey items may lead to a high δ15N of top predator not produced by IGP. To address this issue, researchers can apply the framework using prey individuals at different developmental stages in separate feeding trials to derive stage-specific standard IGP curves. The stage-specific IGP estimates can then be combined to form the overall IGP pattern (see the fourth paragraph of the *Applications* section for more details).

As the reviewer commented, it is necessary to consider several potential limitations and further elaborate on the experimental details. I agree with the reviewer’s opinion and have revised the manuscript accordingly for a more thorough discussion of the concerns raised and the ways to address them. Overall, I believe the proposed framework can serve as a useful tool for studying IGP dynamics in the field.

*Abstract*

**Comment 2** > Line 32: to study.

**Response** > Revised (Line 32).

*Introduction*

**Comment 3** > Line 101: Quinby, Creighton & Flaherty 2020 (add a comma between authors).

**Response** > Thanks for pointing out the mistake. I have changed the original citation “QuinbyCreighton & Flaherty 2020” to “Quinby et al., 2020” (also see my response to Reviewer 1’s comment 2 on the citation format issue).

*The proposed experimental framework*

**Comment 4** > Line 96-108: Have been the arthropods kept under starving for a time before the experimental trial?

**Response** > Yes, the experimental organisms should be starved for a period of time prior to the feeding trials to avoid potential contamination from the previously-consumed food in their guts. Additionally, field-collected organisms should be starved as well (if possible) to empty their gut contents before stable isotope analysis. I have added these experimental details to *The proposed experimental framework* section:

*“All experimental organisms are starved prior to the feeding trial to avoid potential contamination from their gut contents.”* (Line 101-103)

*“(all experimental organisms are starved beforehand as in the first feeding trial)”* (Line 107)

*“These field-collected organisms are kept in starvation to empty their guts before preparation for stable isotope analysis.”* (Line 128-129)

**Comment 5** > Line 108-110: In the case of spiders or other arthropods such as larval green lacewings it would be desirable to consider the cannibalism event in the experiment including individuals from the same species in the proportions of the diet.

**Response** > Thanks for bring up this important point. Yes, cannibalism among top predator individuals should be accounted for in the experimental framework as it could potentially alter the interactions between top predator and the shared prey or mesopredator. I have discussed this in the sixth paragraph of the *Applications* section (Line 198-201):

*“Furthermore, if top predator engages in cannibalism, multiple predator individuals (based on their field density) can be reared together in the same experimental arena to better reflect cannibalism events in the field and to account for their effects on the δ15N of top predator.”*

**Comment 6** > Is the meso-predator fed with the shared prey before the assay or is kept under starving for a time? It would be important to include these two treatments in the experimental assay because the gut content of the meso-predator may affect the N15 content of the top predator.

**Response** > The mesopredator used in the second feeding trial should be starved to minimize the potential effects of consumed shared prey in the gut contents on the isotope signatures of the top predator. I have added this experimental detail to *The proposed experimental framework* section (Line 107):

*“(all experimental organisms are starved beforehand as in the first feeding trial)”*

(Also see my response to Reviewer 2’s comment 4 for more details on starving the experimental organisms.)

**Comment 7** > Line 120-121: Likewise the top predator, the mesopredator is a generalist spider that can eat other resources different from the shared prey used in the experimental trial. Therefore, the N15 content of mesopredators may vary respect to the individuals used in the experimental trial. It would be necessary to collect mesopredator individuals from the field aiming to know the N15 content in field conditions.

**Response** > Thanks for pointing this out. Yes, I agree that it would be helpful to analyze the nitrogen isotope signature of mesopredator in the field since it can feed on prey items other than the shared prey. I have discussed this issue in the fifth paragraph of the *Applications* section (Line 180-188):

*“Mesopredator may consume prey items other than the shared prey in the field, thus leading to deviation of its δ15N from that of the lab-reared mesopredator (which feeds entirely on the shared prey for constructing the standard IGP curve). To account for this potential source of variation in the δ15N of mesopredator, researchers can analyze the nitrogen isotope signatures of mesopredator in the field and calibrate the δ15N values of the field-collected top predator individuals by subtracting the average δ15N difference between field-collected and lab-reared mesopredator individuals (such δ15N difference is due to mesopredator feeding on alternative prey). The calibrated top predator δ15N values can then be interpolated to the standard IGP curve to estimate the degree of IGP more accurately.”*

*Applications*

**Comment 8** > Line 138: to study.

**Response** > Revised (Line 148).