ICES Journal of Marine Science, 2025, Vol. 82, Issue 12, fsaf211
https://doi.org/10.1093/icesjms/fsaf211
Received: 21 October 2025; **revised:** 5 November 2025; **accepted:** 6 November 2025
Reply

ICES CIEM
International Council for the Exploration of the Sea
Conseil International pour l'Exploration de la Mer

Reply to "Ecosystem modeling in the Northeastern United States: a comment on Sivel et al." by J.S. Link

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Abstract

Sivel et al. (2025) provided a review of peer-reviewed literature focused on food-web modeling and highlighted the many important contributions from research focused on the Northeastern US Shelf. Link (2025) raised several points related to our criteria of model evaluation and exclusive use of peer-reviewed literature, among others. We defend our use of peer-reviewed literature, clear search methodology, and our definition of model evaluation in determining whether a model application was validated. Our team that was led by early-career scientists found areas of agreement with Link (2025) but strongly disagree that the involvement of senior scientists or "experts" from outside the author team is needed in conducting analyses such as those presented in Sivel et al. (2025).

Keywords: ecosystem; food-web; models

Introduction

Sivel et al. (2025) conducted a review of published peer-reviewed journal articles that focused on the application of end-to-end food-web models in the Northeastern US Shelf (NEUS). The article described the innovative and scientifically impactful developments in food-web modeling in the NEUS from the pioneering work of Dr Robert Ulanowicz to the more recent applications in the region that build on these seminal works. Sivel et al. (2025) present summaries of research published in the peer-reviewed literature that address many topics ranging from ecosystem structure to the assessment of anthropogenic drivers. In all cases, we present the contributions without a critical evaluation of each paper's strengths and weaknesses with the intent of providing the reader with a survey of what research has been done in the region and published in peer-reviewed journals. Following this literature review, we provide recommendations based on our observations of the published literature to further the development and application of food-web models. Link (2025) argues that we questioned the skill, use, and development of research in the NEUS. We feel the opposite is true, and that Sivel et al. (2025) present a case that some of the most innovative pursuits of food-web modeling did indeed occur in the NEUS, with many of the advancements highlighted in the paper.

Link (2025) goes into great detail on many subjects, both related to Sivel et al. (2025) and unrelated. In this response, we will cover the subjects that are directly related to concerns Link (2025) expressed regarding Sivel et al. (2025), including the following: (i) search criteria and papers included in the analysis; (ii) the % of applications with model validation and evaluation, and (iii) the role established scientists play in informing early-career scientists.

Search criteria

Link (2025) argues that Sivel et al. (2025) missed large amounts of literature focusing on food-web modeling in the NEUS. Sivel et al. (2025) employed an objective approach, using clearly defined keywords and specific search engines, and focused solely on peer-reviewed, published journal articles. It is true that our search, like any other, will likely miss some contributions. Indeed, Link (2025) finds a few papers that our search methods missed. No literature review is likely to be fully comprehensive, but to be useful, it should provide an unbiased sample of papers. In the case of Sivel et al. (2025), we focused on peer-reviewed journal articles and find no reason to support Link's (2025) claim that our assessment of the peer-reviewed literature was biased.

Link (2025) argues that the gray literature contains copious amounts of missed information on food-web modeling. We do not challenge the notion that large amounts of gray literature publications exist, many of which are reviewed in some form and contain important advancements in scientific inquiry. We explored the suggested gray literature provided by Link (2025) when writing this response, and in some cases, we had trouble finding the documents; in others, we could not find the pertinent information. It may also be that some peer-reviewed papers did not cite or highlight important efforts in the development of the model used that were covered in the gray literature. Nonetheless, we agree that it would be an interesting extension to Sivel et al. (2025) to compare the differences in objectives, methods, results, and review processes of the work contained in the gray literature with those published in the primary peer-reviewed literature. That was not the intent of Sivel et al. (2025), and Link provides a good summary of many of those important efforts, which are contained in the gray literature.

Model validation and evaluation

We disagree with Link's (2025) arguments to increase the number of models assumed validated and the sequential model development argument to reduce the total number of models. Link (2025) argues that if a model was validated at a stage prior to its current application, it should be considered validated because of its sequential use. Link (2025) also argues that many models we categorize as not being validated were, in fact, validated during the development stage, and this may not have been reported in the primary literature papers, but instead, such information is presented in the gray literature. Link (2025) further argues that if models built sequentially and used for multiple applications (across multiple papers) were validated in the initial model stage, they should be categorized as such. We disagree and argue that each model application would benefit from evaluation and testing: "Here, we consider model evaluation as the process to assess goodness of fit between model outputs and observation data, and methods assessing how uncertainty can affect users' conclusions" (Sivel et al. 2025). Thus, each application would benefit from such an evaluation. As noted in Sivel et al. (2025), not all models require the same evaluation approach, and our findings do not question whether any previous model was fit for purpose. Instead, we provide recommendations that we believe will further the development of food-web modeling. Model development routines, such as PREBAL, are good practices and were considered a validation/evaluation method in our totals even though they are not fully true to the definition of model evaluation provided in Sivel et al. (2025) noted above. The same criteria were used in categorizing the four models that the lead author of this reply was involved in developing, and not all of them meet these criteria. Again, like our assessment of all the papers, we did not challenge whether they are "fit for purpose"; instead, we highlighted areas where more rigorous model evaluations would benefit the field.

Collaborating with "experts"

Link (2025) suggests a lesson to early-career scientists to reach out to "experts" in the field to avoid the mistakes he argues were made in Sivel et al.'s (2025) use of the literature. These early-career scientists led an effort to review the peer-reviewed literature, the gold standard in academic research, to summarize food-web modeling in a region, and to make recommendations based on the findings. We fully agree that collaborating with colleagues is important. Surely, if they had directly worked with Link (2025), they would have benefited from his vast experience and may have been able to navigate the gray literature; again, that was not the objective of the paper. However, there should be no implied need to include senior "experts," outside our group, as this could hamper the writing of independent inquiries led by researchers at various stages of their careers.

In conclusion, Link (2025) provides a long critique of Sivel et al. (2025) that delves into many areas of ecosystem-based management, climate change, and other related topics. We agree with many of his comments and defense of ecosystem-based management and ecosystem modeling's utility to address a wide range of scientific questions. We disagree with his interpretation that our paper in any way challenged the "operational living marine resource management uses of said models." We share the goal of continuing efforts to develop ecosystem-based management to address pressing questions and applaud the efforts he outlines, as well as the progress that has been made to date, as highlighted in Sivel et al. (2025).

Conflict of interest

None declared.

References

Link JS. Ecosystem modeling in the Northeastern United States: a comment on Sivel et al. *ICES J Mar Sci* 2025

Sivel ET, Eigenberger T, Ervin AC *et al.* Lessons from seventy-five years of ecosystem modeling in the Northeastern US Shelf. *ICES J Mar Sci* 2025;**82**:fsaf078. https://doi.org/10.1093/icesjms/fsaf078

Handling editor: Howard Browman