Senior Editor Comments to Authors

This MS is strongly linked to the recently published one in MEE since it was split from this published one early on. While the links between the 2 obvious, the present MS should be more clearly independent from the other since the overall perspective of the reviews is that the ms adds little to the previous published paper.

> We would like to express our surprise regarding this comment, since the other article was only mentioned twice (in the caption of a figure and in a table). In order to address specific comments by the first reviewer, we actually had to cite this manuscript in a few more places. We also want to point out that the submitted version of this present manuscript includes (i) an overview of embedding techniques and their application to species interaction networks, (ii) a discussion of the properties of metawebs that make them amenable to prediction through embeddings, and (iii) a discussion of the remaining technical and methodological challenges associated with this approach; therefore, we strongly feel like the comments about this manuscript adding little, as all of the aforementioned elements are absent from the previous manuscript, are unfair. Specifically, our initial introduction contained the following statement for this manuscript: "In this contribution, we highlight the power in viewing (and constructing) metawebs as *probabilistic* objects in the context of rare interactions, discuss how a family of machine learning tools (graph embeddings and transfer learning) can be used to overcome data limitations to metaweb inference, and highlight how the use of metawebs introduces important questions for the field of network ecology".

> To be perfectly transparent about our feelings reading the review, we did feel like our manuscript was reviewed with the mindest that it was presented as a Research (and not Perspective) article. We followed the MEE guidelines about Perspective articles, *i.e.* "to stimulate scientific debate, [by offering] conceptual advances or opinions or identity gaps in knowledge", which is a different type of work than a complete methodological treatment of the topic, or a narrative Review of it.

> We also, notably in response to the comments made by the second reviewer, had to defend ourselves against accusations of sensationalism ("marketing"), opportunism, speculation, and general sloppyness. Although we did not assume that this reviewer was acting entirely in bad faith, we hope that the senior editor will understand that we had to take a lot of critical distance from these comments. We did bring specific responses to how a significant numbers of them were unfair, unsubstantiated, or unprofessional, but have nevertheless done our best to respond to them fairly; this is more than the

courtesy extended to us by the second reviewer. It is concerning to receive reviews of this nature; not only do they establish the wrong example by throwing around unfounded accusations of low professional standards, they have a lasting impact on the perception of early career scientists, of which this manuscript has many. We do politely request that the senior editor, should this manuscript be sent for review again, does not asks the second reviewer for their advice.

In summary, they should be complementary but not overlapping to a large extent. In the current ms, a strong message yet different from the other paper would be that graph embedding is an emerging field in machine learning that holds great potential for ecological problems. And this will certainly affect the whole structure of the ms.

We do appreciate this comment, and wholeheartedly agree. Yet, it is extremely difficult for us to address, as to the best of our understanding of the two reviews and the senior editor comments, there is not a single instance where areas of overlap are clearly identified, or indeed identified *at all*. Following the comments of reviewer 1, we do hope that the central argument ("graph embedding is useful for link prediction") will be clearer, but in the absence of specific substantiated comments about what constitutes overlap, we have not made significant alterations to the manuscript.

Adding a section on "how to" approach graph embedding, and a nutshell section on embedding methods vs dimension reduction method, an example dataset to assess with a R-code; all these would be essential for the ms even if it coud go over the word limit. Overall, if these comments are implement together with the suggestions of the reviewers, the current ms would be completely revamped and would need a lot of work from the authors. yet it should produce a strong ms and have potential to influence the field.

We have added this section after the overview of techniques, and we now distribute the code to perform the analysis as Supplementary Material. This resulted in an additional figure, outlining a number of information that can be extracted from an embedding of a metaweb.

Associate Editor Comments to Authors

(There are no comments.)

Reviewer 1 Comments to Authors

General comments

I read this interesting text about the idea of using embeddings to predict metawebs. I understand that this is a perspective. About 2/3 of the manuscript is

dealing with metawebs (very pleasant to read), and the remaining 1/3 recommend the use of network embeddings (actually, the authors have mixed up embedding methods and dimension reduction methods, which are actually two disjoint but connected concepts).

However, I am very suprised that there is no material to support the proposal. Do the authors already use an embedding techniques on their datasets? They claim it is promising to use embedding with metawebs, but is it an intuition? or are there scientifical experiments/analysis/reasoning that could support this idea?

> This is a surprising comment. The table in the original manuscript already had a list of embeddings methods applied to species interactions predictions, which in our opinion should assuage the concern of reviewer 1 by establishing that this is not "an intuition". Indeed, table 1 has a number of empty cases in the last column, which not only outlines what has been done, but what methods are available for application to species interactions. Furthermore, a part of the text discusses recent work from Strydom et al. (2022) wherein graph embeddings have been used to not only predict a metaweb, but do so by transferring information across space. Despite the fact that these information was already in the original manuscript, we have re-worded and expanded a number of sections to make them stand out more.

At some point, I am wondering if it would be have been possible to write the same article replacing "embedding" by "graph neural networks"... These are all interesting techniques, and I do appreciate that the authors try to help ecologists to understand them. But we need more facts to understand the pros/cons of these options. I am afraid the reader can not be convinced reading the manuscript in its present form.

> This is a good question, which we explored during the writing of the initially submitted manuscript. GNN approaches are comparatively younger than the broader family of graph embeddings methods. We have added a rationale for not spending too much time on GNN (which would also require to introduce a solid amount of literature on deep learning in general), but point readers towards recent reviews. In order to help convince the reader, we are confident that the addition of an illustration or embedding on empirical data, alongside the discussion of already published studies, will help. Furthermore, we have revised the last section to bring more concrete suggestions of required work, which will hopefully stimulate further analyses.

I am sincerely annoyed that I can't be more positive, maybe I missed something, and I hope that these comments can be used to improve the manuscript. But I do think this paper is "too light" to be published in MEE.

> We hope that our gentle reminder of the nature of this (Perspective)

paper as given in the response to the Senior Editor, will help assuage the sentiment that the paper is too "light"; it is not expected to be a formal comparative analysis of the methods, not is it expected to be an exhaustive narrative review. Indeed, we point out that these approaches are novel for the prediction of species interactions, and our hope with this manuscript is that more community ecologists will attempt to use them in the future. We are confident that the additions to the text are addressing all major comments raised by reviewer 1.

Detailed comments

Until l.68: a very nice introduction about the concept of metaweb

Thank you.

L.72-83 & Table 1 : here, there is a mixture of graph and node embeddings, i.e. projecting a graph or a node into a low dimensional space. Would require some clarifications.

The second column of the table already specifies (explicitly) whether nodes or graphs (as well as sub-graphs, multiple graphs, and node complexes) are embedded given the different methods; we are not sure what the reviewer wants to see added as a clarification. As the information asked is already given, and in the absence of specific feedback, we have not changed the manuscript (besides splitting the table to separate the type of embedded object and the embedding method).

L.91 & Figure 2 : there is another subtlety here. Indeed, embedding is often followed by a dimension reduction technique (could be PCA, t-SNE). As in figure 2, section D is for dimension reduction. In my opinion, these are two distinct concepts, embedding and dimension reduction. For instance, one can perform an embedding listing a series of metrics, then perform a PCA to obtain a 2-D representation.

This is indeed a subtle point, and very respectfully, we think that the reviewer's confusion justifies the need for our manuscript. Graph embedding and dimensionality reduction are different yet *related* concepts. For example, PCA can be used initially to maximise the information in a dataset then fed into t-SNE; both the PCA and t-SNE step can be parameterized to retain fewer dimensions, but so can neither of them, with the user deciding to visualize a two or three dimensional representation. We have added a paragraph to make sure this point is extremely clear, and we hope that it will help readers decide on the appropriate method to use. As a sidenote (and not discussed in the manuscript), measuring network properties and performing a PCA on those (although common in network ecology)

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is not an embedding on the network, and is therefore not discussed here for the sake of text length.

l. 107 : "large confidence intervals during estimation of the values in the low-rank space" => what does it really mean?

We have clarified that this would hint at the fact that the two species pools are too distinct for transfer learning to work with adequate confidence. Thank you for pointing this out.

L. 109: "resulting embeddings would have interactions" => same thing. Could you be less elusive or give references?

We have clarified that this would lead to higher variance in the original dataset compared to the target dataset, leading to too much noise in the prediction. Thank you for pointing this out.

L. 128-141: this part is interesting at some point, but the reader can loose the point of the article... In particular, mentioning AI here is disconnected to the remaining text. I understand that embedding methods are part of AI at some point, but AI gather so many domains....

We have considerably expanded the last section, to further clarify which issues were "research-based" *v.* "praxis-based" problems, alongside possible avenues to start adressing these problems. We do hope that this will make the point of the last paragraph more obvious.

Reviewer 2 Comments to Authors

General comments

This manuscript consists in a perspective on the use of graph embeddings to infer potential interactions, by using metadata and interaction databases. It is, somehow, linked to the idea that potential interactions involving a new species can be predicted from its traits (vulnerability and foraging) and interactions involving similar species. If such an idea , based on niche conservatism regarding interactions, is not new (e.g. Desjardins-Proulx et al. 2017, Strydom et al. 2021a), the use of graph embeddings to infer potential interactions is a useful step forward.

We agree with this comment; graph embeddings can indeed project networks in a space where the detection of unknown interactions is easier, and we support this claim with several examples throughout the text. No changes requested (none made).

However, I strongly doubt that such a short and speculative paper helps the network ecology community.

We have no choice but to very firmly push back on the idea that the manuscript is speculative. The manuscript is prospective, or forward-looking, but every claim we make is either backed by facts

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(in the forms of references to existing literature in the fields of ecology, machine learning, and environmental governance), or accompanied (notably in the last section) by suggestions of additional work needed to address some of the gaps we have identified. "Speculative" is an unfair assessment of our work, and we do hope that it did not affect the reviewer ability to take it seriously. We would like to point out that despite this sweeping (and serious) accusation, the reviewer is unable to point to a specific instance of a speculative claim.

We furthermore point out that table 1 contains examples of published research using the techniques we discuss, and that the text contains discussion of some of these papers. We are therefore confident that we clear the bar for the manuscript not being "speculative" by a very large margin.

Despite being appealing (it mentions the term 'machine learning' 10 times), I think that it contains too much generic wishful thinking to be useful and implementable.

We would like to question the intent behind this comment. Is there a threshold above which mentioning machine learning too much opens authors to accusations of "generic wishful thinking"? Should a paper specifically about a field of machine learning *not* call the field by its name? Would a predictive method by any other name predict just as well?

We also would like to point out that the doubt expressed by the reviewer about the ability to implement these methods is directly addressed by Table 1 and the main text, in which we discuss examples of research papers using these exact methods for the prediction of species interactions. We are concerned that this comment, besides its inappropriate amount of sarcasm, reflects a bad faith reading of our manuscript.

In other words, I would expect less marketing and much more science for a paper in Methods in Ecology and Evolution.

We do not feel like such a cheap accusation, without any argument to back it up, warrants a response. Again, the reviewer is comfortable making sweeping statements about "marketing" without seemingly being able to identify a single instance that has too much marketing, or too little science.

We would expect less aggresive snark and much more constructive criticism for a review in Methods in Ecology and Evolution.

Moreover, the main idea of the paper (using node metadata to predict new interactions using nodes embedding) comes from a paper of the same authors that is not peer-reviewed and I feel a bit uncomfortable with that (see Strydom et al. 2021b)

This statement – in addition to being arguably incorrect – is no longer relevant as the version of the preprint we cited has now been published. We do hope that this brings reviewer 2 comfort.

Detailed comments

L51: this sentence is a tautological, if interactions are represented by parameter of bernoulli law, there are already weighted appropriately

No changes were made - the interactions are weighed appropriately compared to the binary baseline, as is the point of the sentence.

L62: 'machine learning algortihms' : fancy term but not very specific. What algorithms are you talking about ?

The paper cited at the end of this sentence primarily uses artificial neural networks, but concludes on the potential to use "data-driven techniques", which feels neither less nor more specific than our reference to "ML algorithms"; as we do spell out in the sentence immediately after this one, the core point is that ML algorithms that can be made to generate data have the potential to augment data collected through empirical means. No changes made to the manuscript.

L78: Botella et al. 2022 does not use node embeddings but network embeddings. In this case, each point of the low dimension space is a network and not a node. Consequently, it won't be useful to predict new potential links. Table 1 is mixing nodes and network embeddings that do answer to different ecological questions.

The fact that Table 1 lists different types of embeddings is self-evident from the second column of table 1, which is specifically about the type of objects being embedded. No changes made to the manuscript.

Concerning the Botella et al. 2022 paper, the reviewer is indeed correct that they use graph embedding. We do agree with this interpretation. In fact, nowhere do we argue that their results are relevant to interaction prediction, and instead we summarize the findings of this article: there is a need to evaluate multiple embeddings for each problem, as their performance may differ.

All the information required was already in the manuscript, no changes were made.

L84: This sentence is too generic. What are machine learning techniques here ? If you include node embedding in it, it deals with discrete data. Moreover, there is a broad network science litterature that deals with discrete data.

The techniques we discuss are listed in the reference given immediately at the end of the sentence. There is indeed literature on discrete data (we do not make claims to the contrary); our point remains that projecting discrete data into a dense, continuous space comes with a

list of benefits (that are listed in the remainder of this paragraph, together with references to the relevant literature). No changes were made.

L129: This sounds a bit superficial and opportunistic. I don't think such a serious topic can be adequately covered in a short paragraph. Yes, we all know that machine learning techniques will save the world, thanks.

> We would like to question whether this dismisiveness of our work is contributing in a positive way to the discussion that is supposed to take place as part of the review process, and whether this is the behavior the second reviewer thinks should be demonstrated to early career authors.

> To be candid, the comments by reviewer 2 mostly come across as opposed to our work as a matter of principle, but (more concerningly) as refusing to take it seriously, by chosing to expres a concerning number of points through sarcasm rather than in a professional voice more becoming of the review process.

> We did our best in our response to the specific points below to not reciprocate, and explained why changes were (or were not) made, but we cannot weigh this review equally to the constructive comments made by the first reviewer. "Superficial", but mostly "opportunistic", are egregious attacks on our profesionalism, and insinuate that we did not write the manuscript in good faith. They do not deserve a reply, for they should never have been written.

More seriously, we should not forget that these techniques are built by people and not machines. Consequently, they are also subjects to cultural/national biases since influencing academic centers are located in the western world.

> We do agree with the reviewer, and this is, in fact, a core point of the paragraph, and of most of the articles cited therein. As the reviewer and authors are in agreement, no changes were made.

L135: I doubt that 'algorithmic thinking' is generating new knowledge thanks to machine learning. Actually, ecological knowledge is generated by people, not machines. Of course, statistical techniques help scientists to analyze and structure data. But they must remain tools in the hands of individuals.

> We do agree with the reviewer (but would like to point out that the definition of algorithmic thinking we use comes from the article cited immediately before we first mention "algorithmic thinking"). We have added a reference to "algorithm in the loop" approaches (in other fields, as this notion has been under-explored in ecological sciences), alongside their possible issues, and suggestions as to what the next steps are.

L140: But it should also be recognized that these tools, because they can be

difficult to understand for non-specialists, can pose problems of interpretation. Moreover, they can make those who make decisions dependent on a tool that they do not master.

> This has been adressed as part of the previous comment, by emphasizing the need for training and computational literacy in decision makers.

L145 : Long acknowledgements section for a short paper !

> We refuse to engage with this comment.