Editor's comments

The most salient point is that the preprint is a kind of response/comment to a recent paper of Fründ in Ecosphere.

> I have de-emphasized this component of the manuscript in the revision - it is now framed as a "best practices" recommendations, with a discussion of the consequences of changing the denominator as suggested by Fründ towards the end. This is made clearer in the introduction, by explaining first the difficulties in interpreting the components, then the importance of choices about the denominator.

In addition, the ms does not follow a classical Introduction/M&M/Results/Discussion section, arguably because of the response/comment nature of this contribution.

> I do not think the "classical" structure would be appropriate, as it would essentially disconnect the explanations of how components are calculated from their use in the numerical experiments. To guide the readers I have added an expanded outline to the introduction, and strengthened the rationale for all of the numerical experiments in the text.

As suggested by reviewer 2, the ms would gain clarity if,

an Introduction section more explicitly states the nature of the diverging views between Fründ and you, and thus more clearly exposing the motivation of challenging the recent paper of Fründ,

> As mentionned in a previous comment, I have moved this part of the manuscript towards the end, with more explicit recommendations about the use of the proposed alternative.

a Discussion section synthesizes the pros and cons of both approaches, as it seems that each method can be justified and used in an appropriate context.

> Same answer as above. In addition, I have clarified the mathematical notation throughout, so that it will hopefully be easier to follow. I have also added a conceptual cartoon in the introduction, which I hope will help guide readers.

Reviewer 1

Main comments

While there is no doubt that partitioning interaction turnover is a needed tool for ecologists, there has been recent debate on how to best perform this partitioning. I assess this manuscript from the perspective of the interested user in applying such indexes and doing it correctly. This means that while I mostly follow the maths decomposing the different indexes, I focus my review on their interpretation, and I can't fully assess the more complex mathematical derivations.

> I have attempted to clarify the notation *and* the way the derivations are presented, so that they are easier to follow by interested users as well.

I appreciate the detailed explanation provided and the rationale behind each index. I think the calculation and interpretation of BetaOS are clear. This one can be interpreted unambiguously as interaction rewiring among shared species. To me, this is the key index to interpret for most ecological questions and can be interpreted as a single index (probably along with the proportion of shared species for context).

> Drawing on the numerical experiments, I have emphasized how β_{os} relates to the rewiring probability, and now give an expression to estimate the rewiring probability based on β_{os} for networks of difference connectances. I hope this will further re-inforce the usefulness of this measure.

Next, Beta WN is also intuitive, but as depicted in Fig 2 it needs to be clearly stated that it depends on both interaction rewiring and the proportion of shared species. For most ecological questions it may be of secondary importance.

> The figures have been re-worked extensively following the new structure of numerical experiments, and in doing so, the description of Beta WN has also been expanded.

The most problematic term for me is BetaST. This is also stated in the manuscript, and I agree that it has caused a larger degree of confusion. Interpreting this term (beyond an error term, which is the simplest interpretation) is complex. In fact, the manuscript often describes their behaviour as "as expected" given its mathematical formulation, but looking at the literature it is clear that most researchers (including myself) were not expecting some of those behaviours. However, even when completely understanding how it behaves, the interpretation is still too complex for me in order to be useful. Fig 2 B is ilustrative to me. Note that at q = 1; BetaST = 0 regardless of the proportion of species sharing! Hence, BetaST tells you nothing about the contribution of species turnover when rewiring is high (ST stands for species turnover, so it's normal people get confused). I had this discussion with several researchers, and I can tell this is hard to grasp at first. Following with figure 2B, you can see that the same value can mean two very different things. BetaST can be low if there is high rewiring, or if rewiring is very low, but they share most species. This is less accentuated in the relative importance but is still the case. Hence, I would suggest giving clear recommendations on not interpreting BetaST as a primary index, but only in cases when is pertinent, and giving a clear context (i.e. the proportion of shared species, which I think is much more useful for ecological questions). I know this is suggested in some places of the manuscript, but in my opinion, it can be said stronger. In fact, I would love to see a section on how to interpret each component, recommending the interpretation of BetaOS as the most straightforward, and cautioning that interpretation of beta WN, and especially ST need to be done in context and can't be interpreted alone.

> The new Figure 2 should facilitate the interpretation of Beta ST, and how Beta ST/WN should probably be looked at first.

Minor comments

line 13: Currently it is done in several ways, so maybe better cite here Poisot et al 2012.

> Done.

line 15-21: This is a very long sentence with "i.e", ";", "-", which I had to read twice. Consider splitting it up. At least a point after "Tuomisto 2010)" I think is needed.

> The introduction has been re-written quite extensively, and therefore this sentence was fixed.

lines 72-74: letters used in the text do not match those in the equation. (m = k, I think)

> Fixed.

line 85: The second "similarly" can be replaced by "also".

> Fixed.

line 98: I would name between brackets the abbreviations of Sorensen and Wittaker indexes when first mentioned a couple of lines above.

> This section has been removed from the revised manuscript.

line 111-112: I think the reader needs first to be introduced briefly to what Frund did differently to understand this sentence. This is not done until line

> This section has been pushed further down in he manuscript, and so hopefully the differences will be clearer.

line 150: I understand we do not need species yet, but this first numerical exercise is quite abstract. Maybe guide the reader on the purpose of the experiment. Which I believe is to describe the behaviour of the components.

> This numerical experiment has been replaced by a more illustrative one.

line 165: I agree with this interpretation "it is a quantification of the relative impact of rewiring to overall dissimilarity". My critique is that this measure, as defined here, is hard to interpret ecologically and I may dare to say that irrelevant for most ecological questions.

> This has been expanded upon in the conclusion.

Line 165: But I do not follow the next sentence. What do you mean by "all non-turnover mechanisms being accounted for in the decomposition, can be explained by turnover mechanisms". See my point above on the ambiguity of interpreting its values, which can emerge from very different ecological situations.

> Some mechanisms who do not contribute to turnover have been mentioned to make this section easier to follow.

line 171: Adding the illustration may help a lot. I had to write it myself (and it was not easy for me).

line 228: formatting error when designing the title.

> Fixed.

Fig 3: Maybe expressing the x-axes as connectance dissimilarity would help the reader? I would also appreciate a comment in the text on further partitioning of the components on changes due to differences in link number (related to connectance) and true link turnover. Those are discussed by Frund, and I think it can help interpret those values depending on your ecological question.

> This numerical experiment has been reworked, and the axis is now the relative connectance between the networks.

line 260: Here a caution on interpreting BetaST may be good as stated above.

> This is now part of the discussion.

Reviewer 2

Main comments

The paper is dedicated to an outstanding question, how to compare interaction networks teasing apart the effect of species turnover and interaction rewiring. This question is central in many studies, due to the recent development of efficient data acquisition techniques in the "multiple networks era".

The author did not write a cover letter.

> One was provided as part of the PCI submission process, I apologize if it did not made it to the reviewer.

It would have been important in particular regarding the following observations. From the last paragraph of the first section, we early understand that this manuscript is actually responding another, Fründ 2021, that gave some objective criticisms about the renowed method developed by the author of the present manuscript in Poisot et al 2011. In my opinion, the abstract is not clear about this fact because it does not clearly state the debate which is central in the paper, i.e. the different views proposed by Fründ and Poisot respectively. Indeed, having read the paper by Fründ seems to be a prerequisite to read this manuscript. The paper does not have a classical structure with the usual sections (e.g. no

"Introduction/Discussion" keywords) but is linear in the sense that the arguments are displayed one after the other. Again, this is not clear enough to easily see 1/ what is exactly the debate 2/ what is the author's strategy to convince the reader in this debate. Finally, the notations adopted in this manuscript are completely different from those of Fründ, which does not help the reader in evaluating the different conclusions raised by both authors.

> I appreciate the comments made by reviewer 2, and have re-organized the manuscript a lot, to make sure that the disagreement with the alternative proposal is contained within its own section.

About the numerical experiments, they are well conducted and clear but could be better motivated. The first one consists in redoing the small example proposed in Fründ (here, this is clear). The conclusion is that actually both authors have two different understanding of the betast but in fact the actual incoherence between the two authors comes from the fact that Poisot counts the shared interactions twice, which is actually not an issue as long as the network is large enough. The next two experiments are dedicated to bipartite networks : what is the reason for this choice? In fact, one can question the generalisability of the conclusions to unipartite networks. The first one explores the variation of the different beta measures with rewiring and turnover, and actually shows these measures are responding as expected. The second one is investigating the possible link between the beta measures and connectance. In fact, it is clear that connectance difference can induce a large dissimilarity between two networks (again, not only bipartite networks). The author shows his results for two connectance values in the bipartite case, but one can expect a broader exploration of the link between intrinsic networks properties (for a range of variation, not two values) and the beta measures. Connectance is indeed important, but degree distribution is paramount as well (at a fixed connectance, for instance). Finally the author gives his opinion about the need for a new denominator as suggested by Fründ. However, the previous experiments were not performed with an alternative denominator, and then the reader can not compare different proposals.

> The numerical experiments have been entirely re-done, and now focus on unipartite networks. The comment on connectance has been adressed as part of the response to reviewer 1.

The paper is, again, dealing with an outstanding question using very important methodology. But the reader could expect more systematic experiments (unipartite case?) and a better organized manuscript.

> I hope that the extensive revisions will prove to the satisfaction of the reviewer.

Minor comments

L.32 Maybe citing Ohlman et al, Eco. Lett. as another extension.

> Done.

L.53 Typo “baed”

> Changed.

L.54 Using “x” here and then using “x” L.56-57 could be misleading. Maybe just say “|.|” is the cardinality operator ?

> Done.

L.66-68 This assumes that edges are directed. This can not be the case, for mutualistic interaction (btw, replacing an undirected edge by two directed edges is still possible)

> I clarify that this assumes directed edges. This is a common assumption in ecological networks.

L.70 Well, classically, Vm are the vertices of m (all of them)...

> Noted; no changes made.

L.70 Is Ec with “c” for “in common”? If yes, tell it because it can help memorizing.

> The notation has been updated and the mnemonics explicited.

L.71 Typo: capital N instead of capital M.

> Fixed.

L.87 In my opinion, that’s a good point to call it “rewiring”.

> Fixed.

L.89 a is supposed to be the union, and Ec is the intersection. Is there a problem here in the analogy? Maybe I missed something, my apologies in advance. Also, a/b/c are supposed to be cardinality, and this is not the case in the table. Am I right?

> Yes, the table has been fixed (and now reflects the update notation).

L.98 betaSor and betaw, betat not defined before.

> This section has been re-written.

L.98 Typo : where is (i) ?

> Fixed.

L.93-102 This paragraph is difficult to understand without being an expert of the mentioned papers. What is the message here?

> This paragraph has been re-written to improve the flow.

L.104 OK, here betat is defined. This is related to Dice (1945) or Sorensen (1948)?

> Not a concern following changes made to the previous paragraphs.

L.111-112 “they are using components of the networks that are not part of the networks being compared” is not clear.

> This paragraph has been modified.

L.114 Which calculation?

> Clarified.

L.113-121 This part of the paper looks like an answer to Fründ. Is this an “answer” paper ?

> No - the section on why the normalization they suggest is inappropriate is now its own part of the manuscript.

L.120 Typo : “very point”

> Not a typo.

L.130-143 Straightforward mathematics, details could go in appendix.

> Reviewer 1 expressed the need to have a more didactic approach to the derivations; no changes made.

L.151 Once again, the choice of these capital letters for shared (A), rewired (S), and unique (U) does not help.

> Fixed.

L.151 In fact, ps is a proportion but pr is not, whereas pr X (1-ps) is a proportion… (pr is a proportion of the proportion of non-shared links).

L.153 How can “shared links [be] rewired” if they are shared?

L.164-166 Maybe split this 3 lines sentence, to facilitate understanding ?

L.176 Precise what are these three-fifth (this is because betast/betawn=2/5)

L.178 This “2A” is the key point. This is enough to understand the full paragraph.

L.182 Then, should we move to such a measure that do not amplify the effect of rewiring?

L.184 Why do we switch to bipartite networks here ?

L.184 Again, the choice of capital R for a number of species could be discussed.

L.196-199 Are these conclusions not obvious ? From the definitions of the beta indices.

L.197 Previously, we have “proportions” and now we have “probabilities”. It would be better to harmonize.

Figure2 Please harmonize the axis labels with Figure 1.

L.208-209 Typo: bad copy/paste here.

L.210 Again, why bipartite networks? Also, why varying connectance? It is a relevant idea but one can make the degree distribution vary as well. This distribution can have huge impacts on beta indices.

Also, it is normal to expect huge dissimilarity between network of different connectance because the number of un-shared links is expected to be high. This seems straighforward. Here, the reader can expect a formulation of the hypothesis that will be tested : what is this numerical experiment for?

L.243 The formulation "I do not think" is not appropriate because the reader will not expect an opinion but conclusions drawn for the previous experiments.

L.244 It would be necessary to recall the proposal of Fründ somewhere in the paper. Did the author perform the same numerical experiments with the other numerator?

L.246 "rigorous definition of networks as graphs (as opposed to networks as matrices)". It is unclear why we have to oppose the matrix and the graph view, since graphs and adjacency matrices belong to the same conceptual context.

L.252-254 In fact, one can think that the debate is solved by choising a definition. A matter of perspective, at some point.