Recommendation by the Subject Editor  
The reviewers and I feel that the ms has greatly improved and addresses most of the concerns. There are a few minor comments remaining that need to be addressed and one more major comment from reviewer 2 (listed first below) who would like to see a broader discussion of when the MCT framework can be used, what it can tell us and what its limitations are (as relevant to empiricists).  
  
Reviewer(s)' Comments to Author:  
Reviewer: 2  
  
Comments to the Author  
I would like to thank the authors for their responses – they have addressed my major concerns and have put in substantial revisions which makes this manuscript an enjoyable and thorough read.  
I very much appreciate the changes to the introduction and feel it is both clear and well-written. The review of the experimental methods and models to measure ND and RFD is in-depth and easy to understand, including the limitations of each specific method. Overall I am very satisfied with the quality of the manuscript. I only have one main comment, and that is that I feel the motivation for this guide is missing from the paper. This is largely because there is little discussion as to what empiricists may like to do with these estimates of ND and RFD (except, perhaps, for the last section on future directions but that is more focused on expanding the theory). I think it is important to discuss the context in which the broader framework you are providing a guide to can or cannot be applied, and outline areas where empirical applications of MCT can tell us important things.  
  
Before continuing, I would like to first address a response the authors made to a comment in my previous review which they felt was condescending. I apologise for the comment and any hurt I may have caused to the authors. It was not my intention to question the authors’ intellect, expertise or understanding of their field of work in any way. Rather, my intention was to underline a lack of clarity in the writing of some sections of the manuscript, which I felt muddled the authors’ discussion of the theory. I understand that my comment was both clumsy and inappropriate and would like to thank the authors for bringing this up. Reviewing manuscripts is a difficult task for which little guidance is given, and yet has tremendous impacts on how we as scientists view our work and ourselves. In light of your response, I will be making stronger efforts to ensure that my comments do not unintentionally demean my peers and maintain this awareness for future reviews.   
  
To clarify my previous comment, I do not think that the authors misunderstand how to implement MCT empirically. Rather, I am mainly concerned with what quantifying ND and RFD tells us!   
Predicting mathematically stable coexistence simply requires the negative frequency dependence method you describe, so what is useful about also quantifying ND and RFD ? Addressing this question would add motivation to the paper. I understand that comparing methods to measure ND and RFD is the principal topic, but I think that such a guide should consider why empiricists might be doing this. I recommend it also include an overview of what empiricists should be considering beyond selection of the most appropriate model and design. 

Response: We agree with the reviewer that the ‘why’ may not be obvious to empiricists and, as a result, could use some justification. There is probably not an objective answer to why ND and RFD are useful as opposed to using Chesson’s inequality with inter- and intraspecific interaction coefficients. However, we argue that the utility (and seeming popularity) of these metrics stems from the fact that they reduce complex equations and parameters into two terms that are easily interpreted. We added three sentences to the introduction that help justify the utility of these terms to ecologists.

This ties in to the concerns I raised in regards to applying MCT to empirical communities – MCT makes many simplifying assumptions which, unfortunately, are rarely validated in nature. I should clarify that I do not think empirical work on MCT is dubious and I understand the value of quantifying ND and RFD in an experimental setting. I am concerned however, about empiricists applying the theory beyond the experimental contexts in which ND and RFD are measured, for example by attempting to extrapolate experimental predictions to coexistence between specific plant species in natural conditions. I would not want to lead empiricists astray by omitting often repeated concerns about it’s application to natural, diverse communities. This being said, a full list of the caveats involved with applying MCT to natural communities could indeed derail this paper. Adding a caution against what MCT cannot do (alongside what it can) which is relevant to empiricists would be sufficient to address my concerns. This is partially covered in the second half of Part 3.1 but could also include facilitation (removing the need to repeat that for every method), HOI’s, but also multiple limiting factors etc.   
  
To conclude, I recommend that the authors revisit Part 3 of the manuscript in order to include some discussion of the current applications of MCT in empirical research (and it’s limitations), before discussing future directions (Part 3.3).  If length is a concern, I would suggest condensing Part 2 and perhaps finding ways to integrate 3.1 with Part 2 (though these are suggestions only). My main point is that a broader discussion of when this framework can be used, what it can tell us and what its limitations are (as relevant to empiricists) should be included and go beyond the specifics of experimental or model designs. Other than that, I am very satisfied with the authors’ revisions of the manuscript.

Response: We agree with the Reviewer that the general limitations to all empirical approaches need to be highlighted. We created a new subsection (*3.2 Limitations of methods for measuring ND and RFD*) with two paragraphs outlining key limitations that apply to all five methods for quantifying ND and RFD. Some of this information was previously interspersed in other sections, so this did not dramatically affect the length of the manuscript. We now explicitly caution against using pairwise estimates of ND and RFD to make predictions about coexistence in complex communities (Lines 712-714).   
  
Minor comments:   
  
L 261-262 – If it is likely that empiricists will be using these methods to predict coexistence, I think it is important to make it explicit that Bimler et al’s solutions cannot make these predictions when facilitation is included. I believe Spaak & Laender have been working on finding a way to include facilitation into new definitions of ND and RFD (A unified definition of niche and fitness differences, bioRxiv 2018) but if I remember correctly their framework requires that species must be stable in monoculture.

Response: Thank you for pointing out that Bimler et al’s solutions cannot be used to make a prediction about coexistence. We moved this discussion to our new section 3.3 (on the practical limitations of MCT) since this really applies to all five methods for calculating ND and RFD. To date, Bimler’s paper represents the state of this problem. We checked for Spaak and Laender’s paper is published, but it is not out yet unfortunately.   
  
L 580 – no longer highlighted in blue.

Response: We thank the reviewer for identifying this issue with the figure. Correct, the last two columns should be highlighted in blue. This figure looked normal as a PDF and in the submission proof submitted to Oikos. Whatever the reason, we regret that it did not reproduce correctly during the review process and will ensure that the shading is visible in the submission proofs.   
  
L 651 – 662: I think this would fit better under the ‘model selection’ part. 

Response: We considered this comment carefully. We agree that there is some redundancy between this paragraph and Section 2. We decided to revise this paragraph to lessen the redundancy with the previous section, while emphasizing the terms of the tradeoff. The revised paragraph now read (Lines 651-664):

As discussed previously under Decision Steps Question 1, the most important tradeoff is between the phenomenological methods and the mechanistic methods. While empiricists may have practical constraints which necessitate use of either mechanistic or phenomenological methods, others may have more flexibility and need to consider some key tradeoffs between these approaches.

First, there is a tradeoff between detailed knowledge, or assumptions, regarding the resources that species compete for (mechanistic methods) and the number of pairwise experiments that must be performed (phenomenological methods). Specifically, the phenomenological methods require no assumptions about limiting resources, but the trade-off for this simplicity and generality is that the need for experiments increases exponentially as more species are considered. However, recent papers have shown that the annual plant model can be parameterized from multispecies communities without pairwise experiments (Lanuza et al. 2018, Matías et al. 2018, Zepeda and Martorell 2019), which could make the phenomenological methods more feasible for some situations.

Appendix A   
Figure A2 – graph (c) the measure for alpha\_11 has not quite rendered

Response: We replaced all of our figures with JPEG format images to solve any rendering issues.   
  
Appendix B  
Equation 5 → Equation 7? in main text

Response: Thank you for finding this issue and ambiguity. We revised the supplement text to clearly indicate the appropriate equations in the main text.   
  
Reviewer: 1  
  
Comments to the Author  
I thank the Authors for their very thorough engagement with my earlier comments on the manuscript, which is now much improved. Though my comments were not always followed to the letter, the points of potential disagreement (such as just how much generality is contained in the ND-RFD description), while interesting in their own right, would lead to a nuanced discussion with the the length and complexity of (at least) another paper - thus, I agree with the Authors that this is better left for a different time and place. I am therefore happy with the changes. I just have a handful of minor suggestions for improvement - please see them below.  
  
- l.227-236: I would restructure this section to describe the meaning of eta\_i only after it is introduced in Eq. (6), referring to Godoy & Levine (2014 Ecology) for the details. Right now the text suggests that Eq. (5) will be written in terms of eta\_i and lambda\_i, but eta\_i does not actually appear until Eq. (6). I would rephrase the text before Eq. (5) and move lines 234-236 to after Eq. (6).

Response: We agree that rearranging this text makes it easier to follow. We made the changes specified by the reviewer.   
  
- l.258-262: I assume the Authors are referring to Eqs. (11) and (12) in Bimler et al. (2018 J Ecol) here. If so, there is a bigger problem with those equations than not conforming to Chesson's definitions. Namely, 1) they do not map onto a proper coexistence criterion, and 2) they are not invariant to changing units; i.e., ND and RFD, both of which are unitless quantities in principle, will change their values after rescaling the units of the competition coefficients. Which also implies that the prediction of the equations about whether one gets coexistence can change simply by changing units. (See Spaak & De Laender, bioRxiv, doi: <https://doi.org/10.1101/482703> for details). That said, I don't think this paper is necessarily the place to criticize the approach of Bimler et al. If the Authors agree, then I would simply say here that the inclusion of facilitation in the two-species ND/RFD framework is an unsolved problem to date.

Response: We revised the text to acknowledge this limitation that both reviewers identified. This text is now moved to Section 3.2 (*Limitations of methods for measuring ND and RFD to predict coexistence*).

- l.399-401: There is a tacit and subtle assumption in the derivation of Letten et al. (2017 Ecol Monogr), namely, that the resources are not driven extinct by consumption. I would modify the sentence to read "... for all resource limitation scenarios, provided resources do not go extinct, ..." (or something to that effect).  
  
Response: We are grateful to the reviewer for identifying this ambiguity. We revised these two sentences to make it clear that these calculations only apply to conditions where each species is able to grow as a monoculture (i.e. no washout in the chemostat).

Sincerely,  
Gyuri Barabás