**Reply to the handling editor’s comments**

Original comments appear in straight font, and our answers appear in italic.

Dear Dr Portalier,   
  
The paper has now been reviewed by three reviewers. I agree with Reviewers 2 and 3 in that this manuscript has the potential to be a valuable and interesting contribution, but that substantial revision is required.  
  
Specifically, I agree with Reviewers 1 and 2 in that the manuscript requires quantification of the model fits. While the text and figures are helpful for describing and depicting the general trends, at this stage there is no quantification of how well the modeled parameters match the empirical data which significantly weakens the model validation section of the manuscript.

*We added several elements to evaluate the goodness of fit. First, we computed the root mean square deviation (RMSD) that account for the mean deviation of the predicted and observed data, as recommended in Pineiro et al 2008 (Ecological Modelling). Second, we added a test on the slope and intercept of the regression of observed versus predicted data (i.e., a slope of 1 and an intercept of 0 mean that the model is unbiased) with body size as a cofactor, and the source of data (i.e., the study where the data was originally collected) as a random variable. It appears that the slope and intercept of the regression for speed, attack rate and capture probability do not significantly differ from 1 and 0 respectively, with no significant effect of size nor the source of data. On the other hand, the model fails to accurately predict handling time, which is the only parameter that does not rely on mechanical factor in our model. Results can be found in the main text (l. 252-260 pp. 12-13, l. 297-315 pp. 14-15) and the full details can be found in the supplementary material.*

Please ensure that you provide a complete list of references where you obtained the data for model validation in your Supplementary Material (Line 189-190).   
*We added the full references in the Supplementary material.*

In addition, Reviewer 3 points out a substantial amount of relevant literature that should be considered (and cited) in the revised manuscript. This reviewer further points out that predator foraging strategy (sit-and-wait vs. active foraging) can substantially impact the functional response, which likely has substantial implications for the model presented in this manuscript. For example, depending on predator foraging mode it may be that prey or predator velocity is the main driver of the interaction. Please take some time to review the list of papers provided, and, where appropriate, incorporate some discussion of additional factors not considered in your model that may be relevant.

*We followed suggestion from reviewer 3. We added the relevant literature suggested, and we added a paragraph to discuss the case of sit-and-wait predators.*  
  
Each of the reviewers pointed out that the novelty of this work could be better articulated. Reviewer 3 has specifically asked that you address how this work relates to the previous study Portalier et al (2019), and how the material presented here is novel relative to the processes modeled in that paper.

*We clarified this point.*  
  
Below I have included a few additional comments / questions of my own:  
  
The effects of predator density on a predator’s per capita kill rate are now generally thought to be widespread. This mechanism is mentioned only in pasting towards the end of the manuscript (line 239-241). I agree that one could include other aspects associated to predation such as behavioral features into the model, but the reader is left wondering how this might be done or how it would improve model predictions. Which biological features specifically do you anticipate being important in reconciling the discrepancy between the predicted / observed data you present, and why? My suggestion is that you consider briefly describing how incorporating the biological part (e.g., interference) could help explain discrepancies in observed vs. predicted parameters, and /or be built into this modelling framework.

*As comes out of the manuscript, the first improvement to the model to enact would be to include better mechanism for handling time (especially prey subjugation, and predator satiation). Inclusion of dimensionality, as in Pawar et al (2012) is also among the first improvements to make. We agree that there is the need to include interference between predators. There is certainly a way to do this according to a very similar approach, probably by calculating encounter rates based on the relative speed of the 2 predators, and “capture” and “escape” probabilities as well, and add a time penalty for these encounters in our time-based functional response derivation. However, we felt that the scope and space of this manuscript would not allow to elaborate on this potential development of the theory in a good way. If length limitations are not a stringent constraint, we are happy to include such considerations in the manuscript in a future revision.*  
  
Within the proposed framework the functional response behaves as a Type II response, yet we know that other forms of the functional response better match empirical data sets. Interestingly, there has been some recent work indicating that crustaceans exhibit nearly double the proportion of sigmoidal functional responses (Holling type III) as compared to predatory fishes (Dunn and Hovel 2020 Biol Lett). To what extent could the discrepancy in observed vs. predicted handling times be related to taxonomic differences (e.g., invertebrate / vertebrate)?

*It is possible that anatomic/physiological differences that affect motion play a role (e.g., fish fins do not behave exactly like flagella). However, the goodness of fit analysis did not reveal any significant bias for speed, which is the core of the model, nor for attack rate according to body size. More importantly, the source of data (i.e., the study where the data comes from) does not have a significant effect on the goodness of fit for attack rate nor for handling time. Since these studies were usually done on specific taxa, we can reasonably infer that it is a proof (although indirect) that there was no major difference due to taxonomy. Due to length limitation, we did not develop this point in the main text.*

Moreover, Vucic-Pestic et al (2009) found that variation in predator–prey body-mass ratios could also impact the shape of the functional response (Type II vs. Type III). I think there would be value in briefly discussing if / how your framework might be revised to consider distinct functional response shapes (e.g., by altering assumptions within the underlying model).

*We agree that other types of functional responses should be considered (as reviewer 2 also mentioned). Actually, the model uses an encounter rate model from the literature (Rothschild and Osborn, 1988). This model leads to a type-II functional response. Another encounter rate model would lead to another type of functional response. We addressed this comment in the revised manuscript (l. 285-287 p. 14).*