Reply to reviewer 3's comments

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

EVALUATION
Please list your revision requests for the authors and provide your detailed comments, including highlighting limitations and strengths of the study and evaluating the validity of the results, and data interpretation. If you have additional comments based on Q2 and Q3 you can add them as well.
General comment:
In this manuscript the authors present a model that derives functional response parameters from allometric relationships of predators and prey and physical properties of the surrounding medium. The model is tested against published data from two recent meta-studies and a couple of additional original studies.
I think this is an interesting study but I have a few major concerns and also a couple of minor comments about this manuscript that need to be addressed to improve its quality and comprehensibility. See my comments below. Overall I think this will be a valuable contribution to the field after a solid revision.
General suggestions:
One think that was not really clear to me and which I think is most important for the overall evaluation: how is this new study related to the previous study Portalier et al (2019)? Which parts of the model framework are maintained compared to this study and what part is truly novel here in the new paper? It seems that the consideration of the physical properties of the medium should be the novel part now but some considerations to this respect have been included in the previous study as well, as far as I understand.

The current study actually dos not add to Portalier et al.'s model. Rather, it uses the original equations to derive the parameters of the functional response. The original model computes time expenditure (for searching, capturing and handling) and energetic expenditure. The present model uses only part of the original one to compute time expenditure. We clarified this point (l. 223-229 p. 11).

Moreover, I found that considerable parts of the relevant literature need to be considered (and cited) in the revised manuscript, see detailed examples below.

We added several references, following the reviewer's suggestions.

A minor general point that I see missing from the discussion is the differentiation between foraging modes of predators. There are numerous studies that consider sit-and-wait predators versus actively foraging predators in the context of functional response studies (e.g. Twardochleb et al. 2020). In these either prey or predator velocity are main drivers of interactions. I think this

has important implications for the model presented in your study and the underlying mechanisms need to be discussed here.

We added a paragraph to address this point in the conclusion section (l. 360-367 p. 17). The model assumes that both the predator and its prey move and can detect each other without any interference. This does not hold for sit-and-wait predators. The model can be adapted to compute an encounter rate in the case of a moving prey and a non-moving predator, but behavioral aspects such as hiding are not size-related, and therefore are not included in the model, although it could be a potential way to improve it.

Specific suggestions:
p.2 l.37: I would suggest to cite one of the earlier papers by Holling here, i.e. (Holling 1959)

We added the reference.

p.4 l.68: also see (Pawar et al. 2015)

We added the citation.

p.4 l.68: I suggest to write "or habitat complexity (Barrios ...)" or alternatively "or structural complexity (Barrios ...)"

We added "habitat complexity".

p.4/5 ls.78-91: for the whole paragraph please also consider the publications by Beveridge and colleagues (Beveridge et al. 2010a, b) which seem very relevant in several aspects of this work.

These two papers are relevant indeed. They are now cited in the corresponding paragraph.

p.5 l.98: (Pawar et al. 2019) is not listed in the bibliography

We added the full reference.

p.5 l.99: maybe also consider the very recent publication by Cloyed and colleagues here (Cloyed et al. 2021)

We added the reference.

p.7 l.138: by giving this explicit reference to pelagic organisms do you mean the model does perform worse for all other kinds of interaction types? This is also relevant in terms of the Pawar et al (2012) terminology where pelagic interactions as 3D/3D interactions are different than 2D/3D or 2D/3D interactions. Should be considered.

The model assumes that both predator and prey move within the fluid, and cannot hide from one another. This affects the equation used to calculate encounter rates. This is why the model is well-suited for pelagic or flying organisms. Interactions occurring on the bottom of the system (i.e., benthic or terrestrial systems) violate these. We clarified this point in the corresponding paragraph. Inclusion of dimensionality into Portalier et al.'s model seems the next natural step to undergo within the physics-based approach that we advocate.

p.9 l.176: At least for ectotherms digestion is not independent from the physical property temperature (Rall et al. 2012). As you point out correctly, digestion is an integral part of the mechanisms subsumed in the handling time parameter. Please clarify.

We agree with reviewer that this point needed clarification. We meant that it is independent of the mechanical factors included in the model (i.e., gravity, density, viscosity). We clarify this point in the corresponding paragraph (lines 273-276 p. 13).

p.9 ls.189/190: this seems like instructions from authors to authors themselves. This needs to be cleaned up!

The reviewer is totally right. We removed this part.

p.9 l.190: I see that the references are listed in the data files on GitHub but why not include them in the supplementary materials?

We added the full references in the Supplementary material.

p.11 l.233: please also add (Jeschke 2007)

We added the reference.