Dear editor and reviewers,

We thank the reviewers for taking the time to review our manuscript and providing constructive comments.

Below, we address the reviewers' comments point by point. In the response, black- and blue-colored characters denote reviewers' comments and our responses, respectively.

Response to Reviewer #1

Reviewer [1.1]:
I read the manuscript by Yoshikai et al. with great pleasure and believe that it will be suitable for publication very soon. I thank the authors for addressing my comments and the additional work that they carried out to improve the manuscript. I think the manuscript reads much clearer now and the methods are very clear! However, I have still some suggestions to improve the manuscript and get it ready for publication. My main concern is that the abstract and introduction require framing of the study and presentation of the questions that the manuscript addresses. For example, I think the attention is still focused too much on the role of sedimentation in wetlands. This distracts from the key messages and the novel work that the authors do, so I suggest to streamline the first third of the paper more towards the hydrodynamics (whose representation is by itself very important and therefore represents a significant contribution).

Response [1.1]:
We are grateful to the reviewers for taking the time to review our manuscript again and providing valuable comments.

We agree with the reviewer's suggestion that the manuscript needs framing of the study in the abstract and introduction focusing on the importance of hydrodynamics.

In the revised manuscript, we have reduced the descriptions related to sedimentation in wetlands in the abstract, introduction, and conclusions. Instead, we made it clearer that the focus and contribution of the study is improved modeling of hydrodynamics in *Rhizophora* mangrove forests.

The revision related to this point can be found in the following lines in the marked-up version:

- Abstract: L. 15–20; L21–22; L. 31–33
- Introduction: L. 42–56; L. 80–82; L. 95–96
- Conclusions: L. 550–552

Reviewer [1.2]:
In addition, I suggest to have another read of the text as there are still some ambiguities and repetitions that could be removed to make the manuscript even clearer. For example,

sentences are generally very long and convoluted which makes the text sometimes hard to follow (especially in the conclusions). I included many textual suggestions in the pdf that hopefully are of some use to the authors.

Response [1.2]:

We thank the reviewer for the text suggestions. We have incorporated them in the revised manuscript. We have also carefully read the manuscript again and made some revisions to long or ambiguous sentences (e.g., L. 27–31; L. 236–238; L. 332–339 in the marked-up version).

Reviewer [1.3]:

Another point is the use of the Xie-model. Although I think it is very interesting to compare the presented model to another model, I would be careful with the interpretation of the scenario of the Xie-model, as Xie et al have a spatially and temporally varying stem diameter and density. So, the combined spatial and temporal evolution of the mangrove forest determines the mean hydrodynamics and sediment transport and deposition processes over long time-scales. Here only one scenario with constant diameter and density is tested for two points in time. I think the results can still be presented as is, but I suggest to rename the scenario with a more general term and bring up Xie et al as an example study that uses this type of drag representation.

Response [1.3]:

We agree with the reviewer's suggestion. We have renamed the scenario from "Xie root model" to "Generic root model" throughout the text. Figs. 3, 5, 8, and S6 have also been updated to reflect this change. We have also added the following sentence in L. 334–336 (marked-up version):

> "We use the term "generic" because Xie et al. (2020) used this model to represent root structures of several different mangrove genera including *Rhizophora*."

Reviewer [1.4]:
General comments:
Abstract:
I think the abstract is still very much focussed on the sedimentary processes, which of course are important but here the hydrodynamics are the main focus. I would possibly adjust the text to focus on the fact that we need to better represent the hydrodynamics to in turn better describe the sedimentary processes. For example, the first sentence as it is very general now and could be more streamlined towards mangroves and hydrodynamics instead of sedimentation and transport and sea level rise. I also have several suggestions to make the abstract more concise and less vague in the pdf.

Response [1.4]:

This was addressed in Responses [1.1] and [1.2].

Reviewer [1.5]:
Introduction:
Again, I suggest to focus more on the hydrodynamics and less on the link with sediment and geomorphology (e.g., in lines 71, 79)

Response [1.5]:
This was addressed in Responses [1.1].

Reviewer [1.6]:
line 90: At this point I still was not sure what the differences is between the presented model and the empirical model by Yoshikai et al 2021. The latter is mentioned late in the introduction and it is not entirely clear why both are used. Maybe explain the model upfront and what both scenarios are trying to answer?

Response [1.6]:
We have added an explanation of the empirical model for *Rhizophora* root structures by Yoshikai et al. (2021) in the 5th paragraph (L. 83–88).

We have revised the said sentence as follows:

Previous manuscript:

"Here, we aim to examine the following: (a) how does the new representation of *Rhizophora* mangroves in the hydrodynamic model improve the predictability of flow velocity and turbulence compared to the conventional drag approximation using cylinder arrays or increased bed roughness? (b) how can the new model be effectively applied for an accurate prediction of the flow in *Rhizophora* mangrove forests by incorporation of the *Rhizophora* root model?"

Revised manuscript (L104–109 in the marked-up version):

"Here, we aim to examine the following: (a) how does the consideration of the three-dimensional root structures of *Rhizophora* mangroves in the hydrodynamic model improve the predictability of flow velocity and turbulence compared to the conventional drag approximation using cylinder arrays or increased bed roughness? (b) how can the new model be effectively applied to *Rhizophora* mangrove forests in the field with limited known root parameters?"

Reviewer [1.7]:
Methods:

I believe that a map of the sites would be useful here in the main manuscript. In Figure 2 the names (Bak 1,2, and Fuk) are now not clear and I was not sure where to find the site description. Figure S2 also does not have labels with those names.

Response [1.7]:

We have provided in Fig. R1 of this document the map of the sites.

We have provided the description of the sites in Text S4 in the Supporting Information of the original manuscript, but it was mistakenly referred to as "Text 4" (L. 201 in the marked-up version), and we believe that this caused the confusion of the reviewer.

In the revised manuscript, we included Fig. R1 in the Supporting Information as Fig. S1 in the Supporting Information and revised L. 199–200 (marked-up version) as:

> "We investigated the above assumption using tree census data collected from three sites (Bak1, Bak2, and Fuk; see Fig. S1 and Text S4 in the Supporting Information for the map and description of the sites)."
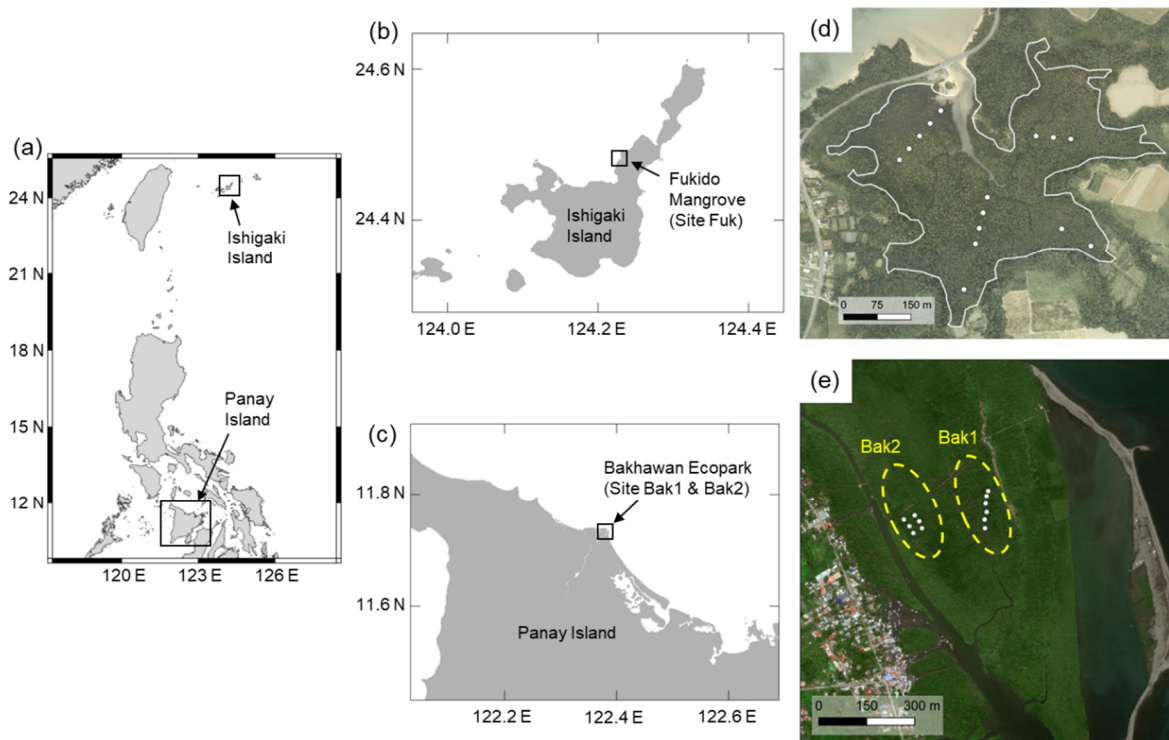

Figure R1. Map of the sites (Bak1, Bak2, and Fuk) indicated in Fig. 2. The white dots in panels "d" and "e" represent the tree census stations from which data are used in Fig. 2. In panel "e", the approximate locations of the 30-year-old (Bak1) and 17-year-old (Bak2) planted stands are also indicated. See Text S4 in the Supporting Information for the description of each site. Shorelines in panel "a–c" are from the Global Self-consistent, Hierarchical, High-resolution Geography (GSHHG) database. The aerial photo in panel "d" is from Asia Air Survey Co. Ltd., Japan, and the satellite image in panel "e" is from Google Earth.

Reviewer [1.8]:

Results:

In the introduction and methods first velocity is presented and then TKE. In the results (paragraphs in lines 317 and 329) it is the other way around. I suggest to swap the two paragraphs to be consistent with the structure.

Response [1.8]:

The said paragraphs already describe the results in the suggested order (velocity and then TKE), thus we think that revision is not necessary here.

We would like to note that the first paragraph describes the results from the *Rh*-model and the second paragraph describe the results from the cylinder model, but in each paragraph, the order of the description is velocity and then TKE.

Reviewer [1.9]:

Conclusions:

Sentences are very long in the conclusions. I also suggest to make the aim of the study clearer in the first sentence.

Response [1.9]:

We have revised the first two sentences in the Conclusions as follows (L. 550–552 in the marked-up version):

> "Modeling flow in *Rhizophora* mangroves has been challenging due to their complex root structures. This manuscript presents a new model to represent the impacts of *Rhizophora* mangroves on flow implemented in the COAWST towards a better understanding of hydrodynamics in mangrove forests."

We have also improved the texts in the Conclusions as mentioned in Response [1.2]. Please see the marked-up revised manuscript for the details of the revision.

Reviewer [1.10]:

Minor comments:

I suggest to revisit the text and make sure it is correct and clear. Below some suggestions of what I think could be rephrased but there are other instances, so please have a thorough look.

Response [1.10]:

This was addressed in Responses [1.1] and [1.2].

Reviewer [1.11]:

line 49: the part with the reference of Nepf et al seems a bit odd.

Response [1.11]:

Please see L.58 –61 in the marked-up version for the revision made to this sentence.

Reviewer [1.12]:

line 95: a very long convoluted sentence that could be rephrased.

Response [1.12]:

The sentence was separated into two as (L. 113–116 in the marked-up version):

> "The vegetation module has been added by Beudin et al. (2017) to account for the drag by vegetation (such as seagrasses and salt marshes) in the momentum equations in ROMS. The equations added by Beudin et al. (2017) are basically in the same form as the cylinder drag model (see Text S1 in the Supporting Information)."

Reviewer [1.13]:

line 97: "We modified the equations introduced by Beudin et al. (2017) to make them suitable for representing the impact of Rhizophora mangroves on flow; these equations are described below. We added a new module in COAWST–Rhizophora root module–that provides the vertical profile of the projected area density of root systems from stem diameter and tree density in each model grid (Fig. 1)." Is the second sentence what you did in the first sentence or did you do two steps here?

Response [1.13]:

These are two steps – the modification of the drag and turbulence model (described in Sections 2.1.1 and 2.1.2), and the incorporation of the empirical *Rhizophora* root model to the modified drag and turbulence model (described in Section 2.1.3).

In the revised manuscript, we have revised the said sentence as follows (L. 116–120 in the marked-up version):

> "We modified these equations to make them suitable for representing the impact of *Rhizophora* mangroves on flow; these equations are described below (Sections 2.1.1 and 2.1.2). We added a new module in COAWST–the *Rhizophora* root module–that provides the vertical profile of the projected area density of root systems from stem diameter and tree density in each model grid (Fig. 1; Section 2.1.3)."

Reviewer [1.14]:

line 110: "The Reynolds number …" – I am not sure if this was found in the publication (Shan et al) or in your work. Please clarify.

Response [1.14]:

It was found in previous studies. We have revised the said sentence as follows (L. 129–131 in the marked-up version):

> "The Reynolds number (Re) defined using the root diameter as length scale <u>could be higher than the value ensuring</u> fully turbulent structures of root-generated wakes ($Re > 120$; Shan et al., 2019) even for weak currents (~1 cm s^{-1}) that could diminish the dependence of drag coefficient (C_D) on Re."

Reviewer [1.15]:
line 301: " We inputted …" Could this sentence be combined with the previous one? Now it seems like the same thing is said twice.

Response [1.15]:
We have combined the said sentence with the previous one as suggested (L. 325–327 in the marked-up version):

> "Among these, the proposed framework (Fig. 1) was used for the case Rh model using the modeled a_{root} (the *Rhizophora* root module provided the a_{root} in the simulation) <u>with input parameters of measured mean stem diameter ($D_{stem,ave}$) and tree density (n_{tree}).</u>"

Reviewer [1.16]:
line 450: very long sentence. Please separate into two or three sentences.

Response [1.16]:
We have separated the sentence into three as suggested. Please see L. 484–489 in the marked-up version for the revision.

Reviewer [1.17]:
line 463: again very long, please separate.

Response [1.17]:
We have separated the sentence into two as suggested. Please see L. 497–500 in the marked-up version for the revision.

Reviewer [1.18]:
line 485: I think you can leave out the first sentence. As a reader I would not expect you to do more work so discussing what can be done in the future suffices here.

Response [1.18]:
We have removed the said sentence as suggested (L. 521–522 in the marked-up version).

Reviewer [1.19]:
line 504: I would acknowledge here studies that use dynamic vegetation models (which you cited in the foregoing line)

Response [1.19]:

We have added the reference as "(e.g., Xie et al., 2020)". Please see L. 542 in the marked-up version.

Response to Reviewer #2

Reviewer [2.1]:
General Comments
This manuscript presents a new approach to modeling the flow of water within Rhizophora mangroves. The key improvements to the COAWST vegetation package are: (1) allowing the vertical varying projected area density (frontal area per unit plan area), (2) using the root and stem length-scales in the turbulence dissipation terms, (3) implementing the Rhizophora module which can calculate projected area density from easily obtainable field measurements. These improvements allow the field to move beyond the conventional cylinder assumption, and are generally applicable to all hydrodynamically rough environments which aren't well described by cylinders.

I like the approach of this paper. The changes the authors have made have increased the clarity, and strength of this paper.

Response [2.1]:
We are grateful to the reviewers for taking the time to review our manuscript again and providing valuable comments.

Reviewer [2.2]:
The runs that are labeled as increased bed roughness (z0=0.02) should be considered with care because the z0 value used in those runs are an order of magnitude less than the authors estimate of the actual (without numerical limitations) increased bed roughness (z0=0.22). I think some text in the manuscript describing z0=0.2 as the maximum amount of bed roughness that logarithmic drag can represent in the model due to numeric limitation would be good. The authors thoroughly explain the numerical limitation in the supplemental information, I believe a few words in the manuscript would make it very clear to readers that z0=0.02 isn't the authors estimate of the enhanced z0 value. Alternatively, I believe that the requirement in COAWST that z0 < zbottom, is only true when using a logarithmic drag law. I have never attempted this, but I think it would be possible to use the equation (R8) relating the manning coefficient and C_{bed,mean} to arrive at a drag coefficient that can be input to COAWST using a quadratic drag law, getting around the z0 < zbottom limitation. Either additional text detailing the numerical limitations of and enhanced z0 value or a quadratic drag law approach would be sufficient in addressing this.

Response [2.2]:
We agree with the reviewer that some additional text describing the numerical limitation explains clearly the reason of choice for the value for z_0.

We have added the following sentence describing the numerical limitation in L. 342–344 (marked-up version) as suggested:

> "We note that the z_0 value equivalent to Manning's coefficient of 0.14 at 0.5 m water depth is z_0 = 0.22 m, but we were able to increase the value up to 0.02 m

due to the numerical limitation of the logarithmic velocity profile assumption implemented in the COAWST (Eq. (S13))."