Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-229

Anonymous Referee #1

Referee comment on "Reconsideration of wind stress, wind waves, and turbulence in simulating wind-driven currents of shallow lakes in the Wave and Current Coupled Model (WCCM) version 1.0" by Tingfeng Wu et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-229-RC1, 2021

General comments

The manuscript considers the effect of parameterization of wind forcing to wind generated waves in a shallow lake in China. The authors have developed a new method for estimating and modelling wind-driven current that is specifically suited for shallow lakes. A measurement campaign of wind and flow conditions was conducted to support the development of a wave-current coupled model.

Overall, the science in the manuscript is described well and the subject fits within the journal's scope. The theory and implementation into the model are clearly written for the most part and relevant material is included in the article and appendices. The readability and structure of the paper are satisfactory. This paper gives interesting insight to 2-D and 3-D modelling of shallow inland lakes where the atmosphere-water interface parameterizations can behave differently to the ones used in general hydrodynamic ocean or coastal models. Especially the significance of turbulence parameterization, wind drag coefficient and the wave-flow model coupling are of benefit to shallow lake modelling development.

After minor to medium improvements to clarify some points and make the paper more readable, I can recommend the paper to be accepted for publication. I also suggest making the source code fully open (without the need to ask for access) as is the standard these days.

Specific comments

- In the abstract at row 19 you say "Comparing with other model…". This should be rephrased to include minimum relevant information about what you are comparing to. E.g. "Compared with a reference model…"
- In row 33 you refer to Sterner et al. 2017 when discussing 3-D ocean model applicability to shallow lakes. I don't think this reference fits here. Please remove this or explain the relevance. On the other hand, the second reference (LükÅ□ et al. 2020) is spot-on.
- At chapter 2.1, provide references for the weather conditions at Lake Taihu.
- At chapter 2.2, provide the height at which the wind measurements were taken.
- At the introduction to Chapter 3, provide some references and examples of 3-D model – SWAN couplings that have already been done and why they are not sufficient for this work. In Chapter 3.1 provide an justification why an LCM is developed from the ground up instead of modifying one of the existing, well tested and freely available open source 3-D ocean coastal/ocean models.
- At row 199 the reference to Koue 2018 is odd at this point. How is this relevant to measuring the performance of WCCM?
- Also at row 200 the reference to Carvalho et al. 2012 seems out of place. Carvalho's paper doesn't mention MAE_{UVD}.
- Chapter 4.1. You refer to this chapter (rows 124 and 181) for more information about the calibration of the model and deriving the wind drag coefficient. However, an explanation about the calibration process is missing. Please add a section describing clearly how the observation data was used to calibrate the model and how the wind drag coefficients were derived based on calibration and the observations.
- Move the EFDC mentions at rows 179-180 to the chapter 3.5.2 to avoid forward-references.
- At row 241 you say that the measured flow speeds were lowest at the surface and highest at the bottom and it seems so also from Fig.7. Also in 2018 (row 262) the measured speed at bottom is highest. In simulations (Figs 6,7) the simulated surface speeds are generally higher than bottom speeds. Discuss why this is so.
- At row 293 (and 345), you say that "WCCM can accurately simulate the wind-driven currents…" and later at row 352 "…correlation between simulated and measured current speed remains low…". Which one is it? I agree that a) according to the data there is a clear improvement over a reference model and b) correlation with flow speeds can be low. Please be more elaborate about which part of WCCM results is accurate and which parts still need work.
- Chapter 5.1, row 299: "…, considering the discontinuity of changing trend and directionality of wind momentum transmission, …" is hard to understand in the middle of the sentence. Please rephrase for more clarity. Almost same sentence appears at row 302. The whole chapter would benefit from rewriting with more clear language.
- At rows 325 to 333, you compare the current fields of WCCM and EFDC. Which one (or neither) produces similar vortices as is observed (if there is observations)? Explain which model fits the reality better qualitatively (not just with current speeds etc).
- Discuss is the model resolution sufficient for this kind of simulation. 1 km x 1 km seems a bit coarse for this.
- What were the blanking distances/dead areas of the ADP measurements? If they are significant, please discuss if it affects the reported flow speeds and therefore the comparisons to simulations. Do you compare the same height layers from model and ADP?

Technical corrections

typo --> suggested correction
• row 28: Naiver-Stokes --> Navier-Stokes
• row 29: "…and solved the equations using…"
• row 41: discontinuity --> discontinuous
• rows 56-57. Sentence here is a bit repeating compared to the previous sentence and unclearly said, please rephrase.
• row 72: '…lakebed slope of 19.7"…': should probably be in degrees °
• row 73: southeast should be capitalized at the start of a sentence
• row 85: remove extraneous mention of (LCWS) after …USA)
• row 135: firstly --> first
• row 176: by 0 m s^{-1} --> to 0 m s^{-1}
• row 220: explain the parameter w_s
• row 308: logistic curve: Should be logarithmic curve?
• Tables 2-5: Consistently use upper or lower case for all p in tables 2-5.
• Please include LCWS location in the pictures in Figs. 7, 11.