Comment on gmd-2021-160
Anonymous Referee #2

Referee comment on "A Moist Quasi-Geostrophic Coupled Model: MQ-GCM2.0" by Sergey Kravtsov et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-160-RC2, 2022

I agree with the review comments from the first reviewer. As the first reviewer said, this paper is very well written and should be published in Geoscienic Model Development. This paper also shows significant improvement over the first version of Q-GCM model. Although I did not check the equations carefully, the authors included the moisture dynamics in this new version and made it a promising tool for studying air–sea interactions. The authors also gave a summary of the differences between the old and new version at the end of this paper. I would thank the authors for contributing this great work to the community. In my opinion, there are a few very minor concerns before publishing this manuscript:

1. In Line 125, the authors said this model has n=3 layers in oceanic and atmospheric modules, but it looks to me that the atmospheric model does not only have 3 layers. Why do equations 1 and 2 only apply to the 1st and 3rd layer in the ocean model?

2. In equations 18 and 31, the authors should explain the biharmonic term (Nabla^4). Is this term added to ensure the numerical stability or to resolve the physics?

3. Line 440 is confusing to me. It seems that the authors run "control", "partially coupled" and "fully coupled" for both dry and moist models. But this paragraph is very confusing while I was reading. Why are you running three simulations for both dry and moist models? It also seems to me that the control run does not go for 130 years (as mentioned in the first sentence of this paragraph).

4. I would recommend the authors adding a few sentences introducing the validation/verification test in Section 5. Is it an idealized regional model or a realistic model? How is the boundary condition? How can this model validate the implementations
in the model?