Reply to Referee #2:

Following reviewers' comments, Figs. 6 and 11 are added for the revision, and Figs. 4, 8 and Table 2 are modified. Suggestions by the reviewer are included as below. The page and line numbers in parentheses indicate the revised places in the new manuscript.

Synopsis

1. *As a minor point, a comment should be added after equation 10 to clarify this intricacy, even though it becomes clear later in the paper. More broadly, I would like to comment on ramifications of equation 10, as an example of the difficulties posed by assuming superposition.*
   A sentence is added to the paragraph about Eqs. (7)-(10): "There are some difficulties posed by the decomposition of the variables, especially $\boldsymbol{\tau}^{\mathrm{btm}}$, and the decomposition is discussed in more detail later." (Pg. 8, Lines 16-18)
2. *However due to the approximations used, it is unclear how the magnitude of the bottom stress, affected by sqrt(U(barotropic)+U(equilibrium tide)) in each friction velocity term, may affect the solution. Greater clarity could be brought to the paper by investigating this further. It is unclear that a 40-day integration is sufficient for the purpose.*
   Following the comment, we executed an additional experiment by replacing the coefficient part of the bottom friction |U(barotropic)+U(equilibrium tide)| by |U(equilibrium tide)|. In one year integration, the tidal heights were almost identical to case TIDE, and the changes were less than 1%. Including the additional case and the reviewer's comment, the explanation about separation of the bottom friction is enriched. (Pg. 11, Lines 1-8)
3. *a more compelling case to support the suggested explicit tidal forcing approach could exist if longer integrations were presented. The work, as it stands, seems not quite complete, and a dearth of time evolution analysis is a weakness of the paper.*
   We continued the experiment of TIDE for one year, and analyzed time series of SSH and barotropic kinetic energy to verify that the model with our tidal scheme runs stably. The result is added to Sect. 3.1. (Pg. 19, Line 21 - Pg.20, Line 7, and Fig. 6)
4. *The problem of barotropic to baroclinic conversion should be better addressed.*
   Since it is difficult to obtain the conversion directly from $\overline{w'\rho'}$, we indirectly guessed it by estimating the energy dissipation D given by the barotropic tidal energy balance. As a result, D is found to be large over rough topographies such as the Izu-Ogasawara Ridge, the Hawaiian Ridge and the Central Atlantic Ridge. This likely reflects barotropic to baroclinic conversion resulting from excitement of internal tides over rough topographies. This new analysis is added to Sect. 3.2. (Pg. 21, Para. 1 and Fig. 8b)

Technical Points noted during this review

The manuscript is revised following all the comments. We really appreciate the advices.