July 29, 1996

Prof. Peter Cornillon
Editor, JFR Oceans
Graduate School of Oceanography
Narragansett Bay Campus
University of Rhode Island
Narragansett, RI 02882

RE: Accuracy Assessment of Recent Ocean Tide Models, Shum et al.
JGR-Oceans, PCC-0137

Dear Professor Cornillon:

Please find enclosed three copies of the revised manuscript of the above referenced paper. We have considered the Reviewers' comment and some of the responses are reflected in the revised manuscript. The following provides a detailed description of the response to the Reviewers:

Response to Reviewer 1:

We disagree with the Reviewer's opinion that the subject of the paper, accuracy assessment of tide models, is inappropriate and that it should be regarded as "comparison of relative performance of models". Data (TOPEX/POSEIDON altimeter and tide gauge data), and different techniques (harmonic analysis using both gauge data, and crossovers) were used to assess the accuracy of the models. In addition, a number of other independent tests (orbit computation, Earth rotation, tidal frictions, etc.) were used, and it is evident to us that this paper is truly evaluating the accuracy of the current tide models. The following is a list of specific response to the Reviewer's comment:

(1) As indicated in the above paragraph, we disagree that the title of the paper should be changed to downplay the significance of results of the paper, which provide an accuracy assessment of the current state of the tide models.

(2) The interdisciplinary applications of tide models, i.e., Earth rotation, tidal dissipation, etc., are instead another independent test to assess the accuracy of the tide models.

(3) The reference, Molines et al. [1994] was added, and the suggested revision applied.

(4) The Le Provost et al. [1995] is itself a review paper, other references on assimilated tide models are not needed.

(5) The tides are immediately apparent (to be used to reduce altimeter measurements to sea surface heights), however, the the use of Geosat altimeter data to observe Rossby waves). The sentences have been restructured to reflect these points.

(6) Same response as (1).

(7) Point (ii) has been revised to reflect the unprecedented collaborative effort between a large number of tidal experts, which is a good reason for the paper. Point (iii) is also a good reason for the paper, may be not viewed by JGR as such, but it is a reason nevertheless.

(8) The response has been indicated in the introductory paragraph above. A number of geophysical tests were conducted by the authors, not merely just quoting existing studies.

(9) Additional descriptions of the models have been used in the paragraphs discussing individual model, to include many of the Reviewers' suggestions. The suggestion to include all these information in Table 1 will probably make the Table much larger and wordy.

(10) The collective terms, JGM-2 or JGM-3 orbits, contain more than just gravity model and (background) tide model differences. The improvement of orbital parameterization, nonconservative models, reference frame, and others all contributed to the overly simplified term that implied "JGM-3 orbits". The current references quoted [POD, 1994; Marshall et al., 1995], which represent a detailed discussion of the above improvement. The Bettadpur and Eanes [1994], and Desai and Wahr [1995] references are relevant, however, they only relect one part of the T/P orbit improvement. The paragraph has been revised to reflect the above points.

(11) We agree that the sentence is misleading to imply that the orbit errors (difference between JGM-2 and JGM-3 orbits) are smaller than the other differences in the tide model. The sentence is revised.

(12) We thought that this paragraph helped better define the terminologies used in the paper.

(13) As indicated in (9), the suggestions to provide additional descriptions of each model have been implemented as much as the information is available. Additional references which become available after the submission of the manuscript have been added.

(14) GSFC94A model is an example of assimilated models, but not SR95. The former model is added to the sentence.

(15) Descriptions of long period tides in particular models have been augmented based on the available information.

(16) We are not aware of any more recent models except some of the revised models, available to us, to specifically answer this comment.

(17) and (18) Analyses in the paper have shown that Proudman function approach provides smoothing to the solution, significantly more than an entirely empirical model (e.g., DW95) based on filtering techniques without any external information (i.e., bathymetry). It is also clear that the strength of using hydrodynamic models (e.g., FES94) as a priori models for empirical solutions significantly helped the smoothness of the model. These

points are emphasized in the paper.

(19) The figure is necessary, and it will be made smaller.

(20) See (16). Newer versions of the original models are indeed used to update some of the analysis results.

(21) I thought Figure 4 provides a more detailed analysis as opposed to Table 3 which provides only an rms number of each constituent.

(22) Individual crossover residuals, i.e., for T/P cycles 90-95 (represents data not used in CSR3.0 model), indicate the same conclusion that it has the lowest crossover residual rms. This point has been included in the discussion. Overall, the best data (crossovers and altimeter data for harmonic analysis) are used for the test, which should be the case. The discussions of the revised models have been updated in the Table and the text.

(23) I disagree. The term "valid" simply means that the model has a predicted tidal height value at that location, in the sense of computer language. There is no implication that the term "valid", means model accuracy. I agree that "available" is an accepted English term, since we have a real English as one of the authors, I opt to retain the term "valid" in this specific discussion.

(24) No. Tables 4 and 5 are not the same. Results for C&R 91 models can be provided also in Table 4, however, Table 5 represents results for both historic models (CR91 and Schwiderski), and should stand on its own.

(25) The along-track test, as other tests, was used to detect possible problems in the tide models, and it may potentially be an alternative way to compute tide solutions. The test was not used in the decision to select the final models.

(26) Results for DW95.1 have been added to the Tables.

(27) Figure 6 has been eliminated.

(28) We agree that may be overlaying of the bottom of Figure 7 may be a better choice of figures. However, this is not a scientific issue and we opt to retain the original set of Figure 7.

(29) The sentence has been modified.

(30) Figure 8 will be reduced.

(31) The sentences have been revised to be more clear.

(32) Some wordings in the text have been modified to respond to the Reviewer's comment. However, we disagree that this section should be removed. One reason is because, again, the tide loading test represents one of the tests which are independent and suffices to qualify to provide accuracy assessments of some of the global tide models.

(33) Same as (1).

(34) Same as (1), (16), and (22).

(35) See also (25).

(36) The long period tides (M_m and M_f) are small, and the solutions (i.e., DW95.0/.1) do not necessarily indicate that they are better than equilibrium values. They are, along with other long period tides, research topics for years to come.

(37) See Section "Global Ocean Tide Models".

(38) The point of Figure 14 is to point out the consistency of the current tide solution (i.e., CSR3.0 and others) with the predicted atmospheric tides. If Schwiderski S_2 tide is used, the "closure" is much worse. This test, again represents an independent accuracy assessment of current tide models in the long wavelength band for S_2. The term "astronomical tides" are used (for the lack of a better term) similar to the term "atmospheric tides", as the consequence of all tides will be gravitational.

(39) Same as (1) and (2) and (22).

Response to Reviewer 2:

With regard to the first point, the GSFC94A model has a spatial resolution of 2 degrees, while the other tested models are 1 degree or finer. This reason forms the basis for the statement. The term "valid" has been changed to hopefully reflect that it does not necessarily imply model accuracy. I agree, however, that the statement is being unfair to imply that GSFC94A model is worse (less accurate) than other models in the shallow ocean. Actually, no model performs well in shallow water. The sentence has been revised.

Reviewer 2 is mistaken concerning his second major item. In a very early version of the model, the basis functions used were indeed "approximations" to Proudman functions. However, this was changed later, and the model studied in our paper used genuine Proudman functions. The original wording should stand.

Please contact me if further information is needed.

Sincerely,

C. K. Shum
Research Scientist

CKS/kjm