

Interactive
Comment

Interactive comment on “Altimetric sampling and mapping procedures induce spatial and temporal aliasing of the signal – characteristics of these aliasing effects in the Mediterranean Sea” by M.-I. Pujol et al.

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Interactive comment on "Altimetric sampling and mapping procedures induce spatial and temporal aliasing of the signal - characteristics of these aliasing effects in the Mediterranean Sea" By M.-I. Pujol et al.

Response to anonymous Referee #1

Specific comments:

1. The title is very long. Practically it is composed of two full sentences. I think it should be shorter.

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Author : The title was changed for a shorted form.

2. A large part of the abstract describes the technical aspects in detail. I think that this part of the abstract should be condensed in order to highlight the main findings of the study.

Author : The abstract was modified removing part of the technical aspects.

3. Generally I think that the manuscript contains a very detailed description of all technical aspects. The authors should try to write these descriptions in a shorter form. Maybe tables describing different experiments can be used instead of text. Now large parts of the manuscript have a form of a detailed technical report.

Author : The manuscript was revised in order to condense technical aspect and insert tables describing the different experiments. In the revised version, part 3.1 and 5 were made shorter as also suggested in comment 8.

4. Page 574, line 2 and page 581, line 16: I think that delayed sea level oscillations due to atmospheric forcing in a semi-enclosed sea are well known in oceanography even before satellites. This phenomenon is described and explained in many textbooks. Some previous studies in the Mediterranean are referenced Le Traon and Guzelin (1997).

Author : References to previous studies (Candela et al., 1989 and Candela, 1991) were added.

5. Page 580, lines 9-14: I think that the statement that there is a high variability of atmospheric forcing in specific areas should be supported with some evidence or by references. I also do not understand why the barotropic signals are favoured by shallow bathymetry? I guess the authors wanted to say that the amplitude of sea level oscillations is larger due to the shallow bathymetry, or that their specific method to estimate the barotropic variability shows high values in those areas.

Author : The formulation of the author was not clear. It was replaced by a new formula-

tion. Of course, the referee correctly interpreted the message that the author wanted to send on : the amplitude of sea level oscillations is larger due to the shallow bathymetry.

6. Page 581, line 13: I think that the paper of Fukumori et al. (2007) links winds at Gibraltar to much shorter scales than intra-annual.

Author : The author used "intra-annual"; directly quoting Fukumori et al. to indicate variability of order 10 days and longer.

7. Page 594, lines 1 and 26, and Page 604, line 23: What is the non-barotropic component of the surface variability?

Author : The term "non-barotropic" was changed for "baroclinic"

8. Section 5: In this section the authors combine two data sets. It is assumed that one data set (observations) estimates well the low frequency signal, and the other (the model) the high frequency signal. Assuming that the model is biased, the authors correct the model outputs by observations using an ad hoc method of statically merging two data sets. As expected the comparison with independent in situ observations shows that the new data set is more accurate. I think that this section describes in too many details all possible experiments that have been performed in order to empirically tune parameters. I think that the section should be much shorter and the detailed description of all steps which arbitrary generated merging parameters should be removed. The authors should also avoid a detailed description of the parameter tuning in sections 3 and 4.

Author : A large part of the technical aspects and description of the signals combined were reduced as also suggested in comment 4. The results of the different combinations tested were also cut down. In particular, section "5.2.2 Impact of the HF MOG2D signal component"; was removed. The results of these experiments were resumed in section 5.1. However, the author voluntary let the different results obtained in section 5.2.3 were different filters were applied to altimetric data. Actually, these results

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contribute to highlight the difficulty of merging complementary signals, pointing out the local sensitivity of the parameters used.

9. Comparison to in situ observations: I think that instead of the table it would be useful to see the position of stations on the map of the Mediterranean. I suspect that most of them are grouped in several isolated geographical regions and have a correlated sea level variability. Therefore, the results for different stations shown in tables 2-4 can be strongly spatially correlated.

Author : A figure of the positions of the stations was added.

Interactive comment on Ocean Sci. Discuss., 4, 571, 2007.

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