Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-328

Anonymous Referee #2

Referee comment on "IBI-CCS: a regional high-resolution model to simulate sea level in western Europe" by Alisée A. Chaigneau et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-328-RC2, 2021

The manuscript entitled "IBI-CCS: a regional high-resolution model to evaluate western Europe sea-level changes" by Chaigneau et al.. deals with the sea level projection for the north-eastern Atlantic region via a dynamical downscaling of a set of CMIP6, single model, simulations. The scenarios considered are the SSP5-8.5 and SSP1-2.6 The manuscript, however, is mainly focused on assessing the impact on the modelled regional sea-level change of (i) increased horizontal resolution (ii) a complete representation of coastal processes, (iii) application of a bias correction to the driving GCM.

Main concerns

The title of the manuscript is misleading. This manuscript mainly focuses on assessing the impact on the modelled regional sea-level change of an increased horizontal resolution of the DD, a more complete representation of coastal processes, and applying a bias correction to the driving GCM. Using only a single GCM downscale is not sufficient to provide a reliable sea-level change estimation. I suggest changing the title of the manuscript to reflect the primary goal of the work.

Paragraph 2.1.2 Regional ocean model IBI-CCS: Tide in the regional configuration is one of the main processes driving SL change in coastal areas. A specific validation of the tides should be included in the manuscript.A reference to a peer-reviewed paper in which the tides have been validated is also enough. Moreover, the authors claim that "Tides are included in the model by calculating the astronomical tidal potential and the tidal harmonic forcing as …". Here the author should be more clear on the way they applied the tidal forcing in the regional model.

Paragraph 2.3.2. In line 308, the authors state that "The GCM GMTSLR term stored in the variable "zostoga" is thus added a posteriori to the RCM modelled SL". My deep concern is

how the authors used this variable in the final SL computation (Figure 9). The global model used in this study is affected by strong temperature (and salinity) drift due to its relatively short spin-up (250 yrs). In particular, the temperature drift affects the local thermosteric component of the SL, and so the global mean thermosteric component (zostoga). Maybe I am wrong, but it seems that the authors used the original zostoga variable provided by the global simulation without any correction. I suggest the authors to indicate in the manuscript how they treated zostoga before using it in the final SL computation.

Line 172: I suspect that mixing due to internal tides is overestimated. So, I suggest to provide more details about the de Lavergne scheme and a more robust justification about its use in the regional model.

Line 249: Sea Surface Height tuning in the Mediterranean Sea. The authors claim that **"**GLORYS2V4 has a mean SSH bias of approximately -0.1 m in the Mediterranean Sea in comparison to the Mean Dynamic Topography observations from CNES- CLS-18". It would be good to show, at least in the supplementary material, the horizontal map showing the differences between GLORYS2V4 and CNES- CLS-18 over the entire domain. The -0.1 value used as a correction seems to result from a tuning exercise. The authors should provide more details on the applied correction if this is the case. Also, looking at Figure 3, it appears that in all simulations (including IBI-CCS_corr) there is a bias on the eastern boundary in the Mediterranean Sea. Do the authors have a valid justification for the bias in IBI-CCS_corr?

As a general comment, the manuscript needs revision for language and grammar.

Minor issues

Line 13: Please, include the name of the model "(Iberian-Biscay-Ireland Climate Change Scenarios)"

Line 63: Please, provide the physical definition for "dynamic sea level".

Line 75: The authors claim that "The DD method can be used to overcome this problem by applying corrections to the GCM outputs before using them as forcing when performing a DD". Actually the bias in GCM simulations can be strongly reduced using bias correction. So, I do not agree in 'the DD methods'. May be this sentence need to be revised or deleted.

Line 114: Paragraph 2.1.1. It would be good to add a specific subparagraph in which is indicated how the SSH is modelled in both global models.

Line 115: I did not find any specific paper in the literature dedicated to the validation of CNRM-CM6-1-HR. Am I wrong? In case you could not provide any reference to the validation of CNRM-CM6-1-HR it would be necessary

to justify the use of this model simulation as driver for the DD.

Line 122: The following sentence is not clear to me: "A polynomial representation of the equation of state (TEOS-10, Roquet et al., 2015) is used but the temperature and salinity outputs are converted into the in-situ temperature and practical salinity needed by the RCM. ". Please, rewrite this sentence.

Line 128: please, insert citation for OASIS-MCT

Line 129 please, insert a citation for ARPEGE-Climat 6.3

Line 130: I would suggest to indicate the exact number of simulations used.

Line 138: In this paragraph should be indicated the model resolution of the regional model

Line 151: I don't think it is relevant to indicate the 1/36° version in the manuscript. It is not used for validation. So, I suggest to remove it.

Line 152: Please, provide the explicit link in the references.

Line 230: Please, provide the link in the references

Line 240: The paragraph is not enough clear. Please, rewrite this paragraph.