08.02.2022

Answer RC2:

The authors wish to thank the anonymous reviewer for his/her comments, questions and remarks that greatly helped us improving the quality of the paper. Please find below the point-by-point answers to your review in blue in the supplement to this comment.

Best regards,

The authors.

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.

We thank the reviewer for this comment highlighting that the title was not reflecting the aim of our work. The purpose of the paper is indeed not to provide a reliable sea-level change estimation not even to characterize uncertainties associated with the simulations. The study is meant to present a regional ocean model that will be used for analyzing the sensitivity of sea level changes, particularly extreme sea level changes, to methodological choices and representation of processes. In the current study, the configuration is presented along with its evaluation and the added value of the regional vs global model. It is from this perspective that we assess the impact of the dynamical downscaling and bias corrections on simulations of sea level changes. As the aim of the manuscript is to present the ocean regional model even more than to evaluate it, the title has been changed to "IBI-CCS: a regional high-resolution model to simulate sea level in Western Europe". The methodology presented in this paper could subsequently be applied to produce an ensemble of simulations using different CMIP6 global models as parent models to provide projections of sea level changes and related uncertainties.

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.

A sentence has been added in the manuscript: "Tides have been validated in section 3.1 of Maraldi et al., 2013 with a 1/36° configuration.". In Figure RC1, we provide the M2 tidal amplitude for FES2014 (a) and IBI-CCS_corr (b) and the difference FES2014 minus IBI-CCS_corr (c). In general, the regional model is close to the FES2004 solution, except north of the Irish Sea and in the German Bight. The figure has been added to the Supplementary Materials.

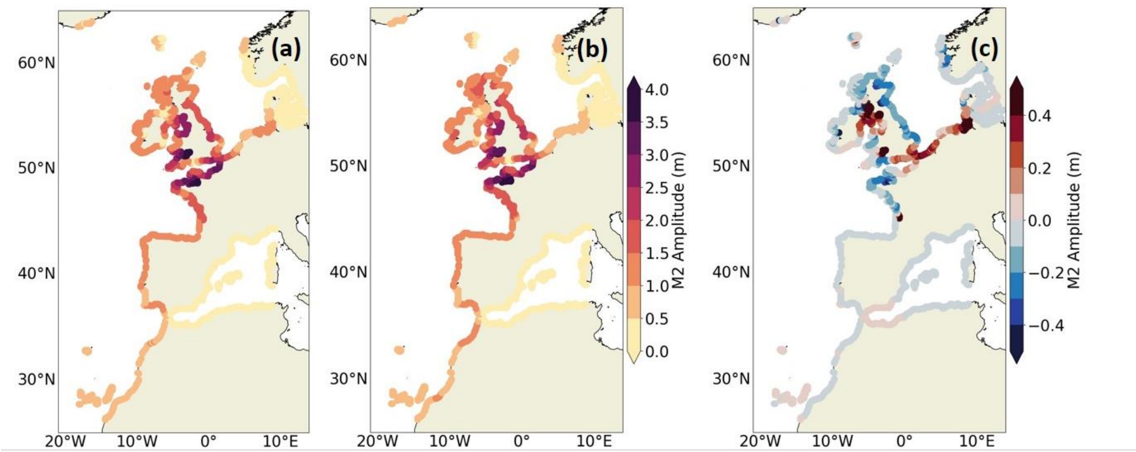

Figure RC1: M2 tidal amplitude for FES2014 (a) and IBI-CCS_corr (1993-2014) (b) and the difference FES2014 minus IBI-CCS_corr (c) for the coastal points of the zone.

Also, the paragraph explaining how the tidal forcing is applied in the regional model has been revised: "Tides are included in the model by calculating the astronomical tidal potential and the tidal harmonic forcing. SSH and barotropic velocities tidal components are added through the open boundaries as the sum of 11 components provided by FES2004 (Lyard et al., 2006) and TPXO7.1 (Egbert and Erofeeva, 2002): diurnal components (K1, O1, P1 and Q1), semi-diurnal constituents (M2, S2, N2 and K2), long-period tides (Mf and Mm) and a nonlinear component M4."

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.

Thank you for pointing this omission. The original variable zostoga has not been used for the reasons you mentioned. The variable is indeed corrected from the drift based on the same method as the corrections applied to the open boundary conditions. The drift is estimated by a linear fit of the full time series of the pre-industrial control simulation (Gupta et al., 2013). Then, the linear fit is subtracted to the corresponding historical simulation and projections at each time step. This method has been used in recent studies for the variable "zostoga" for example in Hermans et al., 2021 and Fox-Kemper et al., 2021 (Supp Mat Chap 9, section 9.SM.4.2). In Figure RC2, we provide the original and corrected variable "zostoga" (monthly outputs) for the two scenarios:

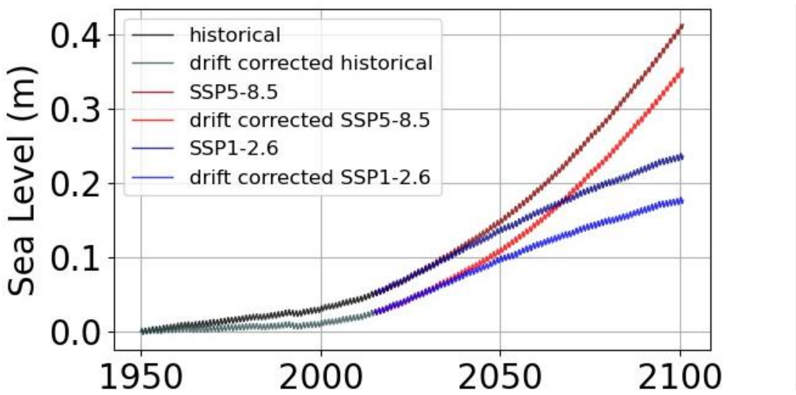

Figure RC2: Original and drift corrected global mean thermosteric sea level over the 1950-2100 period for the two scenarios SSP5-8.5 and SSP1-2.6.

The information has been added in:

- section 2.2.1 "IBI-CCS_corr simulation": "The drift is also removed from the global mean thermosteric sea level (variable "zostoga") of section 2.3.2 using the same method.".
- section 2.3.2 "Thermal expansion": "The GCM GMTSL term stored in the variable "zostoga" is thus added a posteriori to the RCM modeled SL after having removed the drift (section 2.2.1)."

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.

Despite the regional ocean model explicitly resolves tides, the entire spectrum of internal waves is not generated especially at a 1/12° resolution. Actually, at this resolution, only the most energetic modes (mode 1 and 2) of the internal tides are resolved. In the IBI zone, the mode 1 seems to be dominant internal tide mode (Vic et al., 2019) so in our case we might generate a large part of the locally generated internal tide spectrum. However, the model doesn't account for low modes propagating into the IBI region at its boundaries, nor does it have the required physics to dissipate explicitly resolved internal tides correctly. As stated in Melet et al., 2022, "even ocean general circulation models with explicit tides typically do not resolve the generation of high-mode internal tides, scattering of low-mode energy into higher modes, and various processes leading to internal-tide energy dissipation, so parameterizations are still required to get realistic internal tides and dissipation (e.g Arbic et al., 2010; Ansong et al., 2017)." For these reasons, we have applied the de Lavergne et al., 2020 parametrization which is at the state-of-the-art in terms of representation of internal tide induced mixing processes (hence allowing for a molecular background diapycnal mixing).

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?

In Figure RC3, we provide the map showing the bias (c) between GLORYS2V4 (1993-2014) (b) and CNES-CLS-18 (1993-2012) (a) over the entire domain. The bias in the Mediterranean Sea is approximately of 10cm. The figure has been added to the supplementary materials as well. The bias in the Mediterranean Sea of approximately of +10 cm is due to the assimilation of two different sea level anomaly databases in GLORYS2V4: a global one for the Atlantic part and a Mediterranean one. These two databases were not aligned with each other.

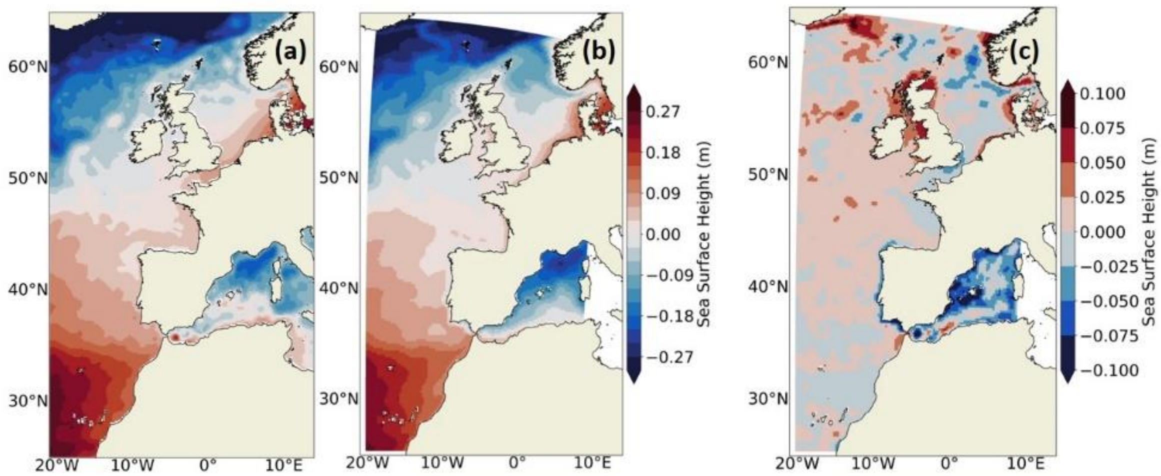

Figure RC3: Sea Surface Height bias (c) between GLORYS2V4 (1993-2014) (b) and CNES-CLS-18 (1993-2012) (a).

As explained L260, the SSH corrective value of +0.1 m is applied to the east Mediterranean boundary at each time step and boundary grid point. A sentence has been added to detail how the correction is applied: "The mass correction is added to the local T/S values."

The +0.1m setting of the SSH is applied in both IBI-CCS_corr and in IBI-ERAi simulations so the biases observed at the eastern boundary in Figure 3 are not related to this setting. The biases of Figure 3 are found in the buffer zone where the bathymetries of CNRM-CM6-1-HR (1/4°) and of the regional model (1/12°) are merged. However, for the IBI-ERAi simulation, the forcings at the open boundaries are GLORYS2V4 (and not CNRM-CM6-1-HR as in IBI-CCS simulations), so there is a small inconsistency in the buffer zone between the forcings and bathymetry. That is why biases are observed for integrated variables like thermosteric or halosteric sea level.

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

The manuscript has been revised for language and grammar.

Minor issues

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

Done.

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

The physical definition has been added above L41: "At regional scales, spatial variations of SL changes are mainly due to changes in dynamic sea level (DSL) i.e. changes in ocean circulations and the associated ocean heat, salt and mass redistribution within the ocean."

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.

The sentence has been modified as follows: "To overcome this problem, bias corrections can be applied to the GCM outputs before using them as forcing when performing a dynamical downscaling"

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.

The subparagraph "2.3.3 Total SL in global and regional simulations" has been modified and explains how the SSH is modelled in both global and regional 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.

Indeed only its lower resolution version (CNRM-CM6-1) has been extensively validated in Voldoire et al., 2019. The CNRM-CM6-1-HR model has been derived from its lower resolution counterpart by only increasing the resolution. This high-resolution version provides results very close to CNRM-CM6-1 which has not motivated a dedicated paper. However, in Saint-Martin et al., 2021, there is a brief description of this model and the supplement information material provides some figures comparing the biases of both model versions (https://agupubs.onlinelibrary.wiley.com/action/downloadSupplement?doi=10.1029%2F2020MS002190&file=2020MS002190-sup-0002-Supporting+Information+SI-S01.pdf). An explanation has been provided L115 to justify the use of this GCM: "The GCM CNRM-CM6-1-HR was chosen for its eddy-permitting high-resolution over the ocean which allows a more realistic regional circulation (section 3.1.2). Moreover, the 0.5° resolution over the atmosphere is also interesting to obtain a less smooth atmospheric forcing (winds) which is very important for the modeling of extreme SLs."

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.

The sentence L122 has been modified: "Seawater thermodynamics uses a polynomial approximation of TEOS-10 (Roquet et al., 2015), therefore the prognostic variables are the absolute salinity and conservative temperature. As the RCM does not use the same approximation for the equation of state (section 2.1.2), the GCM outputs are converted to in-situ temperature and practical salinity to be used in the RCM."

The sentence L171 has been modified: "Seawater thermodynamics uses a polynomial approximation of EOS-80 (Fofonoff and Millard Jr, 1983). Therefore, the RCM requires in-situ temperature and practical salinity from the GCM."

Line 128: please, insert citation for OASIS-MCT

The citation « Craig et al., 2017 » has been added.

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

The citation « Roehrig et al., 2020 » has been added.

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

Done.

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

The title of the section has been changed in "Regional ocean model IBI-CCS at 1/12° resolution".

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.

The 1/36° version is indeed not used for validation. This indication gives an information about the knowledge of the area in the CMEMS framework.

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

Done

Line 230: Please, provide the link in the references

Done.

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

Done.

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