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Reply on RC2

Adama Sylla et al.

Author comment on "Impact of increased resolution on the representation of the Canary upwelling system in climate models" by Adama Sylla et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-130-AC3, 2022

Dear reviewers

We thank you for your comments on our submitted manuscript. We answer below each of the points raised by the reviewers. Our answers appear in bold.We hope that you will be convinced.

Thanks again for your efforts

Adama SYLLA and co-authors

Title: Impact of increased resolution on the representation of the Canary upwelling system in climate models.

Authors: A. Sylla, E.S. Gomez, J. Mignot and J.L. Parages

Summary:

Authors investigate the realism of Canary upwelling system simulation in 6 high-resolution & standard resolution global coupled climate models from the HighResMIP project. Upwelling indices based on sea surface temperature (SST), height (SSH), and surface wind stress during the 1985-2014 period have been analyzed from the models and compared against that from observations. Authors find that increasing spatial resolution of atmosphere and ocean components of coupled models improves upwelling simulation only in the southern part of the upwelling system and while worsening it in the northern part.

The topic addressed is very relevant, since it is crucial to understand the upwelling dynamics in the Canary upwelling system (CUS) in a coupled framework at a high-resolution to better address future climate change and associated societal impacts. However, after considering the scientific merit, analysis methods, novelty, and overall presentation, I have a few major comments as detailed below.

We thank the reviewer for his general comment. We answer below each of his/her points.

Major comment

1) The essential background details for this study are not presented in a clear manner. What resolutions (for the ocean and atmospheric components) are considered standard and high resolutions? According to Chelton et al. 1998), the first baroclinic Rossby radius of deformation varies in the range of 20-60 km and the high-resolution ocean models in this study (0.25o, Table 1) can barely resolve this scale in most parts of the CUS. Most of the high-resolution atmospheric components in this study are about 0.5o which may not be able to resolve realistic wind structure/drop-off near the coast (see Patricola and Chang, 2017). So, even though 0.25 deg ocean model and 0.5 deg atmospheric model are described as "high-resolution" in this study, it is not shown that these resolutions/models can realistically resolve upwelling dynamics in this region (also see comment 2 below). No discussions/insights are offered about possible mechanisms/processes which are not resolved at these resolutions, compared to typical regional model resolutions of 0.1o in the ocean and 0.25o in the atmosphere.

Indeed, the reviewer is right that - "standard" and "high resolution" terms are rather subjective and depend on the context. Here, we use them in the context of global climate modelling, so that standard resolution is around 1° for the ocean, and high-resolution around 0.25° and higher. We acknowledge that the high-resolution ocean models in this study can barely resolve the first baroclinic Rossby radius deformation (20-60km, Chelton et al 1998) in most parts of the CUS. Similarly, the standard atmospheric resolution is 1° to 2.5° while most of the high-resolution atmospheric components in this study are about 0.5° which may not be able to resolve realistic wind structure/drop-off near the coast (Patricola and Chang, 2017). So, even models described here as "high-resolution" can probably realistically not resolve upwelling dynamics in this region, at least not as well as dedicated configurations (for example ROMS model including a high resolution grid 1/60° (~ 2 km) and a standard resolution 1/12° (~ 10 km). This discussion was added in the text.

2) Lack of analysis/discussion of mean upwelling vertical structure (eg. temperature depth- distance sections, see Fig.5 in Capet et al. (2004) and coastal wind structure (eg. wind profiles, see Fig.1 in Capet et al. (2004)) against observations makes it difficult to evaluate the realism of modeled winds and upwelling. The presented seasonal cycle of upwelling indices alone does not help in this regard. How realistic is the coastal wind drop-off (see Capet et al., 2004) in the 0.5o atmospheric model compared to that in the observations? How realistic is the vertical structure of temperature in terms of up-sloping isotherms? How do the high-resolution models differ from low-resolution ones in these aspects?

We agree with the reviewer that the mean modeled oceanic and atmospheric states in the upwelling region have not been precisely qualified in this present study. This point is also underlined in the final manuscript. The focus was on upwelling indices, and we leave it to other studies to link the performance highlighted here to the climatology.

3) The upwelling zone definition (using rectangular regions, especially in nMoUS and sMoUS regions) for analysis is not consistent with the narrow-coastal upwelling pattern (Fig.1, blue box). For example, at about 31oN in the nMoUS region, the coastal zone stretches about 8-9os including the non-upwelling offshore region. At 21oN, it reduces to about 1-2os width. Hence this approach is not consistent, especially for comparing different regions like nMoUS and sMoUS (especially for fields like wind stress curl & models with low-resolution). A fixed-width approach like that in Jacox et al. (2018) (see their Fig.1) will be better suited here.

We thank the reviewer for this remark, we have indeed changed the box in the Morocco region (see Fig. 1 in new manuscript). However the results (Fig.2 in

supplementary material) are very similar to what we have submitted in the previous manuscript. Nevertheless we thus propose to keep this new method in the paper.

4) The estimation of total upwelling intensity (lines 295-296) by simply adding three indices (measuring Ekman transport, Ekman pumping, and geostrophic transport) is not convincing since it is not verified in any manner (say against vertical velocity from the model). Please note that Jacox et al. (2018) (cited in this manuscript) compute the total upwelling index/transport without considering Ekman pumping explicitly (but including its effect by integrating Ekman transport around the perimeter of coastal boxes) and shows that it matches very well with the transport estimated from model's vertical velocity. Such verification is required for the method used here.

We agree that we do not use exactly the same methodology as in Jacox et al. 2018. Nevertheless, the comparison to the latter had been performed with CMIP5 data for the SMUS only and he shown again here (not published, Fig. 1 in supplementary material). Furthermore, the comparison of this indirect estimate to a more direct estimate from vertical velocities was done in Sylla et al 2019 for CMIP5. Main conclusions are now added in this manuscript. Finally, we emphasize the fact that the point of this study is not necessarily to come up with a quantitative assessment of the upwelling but rather to compare various quantitative assessments against resolution. This point is made clearer when estimation of total upwelling intensity is introduced.

5) The available resolution of models (~0.5o to 2.5o in the atmosphere and 0.25o to 1o in the ocean) and the combination of coarse and high-resolution atmospheric and ocean components in this study are not sufficient to draw the conclusion that high-resolution in the atmosphere "has only a limited impact" (eg. lines 434-435) in a general sense. Only a comparison of a high-resolution ocean grid (~10 km to resolve the first baroclinic Rossby radius well) with coarse (~1o) and fine (~1/4o) atmospheric grids can isolate the true impact of a high-resolution in the atmosphere. In other words, the ocean resolution should be fine enough to fully utilize the well-resolved coastal wind drop-off (see Capet et al. (2004) and Patricola and Chang (2017)).

We agree with the reviewer that the range of resolution that is explored here is rather limited. This study is based on an ensemble of opportunity of coupled climate models, which furthermore includes quite a limited number of models and configurations. We have leveled down the conclusions by adding systematically "within the investigated range"

Technical Corrections

1) Please explain the analysis methods in detail (eg. definition of the seasonal cycle, integration steps to compute total upwelling intensity etc.)

We use monthly climatologies built over the 1985–2014 time period for the climate simulations and observations. Thanks.

2) Line 6-7: The sentence "Our analysis shows that an increase of spatial resolution depends on the sub-domain of the CUS considered." is ambiguous.

Thanks, we are changed the line 6-7 into "Ours analysis shows that an improvement in upwelling simulation due to the increased spatial resolution depends on the sub-domain of CUS considered."

3) Line 8: "both components": Though it is mentioned that the models are coupled,

explicitly state "both atmosphere and ocean components" for clarity.

Both components mean here the resolution of the ocean and atmosphere model.

-4)Line 26: Please cite Capet et al. (2004) for the role of coastal wind drop-off in wind stress curl-driven upwelling.

We are grateful to the reviewer for pointing out this paper. We now cite this paper as a reference for the role of coastal wind drop-off in wind stress curl-driven upwelling.

5) Fig.1: Black and magenta stars and dots are mentioned in the caption but are not visible even after trying different PDF viewers. It will be helpful to overlay a few SST contours for highlighting the cooler SSTs in the upwelling region

Thanks for this remark we added on Fig.1 of the new manuscript version the black and magenta dots and also the SST contours for highlighting the cooler SSTs in the upwelling region.

6) Also, show the region over which Ekman pumping has been integrated (line 210).

Ekman pumping was integrated over the latitude and longitude range of each box on Fig. 1 for each sub-regions.

7) Table 2: Use "reanalysis" instead of "reanalyse".

Thanks, reanalyse was changed into reanalysis

8) - Fig.1 & 2: Technical inconsistency:

Fig.1: Various regions extend from 12N to 42N, with a blue dashed line representing the northern boundary of sMoUS region.

The various regions of the CUS are located from the Senegalese coast (12°N) to the Iberian Peninsula region (43°N). The different regions of CUS are also represented of the different boxes as explained in the caption of Fig.1. The Moroccan is separated into two sub-domain based on the seasonality of upwelling (nMoUS: 26°N 32°N and the sMoUS: 21°N-25°N). This division of the Morocco system was correctly represented on Fig.1 in the new manuscript, thanks

9) Fig.2: Some of the panels do not extend to 12N and now the blue dashed line represents the northern boundary of nMoUS region.

Thanks for this remark, but all of the panels in Fig.2 are extended to 12°N in the submitted. This remark that some of the panels do not extend to 12°N in the previously submitted manuscript is due to the fact that the panels for the observations (left column) were different-size from those for models in the subplot. It was correct this mistake and in the new manuscript all panels are the same format and the blue dashed line consistently represents the boundary of nMoUS sub-region.

10) On line 241, explicitly state "observations and reanalysis" as in line 253. Also, which all SST values are contoured in Fig.1? (difficult to read from the colorbar). The dark-red colors in panels (eg. last column,2nd row) is not seen in the colorbar.

Thanks for this remark we added some contours to highlight the dark-red colors values.

11) - Fig.B1: Colors do not have any correspondence to positive/negative values. Make the color scale from -0.14 to 0.14

Thanks for this remark, we are changed the scale color (see new manuscript version).

12)- Line 310: Fig.2: CMCC-CM2 (Group 1 and 2) still shows a high UI_sst index in the summer, though the sign of UI_sst is positive throughout the year.

We agree with the reviewer that CMCC-CM2 family show a high UIsst index in the summer, though the sign of UIsst is positive throughout the year. But it is also important to keep in mind that in this region the upwelling occurs generally in summer as in the previous studies referenced in the introduction of the submitted manuscript. Therefore our analysis was concerning in this period. Additionally this index was used but present disadvantages. In general, the main disadvantage of UI^{sst} comes from the fact that changes in coastal and oceanic temperature cannot always be attributable to upwelling. Indeed UI^{sst} can be strongly influenced both by local scale phenomena (e.g., the presence of rivers with high runoff which can modify the SST signal near the coast) and by macroscale phenomena (e.g., El Nino can result in changes in coastal temperature that are not related to the presence of coastal upwelling, eg Gomez et al 2008).

13)- Line 315: For both groups 1* and 2*, upwelling is present in the nMoUS region indicated by positive values. But the pattern is not the same as in the observations.

The reviewer is right that the Line 315: was not correct, this has been changed into "For both groups 1* and 2*, the upwelling is broadly reproduced in these sub-regions, with an overestimation of UIsst amplitude in MPI-ESM1 over the sMoUS." Thank you.

14)- Line 317: increasing just the atmospheric resolution makes the summer upwelling stronger in the IP region in MPI-ESM1-2 case. This is against the statement in line 332 too.

Thanks for this remark, however the comparison of group 1* and group 2* does not show very clear difference of UIsst amplitude during summer along the IP sub-region (Fig.2) excepted in MPI-ESM. Therefore our analysis in line 317: "Thus, the only increase of the atmospheric resolution in models produces no clear impact on upwelling representation " is similar to what we have noted in line 332: "Thus, the IP does not seem to be very sensitive to these changes in model resolution." Indeed these two lines explain that there are no clear improvement to increasing only atmosphere or both component (ocean and atmosphere) resolution in this sub-region.

-15) Section 3.1 title (Line 238): Change it to "The thermal upwelling indices"

Thanks title was changed

16) Section 3: All figures referred in this section have panels from both observation and models, but figures from models are discussed only in Section.4. The title for section 3 alone is not sufficient to bring this point to readers' attention.

This is true. Nevertheless, to make the manuscript lighter and easier for the reader, we have decided to make these general plots but describe them sequentially in 2 sections. Additionally in each section we noted clearly the position of the observation datasets or models we are analyzing. We add nevertheless this text in the new manuscript "In this section, we describe the upwelling indices defined above computed for the observation datasets. These indices are shown in Fig 2, Fig.3, Fig.4 and Fig.5 for both the data and models but only the observations panels are described here. The results from the modeling experiments will be described in section 4''.

17) line 178-179: Need to provide a basic definition of MLD criteria/method in addition to the reference.

This MLD climatology is based on ARGO profiles where MLD was estimated following a density criterion at a monthly resolution in boxes of 2° latitude by 2° longitude. The selected criterion is a threshold value of temperature from a near-surface value at 10 m depth (DT = 0.2°C)''. We also added this text in section 2.2 in the version of the manuscript. Thanks

18) line 280-281: "We have examined....." edit this sentence for clarity.

Thanks, we are changed the line 280-281 into "We have examined firstly the monthly climatology of the meridional sea surface height gradient from the AVISO satellite data and the GODAS reanalysis (see first two columns in Figure B1 of the appendix B).

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2022-130/gmd-2022-130-AC3-supplement.pdf