Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., author comment AC2
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Reply on RC1

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-AC2, 2022

Dear reviewer

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

Anonymous Referee #1: Review of Manuscript gmd-2022-130

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

Recommendation: major revision

Summary

This study tested the state-of-the-art CMIP-class Earth system models (ESMs) to what extent the models can reproduce the Canary upwelling system along the coast from the Iberian Peninsula to the northwestern Africa, which is one of the areas of marine ecosystem and fishery. In general, ESMs with coarse resolution (1-2 degrees) fail to reproduce the coastal upwelling system due to several causes like wind stress, its curl, heat fluxes, etc. The authors also analysed Highres-MIP data (- 0.25 degree) and showed the benefits of refinements of atmospheric and oceanic horizontal resolution being consist with previous studies that focus on other upwelling areas. Interestingly, the authors employ several metrics to describe the coastal upwelling quantitatively and the results based on this methodology are well summarized. Therefore, I would think that this study would have feedbacks on model development and insightful understandings on the coastal upwelling in model simulation. On the other hand, I have several (most of them should be minor) concerns about plottings and interpretations on the results. As below, I am providing my comments and would expect the authors to address them and revise the

manuscript. After adequate revision, this manuscript will be accepted as a publication in GDM.

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

Minor comments:

1) Line 2. operating => operated?

Thanks, the word operating was replaced by operated

2) Lines 3-4. Might delete For this....was increased.

Sentence was removed, thanks

3) Lines 18-19. Some references should be added.

References added:

Herbland, A., Voituriez, B., 1974. La production primaire dans l'upwelling mauritanien en mars 1973. Cah. O.R.ST.OM., Sér. Océanogr. 12 (3), 187–201.

Minas, H.J., Codispoti, L.A., Dugdale, R.C., 1982. Nutrients and primary production in the upwelling region off Northwest Africa. Rapp. P.-V. Reun., Cons. Int. Explor. Mer 180, 148–183.

Tretkoff, E. (2011). Research Spotlight: Coastal cooling and marine productivity increasing off Peru. Eos Transact. Am. Geophys. Union 92, 184–184. doi: 10.1029/2011eo210009

Huyer, A. (1983). Coastal upwelling in the California Current system. Prog. Oceanogr. 12, 259–284. doi: 10.1016/0079-6611(83)90010-1

4) Line 25. "induces a positive wind stress" talking about only NH? If SH is included, better to say "cyclonic" wind stress curl.

The word "positive" was removed and replaced by cyclonic

5) Line 35. Synoptic. For me, "synoptic" sounds more spatial. But, maybe the authors want to mention temporal variability here, I suppose.

Thanks, for this remark we have reformulated the sentence in the new version into " The variability of this upwelling system has been studied on seasonal time scale (Torres, 2003 and Alvarez et al., 2005)".

6) Line 38. "The latter" denotes Azores High Pressure? I think ITCZ is also a part of Hadley Circulation system.

It was an error, we apologize for that and we have reformulated this sentence into: "In the CUS, the strength of the upwelling favorable winds are associated with latitudinal variation of the Inter-tropical Convergence Zone (ITCZ) and the Azores high pressure system which are both part of the Hadley-circulation. The Azores high pressure migrates from 25°N in late winter and 35°N in late summer."

7) Line 39-40. Any reference?

Thanks, we have added the references below:

Wooster, W.S., Bakun, A., McLain, D., 1976. The seasonal upwelling cycle along the eastern boundary of the North Atlantic. J. Mar. Res. 34 (2), 131–141.

Mittelstaedt, E., 1991. The ocean boundary along the northwest African coast: circulation and oceanographic properties at the sea surface. Prog. Oceanogr. 26, 307–355.

Van Camp, L., Nykjaer, L., Mittelstaedt, E., Schlittenhardt, P., 1991. Upwelling and boundary circulation off northwest Africa as depicted by infrared and visible satellite observations. Prog. Oceanogr. 26, 357–402.

Nykjær, L., Van Camp, L., 1994. Seasonal and interannual variability of coastal upwelling along northwest Africa and Portugal from 1981 to 1991. J. Geophys. Res. 99 (C7), 14197–14207.

Benazzouz, A., Mordane, S., Orbi, A., Chagdali, M., Hilmi, K., Atillah, A., Lluîs Pelegrî, J., and Hervé, D.: An improved coastal upwelling index from sea surface temperature using satellite-based approach – The case of the Canary Current upwelling system, Continental Shelf Research, 81, 38–54, https://doi.org/10.1016/j.csr.2014.03.012, 2014.

8) Line 57. Due by => Due to.

Thanks for this remark ‘’Due by’ was indeed changed into “Due to”

9) Lines 66-68. What data did Bakun use for the study? Might be good to describe it.

We thank the reviewer for the suggestion and we have reformulated this sentence in the new version into “By using the averages of the meridional wind stress component derived from ship reports, Bakun (1990) suggested that coastal upwelling intensification would occur in response to continued global warming.”

10) Line 73. Sea Surface Temperature can be small letter? In Fig.1 I cannot see any black/magenta dots nor any other notifications the caption tells. Probably, forgot to show them in the figure?

Thanks for this remark “Sea Surface Temperature” was changed into sea surface temperature and we apologize for the figure. 1 it was an error, we added the black and magenta dots”.

The action of wind.. should be influence of wind on the upwelling?

Thanks, we have changed these lines into “the influence of wind on the upwelling can be separated into two mechanisms”.

12) Defintion of Tgeo. I am not familiar with this dynamical parameter to describe the vertical transport due to geostrophic flow. However, when there is SSH meridional gradient, MLD would have also meridional gradient, wouldn't it? Could you please explain why it is ok to use a box-averaged MLD?

The cross-shore geostrophic transport (expressed in sverdrup) is computed

following this equation: Tgeo = MLD.g/f (SSHnorth − SSHsouth) where ΔSSH is the coastal SSH difference between the northern and southern ends of our region of interest and we integrate this transport on the mixing layer by assuming that the geostrophic transport is limited to this layer.

13) 2-4. The panels for the observations (left column) are different-size (also lable of latitiude) from those for models. I strongly suggest to have same format among them so that it is easier to compare.

The reviewer is right that panels for the observations are different-size from those for models, this has been corrected in Fig.2, Fig. 3 and Fig. 4.

14) what does the contour denote?

Thank you for this remark. On each panel, the black contour shows the contour zero.

In Fig.3 and Fig.4: the black and grey contours show the contour 0.75 and 0.5 respectively.

15) Line 256. UISST => UISST ?

Thanks '' UISST' was replaced by UI^{sst}

16) Line 257. Not clear "negligible of even negative value of CSET".

Sorry it was a mistake. We corrected the sentence into "negligible or even negative value of CSET"

17) Fig. 4 Along n/s MoUS regions, the upwelling index is almost always negative, indicating downwelling motion is dominant through the whole year. But, this seems contradict against the cool SST there (e.g., Fig1). So, the cool SST comes from horizontal advection, not from upwelling around Moroccan coast?

We thank the review for the remark, but there is not necessarily a contradiction here. Indeed Fig.4 shows the contribution of Ekman pumping only, which is only one of the dynamical drivers of the upwelling. Over the sMoUS and nMoUS this effect is favorable to a downwelling. This situation result to the negative ∂v/∂x (see Eq:4). However, Ekman transport remains favorable to an upwelling and Fig. 5 confirms that an upwelling is indeed taking place in this region.

18) Line 271. null => zero?

The word "null" was removed and replaced by "zero".

19) Line 286. Not clear "sub-regions". Do the authors mean other regions? (IP, nMoUS, sMoUS)?

In this line sub-regions indeed refers to IP, north and south Morocco. We have reformulated this sentence into "In the SMUS (Fig B1, panel d) the SSH difference is also always negative and the related amplitude strongly differs from the others sub-regions (IP, nMoUS and sMoUS)".

20) Line 288. Repetitive.

Thanks, this is removed

21) Fig.5 the xaxis-label data1/2/3 should be AVISO/GODAS/MLD. This can shorten the caption

Thanks for this remark but the xaxis-label data1/2/3 correspond to the transport total which combined different validation datasets as explained in the caption. Therefore it is not possible to attribute them to a specific data. We thus propose to keep our previous caption in the paper.

22) Fig.5.a => Fig.5a. This expression can be seen elsewhere in the manuscript.

Corrected in several places, thanks.

23) Definition of UItotal. The authors add Ekman transport and pumping to estimate the total upwelling intensity. But this summation doesn't double-count Ekman dynamics (transport and upwelling)? In general, the Ekman pumping compensates the divergence of Ekman transport at the upper level. How do the authors interpret this?

We agree that Ekman suction and coastal divergence are added together but are not really independent in the calculation because they overlap spatially. This point is raised in Jacox et al. (2018) who propose to calculate unambiguously the total divergence associated with Ekman divergence + Ekman suction by integrating the Ekman transport along all boundaries (north, south, east) of the region of interest. The comparison of Jacox et al. (2018) method and the estimation proposed in this submitted manuscript was tested after a similar question from a reviewer of my manuscript Sylla et al 2019 (cited in this manuscript). This comparison (Fig.1, supplementary material) shows that both methodologies in general yield very similar results.In the validation data sets, the difference is less than 5%, with the Jacox's et al. (2018) approach leading to slightly stronger results, while the multimodel mean is weakened by approximately 10%. Given the similarity of these results, and the interest, in our view, to discuss the open ocean wind stress curl separately from the offshore transport divergence, we consider that the overlap is weak and decide to keep the estimation described in the manuscript to compute the UI_{total}.

24) Section 4. There is no plot of SST itself from MIP models. I am curious how good/bad the SST climatology (seasonal cycle, location of Senegal-Mauritania Front).

We show in Fig. 2 (Supplementary material) the amplitude of the SST seasonal cycle in the climate models. The magnitude of the seasonal cycle is maximum in SMUS between 12°N and 20°N because the seasonal upwelling con- tributes to wintertime cooling. This figure shows that the simulations from group 2 generally reproduce realistic amplitude in the correct latitudinal band compared to OISST V2 whereas its intensity is less marked for group 1. Additionally for group 1* and 2*, MPI-ESM generally reproduce an intensified amplitude of the SST seasonal cycle in the SMUS latitude band and in CMCC-CM2 it is amplified only in the north of SMUS. These feature affect the thermal upwelling indices.

25) Line 308. "mean state of the mean seasonal cycle", climatological seasonal cycle?

Thanks for this remark "mean state of the mean seasonal cycle'' was indeed changed into climatological seasonal cycle.

26) Lines 319-322. These under/overestimated upwelling (UISST index) is consistent with the SST bias in each model? As I commented, better to show SST or its bias plot among MIP models.

As mentioned above the bias of UIsst index is consistent with the SST bias in each model (see Fig.2 supplementary material).

27) While I can see the reduced overestimation of IP's upwelling, there might be still some overestimation, especially, in ECMWF-IFS-HR?

We are agree with this remark that the upwelling indices are in general sightly overestimated in the group 2 (HR models) along the IP coast. This situation may explain sometimes the higher skill score of group 1 (LR models, eg Fig.6 first column). This remark was now added in the new manuscript.

28) Line 351. Please remove "in these subdomains"

Thanks, "in these subdomains" was removed.

29) Line2 351-352. "being this...established" might be rephrased?

Thanks, we have changed into "Focusing on the sMoUS and nMoUS, group 1 largely overestimates CSET, whereas this overestimation is less well clear for group 1* (Fig.3)".

30) Line 352. "validation dataset" is replaced with observations.

Thanks, ''validation dataset'' was replaced by observations

31) Line 353. "Slightly", at least, between Group 1 and 2 the improvement is very remarkable?

We agree for the review and the word "slightly" was removed.

32) Line 358 and somewhere. "Let's now..." sounds too casual for a scientific paper.

We agree with the reviewer that "Let's now..." sounds too casual for a scientific paper. We have modified the sentence into "We consider now the ability of the different model configurations to reproduce the seasonal variability of the wind stress curl (Fig.4).

33) Line 359. In Fig.4, it seems that Group 1 models do not have large bias in SMUS region. Like Fig. 3. However, Wk and CEST indices are based on wind stress and they may have some coherency, I guess. I am wondering why these indices seem to have different bias in LR models (Group 1).

We agree with the reviewer that the Ekman transport and Ekman pumping are both based on the wind stress and therefore should hold some coherency. However, they also hold differences. For example along the SMUS where the strong difference between CSET and Wek is observed, the zonal component of wind stress is not taken account in CSET because the coast is oriented north to south (Eq: 3). Thus the difference between Wek and CSET biases could come from this component for example.

34) Line 398. "estimation of the upwelling transport", sounds a bit strange in this context.

The estimation of the upwelling transport used here are able to fully capture the estimation of the upwelling transport" -> "estimate quite realistically the upwelling transport.

35) Line 400. “goes in the...UItotal”, rephrase.

Thanks for this suggestion and Line 400 was reformulated into “ Groups 1* and 2* show similar range of UItotal and no clear effects due to the increasing resolution are identified.

36) Line 440-447. This part is a bit redundant.

Thanks for this remark, This part was removed in the new manuscript.

37) Line 455-456. Rephrase.

Thanks, this line was changed into “Globally, our results show that observations and reanalyses yielding a fairly consistent picture of the CUS climatology.

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2022-130/gmd-2022-130-AC2-supplement.pdf