Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-130

Anonymous Referee #1

Referee 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-RC1, 2022

General Comments:

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.

Specific Comments:

- Line 2. operating => operated?
- Lines 3-4. Might delete For this….was increased.
- Lines 18-19. Some references should be added.
- Line 25. "induces a positive wind stress" talking about only NH? If SH is included, better to say "cyclonic" wind stress curl.
- Line 35. Synoptic. For me, "synoptic" sounds more spatial. But, maybe the authors want to mention temporal variability here, I suppose.
- Line 38. "The latter" denotes Azores High Pressure? I think ITCZ is also a part of Hadley Circulation system.
- Line 39-40. Any reference?
- Line 57. Due by => Due to.
- Lines 66-68. What data did Bakun use for the study? Might be good to describe it.
- 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?
- Line 193. The action of wind.. should be influence of wind on the upwelling?
- Defintion of T_{geo}. 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?
- 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.
- 3 what does the contour denote?
- Line 256. UISST => UI^{SST} ?
- Line 257. Not clear "negligible of even negative value of *CSET*".
- 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?
- Line 271. null => zero?
- Line 286. Not clear "sub-regions". Do the authors mean other regions? (IP, nMoUS, sMoUS)?
- Line 288. Repetitive.
- 5 the xaxis-label data1/2/3 should be AVISO/GODAS/MLD. This can shorten the caption.
- 5.a => Fig.5a. This expression can be seen elsewhere in the manuscript.
- Definition of UI_{total}. 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?
- 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).
- Line 308. "mean state of the mean seasonal cycle", climatological seasonal cycle?
- Lines 319-322. These under/overestimated upwelling (UI^{SST} index) is consistent with the SST bias in each model? As I commented, better to show SST or its bias plot among MIP models.
- While I can see the reduced overestimation of IP's upwelling, there might be still some overestimation, especially, in ECMWF-IFS-HR?
- Line 351. Please remove "in these subdomains"
- Line2 351-352. "being this…established" might be rephrased?
- Line 352. "validation dataset" is replaced with observations.
- Line 353. "Slightly", at least, between Group 1 and 2 the improvement is very remarkable?
- Line 358 and somewhere. "Let's now…" sounds too casual for a scientific paper.
- 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).

- Line 398. "estimation of the upwelling transport", sounds a bit strange in this context.
- Line 400. "goes in the…UI_{total}", rephrase.
- Line 440-447. This part is a bit redundant.
- Line 455-456. Rephrase.