Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
https://doi.org/10.5194/gmd-2022-130-RC2, 2022

Comment on gmd-2022-130

Anonymous Referee #2

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-RC2, 2022

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.

Specific Comments

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.

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?

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 31^{o}N in the nMoUS region, the coastal zone stretches about 8-9^{o}s including the non-upwelling offshore region. At 21^{o}N, it reduces to about 1-2^{o}s 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.

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.

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 (~1^{o}) and fine (~1/4^{o}) 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)).

Technical Corrections

- Please explain the analysis methods in detail (eg. definition of the seasonal cycle, integration steps to compute total upwelling intensity etc.)
- 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. It should be "...shows that an improvement in upwelling simulation due to the increased spatial resolution....".
- Line 8: "both components": Though it is mentioned that the models are coupled,

explicitly state "both atmosphere and ocean components" for clarity.
- Line 26: Please cite Capet et al. (2004) for the role of coastal wind drop-off in wind stress curl-driven upwelling.
- 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. Also, show the region over which Ekman pumping has been integrated (line 210).
- Table 2: Use "reanalysis" instead of "reanalyse".
- 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.
  Fig.2: Some of the panels do not extend to 12N and now the blue dashed line represents the northern boundary of nMoUS region.
- 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.
- Fig.B1: Colors do not have any correspondence to positive/negative values. Make the color scale from -0.14 to 0.14
- 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.
- 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.
- 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.
- Section 3.1 title (Line 238): Change it to "The thermal upwelling indices"
- 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.
- line 178-179: Need to provide a basic definition of MLD criteria/method in addition to the reference.
- line 280-281: "We have examined....." edit this sentence for clarity.

References

Capet, X. J., Marchesiello, P., and McWilliams, J. C. (2004), Upwelling response to coastal wind profiles, Geophys. Res. Lett., 31, L13311, doi:10.1029/2004GL020123.
Chelton, D. B., R. A. deSzoeke, M. G. Schlax, K. E. Naggar, and N. Siwertz (1998), Geographical variability of the first-baroclinic rossby radius of deformation, J. Phys. Oceanogr., 28, 433–460.
Patricola CM, Chang P (2017) Structure and Dynamics of the Benguela Low-Level Coastal Jet. Climate Dynamics, 49, 2765-2788.