REVIEWER 1

EGUsphere, referee comment RC1 https://doi.org/10.5194/egusphere-2022-41-RC1, 2022 Comment on egusphere-2022-41 Anonymous Referee #1 Referee comment on "Moana Ocean Hindcast – a 25+ years simulation for New Zealand Waters using the ROMS v3.9 model" by Joao Marcos Azevedo Correia de Souza et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-41-RC1, 2022 General comment: The manuscript entitled “Moana Ocean Hindcast – a 25+ years simulation for New Zealand Waters using the ROMS v3.9 model” by Souza et al. presents an extensive model-data comparison of a high-resolution ocean model for the New Zealand region. The comparisons highlight the improvements of the high-resolution simulation over the global data-assimilative model used for boundary conditions. The manuscript is well-structured, moving from the large-scale to the coastal ocean, however it is my opinion that minor changes are needed before this manuscript can be considered for publication. I appreciated the manuscript for being well-structured and for trying to present a substantial amount of information in a succinct way. However, in doing so some of the results aren’t explored in any detail and some of the model-data comparisons are glossed over (e.g. SST). The Conclusion sections is a bit disappointing. This section could be improved by a) highlighting the strengths and short-comings of the model and b) by including some of the ways in which the model can be improved on.

We would like to thank the reviewer for the detailed comments. They were very helpfull to improve the manuscript. We modified the conclusions to make the current simulation improvements clearer.

Specific comments:

Page 1, Line 20: The authors state that the New Zealand fishing industry is expanding to include coastal aquaculture. New Zealand already has extensive coastal aquaculture activities based around three marine species: salmon, mussels and oysters. However, the Government’s Aquaculture Strategy aims to a) expand and improve the existing aquaculture activities, b) advance land-based aquaculture and c) extend aquaculture into the open ocean by 2035. The aquaculture industry in New Zealand are already exploring moving further offshore (so-called open ocean aquaculture) due to coastal warming and the impacts of marine heat waves.

We modified the text in the introduction to meet the reviewer comment.

Page 2, Line 46: The statement “revolutionize the understanding and prediction of ocean processes in New Zealand” seems to be a bit of a bold claim. Regional models like the one described here are well-used to understand ocean processes in a range of environments. I agree with the author’s underlying view that this simulation will significantly contribute to improve our understanding and predictability of ocean processes but to claim that it will be revolutionary is an exaggeration.

This is actually the objective of the Moana Project, from which this simulation is a component. This phrase is part of the Project proposal aproved by the funding agency (MBIE). We changed the text to make this clear.

Page 2, Lines 48-49: What do the authors mean by “7 days forecasts daily”? Were daily forecasts created for a period of 7 days?

Exactly. Every day we run the model and generate 7 days forecast, that are publicly availabe.

Page 3, Line 74: Please clarify what you mean by mass structure

We are referring to the density strucgure – modified in the text.

Page 6, Line 104: Please be more specific with respect to the local sources that were used for the model bathymetry.

Added: “such as navigation charts and echo sounder surveys”

Page 6, Line 107: The authors mention that PGE associated bottom velocities were used to determine which grid cells required bathymetric smoothing. Since this is not a commonly used method in ROMS, please provide more details about this method.

This is actualy a common approach that is coded in popular libraries that work with the ROMS model, such as pyroms and seapy. We added the reference to 2 papers that use this approach.

Page 6, Line 118: You use Souza et al. (2020) to justify using CFSR for the atmospheric forcing. However, Souza et al. (2020) only compared the ocean component of CFSR to other ocean models and did not look at the atmospheric component. Is there any evidence that the atmospheric component of CFSR out-competes other atmospheric reanalysis products such as JRA-55 in the New Zealand region?

Not that we are aware. We removed the reference to Souza (2020) and added a quick explanation for why we use CFSR (well known and lots of use, and long time series).

Page 10, Lines 208-209: Please be more specific with regards to the location of the high and low SSH centres and the large-scale currents indicated by these SSH centres?

Added the position of the centres and a short description of the associated circulation features.

Page 12, Line 230: The authors note that it is interesting that the hindcast reproduced the 2018 temperature peak. This suggest that they didn’t expect the model to reproduce this event. Please elaborate why this is particularly noteworthy.

It is not that we didn’t expect, but this gives an insight that this was probably forced by anomalous atmospheric fluxes that were represented by CFSR. A deeper analysis is needed to go beyond expeculation, which is included in a couple of papers focusing on marine heatwaves already submitted for publication.

Page 12, Line 234-235: The authors mention that the bias pattern is similar to that found for GLORYS in Figure 7 of Souza et al. (2020) but then don’t give an explicit description of the biases in the Moana Ocean Hindcast shown in Figure 6.

A short description was added.

Page 13, Lines 238-240: While (a) and (b) are the result of intrinsic limitations of the model. Also, the section number for the section addressing (c) is missing. Point (c) is listed as “issues related to the observations of SST from satellites close to the coast and in a region notorious for its high cloud coverage”, yet Section 3.4.2. which I suspect is suppose to look at this limitation in more detail only looks at daily sea surface temperature at a number of coastal stations around New Zealand, and doesn’t examine the limitations of satellite SST close to the coast.

The problem with the section number was related to Latex compilation. This was solved now. The idea is to provide a comparison to an independent in situ observational dataset – what is done in section 3.4.2. Both the in situ and satellite are daily mean, what seemed to make more sense from a compatibility point of view and being this a national scale 5km resolution model.

Page 13, Table 3: RMSE and MAE are standard statistics used in model validation but are often calculated in different ways. Furthermore, maximum-absolute-error is not a commonly used statistic. I recommend that the equations for all the statistics presented in the manuscript are included.

The standard equations for RMSE and MAE were added o teh manuscript.

Page 14, Line 251: Was the model output used for the comparison co-located in space and time with the temperature and salinity profiles or was the time-averaged model output extracted at the location of the profiles?

We co-located by searching for the closest time and linearly interpolating in space.

Page 14, Line 259-260: The phrasing makes this sentence hard to understand. I think the authors are trying to say something along the lines of "Despite the difficulty in asserting the reasons behind such differences, the differences seem to be in part related to the surface forcing from CFSR and in part due to the boundary conditions from GLORYS.

Modifyed following the reviewer suggestion.

Page 17: The section on MLD really gives a nice description of what is observed in the observations and how the model differs from these observations. It would have been really good if this same approach was used throughout the manuscript.

At the time we decided different approaches would give a more general overview of the simulation performance.

Page 19, Lines 309-311: It is clear from the statement here that you are cognisant of the fact that volume transport calculation are sensitive to the horizontal and vertical extent over which it is calculated. Was any effort made to try and match the sections used here to the spatial extent over which transport has been calculated in the literature for a better comparison?

Yes, but we found different methodologies were used accross the literature. Therefore, we decided on an approach that will be used in future publications using this dataset.

Figure 11: Can you provide any explanation for some of the large differences observed between the observations and the model (e.g. stations 10-15 in subplot G)?

We don't think these are large differences - please note the different y axis between subplots. The reason for the differences is hard to estimate, but the relatively coarse representation of the coastal geometry and bathymetry in a 5km grid model should play a role.

Page 27, Line 425-426: "Satellite-derived annual cycles show coastal regions around New Zealand differ from…" This sentence is hard to understand due to the phrasing and requires some clarification.

Modifyed to improve clarity.

Page 28, Line 431-432: Here you claim that additional analysis of the coastal SST time- series are needed to understand the latitudinal decrease in SST and this might certainly be the case. However, one contributing factor to the latitudinal decrease would be a change in the background surface water masses as one moves from north to south with warm subtropical water in the north and cooler sub-Antarctic water in the south.

Thank you for the suggestion.

Page 28, Line 444: “The model results outperform the global models in the coastal region”. This is a very generalised statement that can’t really be backed up by any of the results presented in the manuscript. The results presented here only compares the temporal mean SSH and variance of SSH of the Moana Ocean Hindcast to that of one global model, GLORYS.

Although an improvement can be noticed comparing the present results against the maps presented in Souza et al (2020), we chnaged the text to “The model performs well in the coastal region as demonstrated by the comparison against shore stations.”

The above comments and recommendations along with some technical comments are provided in the attached pdf. Please also note the supplement to this comment: https://egusphere.copernicus.org/preprints/egusphere-2022-41/egusphere-2022-41-RC1-supplement.pdf Powered by TCPDF (www.tcpdf.org)

REVIEWER 2

General comment -

Overall this is a good and detailed paper on a hindcast dataset while providing background information and validation of the dataset. The methods undertaken are robust and I commend the authors for their interesting study. As is mentioned in the manuscript, the work builds on earlier work and extensively compares model results with other data sources (like GLORYS). The work presented in novel and provides a major step in the direction of predictive models and I think it is worthy of publishing in NHESS. However, I have listed a couple of modest comments and technical correction I would like to see addressed.

Modest comments –

In the introduction the authors mention that global reanalysis with DA, because of horizontal resolution have little capacity for complexities like riverine influences and mesoscale variability. While the authors mention mesoscale variability in the manuscript, I was left wondering if the MOANA hindcast is able to capture riverine influences in ocean dynamics and if authors have tried to analyze this.

There are obvious impacts in the Firth of Thames, where horizontal stratification is noticible (and you can see it in the Moana Hindcast). But we had no coastal salinity data to compare to. Our only source was Argo float profiles that don't cover regions shallower than 1000m. This is why choose to not

- I think in general the methodology section can benefit from a summarizing table or flowchart containing the many datasets (including for the model evaluation datasets) the research is using.

A summarizing table was added to the text.

- Line 214-215: Add a sentence or two why and when it is to be expected that higher resolution leads to larger variances (seasonal, peaks, gradient is large).

We added a short explanation.

- Line 240-241: It is stated that patterns of RSME are similar to patterns observed in variance of SST of Fig 3. Whereas there are some similar patterns, notably the highest RMSE (43S-174E and 48S-166E) fall in relatively low variance areas which contradicts the statement. Please elaborate on this.

The 2 regions correspond to fronts of the Southland Current where large SST gradients are present. Therefore, we estimate that the RMSE can be related to difference sin the location of the front in the simulation. Although this can be caused by errors in the model, one should keep in mind that the 1/4 optimal interpolation SST product will have smoothed fronts that will contribute to the large RMSE.

- Line 259-261: Elaborate how the surface forcing and boundary conditions from GLORYS could trickle down to observed difference in salinity.

The boundary conditions from GLORYS set the large scale water masses structure that is fed to the model domain. However, the presence of a water mass formation zone in the Subpropical Front provides a pathway through which atmospheric signals coming from

CFSR can penetrate to depths bellow the thermocline and influence the 3D density structure - especially for central and deep waters.

Technical correction:

All technical corrections were edited in the manuscript.

- Line 68 and 70 uses the abbreviation DA, but it is not written in full. Do the authors mean Data Assimilation?
- Line 6 and 7: “sea surface temperature (SST), sea surface height (SSH)” should be with capitals.
- Line 37: add “Due ‘to’ the large”.
- Line 144: the link is not working for me, maybe better to just provide the link to the product store (https://resources.marine.copernicus.eu/products) and give the dataset name along with it instead of this large link.
- Line 177: SST is already in use in the introduction. - For example, in Figs 2-3-4, what is meant with Moana ‘Backbone’, please provide context.

It is another name for the Moana Ocean Hindcast. A reference was provided on its first apearance in the text.

- Please provide contour boundaries in the caption of Fig 3.
- Line 239: Further explored in which section?
- Line 259-261: Sentence doesn’t flow well.
- Line 285 and 336: Question mark.
- Line 318: sections 1 without the ‘s’. Also line 354
- Figure 10a: section 8 is missing
- Table 4: Denote separations with A-D in the Table