

Interactive comment on “Evaluation of an operational ocean model configuration at 1/12° spatial resolution for the Indonesian seas – Part 1: Ocean physics” by B. Tranchant et al.

Anonymous Referee #1

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Review of "Evaluation of an operational ocean model configuration at 1/12° spatial resolution for the Indonesian seas – Part 1: Ocean physics"

In this manuscript, the authors describe a new model configuration of NEMO, specifically designed for the Indonesian region. They present a validation of the model comparing it so satellite (altimeter, SST, SSS) and in-situ (Argo, CTD, moorings, tide gauges), to find that the model does a reasonable job in some areas (currents, transports, tides) and has significant biases in others (water mass properties and transformations).

The Indonesian Archipelago is one of the most complicated regions in the world to

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model, and it is good to see some dedicated manpower going after this. However, I feel that there are a number of outstanding issues that need attention. In particular, I have the following general concerns:

1) It is unclear how INDO12 differs from the global ORCA12 run. The authors do briefly mention the ORCA12 at the end of the discussion section when they discuss boundary forcing. But the bigger question is: why not simply use ORCA12 in the Indonesian Archipelago? Has the extra work on sill bathymetry, tides etc paid off and improved INDO12 compared to ORCA12? This should be shown. If INDO12 is not better than ORCA12, the whole exercise is rather futile. If INDO12, on the other hand, is better than ORCA12, then the improvements should be fed into the ORCA12 run. In either case, it is unclear to me what the advantage of a regional domain at the same resolution as the global domain is.

2) Given that the INDO12 run is fairly short (2007-2013), to what extend are biases due to initial and/or boundary conditions rather than to the model run itself? In particular for the deeper T/S analysis, how relevant is it to analyse such a short run? Are biases not simply the result of a biased initialisation?

3) The order of the sections is rather odd at times. Why not discuss SST and SSS after SSH/EKE?

4) Some of the figures are very poor quality, and the captions in general need much more careful description of what's shown. Figures 11-15 are hardly readable on the screen; I would strongly suggest improving these.

5) The manuscript text is rather sloppy at times, and needs significant proofreading. This manuscript is clearly the result of the combining of sections provided by different authors, without any attempt to produce a coherent style. For example, La Nina is written in three or four different ways in the different sections (including as El-Nina??). Also, while the grammar in the Introduction is good, it is rather shaky in some of the other sections. I recommend a thorough reread for style and perhaps even professional

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proofreading.

And more specifically:

- page 6613, line 8: mention that there is an associated biogeochemical model in the abstract?
- page 6613, line 15: another complicating factor in the Indonesian Seas is the internal variability associated with ENSO
- page 6616, line 20: a bit more background on the GLS model would be good. At least a reference would be useful
- page 6618, line 16: what is the Global Ocean Forecasting System?
- page 6619: it would make sense to compare the surface currents to products such as OSCAR
- page 6619, line 23: Is there any evidence for these Kelvin waves in the ocean model fields? This would provide some interesting dynamics analysis
- page 6620, line 5: what exactly is meant with these 'high-frequency, oceanographic signals'?
- page 6620, line 10: I don't agree that EKE from INDO12 and AVISO agree everywhere except in coastal regions. E.g. east of the South China Sea around 20N the differences are quite large
- page 6621, line 13: How are 'low frequencies' defined here?
- page 6623, line 2: What is the transport through Torres Strait in the model? Van Sebille et al (2014, JGR) found that the Torres Strait transport is 1Sv in a 1/10 degree NEMO model, which is not insignificant
- page 6623, line 16: Van Sebille et al (2014, JGR) also discuss transport variability due to ENSO

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- page 6624, line 2: It would be good to elaborate a bit more why IOD will 'win' over ENSO? What is the evidence/mechanism for that?
- page 6630, line 20: what does this footnote mean? Why is it here? Where does it come from? How to interpret it, as a reader?
- page 6631, line 16: I don't agree that biases are consistent. This is not true north of 10N, as well as around 2N
- page 6631, line 25: Good to spell out/define RMSD?
- page 6632: I think these seasonal biases in SST, if discussed, should be shown somehow
- page 6632, line 14: This is a strange statement. Is the RMSD smaller or larger?
- page 6634, line 4: It is odd to say that a regional model is 'globally too warm' (same on page 6635, line 12). I would not use the word 'global' in this sense to prevent confusion
- page 6634, line 14: Just because it is the south of the model domain doesn't mean this is the South Indian Ocean
- page 6636, line 26: Is 'wiggles' a technical term?

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