



Interactive comment on “Evaluation of a near-global eddy-resolving ocean model” by P. R. Oke et al.

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Received and published: 6 March 2013

Response to reviewer’s comments on “Evaluation of a near-global eddy-resolving ocean model”, by Oke et al. (gmd-2012-113)

The reviewers’ comments are repeated below, followed by our responses in italics.

Reviewer 2:

The discussion paper describes the settings of a large-scale ocean general circulation model coupled to a biochemistry model. Physical parameters and chlorophyll are verified against global hydrography and satellite fields.

I am surprised that the model configuration is not state-of-the-art in ocean modeling,

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despite the fact that it is based on a modern model architecture. Without a sea-ice model (hence exclusion of the Arctic and important parts of the Southern Ocean) and the application of atmospheric fluxes the model only seems to represent a global, free run - but it isn’t. Even though the authors claim these deficits in the discussion, they show a series of comparisons that cannot be applied here. Before supporting the publication of the manuscript I would demand a more critical verification and discussion of the forecast capabilities. My detailed comments below may give some guidance.

RESPONSE: We thank the reviewer for these comments. They highlight an important shortcoming of the original paper. Namely, that we didn’t adequately describe the motivation for the development of this new model. The new model is the first step in the development of Australia’s next operational short-range ocean forecast system. The focus of this system is on the upper ocean (top 700 m) at the mid- and low-latitudes. With this motivation in mind, we think it is clear why we haven’t included the high-latitudes (eg Arctic), and why the addition of a sea-ice model is unnecessary (and quite a major undertaking in itself, involving a different research team, an additional model, the adoption of a coupler, assembly of different data streams, etc). Also, understanding that this is a staged development towards an operational forecast system explains why the reviewer’s “demand” for a “more critical verification and discussion of the forecast capabilities” is premature. Indeed, the development of a forecast system is quite a different undertaking to the development of a new model. The model is the first step in developing a forecast capability. The forecast capability includes many additional components, including a data assimilation system, observational data streams, etc. We are presenting results from the first stage of this multi-year development.

RESPONSE: We have included a paragraph in the introduction outlining the intended purpose of the new model, and we have included some additional statements about our motivation at the end of the abstract, including the following statements: “We conclude that the model output is suitable for broader analysis to better understand upper ocean dynamics and ocean variability at mid- and low latitudes. The new model is intended

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to underpin a future version of Australia's operational short-range ocean forecasting system."

(1) Most important for the verification is certainly the absence of a sea-ice model in combination with the application of the atmospheric forcing. Provided as heat and freshwater fluxes the model certainly tends to drift enormously in temperature and salinity, hence the needed strong restoring towards observed SST and SSS values. It remains vague and somewhat misleading between sections 2 and A3 what exactly is applied as restoring data. I would read that interannual varying Reynolds SSTs are used, but climatologically averaged CARS SSS. If that is correct, the comparisons with SST (Fig. 3) and NINO indices (Fig. 6) are strongly misleading since these quantities are mainly determined by the restoring term itself (10 days timescale!). In particular, it does not make sense to compare model SSTs with those fields that have been used for restoring. It is also of no surprise that SST is strongly damped, seen in RMS variations (Fig. 9) and seasonal cycle (Fig. 3).

RESPONSE (Re: absence of a sea-ice model): We refer the editor and reviewer to our comments above. Our focus is on short-range (1-day to 1-week) forecasting – not climate applications; and on the upper ocean at mid- and low-latitudes – not high latitudes (hence the exclusion of the Arctic).

RESPONSE (Re: applied restoring): We think there may be some confusion about the "strength" of the heat flux restoring. We have added some discussion of this to the revised paper that should clear this up. The new text follows: "The surface restoring term for temperature scales like $\rho c_p \Delta z_{\text{surf}} / \Delta t = 23 \text{ W m}^{-2} \text{ K}^{-1}$ ($\rho c_p = 4 \times 10^6 \text{ J K}^{-1} \text{ m}^{-3}$; $\Delta z_{\text{surf}} = 5 \text{ m}$ is vertical grid spacing at the surface; and $\Delta t = 10 \times 86400 \text{ s}$, is the restoring time-scale). Each time-step the impact of this restoring term is quickly spread over the surface mixed layer, "diluting" the impact of the restoring term by the ratio of $\Delta z_{\text{surf}} / \text{MLD}$ (where MLD is the mixed layer depth). So, if the MLD is 50 m (10 times Δz_{surf}) then the effective restoring time-scale is ten times greater than the prescribed time-scale."

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RESPONSE (Re: validity of the comparisons with the NINO indices): To quantify the degree to which the surface restoring term influences the model SST, in the revised paper we present a table of the mean plus/minus standard deviation of the area-averaged heat flux components (Table 1, below), including the restoring term, for the entire model domain and for each NINO region. This analysis demonstrates that the surface restoring term for temperature is small compared to most components of the heat flux. It is the smallest term in the global average – and it is either the smallest, or second smallest (term (after the sensible heat flux) in each of the NINO regions. This indicates that the agreement between the modelled and observed NINO indices is not just because of the restoring. It indicates that it is largely because of the ocean dynamics, and the model ocean's response to the other prescribed surface fluxes. We regard these comparisons between the observed and modelled NINO indices as a necessary step in evaluating the model. We expected this comparison to be favourable, but we do not expect readers to accept that this expectation is necessarily met without showing it explicitly. For this reason we have retained this comparison in the revised paper.

(2) The evaluation of the circulation is too sloppy, mainly due to the use of global maps. A closer look at Figs. 1 and 8 shows that the separations of Gulf Stream and Kuroshio are incorrect, a Northwest Corner in the North Atlantic is not visible, and that the path of Agulhas rings is too regular. Model resolution alone is not guarantee for a proper circulation, and an "excellent agreement" (p. 4316, l. 19) is incorrect. It would help to focus into some crucial areas, rather than to argue with global maps alone. The same applies to the MLD (Fig. 2): The excessive deep convections in the Labrador Sea and Weddell Sea are barely mentioned. Some more information (E.g., if the convection reaches unrealistically down to the bottom) and discussion on causes (missing ice model) and (most important) impacts (over/under-representation of Labrador Sea Water and Antarctic Bottom Water) is needed. Owing to the missing sea-ice model there is certainly a special treatment of the surface fluxes for cold temperatures (usually the fluxes are cut off below a certain threshold) - that should be mentioned in the appendix. In addition to a more careful and critical description of the overturning stream function

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(How strong is the drift of the Atlantic MOC over the course of the model run? What is the cause for the locally enhanced AABW cells at low latitudes?) the meridional heat transport is needed (given the strong deviations in zonally mean temperatures seen in Fig. 4).

RESPONSE: Although we note that it is a standard approach to present and compare global maps of model fields and observations, we have included regional maps in the revised paper, as the reviewer requests.

RESPONSE: We acknowledge that the reviewer is correct that the global maps don't show the details well enough. This is evident in the reviewer's comments. The reviewer suggests that the separation point of the Gulf Stream and Kuroshio Current is incorrect. In Figure 1 below, we include two panels from the new figures in the revised paper showing the RMS of the modelled and observed SLA. The regions of high RMS denote high eddy activity associated with the current's separation from the coast. The good agreement between the separation point in the model and the observations is now clearly evident here, and in the revised paper. We note that there are differences between the model and the observations. These differences are discussed at length in the revised paper.

RESPONSE (Re: MLD comparisons at high latitudes): We note that this is not the focus of the new model – as evident by the simple representation of forcing from sea-ice (restoring sub-surface properties). However, we also note that a detailed investigation of the MLD in the new model has been conducted by Schiller and Ridgway (2013; manuscript submitted to JGR). Their paper explores this in detail. We have referred to Schiller and Ridgway (2013; submitted). They note that: "... relatively large differences exist between model and observations at higher latitudes, notably in the Labrador Sea and close to the Antarctic continent where differences reach 100 m. These discrepancies can be partly explained by complex processes involving high-latitude deep convection, which are parameterized in our model and the lack of a sea-ice component in the model, leading to inaccurate MLDs in wintertime. However, these are also areas

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of scarce observations, both of in situ and ocean-atmosphere fluxes. The former influences the accuracy of the observations used for validation and the latter influences the surface fluxes which impact the MLD"

RESPONSE (re: Meridional overturning streamfunction comments): We show the meridional overturning streamfunction in the paper because we recognise that this is a key metric for any global model, nicely summarizing many aspects of the ocean circulation. However, the reviewer is asking here for analyses that are typical for a climate-scale model (e.g., a coarse-resolution model that is to be run for many centuries). The focus of our development is not on high-latitude climate-scale processes, so we regard the suggested analyses as inappropriate for this paper and have opted not to pursue them here.

(3) Volume transports provide a crucial verification. However, section 3.6 remains too vague; figures showing time series would help. Examples: - For the ACC transport a range between 144 and 176 Sv is given. Is that an interannual to decadal variability or rather (as often the case) a downward trend over the course of the integration due to a bad representation of the AABW formation? - There is a lengthy discussion on the INSTANT comparison, with separation into the individual straits. However, a total number for the ITF transport is not given.

RESPONSE: A new figure showing time series of the volume transports of the ACC and the ITF have been added to the revised paper. It is copied below in Figure 2. This new figure clearly shows that the model transports do not have any significant drift.

RESPONSE: An observational estimate for the ITF has been added to the revised paper. It shows good agreement with the total model transport.

(4) Appendix A3 states that "to avoid any significant drift in the deep ocean fields, the temperature and salinity are restored to CARS climatology below 2000 m: : " (p. 4325, l. 25). Does that apply to the closed northern/southern boundaries only? Or is that true for the global ocean? If so, the applicability of the T/S comparison in the deep ocean

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(Fig. 4) would even be more questionable.

RESPONSE: The deep restoring is for the entire ocean, as stated in the paper. The comparisons in Figure 3 are focussed on the upper ocean (note that the top 1000 m takes up $\frac{3}{4}$ of each panel; see the example repeated in Figure 3). But inclusion of the full depth is included for completeness, to provide the reader with an indication of the model performance at the depths of deep restoring.

Minor points:

- p. 4306, l. 1: "18-yr run" is misleading since the run was longer.

RESPONSE: changed to "last 18-years of a 32-year run"

- p. 4318, l. 1: The reference is wrong, Biastoch et al. (2009) used a 1/10_ model.

RESPONSE: Done

- p. 4319, l. 1: I do not agree with the statement that the measurement errors are dominating because of the low signal-to-noise ratio. Here, the application of the SST restoring seems to be the cause.

RESPONSE: The discussion to which the reviewer refers is of a panel (Figure 9b) showing a map of the RMS of observed SST (using AMSR-E). The observed RMS is not influenced by the model – so the SST restoring in the model cannot possibly be the cause. What we say here is that the observations (AMSR-E SST) are reported to have a measurement error of 0.42 degrees. So when the RMS of those observations is 0.5-0.7 degrees – the measurement error of the observations represents a significant fraction of the RMS.

- Table 1: The DiMarco reference is outdated, for the transport through the Mozambique Channel Van der Werf et al. (JGR 2010, doi:10.1029/2009JC005633 is a better reference and actually closer to the model values.

RESPONSE: This has been updated

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- Fig. 11 is wrong, shows the observed (= Fig. 12) instead of modeled chlorophyll

RESPONSE: Done

Interactive comment on Geosci. Model Dev. Discuss., 5, 4305, 2012.

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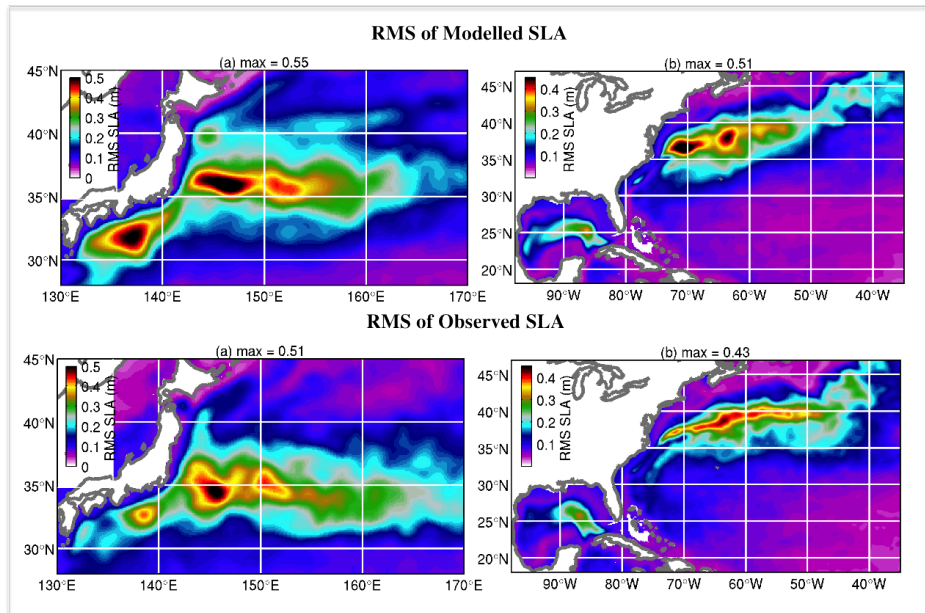


Fig. 1. Regional maps of the RMS of modelled (top) and observed (bottom) SLA in the Kuroshio (left) and Gulf Stream (right) regions.

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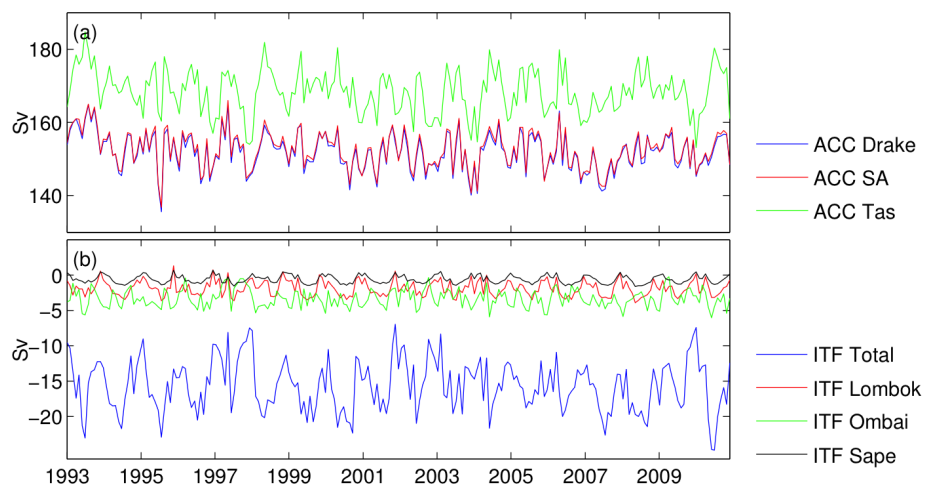


Fig. 2. Time series of the monthly averaged volume transports of the ACC and ITF. This Figure is the new Figure 7 of the revised paper.

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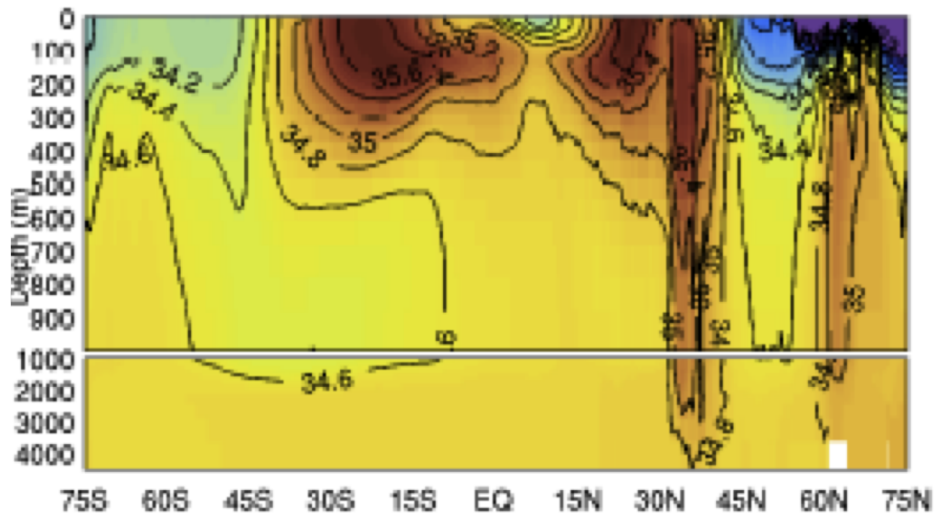


Fig. 3. Example of the zonally-averaged sections presented in the paper. This panel shows the zonally-averaged modelled salinity and is Figure 4a from the original and revised paper.

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Table 1. Time-mean plus/minus the standard deviation (W m^{-2}) of the area-averaged surface heat flux components for the full model domain (column 2), and for each of the NINO regions (columns 3-6) presented in Figure 6.

Heat flux component	Global	NINO1.2	NINO3	NINO4	NINO3.4
Total	4.5±14.4	63.0±40.1	70.8±28.2	32.5±21.9	55.6±27.9
Short-wave	178.4±16.5	233.6±28.4	244.5±14.4	234.3±21.0	247.9±17.8
Long-wave	-61.3±1.3	-49.9±8.5	-61.5±3.0	-63.9±4.1	-64.5±3.7
Latent	-104.0±2.4	-76.4±16.3	-97.8±18.6	-130.5±13.7	-118.0±19.4
Sensible	-13.5±0.8	-8.4±3.2	-6.5±1.3	-9.5±2.0	-7.0±1.5
Restoring	4.9±1.5	-35.9±16.1	-7.9±10.2	2.1±6.6	-2.8±7.6

Fig. 4.

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