Geoscientific

Model Development

Discussions

[GMDD]

Interactive

comment

Interactive comment on "East Australian Current region using the Regional Ocean Modelling System (ROMS 3.4) and Incremental Strong-Constraint 4-Dimensional Variational data assimilation (IS4D-Var)" *by* Colette Kerry et al.

Anonymous Referee #1

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General Comments:

This paper presents the results of an impressive suite of calculations using the ROMS variational data assimilation system to compute a series of ocean circulation estimates for the East Australia Current. The observational coverage appears to unprecedented, and the authors have apparently done a fairly rigorous job of tuning the system and evaluating the system performance.

Overall, this is a well written paper, although it suffers in a few places from being imprecise (detailed comments below).

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[Discussion paper]

Specific comments:

(1) Sections 3.2 and 3.3 In general I found sections 3.2 and 3.3 to be unsatisfying. To the 4dvar expert they are not really very illuminating, and to the non-expert they conveys no real useful information in that technical terms like "inner loops", "outer loops" and "cost function" are used with no useful context. I recommend that the authors rework these sections, perhaps adding an equation or two - this would help to clarify the text. More technical details could also go in an appendix. In contrast the authors devote a great deal of text and detail to the observations, but very little to the 4dvar machinery which is doing all the heavy lifting here.

(2) Section 4.1 This section discusses the consistency between the a priori specified error variances for the background and observations, and those diagnosed a posteriori from the innovation statistics following the methods introduced by Descroziers et al (2005). There is a problem with the language in this section that needs to cleared up. Throughout, the authors refer to "posterior errors" when what they really mean are the "diagnosed prior errors." The term "posterior errors" implies that these are the errors in the analysis, but that is not what is being computed here.

Detailed and technical comments:

Page 1, line 1: "inherently dynamic" - are all circulations inherently dynamic, not just the EAC?

Page 1, line 19: "model dynamics to determine covariance" - what do you mean by the statement?

Page 2, line 14: Reword to say "submesoscale and mesoscale eddies"

Page 2, line 16: You say that barotropic and baroclinic instabilities are unpredictable - this is not true in either the atmosphere or the ocean.

Page 2, line 26: "adjoint to compute covariance" - what do you mean here (and elsewhere)?

Page 2, line 29: "without requiring ensemble or long-run statistics" - this seems like a strange statement in this sentence.

Page 3, line 14: Why is High Frequency capitalized here?

Page 3, line 32: "eddying general circulation" - clumsy wording.

Page 5, line 6: "clamped in the baroclinic" - this does not make grammatical sense.

Page 5, lines 9 and 10: Why does the heat flux need to be consistent with BRAN3? BRAN3 is only being used at the open boundaries right?

Figure 3: Instead of plotting both the ROMS solution and CARS, why not plot ROMS and (ROMS-CARS). From the difference plots it will be clearer where the model is deficient and where it is doing well.

Page 6, lines 22-24: Does the 2 year free run start from the end of the 10 year free run?

Page 7, line 10: Reword as "... deviations OF THE MODEL from the observations..."

Page 7, line 11: Reword as "J comprises a term that represents the difference..."

Page 7, line 14: Reword as "covariance, and a term that penalizes...."

Page 7, line 34: "free within the known uncertainties in the system" - this does not make grammatical sense.

Page 8, line 8: Reword as "...INDICATED that for this MODEL configuration, the linear ..."

Page 8, line 10: Replace "feasible" with "reasonable" or "affordable"

Page 9: lines 13, 15 and elsewhere: You imply here, and elsewhere, that the model is capturing submesoscale variability. The submesoscale is generally viewed as being in the 1-10km scale range, so your model will not resolving the submesoscale. You should remove the references to the submesoscale circulation.

Page 9, line 23: Reword as "... observation ERROR VARIANCE for the assimilation is CHOSEN to BE the square..."

Page 9, line 29: "not resolved by the model" - I think you mean not resolved by the data

Page 11, line 23: "covariance" misspelled.

Page 13, line 17: lower than the model AND OBSERVATION prior

Page 14, line 19: Reword as "These climatological varinces provide..."

Page 14, line 22: You say here that because you have only estimates of the variances, the background covariance of each field is estimated as a diffusion operator. This is not the reason that covariances are estimated this way. I suggest you go back to the original papers on modeling covariances using diffusion equations and brush up on some of the ideas (e.g. balanced versus unbalanced flows, etc).

Page 14, lines 24 and 25: Moore et al (2011) is not appropriate reference - you should refer to the appropriate equations in the orginal paper by Desroziers et al (2005).

Page 15, line 4: The authors claim that they have generated a "near-optimum" minimization. This is most certainly not the case since the background and observation error covariances they use are very far from being the true error covariances. The authors should tone this down or remove it.

Page 15, line 13: Reword as $t_1, t_2, ... t_n$ instead as t_1-t_2 since this looks like the time difference.

Figure 12 and 13: You show only the complex correlations - what are the rms errors in the current speeds? It would interesting to know this also.

Page 18, line 18: Say "diagnosed" instead of "posterior" - see specific comment (2) above.

Figure 4: The light grey points are almost illegible (they are in legend).

All figure: The fonts are tooo small to legible in many cases (eg Fig. 3, Fig. 8, Fig. 9, Fig. 12). The legends in particular are difficult to read.

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