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Ocean Science Discussions

Interactive comment on "Study of the combined effects of data assimilation and grid nesting in ocean models – application to the Gulf of Lions" *by* L. Vandenbulcke et al.

Anonymous Referee #1

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The paper attempts to identify the benefits of assimilating in a 2-way nesting configuration as compared to the classic 1-way nesting where the coarse grid model forces the fine grid and does not receive feedback from it. Twin experiments are shown in the Mediterranean, with three grids (coarse covering all basin, intermediate covering the Northwestern Mediterranean, and the Gulf of Lions shelf). The assimilation method is a variant of reduced-order OI using model variability EOFs. Pseudo-observations are SST and sea-level anomaly along one track within the Gulf of Lions shelf domain. The paper convincingly shows the advantages of 2-way nesting in that context. In particular, it is shown the any corrections carried out in the coarse grid are not sufficient to improve the boundary conditions of the finer grids, while simultaneous assimilation in

the finer grid with 2-way nesting lead to much more satisfying results.

The overall quality is good to very good. Most of the paper is written in a very clear and concise form, although some improvements would be useful (see comments below). I would have wished to see a short historical section on previous two-way nesting attempts, a clearer statement of the data assimilation configuration, and perspectives in the conclusion as to how the assimilation system can be improved.

Specific comments:

page 298 line 25: unclear at this point whether the state vector and the EOFs are multigrid or calculated separately over each grid; in Fig.5, are (a,c,e) and (b,d,f) parts of the same EOF? – also in same paragraph would have been nice to know from the start that P is constant.

page 299 para 1: there are two limitations to be stated here, (1) P being constant, the errors will not be the errors of the day and you can expect the model not to fully cooperate, and (2) P being calculated from model variability comes back to say that the method used here seeks errors in the state space of the simulation at hand, which might not represent the difference between 'free' and 'control' which also contains responses to atmospheric perturbations

page 299 para 3: why don't you include currents in the state vector? some coastal processes are ageostrophic or semigeostrophic (e.g. Kelvin waves)

page 300 para 3: Random errors consistent with R should in all cases be added to the pseudo-observations – is this the case?

page 303 lines 8-10: "the corrections made...significantly" This seems to me as the main result of the paper. However the subsequent discussion is not so clear, in particular on lines 12-13 "these fields are already...covariances": why is that so? It is important to make the point here because your main conclusion depends on this.

Minor comments and typos:

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page 292 line 13: high resolution is an issue, but fine grid models usually also have different physics (free surface for instance, or different mixing schemes) – same comment page 293 line 1

page 292 line 21: "known initial conditions" and boundary conditions, as well as for reasons linked to inherent limits to predictability (turbulence)

section 1: missing brief historical overview of two-way nesting

page 293 line 25: canyons (missing "s")

page 294 line 8: Provençal (missing "ç")

page 294 lines 13-14: "In order...shelf break" this is not an exact statement the way it is written

page 295 line 3: do you mean k-epsilon?

page 296 line 24: what does this paper add over Barth et al.?

page 297 lines 3-4: "We use...climatology" in all grids?

page 297 line 22: "detaching" is not the right word for meanders which are part of the LPC current

section 2.4: Are internal Rossby radii in your simulation consistent with the estimates you give earlier?

page 298 line 11: "suppose that the" *error* "processes can be considered quasi-linear"

page 300 line 19: you say that the assimilation cycle is 2 days, but you don't say how often the observations occur – every 2 days too?

page 300 section 3: What the actual differences are between 'control' and 'free' is still unclear to me after rereading the text. (1) wrong ocean state, 23 jan instead of 30 jan in free run? (2) slightly perturbed atmospheric forcings? In Fig.10 there seems to be a

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natural tendency for free-control to decrease with time. Can you comment on this?

page 301 case 1 etc.: do you use the fine-grid part of the multigrid EOFs? or fine-grid EOFs? This is unclear. Then on line 23 you write "Since the EOFs have been calculated over the 3 grid together *too*", does *too* mean that it is done that way in all cases? Also for case 5 you do not mention if you use 1-way or 2-way nesting.

page 303 line 1: coherent -> consistent

page 303 line 2: "in the other 4 cases" According to me, only in case 3, since in the other cases P would only cover one grid anyway.

page 303 line 23: "the model causes new errors..." This I believe is due to using a constant P which is not representative of the errors of the day.

page 303 line 29 and page 318: free run missing on Fig.11.

page 304 line 19: The RRSQRT is not implemented as a filter here since the errors are not predicted. It is more like OI, and very close to Fixed SEEK.

page 309 middle panel: what bearing is 0° ?

page 310 right panel: a better choice in my view would have been to show the salinity in color, because of the Rhône plume.

Interactive comment on Ocean Sci. Discuss., 3, 291, 2006.