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Interactive comment on "ECCO version 4: an integrated framework for non-linear inverse modeling and global ocean state estimation" *by* G. Forget et al.

Anonymous Referee #2

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This contribution is appropriate to the journal, it reports significant new work and new concepts, and I recommend that it be accepted, although I ask for some minor revisions.

This contribution has two aspects.

The first is a noble effort to document a very complicated modeling and assimilation system so others can both understand and work with the output and even reproduce the runs. It provides a comprehensive (and mostly well-written) description of the model and state estimation framework, and it is hard to suggest much that has been left out or needs more explanation. I am not enough of an expert to challenge, or even deeply check, many of the detailed model equations, but I trust that they are correct.

The system is working, as the fits show, and the recent improvements to the system are very impressive. Their discussion of weaknesses is particularly interesting, and it is good they acknowledge that error analysis is a weakness. This is in contrast to the "competing" ROMS assimilation system, which has well-developed uncertainty tools, even though it is not automatically-generated. This sparks a comment that the only reference to ROMS is Shchepetkin and McWilliams for the modified Adams-Bashforth time step, but I guess that given the purpose of this contribution, it is not necessary to provide a survey of models.

The second aspect is a detailed analysis of many runs of the same assimilation system to look at the sensitivity to a wide variety of model changes: "structural" and "parametric". This is made possible by the impressive the modesty of the v4 computation requirements. I had somehow thought that this computation would be beyond reach, but it is not.

quoting from the text: "Advanced usage of ECCO v4 may include re-running forward model solutions (the state estimate in particular) or its adjoint. Computational requirements are modest –the 20 year forward model integration typically takes between 6 and 12 h on 96 processors."

Although the analysis here is welcome and very well done, there are a few things left out. A main question I would suggest be addressed is whether the adjustments of the mixing parameters are needed to compensate for the resolution of the model, in which case their spatial patterns may have little to do with real mixing. I agree that they are a very convenient way to allow model errors, and, combined with adjustment of fluxes at the ocean surface, presumably allow the elimination of biases in temperature or salinity. While these adjustments produce a large reduction in the misfit to hydrographic data, it would help the curious reader to have the author's discussion of the likelihood of compensating errors. Phrased another way, the inclusion of these controls moves the

ECCO framework closer to the "weak constraint 4D-Var" used elsewhere, including ROMS, where error (control) terms can be included in every model equation at every grid point and time. This can make it easy to fit observations by deprecating the model dynamics, so a cautious approach is needed.

In fact, the entire discussion of the model metrics responding to various controls and configurations could be made clearer. (and please write "Tables 3 and 8" instead of "Tables 3–8", which implies all the tables in between, too.) For example, is global mean temperature and salinity surface or total depth? If total depth, it's hard to see how mixing parameters could affect them much, since they redistribute the heat and the model is forced with fluxes. In addition, the conclusions drawn from the comparing the response magnitudes across different metrics were not convincing. I am especially skeptical of the bootstrap correlation calculations. So a bit more clarity here would help the dumber readers like me.

Detailed suggestions:

first, please spell-check the manuscript. e.g. "abscence" on page 3662. "estime", and so on.

also there are many mismatched plurals, e.g. "slow models drifts" on page 3681. or page 3693: "internal-waves dynamics" (should be: internal-wave dynamics)

Most of these are trivial, but can slow down the reading.

Finally, the Conclusion section seems to be a bit redundant in places, repeating "expert choices" when it doesn't seem necessary.

Abstract:

I found the following phrase confusing: "Both components are publicly available and highly integrated with the MITgcm" I suggest: "Both components are publicly available. The ECCO framework is highly integrated with the MITgcm"

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I also found the phrase "model-data constraints" to be confusing. I suggest "observational and model constraints", or something more specific.

In fact, the word "constraints" is often added to "observational" in the text, where "observations" alone might be better. Much of the time "observational constraints" is correct, referring to constraining the model to match observations, but sometimes it seems to just mean observations. There are many variations, e.g. "observed data constraints", and I suggest going through and regularizing the terminology so that it is always the same, and observations are just "observations".

for example on page 3684: "A permanent issue is the need for additional data constraints, particularly in the abyss (Wunsch and Heimbach, 2014)." This implies that the observations exist, but were not used as constraints. I don't think this is what is meant, however.

I suggest: "A permanent issue is the need for additional observations, particularly in the abyss (Wunsch and Heimbach, 2014)."

The description of the grid could be a little clearer: Page 3658: "Poleward of 57 N, LLC is topologically equivalent to CS minus one cube face (Fig. 1)." then, later: "Between the LL sector and the Arctic cap, the grid makes a gradual, conformal transition that is evident in Fig. 3 between 57 and 67 N."

I suggest these two sentences be put adjacent, and perhaps a little more detail on how the grid transitions from LL in a sector to something like the CS, if that is indeed what happens. And/or just move up the reference to Appendix A so it comes at the start instead of end, and the reader can look at that before puzzling about the short summary.

This also applies to the Southern Cap, where the text says: "To the South of 70 S, LLC is topologically equivalent to a pillow case that would have two vertices in each hemisphere (Fig. 2, right panel)."

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I first thought "hemisphere" meant east and west, and was confused, but they mean north and south; this could be rephrased just to say there are two vertices for the conformal grid on the southern cap and they are on land. No need for the pillow case.

just a comment: The downside of the specialized grid is the complication of the analysis (page 3656), and I would be surprised if the Matlab toolbox could eliminate that entirely, but it is a very important part of the package.

page 3665: the "C-D" scheme is referenced without defining it. Please move up the definition, or refer to the defining section.

Figure 4: caption for lower panels should be clarified. Is it streamfunction at the surface or at 2000m? The text should perhaps also say explicitly that the streamfunction is changed more by the C-D scheme than the customized viscosity, although the rms vertical velocity is also reduced more by the C-D scheme.

I was surprised that the v4 solution was forced directly by wind stress instead of a bulk formula. I had thought that the ECCO framework had previous specified atmospheric fields and let bulk formulae compute the fluxes. Is this the case? If so, does the new approach not take account of stress modifications by surface currents or temperature fronts? Please give a little more detail.

section 4, page 3668: J is a SQUARED distance from the obs. same comment on next page, section 4.1

figure 6 caption needs some editing for grammar (e.g. "bottom panel show" (add s), "all biweekly period" (add s))

in fact, all the captions could do with a little proof-reading. It is tedious to list the typos.

term: "regression" tests: This is not regression as I usually see the word. I would characterize these as checksum experiments, or reproducibility checks...? Maybe this could be explained a little.

I particularly like Appendix G: the "solution history" section, as that is often a dark secret in state estimation. The model, parameters, and observations evolve during the iterations, and so reproducibility is difficult or impossible. I applaud the authors for listing the details, as part of their documentation of the solution. This disclosure could be helpful to others attempting related projects.

page 3702: "Revision 4 iteration 10 consisted in a filtering of atmospheric control parameters adjustments to reduce irregularities in the forcing that had appeared during adjoint iterations."

I suggest rephrasing or adding details to be clearer: was it space and/or time filtering?

Interactive comment on Geosci. Model Dev. Discuss., 8, 3653, 2015.