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Ocean Science Discussions

Interactive comment on "On the observability of turbulent transport rates by Argo: supporting evidence from an inversion experiment" *by* G. Forget et al.

Anonymous Referee #1

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GENERAL COMMENTS I summarize this paper as follows:

Turbulent transport estimates may be inferred from observations of the large-scale hydrography, but may not be useful or meaningful because they often neglect crucial physics. Using inverse methods on GCMS and complimentary datasets can help, but the method itself remains unevaluated. This paper uses a state estimate inversion, augmented by the inclusion of Argo profiles, to estimate turbulent transport rates. It also addresses questions about the precision and generality of such an inversion.

A key finding is that the inferred diffusivities (GM, iso- and diapycnal) strongly improve the ocean stratification of the state estimate in relation to the in situ Argo profiles. The

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stratification in the upper ocean is shown to be highly sensitive to changes in the eddy transport coefficients. The new coefficients significantly improve model bias of passive, biogeochemical tracers. The state estimate is compared to similar, earlier estimates to gauge the robustness of the results, without providing a formal error estimate (which would be intractable).

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This work satisfies all requirements of a publishable manuscript as listed on the OSD peer review guide (http://www.ocean-science.net/peer_review/review_criteria.html). The paper as a whole is well written, and the authors provide a nice set of comparisons between new and old state estimates (and in situ data) to show where the Argo data improves things. The paper is straightforward to read and the figures are organized in a way that supports the main arguments well. Aside from a few points (see below) where I feel the authors could have refined their choice of words, I find this paper to be of high scientific and educational value. It does provide a significant improvement over previous work on the same theme, and for the most part justifies why the Argo profiles provide this improvement while remaining cautious about the overall limitations of the method.

SPECIFIC COMMENTS On pg. 1113, line 26, the authors claim that the large adjustments in the top 2000 m may be due to the inclusion of the Argo observations. I think this claim would be stronger if there was an additional figure showing the same plots from an inversion without Argo. Would the magnitude of the adjustments decrease substantially?

Pg. 1115, line 10: This statement is too strong, or perhaps "constrained" is not the right word here. The Argo profiles themselves are not constraining the oxygen minimum, though they do constrain the inversion for the diffusivities that affect the minimum (either directly through improved eddy transport rates, or indirectly through improvement of other physical variables that impact biogeochemistry).

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Pg. 1116, line 6: Another strongly worded statement. I'd change "is largely due" to "is noticeably improved by the use of".

TECHNICAL CORRECTIONS Pg. 1111, line 29: "reduces" should be "reduce". Pg. 1112, line 20: remove "and" Figure 1: I see that the same figure appears in Forget et al. (2015), but I think it could use more information in the caption here. Does the color scale refer to percentage differences? Pg. 1113, line 21: "energetic" should be "energetics" Pg. 1114, line 4: should be "two model integrations are carried out for 500 years" Pg. 1115, line 3: "parameters" should be "parameter" Pg. 1115, line 6: "maintaining" should be "maintenance" Pg. 1118, line 14: "defended observability proposition" is kind of strange wording and is vague. I think adding a sentence reviewing the "observability proposition" and stating it very clearly would help here. Pg. 1122, line 17: "coast of Antarctica" Pg. 1126, line 5: "variety of numerical models"

Interactive comment on Ocean Sci. Discuss., 12, 1107, 2015.