

Interactive comment on “Comparison of CO₂ fluxes estimated using atmospheric and oceanic inversions, and role of fluxes and their interannual variability in simulating atmospheric CO₂ concentrations” by P. K. Patra et al.

Anonymous Referee #2

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Patra et al. present a comprehensive comparison of CO₂ fluxes between the ocean and atmosphere, and terrestrial biosphere and atmosphere using time dependent inverse (TDI) models. They conclude that their approach of increasing the station constraints, and increasing the number of regions in the inverse along with using interannually varying meteorology (IAV) reproduces the interannual variability in CO₂ fluxes realistically.

The first referee provides a comprehensive review of the technical issues and comparisons. I am not an atmospheric modeler so I'll limit my comments to the perspective

of an observational ocean carbon investigator. The manuscript has the potential of being very useful for comparison with regional ocean CO₂ flux estimates but as written it tries to compare too many parameters and too many different approaches that the message gets diluted and confusing. The writing style is a bit too terse including frequent use of acronyms and recurring references. While the authors attribute their successful comparisons with the atmospheric flask network and the ocean inverse model to the TDI/TTM approach and IAV, the comparisons provided are not exact in that most models that are compared are run for other time periods. While the authors correctly quote the original sources on the robustness of the oceanic inverse there is a significant issue with a mismatch of integration time. By nature of the constraints of the ocean inverse, the solution is an ill constrained average of several decades. It is therefore unclear how applicable is it for comparison of time-varying fluxes, particularly if it is used as an indication of the proposed optimum.

I believe that this paper is a valuable contribution to the literature but I think it would be most useful to the audience that is not experts in atmospheric inverse modeling if it were simplified with fewer varying parameters and a clearer indication and how changing the parameterizations affect the outcomes. More attention should be placed on making comparisons with other work easier. This would include: 1. Reorganization of tables and clearer headers. For instance, in table 1 all boundaries of flux regions should be provided. The headers should clearly indicate which inverse is discussed. Table 1 heading is nearly incomprehensible to me. SMF06 and PKP05 appear to be ocean inverses but the global uptakes are very different from the 76/75 station atmospheric inverse that I thought was the best comparison. 2. Figure 3. Where are the results of the present study? The caption suggests they are the same as those of Rodenbeck (2003).

This paper has a lot of potential. However, as it stands it seems to neither fully satisfy the group of experts (see review R. Law) or those who would like to compare the results with their own regional observations.

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