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Ocean Science Discussions

Interactive comment on "Interannual response of global ocean hindcasts to a satellite-based correction of precipitation fluxes" *by* A. Storto et al.

Anonymous Referee #1

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The authors address the important issue of getting a suitable freshwater input for interannual OGCM experiments and, more precisely, a realistic daily precipitation field. Considering the ERA-Interim field, they propose a simple objective climatological satellite-based correction. They perform then impact assessment with experiments with and without the correction. They finally discuss the potential improvements of the method in terms of salinity impact, freshwater budget, sea surface height adjustment and volume and freshwater transport.

The paper is comprehensive and shows clearly the positive impact of the method on the salinity for instance. The part of the impact study which separates the baroclinic

and barotropic contribution is, as far as I know, an original work. But for my point of view, this paper is subject to major revision considering the followings issues : (1) the authors should explain why the amplitude of the correction (see comment in paragraph 4.1) is weak and still far from the satellite data value, (2) the impact of zeroing the EMP budget should be precisely quantified (see comment in paragraph 4.1), (3) we cannot discern if the various impacts in the Southern Ocean discussed in the paper are from the correction itself or from the EMP redistribution (see comment in paragraph 4.2).

For some quantities such as the current speed and some transport across sections, the weakness of the improvement compared to the bias raises the significance of the result.

Inconsistencies in some figures also prevent the reader to appreciate the discussion.

1. Introduction

The authors partly explain the choice of using the PMWC data because 'PMWC aims at closing the atmospheric hydrological cycle…', but the evaporation field is not used in OGCM experiments using bulk formulation, then this point can't be a criterion choice.

References: add the Dee et al. ('The ERA-Interim reanalysis: configuration and performance of the data assimilation system', 2011, QJRMS) paper for ERA-Interim reference. And also mention the more recent work of Brodeau et al. ('An ERA40-based atmospheric forcing for global ocean circulation models', 2009, Ocean Modelling) for example of atmospheric correction at global scale.

2. Correction of precipitation.

The linear interpolation of the corrective factor between two consecutive months may limit the day-to-day variability of the original ERA-Interim field. How the authors checked this interpolation doesn't alter the ERA-Interim daily variability? It is worth to calculate for example a variance before and after the correction, not necessarily into the paper but send a figure to the reviewers.

The authors mentioned the 'potential applicability in an operational framework' of the method but, by construction, this method needs to establish a climatology of the numerical weather prediction field. And it is known that due to the numerous releases of the real time atmospheric systems, such as the IFS ECMWF system for instance, the error is not constant in time. Unless we've miss understood the meanings, this method cannot be easily applied to the real time, the authors may not mention it.

Figure 2: the authors mentioned the Arctic (domain northward 60°N) and Antarctica (domain southward 60°S) areas but they are not visible in the figure.

3. Ocean model description

The NSIDC doesn't provide full initial conditions for the sea ice model, where the initial sea ice thickness comes from?

Add the following reference for a more detailed description of the physics : Barnier et al. ('Impact of partial steps and momentum advection schemes in a global ocean circulation model at eddy permitting resolution, 2006, Ocean Dynamics)

4. Impact on ocean state and variability

This chapter is the most important part in which authors are asked to clarify major issues. 4.1 Freshwater budget

For a better clarity, PMWC data (trend etc. . .) from Lagerloef et al. (2010) is worth to be mentioned in the Table2.

The corrected precipitation (13.487Sv) is surprisingly far (+10%) from the mean PMWC data (12.2Sv), this may explains largely the weak impact on the EMP imbalance before the zeroing redistribution. The authors should explain in more details why the corrected precipitation is not closest to the observation.

The global EMP is set to zero at each time step. Redistributing this value is then equivalent to put uniformly freshwater everywhere. Then the mass balance is equilibrated

ending with no SSH trend but, unless this redistribution is not applied in the concentration/dilution term of the salinity, this should also impact the SSS trend. Seeing the 'zonal' distribution bias of the ERA-Interim precipitation field, this will then reinforce the correction in the mid-latitudes and weakens it in the tropics for the salt. The authors should quantify the impact of this redistribution and remove it from their impact analysis. Although we understand this redistribution is applied in both experiments, the value of the redistribution is different. This is an important issue of the paper, the authors must clarify this point to my point of view.

4.2 Salinity and Temperature

See discussion above for the salinity.

The trend of the ERA-Interim precipitation field exhibits peculiar long-term trend related to the observations network (Dee et al., 2011) which is not in accordance with GPCP trend and the authors mentioned 'the correction . . . constantly moves the salinity bias closer to zero', as the correction is not designed to correct this long-term trend, does it mean the salinity trend can be realistic despite the unrealistic precipitation trend? This may not be true.

No changes are reported for the temperature except in the Antarctica where the SST is colder. The values of the correction coefficient southward 60°S (Figure 2) are not visible; is the correction plays a role in the stratification by preventing inadequate vertical mixing or is it due to the freshwater input from the EMP zeroing redistribution?

More generally, the values of the corrective coefficient in the Southern Ocean are null (Figure 2), this challenges clearly all the discussions made on the impact of the correction on the ACC results.

4.3 Sea level

'we will assume this remote effect to be negligible' : to be re-considered in the light of the discussion above.

The improvement mentioned in the text is not obvious in the Figure 5, middle and bottom figures seem to be the same, if so, discussing this paragraph is impossible.

Figure 6: The improvement concerning the anomalies is less clear than the clear one depicted for the RMSE at global scale. Again, viewing the very weak (nearly null) correction in the ACC (Figure 2), the result discussed for Figure 6 is subject to caution regarding the EMP redistribution issue

The approach to try to understand how the effect of the correction is distributed along the baroclinic (steric) and barotropic signals is simple and really comprehensive. Figure 8e) and 8f) are the same plots.

4.4 Circulation and Transport

Inconsistency between the Figures 9 and 10 in which the mean zonal speed has increased with the correction when compared to the experiment without the correction and has decreased in Figure 10.

The origin of the decrease of the surface speed in the ACC and, then, on the volume transport, has to be clarified in the light of the EMP zeroing redistribution.

Figure 9: Is the reduction of 4% error for the RMSE (global domain) significant especially when the mean bias is increased?

Figure 10: Scale of arrows needs to be increased and specified.

Table 2: Changes in transports through Bering and Fram straits are fairly weak compared to the mean value, are they significant in the scope of their variability? The variability of the transport has systematically increased when the correction is implemented, any comment on this increase?

Interactive comment on Ocean Sci. Discuss., 9, 611, 2012.