Response to Anonymous Referee #2
Manuscript "Interannual response of global ocean hindcasts to a satellite-based correction of precipitation fluxes" by A. Storto et al.

First of all we would like to thank the reviewer for the careful reading and the valuable comments that led us to a much improved and readable version of the manuscript.

Below we address the concerns that Anonymous Referee #2 (hereafter AR2) reported in his review, and provide, for each point, the modifications that were made in the revised version of the manuscript.

As noted also in the Response to Anonymous Referee #1 (hereafter AR1), many points are in common between the two Responses, which consequently should be considered simultaneously.

MAJOR POINTS

1) Need for zeroing E-P-R at each timestep.
AR2 wonders why this closure is needed. We mention in the Introduction that without this correction, the (globally averaged) sea-surface height (SSH) would exhibit an unrealistic drift of about 2.2 cm/year. Since one of the in-house climate applications of our ¼ degree ocean simulations is the study of the interannual variability of SSH, its causes and effects, we have decided to adopt this correction in order to avoid drifts in the globally averaged SSH. Furthermore, even if it is likely that the balance of the E-P-R occurs at scales of several years, we decided to adopt this strategy because i) an objectively tuned timescale for the E-P-R closure timescale is still not available and ii) because our model system, and the correction itself, was conceived primarily for reanalysis applications, as we are providing global ocean reanalysis at ¼ degree resolution within the MyOcean European project using the same model configuration as in the manuscript. Our data assimilation system (a previous version of which, at coarser resolution, is detailed in Storto et al., 2011) aims at assimilating altimetric observations along with in-situ profiles and space-borne observations of SST. In particular, if the model sea-level presents a drift, such as the one coming from an EMP imbalance, sea-level anomaly observations cannot be successfully assimilated as the observations minus model-equivalents will show a bias increasing with time. In the assimilation strategy (see again Storto et al. 2011), the basic idea is to firstly remove the SSH global averaged from the model field of SSH (representing the bias from EMP imbalance), and eventually remove either global average observations bias (representing MDT biases) or the along-track bias. Removing only the observation minus background bias was found inaccurate as it relies too much on the distribution of the observations (e.g. all the biases in polar and coastal regions are not taken into account), which is far to be homogeneous on the Global Ocean. For this reason, we always keep this correction in our experiments, even when no data assimilation is run.

Furthermore, we recall here the effects that neglecting the EMP imbalance would have on the global salinity trends.

The effect of the global freshwater flux on the global salinity S at time t can be approximated by:

$$S_{EMP}(t) = S_{EMP}(t-1) + \Delta t \frac{-EMP}{\rho_f} S(t-1) \frac{A}{V}$$

where EMP is in Kg m^{-2} s^{-1}, ρ_f is the freshwater density, Δt the timestep in s, and A and V are the area and volume of the ocean, respectively. The previous equation diagnoses the globally averaged salinity changes due to the EMP only.

By using this simple relation we can diagnose the effect of an EMP imbalance on the global salinity. After 21 years with an imbalance of 0.9 in the EMP - as in our second experiment, see the new Table 1 in the Response to AR1 – and starting from a salinity of 34.4 psu, the salinity would be 34.38469 (-0.77 E-03 psu/y). Although this may seem a very small decrease, it should be noted that it is about 15 times larger than the global salinity variation rate found by Durack and Wijffels (2010) from in-situ measurements. Therefore, without EMP redistribution, there would be a large and unrealistic model drift not only in the SSH but also in the salinity.

The effect of the EMP redistribution is discussed in the "Response to Anonymous Referee 1" and is not repeated here.

Given the need of having zero EMP to avoid artificial model drifts, it is important to note that zeroing the EMP as we do is equivalent to rescale the precipitation flux in order to have EMP equal to 0. This is preformed in all similar correction studies. It can be thought of as an adaptive constraint incorporated in the kinematic (SSH) and salt flux surface boundary conditions.

Large and Yeager (2009) found a smaller EMP imbalance by correcting atmospheric humidity and winds, but still they needed to arbitrarily rescale the precipitation to have zero EMP, which is conceptually the same procedure of the EMP redistribution, except for the fact that we perform it online within the model equations.

Similarly, Brodeau et al. (2006) re-calibrated subjectively the precipitation to have EMP equal to 0, namely subtracting the imbalance to the precipitation fields. A slightly different approach is used by Troccoli and Kallberg (2004), where the correction is time-varying and constrained to have EMP=0, but on the other hand their correction is arbitrarily limited to the 30S-30N and only latitude-dependent (ie it would fail in reducing the large bias in the Indonesian Throughflow) and also based on the GPCP climatology, namely while the correction is time-varying the reference data are still not time-varying. Troccoli and Kallberg (2004) (see their Table 1, here the values are converted in Sv from mm/day) found that the precipitation over the ocean for the period (1989-2001) moves from 15.57 Sv to 12.97 Sverdrups after their correction, thus resulting still overestimated. In other words, unless to have perfect precipitation fluxes and perfect air-sea fluxes calculation, EMP will result in an imbalance to be considered somehow.

Since the bulk parametrization was found to ameliorate the air-sea interactions with respect to prescribed fluxes, in particular by improving a lot the representation of the upper ocean temperature variability, we are confident in using this formulation. However, while the precipitation flux is an input field, yet the evaporation flux is free to vary depending on the upper ocean and near-surface air fields. There is therefore no control on the global EMP, given the fact that the evaporation field cannot be constrained, therefore the EMP redistribution acts as a simple constraint on the evaporation if (as with our correction) the precipitation is well specified.

In order to improve the understanding of the EMP redistribution, we have more exhaustively described the needs for the EMP zeroing in the revised manuscript, along with a better quantification of its impact (as detailed in the Response to AR1)

2) Construction of the equation 1.

One major concern of AR2 is the derivation of Equation 1. We firstly want to emphasize that the correction was defined arbitrary in the text and always referred to it as "empirical correction" or "bias reduction procedure" and never as "bias-correction" because there is no warranty from Equation 1 that the bias is actually corrected. A rigorous formulation of a statistical bias correction might be found, for instance, in Piani et al. (2010a, 2010b), and references therein, where the precipitation bias correction

procedure is defined such that the cumulative probability distribution function of the uncorrected values equals that of reference (observed) values, after a proper definition of the distribution function for the precipitation.

In our case, the derivation of the corrective coefficient is arbitrary and does not aim at preserving any statistical property of the precipitation dataset.

Thus, while Troccoli and Kallberg (2004) arbitrarily decided to subtract a given amount of precipitation from the initial ERA-40 dataset, and Brodeau et al. (2006) and Large and Yeager (2009) instead had chosen a multiplicative coefficient to amplify/reduce the ERA-40 initial values, we preferred to use an "exponential" form for the corrective coefficient in order to avoid discontinuities in the case of ERA-Interim or PMWC (or both) zero precipitation. Please note that the 20-year simulations at ¼ degree resolution are very expensive in terms of computational resources, so no other experiment with alternative formulation of the corrective coefficient can be taken into account with our actual resources. Although there is no theoretical evidence why an exponential form of the correction should be preferred to a difference or a ratio (except for the practical advantage of having continuous corrective coefficeint values), the new Figure 1b (Figure 2b of this Response) along with the new Table 1 (see the Response to AR1) clearly indicate that our empirical formulation succeeds in correcting the bias of ERA-Interim against PMWC. This basic demonstration was missing in the original version of the manuscript. Because of this, we are confident in saying that our "empirical" correction is able to remove the bias between ERA-Interim and PMWC.

We have rewritten some sentences to emphasize this concept.

Specific comments

Below we address the specific comments of AR2:

Page 613, line 1: "Usually, oceanographers delegate ". This is awkwardly presented.
Reformulated

Page 613, line 18: Sv is a measure of volume transport, as such its units are m3/s not m3.
Corrected, seconds were taken out by mistake

Page 613, line 19: What would be the river runoff derived from ERA-Interim? Would this be in a better balance with the E-P from ERA-Interim?
This would be a very interesting point to analyze. Unfortunately, it is not straight-forward to estimate ERA-Interim runoff due to the fact that there runoff values are given on land points, requiring a proper construction of a "coastal-point/runoff-active" mask. Results may therefore vary a lot.

In practice, calculating the ERA-Interim runoff on a "shoreline" mask yields a 1989-2009 time-mean runoff of about 0.2 Sv, which is clearly unrealistic.

This is due to the fact that for converting the river runoff of the IFS/ERA-Interim land scheme into river discharge data, a routing scheme must be used (Emanuel Dutra, ECMWF, personal communication), and in the future we plan to test the usage of the ERA-Interim river discharge data.

For the time being, we extended Table 1 in order to give a more complete view of the balances introduced by different datasets, e.g. we calculate the evaporation from ERA-Interim and compared with that from PMWC and our ocean model (see AR1 Response). However, in order to compute the freshwater balance, we have used for all datasets of P and E the climatological runoff dataset as used in the ocean model.

Page 614, line 28: What does "assimilation-blind" mean?

Corrected, we meant an ocean simulation without data assimilation.

Page 616, line 6: " allows us to apply the correction to any period ". This statement
has not been tested. Indeed, it is likely that, because of the inhomogeneity in the
atmospheric observational system, different periods would require different coefficients
(see e.g. Precipitation correction in the ERA-40 reanalysis,
ERA-40 Project Report Series, 13)

We agree with AR2 that the sentence might appear too generic. The idea behind that is that precipitation biases are essentially driven by atmospheric forecast model biases in the ITCZ and spin-up/spin-down problems in the atmospheric reanalysis (Janoviak et al. 2010), which might thought to be independent from the observing network. It should be proven at which extent this approximation holds. However, while ERA-40 precipitation biases were documented to be due to inaccuracies of the humidity analysis and the satellite radiances bias-correction schemes (Troccoli and Kallberg, 2004; Uppala et al., 2005) and present significant drifts, ERA-Interim shows a much more stable behavior and much smaller drifts, because of a much improved humidity analysis and the introduction of a variational bias correction for satellite radiances (Dee et al., 2011). However, remaining drifts are present in ERA-Interim, and were demonstrated to be caused by problems in the rain-affected SSM/I radiance assimilation (Dee et al. 2011). Note that SSM/I radiances are assimilated throughout the 1989-2009 period.

Since it is not possible to prove the applicability of the correction to any period (would it be valid in the 1979-1989 ERA-Interim extension period?), we have corrected the sentence by indicating that we assume that in the 1989-2009 study period precipitation drifts are less crucial than the systematic precipitation over-estimation in the Tropics for our applications.

This is in line with the fact that maximum drifts in the ocean precipitation are at the maximum of the order of 0.4 mm/day in 4 years (Figure 24 of Dee et al. 2011), while tropical bias reaches 3 mm/day (Figure 1 of our manuscript).

Page 616, lines 8-9: Not obvious why the correction would by construction not alter
inter-annual variability. The authors would need to prove this, by also stating what is
meant by inter-annual variability.

This sentence was not clear. For inter-annual variability we actually meant both the interannual trends and the year-to-year variability. For the former, we showed in Table 1 (revised version in Response to AR1) that the trends change very slightly only. For the year-to-year variability, please refer to Figure 1 of this Response, where the zonal means of the year-to-year standard deviation before and after the correction. The correction has the effect of reducing very slightly the values of the variability in proximity of the Equator, while keeping the same variability out of the Tropics. We anyway agree with AR2 that the sentence is too strong and needs better explanation, and that is what has been done in the revised version.

Page 616, line 10: There is no explanation of how such a relationship was derived. This
relationship is at the heart of this work and needs a much more detailed discussion
and justification. Also, it is unclear why an unconstrained approach is adopted. Later
though a fix needs to be introduced to force E-(P+R) to be zero globally. It would be
better to devise a correction that considers such a constraint in a more congruous way.

See the part about General Comments of this Response.

Page 616, second paragraph: Why not show lat-lon maps of the correction coefficient?
The zonally-averaged figure, though useful, is not as informative. For instance, are the
very small values south of 30S due to averaging or are they small all around at those

latitudes?
The time-mean correction factor strictly follows the precipitation difference given in Figure 1 of the manuscript, therefore we would prefer to keep the month-latitude plot that provides also information on the month-to-month variations of the correction. But we added in the revised version a Figure 1b that provides the difference with PMWC after the correction (see Figure 2 of this response). It confirms that the correction successfully reduces the bias. Here, for the reviewer (Figure 3 of this response), we attach the lon-lat map of the correction. It clearly shows that the correction is not effective at high latitudes, as explained in the Response to AR1.

Page 617, line 14: Forcing the E-(P+R) to zero at every time step seems much too strong? What's the physical rational for this? Also, how is the fix introduced in the model? And what is the size of these fixes?
See the part about General Comments of this Response.

Page 619, line 8: It is hardly surprising that the difference in precipitation is about 0.3 percent when the correction factor is of the order of 0.1-0.01 (albeit on zonal averages, Fig 2) and this is added to 1,000 (see 1,000+c in equation 2). It is unclear why the correction factors could not be larger, especially since the computation of the factor is not constrained (eq. 1).
Please see the new Table 1 attached to the Response to AR1, where corrected values are given and where the correction is found to decrease the ERA-Interim precipitation by a 9.3%.

Page 619, line 20: The reason for the 15x increase in the amplitude of the annual cycle (0.032 Sv to 0.484 Sv) needs to be better explained.
Please refer to the recalculated values (new Table 1 in the Response to AR1). With the corrected computation, the amplitude of the annual cycle doubles from 0.033 to 0.066, passing from 15x to 2x.

Page 619, line 27: What is meant by "remote effect"?
All the comments are reformulated.

Page 619, line 29: It is unclear why such an improvement could not be achieved with this work.
Reformulated, however we implicitly meant that usually any correction approach has its own advantages and disadvantages. The motivation of this work was correcting the precipitation to remove the tropical fresh bias, not achieving the freshwater balance. This concept has been better underlined in the revised version (Introduction).

Page 620, line 4: When assessing the impact of the correction using independent datasets, it should be noted that the other variables (e.g. SSS) may not be in balance with the observed (or corrected) precipitation.
Unfortunately, it is not clear to us what AR1 means with this comment. We guess he refers to the effect that the EMP redistribution has on the experiment with the precipitation correction. This is discussed here and in the Response to AR1 (see e.g. the discussion about the effect on the ACC) and better deepened in the revised version of the manuscript.

Page 621, line 11: "may induce many secondary effects". What are these effects?
Reformulated by explicating potential impact on thermo-haline circulation.

Page 622, line 6: I do not think "clearly" is justified here.

This was because the panel of Figure 5c was by mistake the same of 5b.

Page 622, line 6: "... improves ... interannual sea level variability ...". Figure 5 shows
the sea level linear trend, which is not the same as interannual variability (related to
the year-to-year variance).
Corrected.

Page 626, line 9: The statement of a "4% error decrease" (and later 9%) is ambiguous.
It should be clarified that a reduction in RMSE is a reflection of change in variability not
in mean bias. Note also that the global mean actually increases (Fig 9a).
We corrected the sentence by stating what is meant by 4% error decrease (with respect to the control experiment), and mentioning that the bias is not reduced. Note also that since

$$RMSE^2 = BIAS^2 + St.Dev.^2$$

with
$$St.Dev.^2 = \langle [(mod - obs) - BIAS]^2 \rangle$$

the RMSE includes both the bias and the variability of the misfits between the model fields and the verifying observations, thus if the bias slightly increases but the RMSE decreases, the variability of the differences between model and observations is largely reduced.
We modified the sentences to account for these comments and about the significance of these results, as also noted by AR1.

Page 629, line 26: "The correction also yields a 16% reduction ...". This was achieved
by construction of equation 1.
According to the new Table 1, these comments have been rewritten.

Page 630, line 1: "the remote effect of the superimposed ...". Unclear.
Reformulated

Page 630, line 4: "One of the most appreciable ...". This is in spite of having what appears to be a
negligible correction factor south of 30S. Please explain.t
Comments on the impact on the ACC are reformulated as explained in the Response to AR1.

Page 630, line 9: As mentioned above a more advanced precipitation correction approach should be
the focus of this work.
Removed.

Page 630, line 11: "since the main motivation ...". Unclear.
Reformulated

Figure 1: "difference" instead of "bias". It would be useful to compute and overplot global statistics
such as mean and standard deviation.
Corrected.

Figure 3: Specify averaging period.
Corrected

Figure 4: "difference (cm) due" instead of "decrease (cm) due". It would be useful to compute and overplot the mean difference in Fig 4a. Specify averaging period.
Corrected

Figure 5: Mid and bottom panels look the same.
Corrected (see the figures posted in the Response to AR1)

Figure 6: Difference between corrected and non-corrected sea level over the Antarcticregion is noticeable only for the first three years (bottom left).
As also requested by AR1, we have better discussed this Figure and the implications of the precipitation correction and the different EMP redistribution on the SSH. Note also that in Figure 6 we compare the sea-level anomalies, not the SSH, ie point-by-point we subtract the time mean SSH to the complete timeseries. This is done for comparison with observed SLA that are by definition referenced to a mean ocean topography (originally to the 1993-1999 period for convention by AVISO, referenced to the 1993-2009 in the comparison by us). The time-mean values of the two experiments are different; this means that a change that seems more marked in the first years does not necessarily mean that the variations between the experiments are confined to those first years.
For sake of clarity, we have substituted and commented accordingly Figure 6 with a new Figure where the left panels show the basin-average SSH rather than SLA (Figure 4 of this Response). Thus, the AVISO is kept for the RMSE only, and the plot confirms our previous statement.

Figure 8: Panel (f) is the same as (f).
Corrected (as asked also by AR1).

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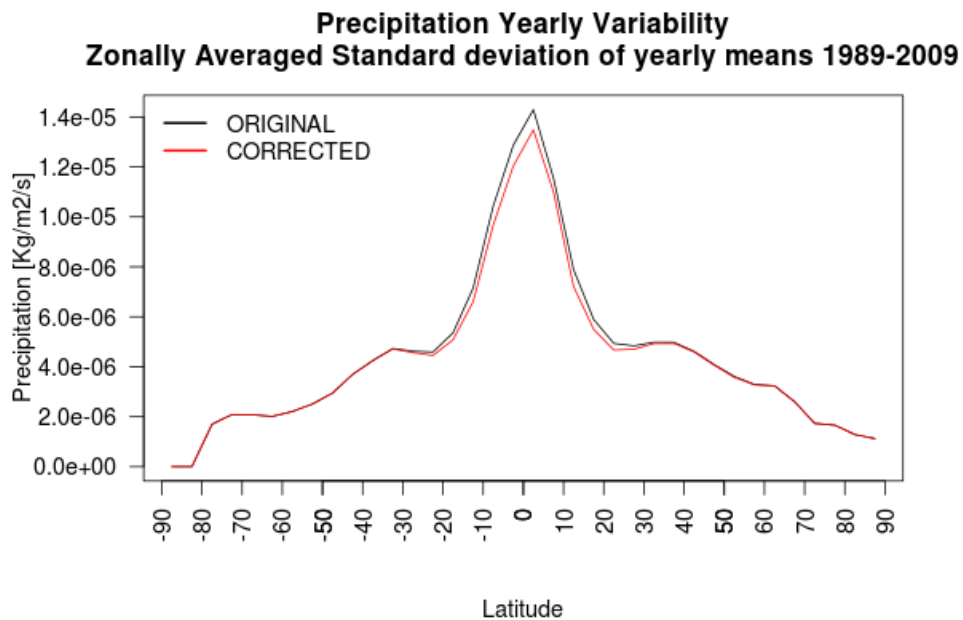
Figure 1. Zonal Averages of the year-to-year variability of the precipitation without and with the application of the precipitation correction.
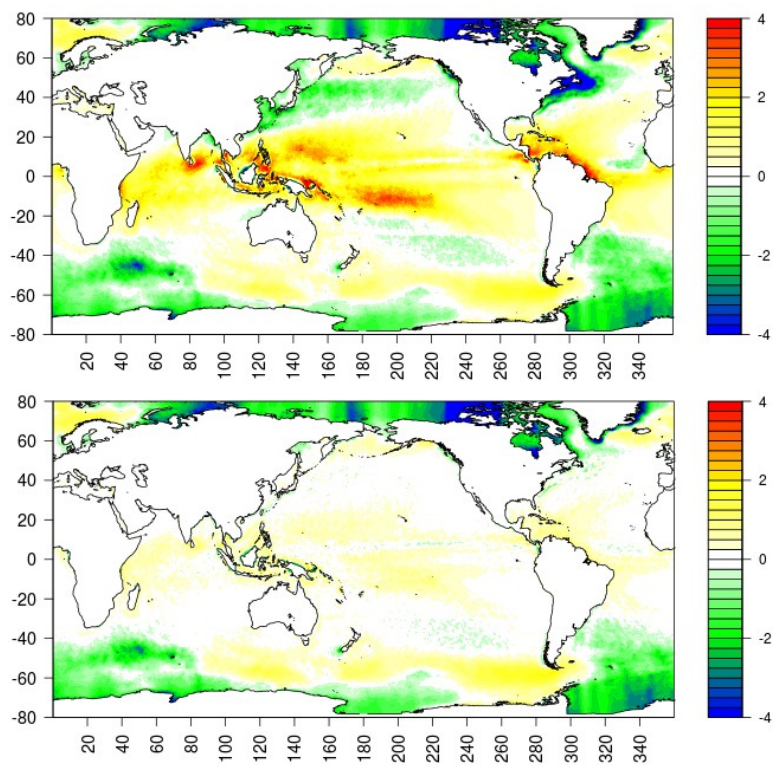

Figure 2. Difference between ERA-Interim and PMWC precipitation (top) and between corrected ERA-Interim and PMWC (bottom) for the period 1989–2008 in mmd−1.
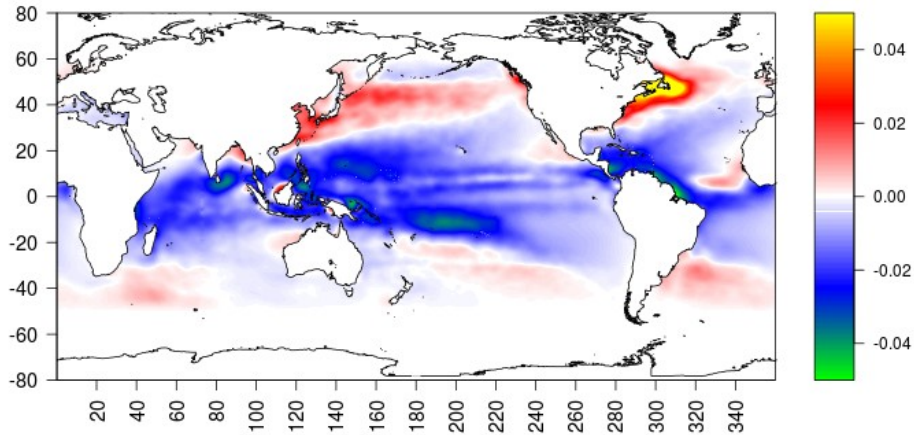
Figure 3. Time-mean precipitation corrective coefficient. Definition is given in Equations 1 and 2 of the manuscript.
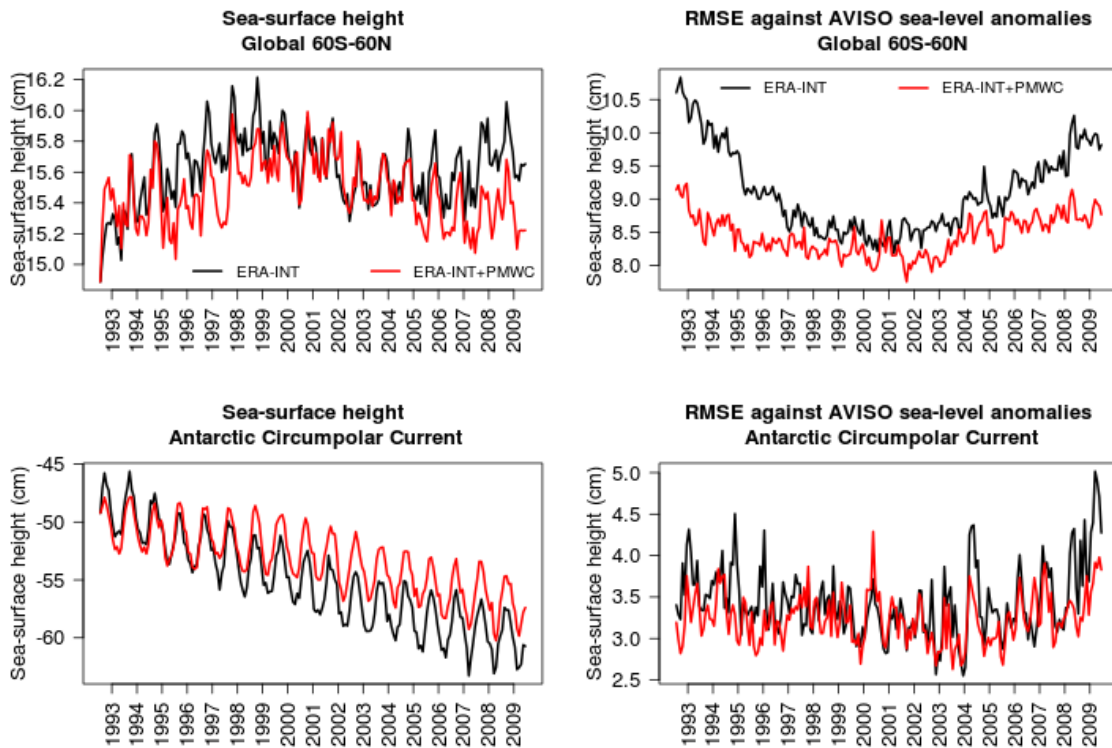

Figure 4. Basin averaged sea surface height timeseries (left panles) and RMSE against the AVISO monthly gridded altimetric data (right panels) for both the Global Ocean (60S-60N) and the ACC.