

Interactive comment on “Intercomparison of freshwater fluxes over ocean and investigations into water budget closure” by Marloes Gutenstein et al.

Anonymous Referee #2

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Review of “Intercomparison of freshwater fluxes over ocean and investigations into water budget closure” by M. Gutenstein et al.

This is a nice intercomparison study of various satellite-based precipitation and evaporation products and ERA5. The authors show that there is a large spread among the different products, and most of them fail to satisfy global budget constraints, especially when combining different products for estimation of P, E, and the transports. ERA5 performs best with a remarkably good agreement of forecast-based fluxes (P and E) and analysis-based transports (moisture flux divergence). The paper is well-structured and –written and therefore easy to follow. My only major comment is on the obvious

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error in the computation of ERA5 moisture transports as detailed below. Moreover, the reader would probably like to see stronger conclusions. I know it is hard to make a ranking, but e.g. SEAFLEX with its clearly unphysical P-E over ocean could be ruled out as clearly unrealistic. Also HOAPS appears to be a bit of an outlier, especially in terms of variability. The budget constraints are an objective measure to rule out poor data, and this helps to better constrain the best estimate of the water budget, without unnecessary inflation of the error bars. Otherwise I only have a number of minor comments.

Major comment: The authors use VIMD from ERA5 to compute ocean-to-land moisture transports. For this it should not matter whether one integrates VIMD over all land points or over all ocean points. Another constraint is that the global average of VIMD must be zero. This is a mathematical constraint independent of data quality. However, the authors obtain inconsistent results for land and ocean integrals of VIMD (table 4). So either the archived VIMD fields are flawed (which I doubt as I am using the ERA5 data myself and could not find a similar problem) or there is some error in the author’s processing chain that leads to these erroneous results. If this problem is really due to interpolation errors, as suspected by the authors, these interpolation errors are clearly unacceptably large. In short, this error must be corrected.

Minor comments: Equation 1 and everywhere else: as it stands, the VIMD term looks like the moisture gradient. I suggest to replace with the more appropriate $\nabla \cdot (vQ)$. L17: I presume you use monthly values. Please say it clearly, as the correlation strongly depends on the considered timescales. L37: The term “model reanalysis” seems an uncommon term to me. I suggest to drop “model”. If you want to give an attribute, it may be better to say “climate reanalysis” or “dynamical reanalysis”. L46: Isn’t there an author on the GPCP document? L58: “moisture divergence” is sloppy terminology. Moisture itself cannot diverge. It should be “moisture flux divergence”. L58: another nitpicky comment: VIMD is technically not identical with advection, although it is an excellent approximation of it. I suggest a slightly more cautious wording. L85-86:

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“model runs” sounds it does not use any observational info. Simply say it consists of ten ensemble members. L108: L_e is usually called “latent heat of evaporation”. L133: Do you mean SST averaged over the top 0.5m? Please clarify. L183-184: The statement about forecast skill is hard to understand for a non-expert. L184: “model runs” → see comment above. L184: It should be mentioned that the ERA5 ensemble members have a lower resolution than the stated 30km. How would the results change when using the high-resolution ERA5 data? L189: I suggest to delete “on single levels” L192: It should be noted that monthly P and E from ERA5 is averaged from short-term forecasts (12 or 24 hours? Needs to be clarified as well!) L198: If the TCWV tendency is computed from monthly means (rather than instantaneous values at beginning and end of the month), one should use centered differences. L243: When deriving E from monthly Q fields, what is the error from neglect of sub-monthly covariance between E and SST? L258: I suggest to replace “relative” with “area-specific” and “total” with “area-integrated”. L260: Please clarify how sea ice is treated in general. I presume it is masked out? Is this a seasonally varying mask? L273: “at the ITCZ” maybe better “in the ITCZ”? L280: Figure 2: One could make the simple statement that HOAPS differences are generally larger (RMS values of the field would be useful), but areas of disagreement are smaller because of the larger uncertainties. L285: similarly → similar L317: In terms of flow of reading, it may be better to move the sentence about ENSO correlation further down to around L340. Figure 3 and in general: I suggest to change the panel labelling to small letters, as capital letters have potential for confusion, especially “E”. Best would be E → (e) Figure 3: It would be interesting to see the ENSO correlation for every curve. This could be given in the legend, ideally with the lag at which the maximum correlation occurs. L319: Is “bias” the right term? We see differences, but still one of the datasets could be unbiased. L337: “biased low”. SEAFUX seems to be low in general (according to the mean annual cycle figure). So better to change to something like “particularly low”. L373: Is there a reference for the statement on detection of snow in HOAPS? L389: remove “are” L413: This statement would be correct if VIMD was the 3D-divergence, i.e. including fluxes at top of the atmosphere, where there theoretically

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could be an exchange with space. However, your VIMD is 2D and its global average is 0 according to the sentence of Gauss. L415: See my major comment. L503: I think it should be “right-most” L575-576: Please provide a reference for this statement. L586: This statement is on “observation-based attempts”. Please clarify. Figure 4: middle and right columns: Would it be possible to use color schemes that are really white in the middle? Table 4: How is runoff from ERA5 obtained? Is this the area-integral of all grid point values?

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