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Hydrology and Earth System Sciences
Discussions

Interactive comment on "ERA-Interim/Land: a global land water resources dataset" *by* G. Balsamo et al.

Anonymous Referee #4

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The paper describes a ∼32-year, global land reanalysis dataset produced by ECMWF and based on their ERA-Interim reanalysis. The land reanalysis dataset, ERA-Interim/Land, benefits from an improved land surface model (similar to that currently used in the ECMWF global NWP analysis system) and from precipitation corrections based on GPCPv2.1 observations. The authors validate ERA-Interim/Land with an array of independent data based on in situ measurements, remote sensing observations, and analysis data products. The authors find that generally ERA-Interim/Land outperforms the land component of ERA-Interim.

The paper is of interest to HESS readers and discusses an important new data set that will be used by many for a variety of purposes. The paper would therefore be suitable for publication in HESS. However, there are severe shortcomings that need

to be addressed before the paper can be published in HESS. Most importantly, the discussion must make it much clearer whether the improvements in ERA-Interim/Land over ERA-Interim are due to the improved model physics and/or the precipitation corrections. Moreover, it would be useful for readers and data users to get a sense of the performance of ERA-Interim/Land compared to that of other land reanalysis datasets such as MERRA-Land. There are also a number of issues in the presentation of the material. See comments below for more details.

I recommend that the paper be returned to the authors for MAJOR revisions.

Major comments:

1) Throughout the paper, the authors only discuss two datasets, ERA-Interim/Land and ERA-Interim. By comparing only those two datasets, it is never really clear whether the differences between ERA-Interim/Land and ERA-Interim are due to the changes in the land surface model or whether they are due to changes in the precipitation forcing (GPCPv2.1 corrections). In many cases, the text just states that differences are due to model changes (eg., page 14716, lines 8, 10; page 14717, lines 5-7) when it is not clear at all what role the changes in the precipitation forcing might play. Ideally, this could be resolved by adding a third dataset, say HTESSEL forced with ERA-Interim (without precipitation corrections). Such a dataset was analyzed in Albergel et al (2013) (see page 14722, lines 4-6). Why isn't the skill of those data included in the present paper?

2) For a user in search of a global land reanalysis dataset suitable for their application, it would be tremendously helpful to include other available datasets in the comparison. Primarily, I would think that this should include MERRA-Land.

3) The title and the last sentence of the abstract mention "water resources". While soil moisture and SWE can be considered water resources, the perhaps more common understanding would also include lakes and reservoirs as well as groundwater. The latter components are not part of ERA-Interim/Land, however, and I think the term "water resources" is not appropriate here. How about "a global land surface reanalysis

dataset"?

4) Section 2.1.4, Fig 9, Table 3: Why is the validation vs. in situ soil moisture observations limited to a single year (2010)? Many of the in situ datasets cover much longer periods that should be used for validation.

5) Figure 2: If the underlying distribution is based on the 32 values for January 1 (or July 1) 1979-2010, then the 95th percentile is essentially the same as the second-largest value (because of the granularity of the distribution). It does not make sense to me to derive the "95th percentile" from just 32 values.

6) Eg., page 14717, lines 1-2; page 14720, lines 18-20; caption of Fig 11: It is not always clear whether the "ERA-Interim" data that are analyzed here are from the original ERA-Interim dataset (derived with the coupled atmosphere-land modeling and assimilation system) or from offline simulations of TESSEL with ERA-Interim forcing. The latter should be very similar to the original ERA-Interim dataset, but it cannot be identical. At the very least, the difference needs to be mentioned clearly. The authors should also state how different the two "ERA-Interim" dataset are.

7) page 14718, line 21 - page 14719, line 2 and Figure 7: While the improvement in soil moisture at this particular site is impressive, it is a bit misleading since on average, soil moisture from ERA-Interim/Land does *not* appear to be much improved w.r.t. soil moisture from ERA-Interim, except perhaps in the variability (Table 3, Fig 8). Per Table 3, for a number of networks and metrics, ERA-Interim actually outperforms ERA-Interim/Land. It would be useful to understand why that is.

8) Figure 9 includes confidence intervals and nicely demonstrates that for soil moisture, ERA-Interim/Land and ERA-Interim have essentially the same skill in terms of R. The same information is not available for other comparisons, e.g., Table 2, Fig 5, Fig 6, where it is not clear whether the skill differences between ERA-Interim/Land and ERA-Interim are significant.

Minor comments:

a) Abstract line 9: Replace "makes it suitable" with "makes it more suitable" or "makes it potentially suitable". There is no proof that ERA-Interim/Land is in fact suitable for climate studies.

b) Abstract lines 10-13: This sentence is very generic. Please rewrite and provide the most important quantitative results.

c) Page 14709, line 22: replace "present" with "are subject to"

d) Page 14709, line 23: replace "effects," with "effects on the offline land surface simulation,"

e) I do not understand the caption of Figure 1. Is the top panel for SWE on 1 January and the bottom panel for soil moisture on 1 July? I guess the "(a)" and "(b)" are in the wrong places.

f) I do not understand Figure 6 and page 14717, lines 16-17): If the y-axis shows the cdf, then a horizontal line through y=0.5 should intersect the dotted blue line where the correlation is at the median value (x-axis). Similarly for the red line. But then the fact that the dotted blue line is "above" the red line would suggest that ERA-Interim has higher skill. Put differently, a cdf that rises more slowly has fewer low (correlation) values. I am getting this wrong? Or is the figure mislabeled? Also, why do some of the cdfs not end up at y=1 (for x=1)??

g) page 14718, lines 1-2: Perhaps mention that deeper soil layers/groundwater is also harder to verify because of the lack of suitable observations at the global scale?

h) page 14718, lines 11-20. Are the RMSD values cited here quoted from Albergel et al 2013, or are they for the datasets analyzed here? Please rephrase to clarify the text.

i) page 14720, lines 15 and 27: The best value for FCA is 1, but below FCA values are 80, 86, 76, and 83. I suppose the latter are in "percent"?

j) Figure 12 should show only land north of 45N. Why include the Sahara desert in a discussion of snow?

k) page 14722: "reducing the bias over [the] US" Bias of what? Please clarify.

l) Table 3: Metric "E" (header of last column) should be tied to the "centered RMSD" in the caption. (My best guess for the meaning of E.)

m) Fig 3,4: The axis labels are too small and essentially unreadable.

n) Fig 8: Are the "black solid curve" and "black dots" labels in the caption switched by accident? I would have expected the RMSD values to be the "black dots".

o) Fig 10, caption, 2nd sentence: "Circles are for the operational..." This seems to have been copied verbatim (and erroneously) from another paper or report. Also, replace "delta" with "inverted triangle"?

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