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Hydrology and Earth System Sciences
Discussions

Interactive comment on "MSWEP: 3-hourly 0.25 global gridded precipitation (1979–2015) by merging gauge, satellite, and reanalysis data" *by* H. E. Beck et al.

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The manuscript describes a newly developed global precipitation data, that takes advantage of existing products and uses a weighing approach to merge that into on consistent product. The manuscript is well written and I think the authors did a great job in creating this new MSWEP product. Nonetheless, I have some comments that I think should be thought about and or addressed in a revised version of the manuscript before publication.

Major comments

Figure 1, If I understand correctly, the authors use the monthly performance weights

for the daily and 3-hourly merging? It is unclear if the authors first compute 3-day precipitation timeseries and then compute 1 annual correlation values between those 3-day precipitation values and the observations or that the 3-day precipitation timeseries are used to compute an annual estimate of the correlations which is dependent on the day of the year (i.e. varies throughout the year).

In addition to the previous comment, if annual correlations are used or monthly weights are computed as indicated in Figure 1 this will have an impact on the daily merging. I can imagine that some products better capture the 3-hourly variability than the 3-day precipitation average. For example, some of the latest satellite precipitation products might be in this latter category, while I think a product like WFDEI, does a better job in monthly totals. A product like WFDEI is more heavily bias corrected and other observations are assimilated into the product (e.g. soil moisture). Therefore, WFDEI will get a high weight from the monthly analysis, while on the 3-hourly resolution the performance might not be as good as for a satellite product.

Why do the authors perform a product validation at 0.5 degree? I understand that the gauge density might be low at 0.25 and that more erroneous observations might be included, however, that is the resolution that product is going to be used at by users. I think including a validation at that resolution might prove valuable, if not in the main manuscript, maybe in a supplement.

Why did the authors select this merging procedure and not a more standard Bayesian methods where the errors between the products are weighted and their cross-correlation is taken into account. I might have missed it, but reading the manuscript, I come to the impression that the cross-correlation in the errors between the different products is not taken into account. I think that a large part of the errors in most of the product show a strong cross-correlation, which could be exploited in the merging of the products. That could further strengthen the added value of the MSWEP product compared to the existing data.

One thing that I missed after reading the manuscript is the development of an uncertainty product. The authors have P anomalies from all products and they have the weights, so they can indicate an uncertainty on their product. This would significantly strengthen the MSWEP product, especially in terms of ensemble modelling. Many studies just make assumptions on the uncertainty of a precipitation dataset when they use them for their modelling studies, while the authors are in the position to actually provide this valuable information to the reader and data user. I understand this might be some undertaking and too much for this manuscript, but I think it could be a valuable addition in the future.

Minor comments

Table 1 PRISM is missing from the table

I feel it would be good to elaborate a bit more on the merging in the manuscript, the assumptions made here are very important for the final product. Why is this method chosen over others etc.

How is the performance of the product over mountainous regions, and more specific the Hindu Kush – Himalaya region? Immerzeel et al. (2015) showed in a recent study that most the annual totals of a selection precipitation datasets does not even match the annual discharge (Q > P), which indicates some severe biases in the products. Is it possible with MSWEP to correct for these biases or would MSWEP suffer from the same problems? No HBV validation has been done in this region, while it is a region of major importance with regard to water demand, availability etc. Why not perform a quick check to see if Q < P (long-term average to excluding changes in storage) for most of the GRDC stations and see if the annual totals could at least account for the observed discharge. For some of the original products, this would definitely not be the case. This makes me curious to see if MSWEP can overcome that problem.

Reference: Immerzeel, W. W., Wanders, N., Lutz, A. F., Shea, J. M., and Bierkens, M. F. P.: Reconciling high-altitude precipitation in the upper Indus basin with glacier mass

balances and runoff, Hydrol. Earth Syst. Sci., 19, 4673-4687, doi:10.5194/hess-19-4673-2015, 2015.

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