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Hydrology and Earth System Sciences Discussions

Interactive comment on "Evaluation of areal precipitation estimates based on downscaled reanalysis and station data by hydrological modelling" *by* D. Duethmann et al.

Anonymous Referee #2

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Review of "Evaluation of areal precipitation estimates based on downscaled reanalysis and station data by hydrological modelling" by Duethmann et al.

This paper compares different ways of estimating areal precipitation from a number of precipitation sources and states that the methodology is especially useful for data-sparse areas. I agree with the authors that these kind of analysis are important, but there are major flaws in the methods used, and I recommend a major revision.

Major comments

1.The Introduction is very long and can be shortened substantially. For example,

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Discussion Paper

the section on orography effects on precipitation is too long and detailed, and it also lacks references. For example, there is no mention of the all the regional climate models that downscaled ERA40 data (van der Linden and Mitchell, 2009, http://ensemblesrt3.dmi.dk/). Also, a paper should not have more than one main aim. The main aim of this paper is to develop and test methods to interpolate station data with the aid of downscaled reanalysis. The hydrological modelling is an application and evaluation of the data. However, I would urge the authors to stress more in the introduction what is the main purpose of the paper and why it is important. What is the novelty of the methods?

2. You state in section 2.3 that a direct comparison between station data and model output from models are not comparable, but you still carry out the comparison. What does this bring to the analysis? Is there any value in such a comparison? In this comparison, I would also suggest to look at the distribution of the data series, as well as linear trends and anomaly correlations.

3. The discussion on using hydrological model to assess the quality of precipitation (2.4.1) is not clear to me. What is meant by the statement: "precipitation data set with larger differences from the true precipitation might lead to lower mean deviations between simulated and observed flow and would therefore be classified as the better one." And also the following statement on systematic errors is very strange. Monte Carlo calibration of model parameters using different data sources is a good way of detecting systematic errors and to estimate the parameter sensitivity if the right objective functions are used. If goodness-of-fit (for example Nash-Sutcliffe) is plotted against a measure of the water balance, systematic errors can be detected.

4. The inclusion of a "precipitation bias factor" is often used in operational hydrological forecasting, and it is a way to overcome underprediction of precipitation. However, it also affects the water balance, and if you have other parameters that affect the balance, such as evaporisation parameters, the parameter values are not independent, and changing one set of parameters will affect others. You argue in 2.4.1 the benefits of

including such a parameter, but I believe that this has too much of an impact on the parameter values and the whole hydrological model exercise. This is apparent in Fig 13, where the parameters that are used to calculate evapotranspiration are directly affecting the bias factor. This means that you are removing water from the model, either by changing the ET parameters of the bias coefficient. There are other ways of investigating the bias of the data sets, for example by a simple scatter plot between NS and mean bias, as I suggested earlier.

5. Why are you using a runoff model to look at precipitation characteristics as linear trends, such as in the WRF data? This should be obvious through a check for linear trends in for example annual means. ERA40 has problems with trends over certain regions, and this could be such an example. I would suggest adding this analysis to the precipitation evaluation.

6. In your conclusion you state that evaluation of the bias factor adds an additional performance measure to the analysis, but I do not agree. The misrepresentation of physical features as well as linear trends in data should be easily detected with precipitation statistics and metrics. The added value of using a runoff model would be useful to distinguish more in detail between datasets, for example how high flows; low flows are modelled et cetera. The conclusion that the deficit is due to a linear trend in the precipitation and not changes in the catchment is trivial, since there are no such evidence in the other data.

Specific comments

1. P 10720, L6. You say that the study has a second aim, but I would argue that this is not an aim to test it for hydrological modelling purposes, that is an application of the method.

2. P10723, L10. I disagree that discharge measurements are inflicted with smaller measurement errors than precipitation. It might be true for flows close to mean flows, but for low flows and especially high flows, the uncertainties can be huge.

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3. P10726, Section 2.2.1. Why was ERA40 used? There are newer products out, for example ERA-Interim. Was it because of the available data for precipitation? And what was the data period for the data used?

4. P10727, L1-9. Please describe the method to correct for undercatch by Yang el at.

5. P10727. Please describe the interpolation method in 2.2.3 with equations. I found it difficult to understand the method from the text itself.

6. P10728. Please describe the description in section 2.2.4 with equations

7. P10728. Merge section 2.2.4 and 2.2.5

8. P10728. Please spell out the acronyms GTS, GHCN, NCDC andFAO.

9. P10734. Please mention the used time periods for calibration earlier in the text

10. P10734 eq 1. As mentioned earlier, the used objective function is suboptimal, since it mixes two different aspects of the modelled discharge, and you therefore loose information of your system.

11. P10735. How much does the glaciers affect the water balance?

12. P10736, L7. “at the time” should be “at a time”

13. P10737, L5-10. Changing just one parameter at a time is hardly a sensitivity test. And as discussed earlier and what is apparent from Fig. 13, the parameter values of the ET are highly correlated with the precipitation bias factor.

14. P10739. L5. Why was such a coarse resolution selected for the WRF model? Could it not be run with a higher resolution?

15. P10740. L1-4. Please make a reference to Fig. 7 already here when it is first discussed.

16. P10741. L4. As seen in fig. 9, a preciptiation bias factor of 0.6 is physically not reasonable.

17. P10743, L7. Something is missing "the best, the best 20"

18. P10743, L13. As you mention here, the model structure might have a major impact on your results, especially the chosen scheme for evapotranspiration.

19. I would suggest to merge figure 1 and 2, since they are very similar.

20. Please put Figure captions (a, b c etc) in the figure instead of the name of the method. The text is also very small and difficult to read.

References van der Linden P and JFB Mitchell (eds.), ENSEMBLES: climate change and its impacts. Summary of research and results from the ENSEMBLES project. Met Office Hadley Centre, Exeter, 2009.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 10719, 2012.