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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A large-scale, high-resolution hydrological model parameter dataset for climate change impact assessment for the conterminous United States" *by* A. A. Oubeidillah et al.

Anonymous Referee #2

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***Please see attached pdf for formatted comments.

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General comments

This paper outlines the approach and methods used to develop a new set of historical hydrologic simulations for the conterminous U.S. at a high spatial resolution (HUC8 watersheds, using 1/24th degree data as a basis). This is a methodological paper,

in which the data sources, calibration approach, and initial sensitivity testing are described – the stated intention being to provide a basis on which further improvements can be applied. The authors describe the considerable computational demand of such an undertaking, and note the substantial resources committed to the work at ORNL.

Overall, the effort has potential to be useful to the research community if the model, datasets, and future updates are made public. However, I have four chief concerns: (1) Although potentially useful in vague terms, it is not clear exactly how this dataset (or the results) could be used to advance research, either in hydrology or climate impacts (2) This is not the first effort of this kind. Indeed, the authors cite Maurer et al., but do not contrast it with their approach. A more thorough literature review should be included, along with a justification for this newer approach: how is this different from reinventing the wheel? (3) Quality of the input datasets is key to obtaining reliable estimates of hydrology – the authors appear to have paid insufficient attention to the quality of the input datasets (e.g., meteorology, soil parameters) – e.g., alternative options, differences in quality/methodology, past validation studies, etc. (4) Calibration on such small scales may be problematic, since it may result in simulations that "get the right answer for the wrong reason" and therefore have incorrect sensitivities. I'm not convinced that calibration at the HUC8 level – in particular given the approximations of WaterWatch – is the right approach.

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Specific comments:

p. 9578, line 14: it is false to say that spatial resolution is not important for climate modeling – it may be less critical than for hydrology, but typical GCM resolutions are far from adequate at the scale of HUC8 watersheds.

p. 9579, line 15: this is a very vague statement. Be more concrete – e.g., median and range of NSE, etc.

p. 9580, line 11: the 1/24th degree grids should require exactly 9 times the computational resources needed for the 1/8th degree grid, not “more than 10 times”.

Section 2.2: All of the met datasets listed have major limitations, and ALL (including DAYMET) involve some sort of interpolation from sparse station observations. It is certainly not sufficient to simply interpolate Maurer and NARR, since at minimum some crude correction should be made for differences in elevation. In addition, some degree of validation is certainly possible and necessary to include in a study such as this one – at minimum a discussion of the validation presented in the main reference for each dataset. I’m concerned by the fact that the authors seem to have applied very little rigor to the choice of meteorological dataset – this is likely a dominant source of error in hydrologic simulations – a problem that calibration rarely addresses, instead ensuring that you get the “right answer for the wrong reason”.

In addition to the monthly analysis, some analysis should be included regarding differences among *daily data from DAYMET, NARR, and Maurer – these comparisons would likely show much larger discrepancies. Furthermore, a review of previous validation efforts should be included – surely all of these datasets have been subject to scrutiny elsewhere in the literature.

Section 2.3: Why not use the new dataset of Livneh et al., 2013?

Section 2.3: Given the approximations used to estimate HUC8 flows via WaterWatch, the discussion should include some information about how well this method performs, and when/where it is most at risk of exhibiting errors. For instance, the authors hint at the problem of regulated gauge stations – this has potential to result in large errors in places with small reservoirs, diversions, etc. Similarly, there are other concerns – basins far from gauge observations, adjacent basins with very different exposures to precipitation, etc. Such questions should at least be acknowledge in the text – ideally, results of validation tests can be included.

p. 9586, line 9: the 3 hourly time-step in VIC is a user-defined setting. I believe the

default is for MTCLIM to run on a 1-hourly time step.

Section 3.1, lines 3-7: Use of the term “daily” for each term is misleading since the comparison involves monthly means. This should be clarified to avoid confusion – both in the text and in the caption to Figure 3. Figure 4: the mean daily wind speed from NARR appears to be quite noisy – e.g., no clear delineation of the Rockies, Cascades, or Sierras, where I would expect to see large increases in wind speeds. Could this be due to an artifact of the gridding process? Have NARR winds been validated elsewhere?

p. 9593: What about groundwater? Managed flows? Could these be affecting the erroneous runoff simulations in Fig 8?

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Technical corrections:

p. 9580, line 10: replace “a key” with “key” and “resource” with “resources”

p. 9580, lines 11-12: replace “of computation resources comparing to” with “the computational resources required for”

(There were other such grammatical errors and stylistically awkward phrasings throughout the text – please identify and correct these)

p. 9580, line 27: weather station observations are not referred to as “gauge” observations – for one thing because the measurements are not made by gauges, but by instruments: temperature, precipitation, and wind sensors, for instance.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C4751/2013/hessd-10-C4751-2013-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 9575, 2013.