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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The Future of Earth Observation in Hydrology" *by* Matthew F. McCabe et al.

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First off, the paper is excellent and extremely valuable to the community, and, frankly, thought provoking. I honestly had no idea about more than half of the issues raised in the paper. I can imagine that, for younger scientists/engineers, both hydrologic practitioners and researchers, the paper will be especially motivating. With faculty positions and government research tight, the paper gives some hope for a larger industry including the private sector where many of our current and recent Ph.D. students could find employment, and most importantly, contribute significantly.

[Response]. We thank Prof Salvucci for his kind words and appreciate his review of our manuscript.

The paper is long, but that's ok. It is very well written. I have only a few short comments

that could be considered by the authors before publication:

1) In section 2.1, fundamental challenge #3, where stove-piping is criticized, i wonder if the EOS project isn't a counter-example (at least partly).

[Response]. Noted and it may be worth highlighting such a counter example.

2) The point made in section 2.1, fundamental challenge #4, that due to merging "observed inter-annual fluctuations may reflect discontinuities in the constellation of satellites" is an important one, and thus could use a few citations, as this has been discussed a fair amount, and many merged products exist to attempt to overcome it.

[Response]. This is a good point and some appropriate examples and references can be included.

3) in the future agency missions discussion (3.1), the statement "Deep soil moisture could also be on the list, although soil moisture algorithms that make use of wavelengths longer than L-band are less than mature." is made without evidence. I honestly do not know the status of that research, but I support its potential importance, and am not comfortable with it being dismissed without backing.

[Response]. A suitable reference can be included to support this statement.

4) Two sources of data that I was surprised were not discussed at all (or barely), are: 1) Ameriflux/Fluxnet (I believe there was one citation to a paper that used fluxnet). I believe fluxnet has had a huge impact on hydrologic science, and hope to see it continue and grow in scope. I think it could be argued, as well, that for the cost of a space mission (billion dollars ?), one could put together a pretty amazing network of eddy covariance stations (perhaps 5,000 stations running for 5-10 years ?)

[Response]. Fluxnet is a great example of a community-led approach to sensing that has provided an invaluable source of ground-based data. Within the evaporation community, for instance, it is the gold-standard monitoring network (n.b. we cite three papers in this area). However, the focus of this paper leans more towards emerging

approaches and newer sources of observation data. Fluxnet is a far more mature and well developed technology. The point on the relative cost-allocation of ground-based infrastructure versus space-based missions is an interesting one, and can perhaps be included in a relevant section of the paper. Of course, identifying who funds such an effort is the challenge: it is extremely unlikely that a space agency will seek to allocate their limited resources to such an activity.

2) AMDAR/ACARS observations (e.g. Drue et al, 2008, QUARTERLY JOURNAL OF THE ROYAL METEOROLOGICAL SOCIETY Q. J. R. Meteorol. Soc. 134: 229–239 (2008)) which provide extremely high resolution profiles of temperature, wind and humidity that well sample the atmospheric boundary layer during takeoff and landing many hundreds of times per day all over the planet.

[Response]. Thank you for this citation. We briefly discuss AMDAR in Section 3.6 (Signals of Opportunity) but lacked a published reference.

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