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

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Overall, I find the paper suitable for publication in that I though it did a good job at reviewing the current space-based observing systems, and the emerging capabilities and technologies. I found the last section a bit unbalanced as I explain below.

The author lists existing and planned observations together with a comprehensive look at emerging technologies to argue that the future of hydrology is not in individual datasets but instead in the integration of large clouds of emerging data sources. The author makes a good case that the emerging data sources indeed hold promise and that storage and computing may be available as well.

Where the paper falls short, in my view, is in assuming that if the pieces exist, the implementation is easy. Space agencies currently devote a significant amount of their

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funding to integrate satellite measurements into a coherent global framework. As more and more data sources shift the focus away from specific space agencies, it would be hard to imagine that they would be able to fund such large data storage and management objectives unless they are tied directly to their mission. The real question becomes – who funds such a large and integrating hydrological sciences data structure objective? The answer is that nobody is in a position to step up to that task at the moment, and without it, the holistic approach is not in easy reach.

I don't think the authors need to answer this question. On the other hand, it is an immense challenge that cannot simply be ignored. The challenge before us is not simply in making measurements, but also rethinking how we work as a community and how we fund this community. That needs to be better conveyed in this paper.

Some places where corollaries of this show up: p. 5, line 1: the bottom of page 4 argues that it would be great if there was a continuous, holistic water budget strategy. The author then suggest that aside from a paradigm shift in how we undertake much of our research, the big hurdle would be the need to have much more and much cheaper observations. Lots of cheap observations are a problem for space agencies who's mission it is to advance space-borne technology.

p. 7, line 10: The notion that we need a more appropriate error analysis before we can merge multiple sensors into a coherent framework is often repeated and yet to be implemented. Since this is a large part of what the paper is advocating for the future, the authors should emphasize the critical need to advance this field rather than simply stating it as a necessity.

p. 29, line 22: Google's Earth Engine was developed by Google and follows Google's rules. Without a coordinating body in global hydrology, it becomes difficult to reproduce something like this.

p. 34, line 27: The fallacy here is that it assumes that if SpaceX develops a very cheap launch vehicle, then NASA would launch its satellites on it. That is probably not the case. NASA would continue to develop new technologies that in its early stages, might be just as expensive as before.

p. 30, line 22: Storage can get very large. While commercial vendors often discard old data which generates little revenue, this has never been the case in Earth Sciences. It again goes back to the question of funding data storage that belongs to the community rather than an “agency”.

Some minor text issues:

Abstract, line 10: The real time, high resolution video capabilities are surely new, but equally important for cloud development are the newly available JMA and NOAA 1min. geostationary data with 1 km resolution. CMA even has 60m resolution geostationary data with 30 sec. resolution. Given the size and speed with which clouds naturally evolve, it is not clear to me that 10 m data with 30 frames per second would revolutionize our understanding of clouds. I can better see the utility in tracking small scale, fast flows. In any case some careful rewording here would help.

p. 6, line 5: Eliminate “radiative” – just visible and NIR frequencies is enough.

p. 6, line 6: It is now GOES-16 and authors should probably mention Himawari-8 as well. They do the same thing.

p. 9, Line 16: Doppler radars are good for judging speeds and rotation of convective elements. They don’t do anything for rainfall estimation. I suggest that the author drop “Doppler” or replace it with “polarimetric”.

p. 14, Line 5: Section 2 discussed variables independently. The author mentions that integration and a more holistic approach is necessary – but emergent capabilities and technologies are not the answer. They merely add observations. The text is not very clear here.

p. 14, line 11: The text starts out boldly predicting hydrology 50 years from now. The paper, however, focuses on things that are real today and probably represents the next

10-15 years. Perhaps that would be a better into?

p. 14: Line 33: Perhaps Japan should be mentioned as well.

p. 18, line 25: The balloon section needs a few more caveats. Since high altitude balloons have been around for a long time, and the authors fail to make a case that commercial balloons for telecom would somehow change their utility or cost, some argument for why we might see a resurgence of balloon-based observing systems should be put forth.

p. 18, line 32: “constructed” instead of “constructing”.

p. 21, line 19: perhaps the authors need to still remind readers that while passive systems in the VIS/IR are quite feasible, the power needed by active sensors still limits these systems today.

p. 23, line 5: I think the authors could come up with a better example here. Rain gauge records are often protected for various geopolitical reasons. It is not clear to me that the rain gauge records from the Congo have not been released simply because nobody in the office has a smartphone.

p. 24, line 12: The authors might want to include CoCoRAHs https://www.cocorahs.org/ Which, on a given day, receives roughly 7000 daily rain gauge reports) from citizen scientists.

p. 25, line 20: Perhaps it is worth noting that the “entire country” was the Netherlands and that this would not work for countries like the US where many rural areas do not have nearly enough cell phone towers to apply this technique.

p. 27, line 3: Commercial aircraft started making turbulence measurements as well. See Sharman et al., Description and Derived Climatologies of Automated In Situ Eddy-Dissipation-Rate, 2014. Appl. Met. and Clim., 53, 1416-1432

p. 27, line 25. The same issue pointed out in the abstract.

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p. 33, line 4: The risk averse nature of space agencies is true. The rationale is the authors' interpretation. I would argue that they are risk averse because it takes so long to reach scientific consensus. The PI led missions have very short time frames – similar to industry. Perhaps pointing out that facility type missions are risk averse while PI missions can be much more nimble is a better way of expressing this.

p. 35, line 20: At least NASA is shifting into smaller missions via Earth Ventures. This should be acknowledged although it is equally true that NASA's mission would probably not support a much greater emphasis in small Earth Venture missions.

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