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Interactive comment on "High resolution land surface fluxes from satellite data (HOLAPS v1.0): evaluation and uncertainty assessment" *by* A. Loew et al.

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Reviewer: Matthew McCabe, January 17, 2016.

Overview. I have been familiar with the HOLAPS effort for a number of years now and was very pleased to see this first publication put forward for consideration in GMDD. The HOLAPS project has been a leading effort in large scale flux estimation, utilizing geostationary data in a novel modeling framework to provide high spatial and temporal resolution flux estimates. Over the course of this ongoing project, there have been a number of related flux products put forward in the literature and also being distributed as products to the community. As such, it would be instructive if this HOLAPS effort

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could be better placed in this context, outlining some of the previous efforts and highlighting the novelty and innovation of the present contribution.

As it stands, the manuscript only partly delivers on the stated objectives to 1) introduce and validate the fluxes at a global scale and 2) perform a thorough uncertainty assessment. The validation and assessment of flux behavior could be strengthened by additional analysis, coupled with a more detailed interpretation of results. I would encourage the authors to explore aspects of the analysis beyond the use of entire-period (or global) statistics and to really disentangle some of the variability in model performance as a function of time (and space). In terms of the uncertainty analysis, while this is certainly an interesting element of the work, it is more an assessment of the impact of forcing variability rather than a concerted effort to characterize model uncertainty or even sensitivity. It may be worth rephrasing this objective or alternatively exploring it in a deeper manner. Such efforts are a badly needed element of flux assessment in the community.

Following is a summary of some comments and suggestions that might help to strengthen or re-focus the manuscript. With attention to identifying the key contributions and novelty, as well as teasing out the underlying model behavior and response through additional analysis, I believe that this work has potential to advance our understanding of global water and energy cycles. With some fine-tuning, it will make a valuable contribution to flux estimation efforts.

Introduction.

The Introduction provides a rather narrow review of the literature and misses a number of recent (last 5 years) contributions towards global flux estimation. It would be helpful to provide a more thorough review of this literature to define the context within which HOLAPS is being proposed. For example, what are the current knowledge gaps or limitations in global flux estimation; what are the key advantages and contributions of this dataset; how does HOLAPS advance or improve upon these past efforts? Basically, a

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clearer expression of how this work progresses upon recent efforts is needed. The fact that HOLAPS is proposing high temporal (<1 hour) and spatial (5km) flux estimates globally is clearly novel (although the authors should review related ALEXI/disALEXI research) and needs to be highlighted further. So do the advantages of such resolutions for studies that other existing global products (e.g. GEWEX Landflux) are unable to offer insights into (i.e. state the knowledge gap that HOLAPS is filling).

Page 10784, Line 19: perhaps rephrase by removing "In the last years" from the start of this sentence – just state that local scales fluxes are (predominantly) measured by EC systems – in terms of the FLUXNET collection at least.

Page 10785, Line 1. I'm not sure Fisher et al. 2008 is best described as a surface energy balance approach.

Page 10785, Line 2. Ultimately, almost all approaches can be described by point 4, since E cannot be inferred directly (hence rely on spatially variable surface parameters as proxies). If you are referring to techniques such as the triangle method or SEBAL/METRIC type approaches, perhaps better to rephrase this to make it more explicit to this family of techniques.

Page 10785, Line 5. You can reference the most recent GEWEX-Landflux paper:

McCabe et al. (2016) "The GEWEX LandFlux project: evaluation of model evaporation using tower-based and globally-gridded forcing data", GMD (accepted).

Page 10785, from Line 15. There are quite a few "high-resolution" data sets currently available that exceed the stated resolutions (0.25 – 2 degrees). It would be worth examining the recent literature to update these values and place the HOLAPS contribution in context.

Page 10785, Line 29. Probably remove "exclusively", as again, most products derive their data from remote sensing and some form of meteorological forcing, so it's not clear what is exclusive about this.

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Page 10786, Line 8. Rephrase this sentence (perhaps by full-colon after question?)

Model.

Page 10786, Line 19. I would advise including some clearer description of what precisely is “state-of-the-art” about the land surface model underlying HOLAPS. The coupling of the land surface scheme to a 1D mixed layer model of the PBL is a nice feature of the HOLAPS approach: is this the state-of-the-art aspect?

It would be helpful to see how this coupling methodology relates to similar approaches used by researchers over the years (e.g. McNaughton and Spriggs, 1986 for an early example, but up to and including the referenced Anderson et al. 2007 and more recent works by those authors). After reviewing, I noted that much of the model description is listed in Appendix B, including the PBL component. However, to justify use of “state of the art” it would still be good to explicitly describe these distinguishing model features relative to other approaches (this may also be an element that is reflected in the Introduction).

Anderson, MC, Norman JM, Diak GR, Kustas WP and Mecikalski JR (1997). "A two-source time-integrated model for estimating surface fluxes using thermal infrared remote sensing." Remote Sensing of Environment 60(2): 195-216.

McNaughton, KG and Spriggs TW (1986). "A mixed-layer model for regional evaporation." Boundary-Layer Meteorology 34(3): 243-262.

After reading further sections, I think it is important to introduce some model description into the main-body of the text, leaving the more explicit technical details in the Appendix. An overview of the approach, a schematic of model elements and processes, together with a description of flux product development would be valuable to the reader (especially as I believe this is the first HOLAPS publication). I note that Figure B1 presents a runtime environment, but this does not describe the schematic in terms of model components and their inter-relations.

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What is precipitation used for exactly: interception, presumably, but I assume also the water balance component of the model? Given the variability in available global precipitation products, wouldn't this also be an important contribution to examining product "uncertainty"? Certainly this will impact considerably on flux-partitioning!

Data.

It has not been stated yet, but what is the time period over which these HOLAPS simulations were run (such information would also be helpful for the products in Table 1). I understood (perhaps incorrectly) that HOLAPS was an operational product and that long-term simulations were available: if so, this should certainly be one feature that is highlighted in the Introduction, as a global long-term high spatial/temporal product is certainly an advance.

Section 3.1.

Was there a reason why the evaluation period only covered 2003-2005? What other constraints on the quality of the Fluxnet forcing were used (i.e. was the data filtered for rainfall, freezing conditions, night-time etc..)?

Table C1 lists the Fluxnet towers, but it is not clear from the caption (or to the reader up to this point in the manuscript) what is meant by the Coverage option in this table? Fluxnet are obviously point scale locations, so it is unclear what the Coverage information refers to: perhaps state more explicitly in the caption (likewise for other Figures, were necessary).

Page 10790. 3.2.1. Surface radiation data. The internal consistency of the radiation components is a nice feature of this approach. The downward shortwave is available from a number of different sources and is the focus of uncertainty assessments. However, from a flux partitioning perspective, knowledge of the surface albedo (and subsequent outgoing SW flux) is one of the most important aspects of flux-estimation. Was there any effort to assess the impact of uncertainties in this variable on flux estimation?

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Page 10791. 3.2.2. Precipitation data. I'm a little unclear on the global extent of the HOLAPS product if it is using TRMM satellite data? Are other data being used to ensure that this is a global product, or is it constrained (as the text suggests in Line 17) to the same geographic restrictions as TRMM? If so, some adjustment of the claim of a global product is probably required.

Page 10792. Line 3. Surface albedo is derived from the ESA GlobAlbedo project. I'm not familiar with the mechanics of this approach, but it presumably uses some other shortwave data-set to assist in deriving the albedo. How does this affect the internal consistency of your radiation flux estimates (Page 10789) given that you are using a different shortwave product to the albedo employed in the model?

Page 10792. 3.2.4. Reanalysis data. The data are available every 6 hours, yet E retrievals are provided approximately every 1-hour? Some model description in the main part of the text is probably required, especially in order to describe how the 6-hourly meteorological data are used to force this required model resolution? Has there been any attempt to compare the reanalysis forcing with the tower data (e.g. some simple statistical comparison as was done in McCabe et al. 2016 for instance).

Given that ERA Interim data are already being used, it would be interesting to run the HOLAPS data using the full-suite of available forcing (i.e. radiation) and then compare against 1) the tower data and 2) the reanalysis flux estimate. This would also allow some separation of model versus forcing uncertainty to be examined.

Methods

Page 10793. Experimental set-up. The "uncertainty analysis" is more akin to the parallel forcing study undertaken by McCabe et al. 2016, whereby gridded forcing and local scale forcing are compared, along with the impact on flux estimation. The terms "sensitivity" and "uncertainty" seem to be used interchangeably: the authors may wish to define what is meant by these terms early in the manuscript (the title has uncertainty analysis, but much of the methods mentions a sensitivity analysis).

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While different “scales” of forcing are examined (this is certainly worth highlighting in the Introduction and Discussion) it is not really a true uncertainty analysis, as there is no capacity to actually attribute model sensitivity to a particular forcing. When mentioned in the Introduction, I had imagined a rigorous uncertainty assessment that sought to disentangle the issues of forcing uncertainty (distinct from variability in the type of forcing data used) on flux estimation. This is a much needed (and mostly missing) aspect of global flux estimation. It may be worth rephrasing the discussion of “uncertainty assessment” in line with what is actually undertaken here (this is not suggesting that the analysis is not useful, just that it does not really identify impacts of actual uncertainty in forcing, as opposed to impacts of changing forcing data). These ideas are stated well in the first paragraph of Section 4.2, but the analysis does not discriminate between them (which would provide a true uncertainty analysis).

Results.

The section presents a rather standard statistical assessment of the HOLAPS flux retrievals, essentially reconfiguring HOLAPS forcing using a number of available sources and providing a brief summary of subsequent model response. No real understanding of the impact of uncertainty is able to be determined here: rather it is more a perturbation-simulation experiment (see the earlier points above on uncertainty). I was hoping to see a detailed sensitivity analysis, but instead we see that using different forcing results in different responses. I do not mean to be flippant here, but it is a common criticism of such papers (see reviewer comments and response to the GEWEX Landflux paper in GMDD for example) and I am sympathetic to the effort involved in product assessment being undertaken. But there are many possible causes for “error” and “uncertainty” in flux estimation: variable forcing data sets being just one. It would be good to see some of these issues expanded upon further, either here or in the discussion sections.

Likewise, it would be great to see something other than just “whole-period” summary statistics. How do night-time versus day-time fluxes compared? What is the impact

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of seasonal cycles? What about cloudy-versus non-cloudy conditions (in comparing the radiative fluxes especially, it would be good to see some capacity for distinction of these between the different products)? Dry climates versus wet-climates? There are many ways to make the statistical assessment of these towers more informative than just providing such "global" evaluation and I would encourage the authors to really be creative in this aspect. One of the common criticisms of flux evaluation papers is how does it advance upon the study of XYZ et al. – so here is an opportunity to avoid that.

Given HOLAPS provides estimates of sensible-heat fluxes, it would be interesting to see how the partitioning results compare with those estimates at the tower (as well as comparisons with H itself).

I note the negative fluxes in Figure 4 (for hourly) but not so in the corresponding evaporation values in Figure 7? Is there some filtering of model results for this period?

I do not recall seeing any mention of ground heat flux, outside of its appearance in equation B1? How is this accounted for in the model (and how is it calculated). This would seem to be important, especially with the focus on sub-daily simulation, where the role of G is not insignificant?

Figure 5 and Figure 6 need to be scaled to make it easier to read and interpret. Likewise the similar Figures in the Appendices.

Discussion.

Page 10800, Line 10. Perhaps provide some key references and benchmarks for the statement "...to those obtained in other studies".

In addition to the WACMOS (not WACHMOS) study of Michel et al. 2015, you may also wish to compare your results to the McCabe et al. 2016 GMDD study, as the analysis reflects a similar approach to that undertaken here. For a comprehensive multi-model evaluation at the tower-scale, the work of Ershadi et al (2014) may also be of interest (not just shameless plugging: they are quite pertinent to this analysis).

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McCabe et al. (2016) “The GEWEX LandFlux project: evaluation of model evaporation using tower-based and globally-gridded forcing data”, GMD (accepted).

Ershadi et al. (2014). "Multi-site evaluation of terrestrial evaporation models using FLUXNET data." Agricultural and Forest Meteorology 187: 46-61.

It would be good to see some further insight and discussion of some of the issues related to global flux development, and how the HOLAPS effort is addressing these, in the Discussion section. What about other sources of uncertainty? What is the (realistic) potential for an operational product and its accuracy? What are the impacts and importance of these issues on flux development and the ultimate utility of such products? What remains to be done to achieve some (stated?) objectives? Where are the major challenges and how might these be addressed? Basically, this section needs some implications and further analysis (of this and related works) to provide a useful summary of the HOLAPS contribution and a context within which the effort can be placed.

Conclusions.

Page 10802, Line 10. I remain a little unclear on the “consistent global water and energy fluxes” statement. I do not see how albedo (derived from ESA GlobAlbeo?) is assured to be consistent with varying vegetation cover from a separate product? Surely this will have a considerable impact on derived radiative fluxes and inevitably related water and energy flux retrievals? Perhaps this would be clearer with a more detailed modeling description section.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10783, 2015.