

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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Dear authors and editor,

I have read this manuscript with great interest. The method has a good potential, and I am aware that the authors have put several years of efforts behind the development of this methodology. I think that the consideration of the atmospheric boundary layer dynamics and the consistent radiative fluxes, set the uniqueness of the model.

Nonetheless, there are a few points that need to be reconsidered, better put in context, or simply clarified, to make the paper a better manuscript. In the following I provide a

C3625

summary of my assessment.

Some general comments:

1. – On the satellite nature

The title '*High resolution land surface fluxes from satellite data*', or sentences like '*The required drivers for HOLAPS comprise satellite data at different processing levels as well as re-analysis data for a limited number of variables*' may feel to the reader a bit... 'optimistic'. The truth is that about half of your forcing variables (i.e., air temperature, air humidity, windspeed, air pressure) come from ERA-Interim (which is fine!). I would, therefore, try to soften these statements all across the manuscript, starting from the title. Perhaps 'observational-based' is a more accurate and still relevant way to refer to the data sets, as it reflects the nature of both satellite and reanalysis forcing.

Along those lines as well, I feel a bit uncomfortable with the recurrent sentence (appearing up to four times...) stating that HOLAPS is designed to maximise the use of satellite data. If it was, it would use no reanalysis data at all. This is possible by just using a Priestley and Taylor approach, which in principle does not rely on humidity or windspeed data – see e.g. some GLEAM versions using satellite data only in Miralles *et al.*, 2014 (S.I.). However, I am not claiming that considering humidity or windspeed is not a good idea; yet, I do not think it is steered by the rationale of maximising the satellite nature of the model. On the other hand, if by 'maximising' the authors actually mean that all relevant satellite observable E drivers are exploited, I tend to disagree once more, since important variables for evaporation – like soil moisture, vegetation optical depth, or fluorescence – are observed from satellites and not included in HOLAPS. Therefore, I would avoid the use of 'maximising' in this context, and try to find a less ambitious but more accurate way of phrasing it. Perhaps what you achieved is a physically-sound algorithm that relies to a large extent on observable forcing. I'd be supportive of statements along those lines.

2. – The succinct abstract

C3626

The abstract could be a lot more comprehensive and include: (a) the period covered by the data set, (b) the statistics of the validation (avoiding the use of 'very good' in reference to these statistics and noting the numerical value of the inferences), (c) state the spatial and temporal resolution, (d) can anything be said about the global results in Fig. 1?

3. – The way to frame the need for HOLAPS

This has to be rephrased in the Introduction to some extent. The argument of higher spatial and temporal resolution was very valid five years ago, when this effort started. However, as of 2016, existing global heat flux models have been run already at finer resolutions than 5km, and at sub-daily scales. Moreover, statements in the lines of '*The currently existing datasets have spatial resolutions between 0.25 and 2.5 and are focused on daily to monthly timescales (Miralles et al., 2011; Mu et al., 2007; Vinukollu et al., 2011)*' are in fact not really accurate. Global datasets have currently resolutions of 1km–25km, and most have been run at 3-hourly resolution or even higher (see e.g. McCabe et al., 2015; Michel et al., 2015; Miralles et al., 2015). In fact, even the papers cited by the authors – which date from at least five years ago – report results at resolutions that differ from the ones stated in the manuscript: Miralles et al. (2011) was daily 0.25 degrees, Vinukollu et al. (2011) was daily 5km, and Mu et al. (2007) was 8-daily 0.05 degree resolution.

Therefore, based on these citations, the reported range '*between 0.25 and 2.5*' and '*daily to monthly timescales*' should be '*between 0.05 and 0.25*' and '*daily to 8-daily timescales*'. This is choosing these early efforts and without taking into consideration any progress over the last five years. In summary: please, highlight the high resolution of HOLAPS without stating that it is notoriously higher than for any existing analogous data sets, and specially try to reflect more accurately the state of the art as of 2016.

4. – The added value upon ERA-Interim

The authors design several interesting experiments altering the observations used as

C3627

radiative forcing in HOLAPS. The reader may wonder at this stage what the result of using ERA-Interim radiation in HOLAPS would be. This is an important question, because the added value of having a radiative module in HOLAPS should be explored and understood, and ERA-Interim is used for several other forcing variables making it consistent with the current setting. My feeling is that results should deteriorate in general when using ERA-Interim, even if it is just due to tower-to-footprint errors.

Then, an equally interesting question for most readers would be how the validation of the latent heat flux from HOLAPS compares to the validation results of the latent heat flux from ERA Interim (e.g. adding a sentence in the description of the results, or even a new panel to Figure 7). This will not only provide an estimate of the added value of having a radiative module but of the entire methodology in general. These two experiments appear important, even if HOLAPS does not outperform ERA-Interim dramatically.

5. – On the low values in tropics

I couldn't help noticing that the values in the tropics are low; 60–70 W m⁻² is about 800–900 mm year⁻¹, which it is almost a two-fold difference from what I would expect based on literature. As an example, the Amazonia evaporation is about the same as the evaporation in the Pampas region now. That is not normal, and it may be partly due to the calculation of interception loss based on a Priestley and Taylor equation, which can lead to a several-fold underestimation of the flux. This is a practice that has long been criticized (see e.g. Shuttleworth and Calder, 1979), given that the energy used for evaporation in wet canopies does not come from net radiation (Holwerda et al., 2011; van Dijk et al., 2015; Savenije, 2004). This may explain the low gradient of evaporation in the tropics observed in Figure 1. In addition, since there are no towers in tropical regions this issue would not show in current validations.

6. – No sensible heat flux results

The title is very promising, but the paper soon tilts towards evaporation only. If latent

C3628

and sensible turbulent heat fluxes are calculated by the model separately, and they are available at the tower level, why do validations and maps refer to the latent heat flux only?

Comments on Figures and Tables:

– Figure 1: I would add a colormap with the units in mm/year. That would make it more comparable to other studies. How does this map qualitatively compare to other datasets? (just one line or two in the text). I would also be interested in seeing the analogous map of H. Also, why only 2001–2005?

– Table 1: It may be a good idea to indicate the temporal coverage and spatial domain of the forcing variables in the table.

– Table A1: It is very useful to have this table of acronyms. There are however several acronyms that are not defined in the main text. I am not sure whether that is fine with the policies of the journal, but it does make the reader need to rely on this table. I pointed to some of the acronyms that are undefined under the list of minor comments below.

– Figure 2. Within the diagram of HOLAPS estimated fluxes and modules, one can find precipitation now. It may be an error.

– Figure 4. Note the experiment name in the caption. It seems that the issues with the radiation come at nighttime; could you comment somewhere on that? Other than nighttime it seems that the correspondence is extremely good.

– Figure 7. It seems that evaporation is always positive. Can you comment on that as well? Nonetheless, the axes are cropped to zero in b) but not in a); any reason for that?

– A general comment about the figures and tables. I have the impression that the statistics are now calculated by plotting together all the stations at the same time, in a single scatter plot. My apologies if this isn't the case. That is problematic, because

C3629

the energy imbalance in FLUXNET towers is rather systematic but location-dependent. That would penalise the statistics. On the other hand, another confounding effect will unfairly favour the statistics instead: there are towers at the wet-end and at the dry-end of your scatters, meaning that a correlation now does not necessarily mean a correlation at the tower level. As the authors know, a better idea to evaluate the quality of the model is to provide the mean of the statistical inferences calculated at each tower. In other words: the mean of the 49 correlations is better than the correlation of the scatterplot constructed by adding together the 49 towers with their own climatological mean and biases (as in Figure 7). This would in addition make the validation comparable to most literature studies. If this is the way it is done now, please describe it in the paper and captions. I have the impression that that may be the way the whiskers plots are designed but I could not find the information.

– One final comment for the scatterplots: labels should be bigger!

Comments on the model:

– The terms used in equation B1, B2, etc. Do they need to be defined after the equations, or is it clear enough with the acronyms table?

– L18 P10804: Italics in 'L'.

– As mentioned below, calculating interception loss based on a Priestley and Taylor equation can lead to a several-fold underestimation of the flux, and this may partly explain the low values in rainforests (Figure 1).

– I may be missing where windspeed enters the calculation of LE, or whether it is only used in the calculation of H, given that the aerodynamic resistance does not appear directly in the latent heat flux calculation. Since the input variable is already there, wouldn't it be interesting to use this resistance in the latent heat calculation? – maybe even within your inhibition function? My apologies if I am just not finding the equation.

– L10 P10807: in the partitioning of net radiation within the fractional covers, is the

C3630

albedo of vegetation and bare soil considered to be the same?

- How are snow times considered?
- What does the model do at nighttime?

Other minor comments:

– L14 P10784: remove 'global'.

– The model classification in the second paragraph of the Introduction: many of the current global models that are based on Penman-Monteith or Priestley and Taylor approaches (e.g., Mu *et al.* 2007; Fisher *et al.*, 2008; Miralles *et al.*, 2011a,b; Zhang *et al.*, 2015; Zhang *et al.*, 2016) do not fit within any of these categories. Note that Fisher *et al.* (2008) is certainly not an energy balance model, as it does not close the energy balance.

– The part mentioning WACMOS-ET in the Introduction feels like if it was added afterwards, and needs to be better integrated within the description of the state of the art. Again, within this description I would try to be more inclusive in what comes to other existing efforts (e.g. Zhang *et al.*, 2015; Zhang *et al.*, 2016; Anderson *et al.*, 2007), and highlight the two main features of HOLAPS: (a) a consistent radiative forcing thanks to the ABL modelling, (b) a high spatiotemporal resolution allowed by the use of geostationary satellites. There is certainly room (and need) for new approaches like HOLAPS, but the way it should be embedded within the existing body of models needs to be phrased more precisely.

– L5 P10788: 'is' for 'are'.

– L15 P10788: same thing.

– Sentence: '*The drivers required to force HOLAPS are summarized in Table 1. These consist of satellite remote sensing data products*'. Again, try to be more accurate and objective in these statements. Half of the driving data sets are not satellite-based (note

C3631

also that you miss ERA-Interim pressure from Table 1, add). It is not something to be ashamed of... I personally do not see the issue with this. Try to be a bit more clear about it.

– L5 P10789: you may need to define these symbols.

– L10 P10789: '*a simple energy balance correction is applied*', put in brackets what it consists on.

– '*stable conditions. As the eddy covariance*' for '*stable conditions, as the eddy covariance*'.

– L12 P10789: '*dataset*' for '*datasets*'.

– '*maximum consistency between the shortwave and longwave radiation fluxes is ensured as the same ancillary data is used*'. The authors state that this is a unique feature of HOLAPS, but could you add a sentence stating why this same feature does not apply to other net radiation datasets from satellite observations, like SRB or CERES (or even reanalysis data, like ERA-Interim). My feeling is that other turbulent flux models that calculate air temperature, like ALEXI (Anderson *et al.*, 2007), may be even more consistent in their shortwave and longwave forcing. I am quite sure I did not understand well this description; since this feature is a uniqueness of the model it may be appropriate to elaborate this part a bit more in the main text.

– P10791: If the authors are using TRMM data, the references to global model output should be corrected. I don't think that 50N–50S can be considered as global... living in Brussels I find a bit excluded by that statement ;). Maybe 'quasi-global' is the right way to refer to TRMM data; we have used this in the past in the same context (see e.g. Crow, Miralles and Cosh, 2010 '*A Quasi-Global Evaluation System for Satellite-Based Surface Soil Moisture Retrievals*').

– P10792: there are several one-line paragraphs in the paper that may be grouped into longer paragraphs.

C3632

– P10792: 'A limited number of additional fields (temperature, wind speed, total column water vapor path, pressure) are required from global re-analysis'. Please, remove 'limited'. Referring to 4 satellite input variables as 'maximising the use of satellite data', while then stating that 4 reanalysis variables are 'a limited number', does not feel too objective... Again, there is no reason to feel uncomfortable about using reanalysis data, especially since there are no 3-hourly global satellite fields for these variables.

– 'which corresponds to a spatial sampling of 0.7', despite this forcing resolution the final output of HOLAPS comes at more than an order of magnitude higher resolution. Therefore, I think a sentence is required somewhere to explain this issue or some readers may get a bit confused at this stage.

– 'However for the present study, no global land cover dataset is used as the experiments conducted are only performed on the point scale', but were not they used in the global map in Figure 1?

– 'Currently the HWSO is the only globally available soil information.' This is a bit of a weird (and inaccurate) statement. Could the authors elaborate a bit more on what they mean by 'information'? Otherwise just delete, because there are quite a few global datasets of soil characteristics available out there...

– L14 P10793: 'related to the usage of satellite data...'. Add: 'and reanalysis'.

– Forcing the model with FLUXNET data 'allows to quantify HOLAPS accuracies without additional uncertainties from the driver variables'. The authors spent quite some effort discussing the uncertainties FLUXNET measurements, and the plenty of representativeness and resolution issues in the forcing and ancillary datasets. This statement needs to be deleted or corrected.

– P10795: a fourth source of errors is the one I just mentioned, the differences in footprint depending on the measured variable, etc. The authors may add this as 'd'). This error occurs even if each measurement is perfect, so it is not to be considered as

C3633

the same as forcing uncertainty...

– I do not understand the need to refer to the MSD, RMSD, R, etc. as E^2 , E' ... For me E^2 is evaporation squared (?). Is this needed?

– 'A monthly mean is calculated if at least 2/3 of the days of a month contained valid values'. Are these times also deleted from the gridded forcing when performing comparisons between tower-forced output and gridded-forced output?

– What do you do with rainfall times in the validations? The latent heat flux measurements from eddy covariance are typically removed when it rains in this kind of comparisons (see e.g. Ershadi *et al.*, 2014; Miralles *et al.*, 2011; Michel *et al.*, 2015), because of their little reliability when humidity approaches saturation (see e.g. van Dijk *et al.*, 2015). This also means that the interception loss of the model cannot be validated through comparisons to FLUXNET data... These issues should be acknowledged somewhere.

– Again, there is a bias towards latent heat, which in the case of FLUXNET is in fact more unreliable than the sensible heat flux measurement. Is there any way to include a figure on H, apart from a global map, showing the statistics of the validation? Note that the current title and the name of the model do not reflect this bias towards evaporation.

– P10799: 'Error statistics for all experiments is', change 'is' for 'are'.

– 'are smallest' for 'are smaller' or 'are the smallest'.

– Discussion section: I believe you are comparing your HOLAPS R to the R^2 reported by Michel *et al.* (2015). That may explain some differences. Please double-check this entire discussion...

– P10801: 'similar approach like (Michel *et al.*, 2015)' for 'similar approach to the one by Michel *et al.* (2015)'.

– Last few sentences of the Discussion, the word 'further' is repeated too many times.

C3634

– Conclusion section: I would try to elaborate a bit more again on the main characteristics of HOLAPS and what it a priori adds upon existing methods. I think doing it in the form of a list (e.g., 'a', 'b', 'c') may help.

– Regarding the comparison to other datasets mentioned in the Conclusion, I would rather recommend the comparison to the more recent and carefully developed WACMOS-ET (Michel *et al.*, 2015; Miralles *et al.*, 2015) and GEWEX LandFlux (McCabe *et al.*, 2015) databases.

I hope some of these suggestions help improve the manuscript. Despite the length of the review I believe the authors have done a very good job at developing the model.

Best regards,

Diego

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C3635

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C3636

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