Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-5-RC1, 2017

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Parameter sensitivity analysis of a 1-D cold region lake model for land-surface schemes" *by* José-Luis Guerrero et al.

Anonymous Referee #1

Received and published: 5 February 2017

The authors present an application of a third-party toolbox and the GLUE methodology to perform sensitivity and uncertainty assessments of heat exchange fluxes simulated by a 1D lake model during the open water season for a small lake in northern Canada. While some interesting material is presented, there is a lack of focus which makes it difficult to evaluate the usefulness of the results in a more general context.

In their parameter sensitivity analysis, the authors identify Kd, the light extinction coefficient, as the most important parameter controlling model performance. They then suggest that Kd should be measured more widely as part of routine limnologic monitoring programs. While I agree with this sentiment, I am concerned that the authors do not present any measurements of Kd but only note that the lake has "an expected Kd

value of ∼2m-1" (p.4 l.23). From the results presented, I can see that Kd is the most sensitive parameter controlling model performance, the failure of the authors to present Kd measurements and then argue for its widespread measurement is not logical and must be re-thought. It is possible that the lake has a markedly different measured Kd than that which resulted in the best model performance. If I am interpreting Figure 9 correctly, this is exactly what the authors show as the MAE for sensible heat flux is minimal when Kd is approximately 0.5. This discrepancy between expected Kd and simulated Kd leading to best model performance seriously undermines the results presented here and calls into question the overall validity of the modelling, suggesting that the authors have obtained the right results (i.e. a good fit to latent and sensible heat fluxes) for the wrong reasons (a model parameter Kd value of 0.5 when the authors expect the true value to be closer to 2.0).

On p.13 l.4-5, the authors note the all too common disconnect between experimentalists and modellers and suggest theirs is a contribution to addressing this problem. I am afraid the results presented which emphasize the importance of Kd for simulating heat transfer and then fail to remark on the disconnect between a hypothesized Kd value of around 2.0 and best model performance with a Kd value of approximately 0.5 only serve to highlight the deep and ongoing disconnection, even amongst co-authors on the same paper.

This difference in Kd values could serve as the starting point for an improved dialog between modeller and experimentalist. For example, what would the consequences have been for model performance if Kd had been fixed at 2.0, and under what circumstances would a hypothesized Kd of 0.5 have seemed reasonable? Throughout the manuscript, I am concerned that the authors are not aware of the relevant literature. For example, on p.1 l.14-15, the authors note that Kd is seldom measured. This is not entirely true, see e.g. Kalff (1992) , Ask et al. (1999), but secchi disk transparency and/or dissolved organic carbon are widely measured, and can be used to estimate Kd, an observation first published in 1929 for marine systems (Poole and Atkins 1929) and later refined for lakes (Carlson 1977; Graneli et al. 1996). Furthermore, while Perez-Fuentetaja et al. (1999) and Tanetzap et al. (2008) are nice papers, I do not believe they are the best ones to support the authors' assertion that multiple processes operate in lakes at multiple time scales. This might be taken as a given from e.g. Kalff (2002).

The model description is inadequate to evaluate the significance of the findings presented. As the original model description paper is not open access, the authors must provide more detail in the present paper. Specifically, they need to provide a description of the manner in which Kd is used in model calculations. The authors also need to provide more detail about model execution. On what time scale and over what date range was the model run? It appears that field observations made at a 30 minute resolution were available. Was the model run on the same time step?

There is too little information provided about the empirical data collection. Over what time period were samples collected and what is the uncertainty in estimated heat fluxes? Using these uncertainties to inform model calibration and sensitivity analysis would have made for a much more informative paper than one which appears to compare modelled values to daily average flux estimates (as seems to be the case from Figure 8) using MAE and NS statistics.

The overview of sensitivity analysis needs to be rethought. In its present format, it is not sufficiently informative. P.6 l.25-26 makes an important and under-appreciated point, but apart from that, much of the text could be deleted and the reader referred to the more thorough discussions identified on p.5 l.23. The authors' description of PSUADE on p. 6 l.15-20 is inadequate. No indication is given as to code availability, language it is written in, etc.

I am of two minds about the description of sensitivity metrics. They are too short, but in light of the authors' subsequent findings, this may not matter as for the task at hand, they do not provide any real advance over older methods. The conclusion I draw from the authors' results is that sophisticated sensitivity analysis toolboxes such as

the PSUADE package they used are not needed for environmental modelling as one can derive the same information from an "old school" GLUE analysis. The dotty plots (if I am interpreting them correctly) suggest that Kd is the only sensitive parameter for both sensible and latent heat fluxes. It does not appear that application of the PSUADE package offers any additional insight above and beyond that obtained from the GLUE analysis. This, in and of itself, is a useful finding as it suggests researchers can concentrate on tried and true methods of sensitivity analysis instead of following the latest fads and fashions.

Minor Comments

P.1 authors – is there an error here and should the third author be Howard Wheater?

p.2 l.6-19 – This discussion of the manner in which lakes are incorporated into climate models is interesting but irrelevant to the authors' stated objectives of performing a sensitivity analysis. While the CSLM has been developed for climate change studies, this is outside the scope of its use in the current paper. Thus, I would ask that the authors delete or greatly shorten this section. Expanding upon the statement on p.2 l. 28-30 would provide more relevant background information.

p.3 l.23 – higher values of Kd do not necessarily indicate more turbid lakes. High dissolved organic carbon concentrations and an absence of turbidity can also result in high Kd.

p.6 l.27 – I dispute the authors' assertion that ". . .evaluation of heat fluxes over northern lakes remain uncommon . . .". I would encourage the authors to consult Rouse et al. (2005), if only to put their results into context.

Figures

Please replace Figure 1 with a bathymetric map of the lake showing the location of the thermistor arrays. This would help the reader to judge the statement made on p.5 l.10 and to better understand the relationship between mean and max depth presented on p.4 l.21.

Figures 2-5 are not terribly useful. Please delete them as one can derive the same information from Table 4.

The information in Figures 6 and 7 could be presented more succinctly as a table.

Figure 8 is encouraging as it shows the model is able to reproduce the observations. I do have some concerns, however. Does Figure 8 present data for a single year? If so, which one? Please also provide some estimate of uncertainties in the latent and sensible heat fluxes.

I have to admit that figure 9 confuses me. I assume that the MAE has units of W/m2? If so, please clarify this in the figure caption. I would like to see a similar set of plots based on the NS statistic.

Figure 10 deserves more consideration in the paper. It is a really useful piece of information that there is a non-monotonic relationship between the MAEs for latent and sensible heat flux. I would strongly encourage the authors to explore how this looks when using the NS, also.

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