Reply to comments on “Parameter sensitivity analysis of 1-d cold region lake model for land-surface schemes” by José-Luis Guerrero et al.

We’d like to start by thanking Zeli Tan and two anonymous reviewers for the time they spent going through this work and providing feedback.

We also would like to than the editorial office and the editor for the prompt handling of this paper.

In this document we address the reviewers’ concerns. The original comments are in boldface and our answers in plain text.

Reviewer Z. Tan, short comment

Dear,

It is a very nice study and important for understanding the role of lakes in the energy balance of cold regions. Just to make you aware that there are already several 1-D lake models for cold regions, especially the pan-Arctic, such as Stepanenko et al. (2011) and Tan et al. (2015).

Stepanenko, V. M., Machul’skaya, E. E., Glagolev, M. V., and Lykossov, V. N.: Numerical modeling of methane emissions from lakes in the permafrost zone, Izvestiya, Atmos. Oceanic Phys., 47(2), 252–264, 2011.

Tan, Z., Zhuang, Q., and Walter Anthony, K.: Modeling methane emissions from arctic lakes: Model development and site-level study, J. Adv. Model. Earth Syst., 7, 459–483, 2015.

Best regards Zeli Tan (tan80@purdue.edu)

Thank you for the kind comments. Here we focused on energy feedbacks. Greenhouse gases such as methane are of course important in the climate system and should eventually be included in large-scale modeling systems. The provided literature could be cited as examples of other 1-D models.

Anonymous reviewer #1.

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.

Thank you for your input and we are glad you found some of the material interesting. We will attempt in the replies to the rest of your comments to clarify the focus and usefulness and the paper.

Modifications of the text will be undertaken upon editorial input, as per HESS guidelines.

The background behind the inception of this paper was to use the relatively scarce latent and heat fluxes modeling data to improve lake modeling within land-surface schemes. This led us to stress the importance of Kd for good modeling results, showing that a wide array of sensitivity analysis methods agree on this result.

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).

And this is precisely the problem: oftentimes Kd is not measured. We were not involved in the field measurement campaign and only made use of the final product. Our point is that Kd should be a routine measurement in all such campaigns, a continuous measurement if possible.

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.

Please refer to our previous comment. We only have an approximate value of Kd, which does not even take into account the temporal variability.

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).

A clearer explanation of the dotty plots will be provided. What Fig. 9 shows is that *MAE* is minimal when Kd is around 0.5.

We do not believe this undermines our results. We might in fact argue that this is the curse of most, if not all, models: equifinality and the disconnect between model parameters and the variable they are supposed to represent.

In this case, the divergence could be explained, e.g., by a possible temporal variability in the value of Kd.

Even if we accept that the measurement was accurate at the time it was taken, this did not coincide with our modeling period and the light attenuation might vary over the course of a season so the parameter in the model is sort of like an "average".

If Kd was actually measured, it could with confidence be set as a constant (perhaps time-dependent) in the model as opposed to be considered a calibration parameter.

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.

Our contribution to this dialog could be resumed as follows: "please measure Kd continuously, this would allow furthering and improving modeling efforts". Please let us insist on the fact that we did not take the measurements ourselves but only made use of them. This should be explicitly stated in the 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 refinedfor 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 reviewer is undeniably right and these references should be added and some of our assertions modified accordingly.

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?

While not open-access, it is still a part of the literature and we did not feel it was required to retake the detailed description already available, albeit behind a paywall. We whole-heartedly agree on the importance of open access, but this is perhaps not the best forum for the issue. We agree that we should provide more details on how Kd affects model calculations and more detail about model execution.

The model was run on a 30 min time step and the results aggregated to an daily time step due to inherent limitation of the eddy covariance data.

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 modeled values to daily average flux estimates (as seems to be the case from Figure 8) using MAE and NS statistics.

We will clarify data description. There are many ways of tackling uncertainty analysis. Using the uncertainty in measurements to assess modeling uncertainty is known as the limits-of acceptability approach. This was not our objective here but should indeed by the basis of a more robust approach to uncertainty evaluation, in our opinion.

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.

This section as originally written, was much more technical in the description. Before submission it was decided to give a more conceptual overview of the different methods instead of focusing on the technical details, which would have yielded a much longer and perhaps murkier paper.

To address the reviewer's concern we could better highlight which of the cited papers provide detailed descriptions of the methods used. Please also note that table 2 provides the original sources for the methods used.

PSUADE is an open-source package written in C++. This should be made clearer.

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 asthe PSUADE package they used are not needed for environmental modelling as one can derive the same information from an "old school" GLUE analysis.

The reviewer is right in it was not our intention to advance existing methods but to apply them.

We strongly disagree that packages such as PSUADE are not needed for environmental modeling. On one hand, different methods make different assumptions that may be more or less warranted and in that sense, to have an entire array of methods available con only be an advantage.

Also, taking only practical considerations into account, the choice of an SA method might boil down to familiarity with these methods and having an array of methods under one roof can help overcome this hurdle through a united framework.

Also, GLUE is computationally demanding and for other models might it might be a practical impossibility to use it.

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.

We would not say that the SA methods do not provide additional insight, but that they agree with the GLUE results and the results are more robust as a consequence of this since the different methods are based on different assumptions, as described in the Methods section.

Minor Comments

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

Thank you for pointing out the typo.

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.

We felt it should be mentioned that this paper came to be as part of an effort of improving land-surface schemes, although we agree this has no direct impact on our narrative. We feel that this section provides necessary background on the evolution of lake modeling of which the CSLM is an example of currently existing approaches. We can expand on fluxes over lakes (p2,l28-30)

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.

The reviewer is right. Algal blooms might also change Kd. A better discussion of this should be provided

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.

The reviewer is right.

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

A revised figure could be provided.

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

We appreciated the visual impact of the shown figures to highlight the importance of Kd.

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.

Results for 2007 are presented. This should be explicitly stated in the caption.

The estimate of the uncertainties can be read from Fig. 23. There is a trade-off in the modelling of the 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.

We did not include a plot for NSE since the results were very similar. This should be explicitly stated.

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.

We should expand the results sections to highlight this.

References

Ask, J., Karlsson, J., Persson, L., Ask, P., Byström, P. and Jansson, M., 2009. Terrestrial organic matter and light penetration: Effects on bacterial and primary production in lakes. Limnol. Oceanogr, 54(6), pp.2034-2040.

Carlson, R.E., 1977. A trophic state index for lakes. Limnol. Oceanogr., 22(2), pp.361- 369.

Graneli, W., Lindell, M. and Tranvik, L., 1996. Photo-oxidative production of dissolved inorganic carbon in lakes of different humic content. Limnology and Oceanography, 41, pp.698-706.

Kalff, J., 2002. Limnology: inland water ecosystems (Vol. 592). New Jersey: Prentice Hall.

Poole, H.H. and Atkins, W.R.G., 1929. Photo-electric measurements of submarine illumination throughout the year. Journal of the Marine Biological Association of the United Kingdom (New Series), 16(01), pp.297-324.

Rouse, W.R., Oswald, C.J., Binyamin, J., Spence, C., Schertzer, W.M., Blanken, P.D., Bussières, N. and Duguay, C.R., 2005. The role of northern lakes in a regional energy balance. Journal of Hydrometeorology, 6(3), pp.291-305.

Anonymous reviewer #2.

Review of ‘Parameters sensitivity analysis of a 1-D cold region lake model for landsurface schemes’ by Guerrero et al.

General comments

This is a reasonably written paper describing an interesting topic in environmental modeling and numerical weather prediction: ‘How do lakes interact with their overlying atmosphere and to what extent can lakes modify their surrounding climate, and the uncertainties in these interactions’. A number of previous papers have addressed similar topics in the past (e.g. Dutra et al. 2010; Balsamo et al. 2012), but the strength of this current paper is the

uncertainty estimation that it provides. Specifically, the authors introduce a third-party toolbox and the GLUE methodology to perform a sensitivity and uncertainty analysis of the different surface heat fluxes simulated by the Canadian Small Lake Model (CSLM), a one-dimensional integral lake model. The authors focus their study on a small lake in northern Canada, which is a good study site as small lakes are the most abundant at the global scale (see further my notes in the specific comments below). Within their sensitivity analysis, the authors find that the light attenuation coefficient, Kd, is the most important parameter controlling model performance and that variable Kd provides the highest uncertainty in surface flux estimates.

Thank you for the time you took understanding the paper.

I don't particularly find this surprising, as others have found that water clarity can have a considerable influence on lake stratification and the turbulent heat fluxes (see Heiskanen et al. 2015) and can also considerably influence the diurnal cycles of heating and cooling in lakes (Woolway et al. 2016), but I do find this an important point to highlight and one that deserves some attention.

Thank you for pointing out these two recent publications. We did make reference to other paper such as Fuentetaja et al. (1999) and Rinke et al. (2010) who highlight the importance of light attenuation in the functioning of the lake. We build upon these contributions by providing numerical estimates of the related uncertainties as well as qualifying and quantifying the importance of the light extinction coefficient.

We should definitely add more recent papers and we would like to thank the reviewer for pointing them out.

While I think this paper will be of interest to those who focus on the integration of lakes within the climate system and for Numerical Weather Prediction, I strongly believe that the paper would be improved if there were more focus on the analysis and the results were put into context of the published literature.

We agree that more emphasis should be put on the more recent literature and we would like to thank the reviewer for providing these relevant references.

Often I found some of the most relevant literature being ignored and/or overlooked and some references, which were included in the text, seem inappropriate or irrelevant. One of my main criticisms is that a thorough literature review is needed to strengthen the introduction and discussion of the results. I provide some examples of relevant studies in this review, but there are many others which the authors should also look into. I strongly suggest a thorough review of the current literature prior to publication.

Please refer to our previous comment.

I find it surprising that the authors specify that Kd is the most important parameter controlling model performance, but do not include any detailed measurements of Kd. In particular, it is very likely that the lake has a different Kd to that estimated from the model sensitivity analysis.

The reviewer is right. Please refer to our reply to the first anonymous reviewer where we stated that we did not make the measurements ourselves.

Overall, I think there is some potential for this paper to be revised sufficiently to make it a valuable contribution to the scientific literature. However, addressing all of the points raised below are needed, in my opinion, prior to this paper being considered for publication in HESS.

Thank you.

Specific Comments Unfortunately the Downing et al. (2006) estimates of global lake size and abundance are no longer supported. Many studies have since shown that the Pareto distribution does not adequately describe the global distribution of lakes. For example, see Seekell and Pace (2011) and McDonald et al. (2012). A more detailed description of the global abundance and size distribution of lakes are provided by Verpoorter et al. (2014) and more recently by Cael and Seekell (2016). Granted that these recent studies to do not consider the smallest lakes of the world (for example, Verpoorter et al. only consider lakes larger than 0.002 km2), but still the authors should read up on these papers and include the relevant citations.

They also provide a more immediate feedback through mass and energy exchanges with the atmosphere' - you need some reference for this. As I'm sure you're aware, these fluxes are quite difficult to calculate (see Woolway et al. 2015a). Further information on these fluxes is needed, in my opinion. Additional information here will allow others who are not experts in the field to understand better the kind of interactions you are talking bout.

Please refer to our previous comments agreeing that a revision of more recent literature is necessary.

"With different albedo, heat capacity and surface roughness than the surrounding land areas lakes also provide more immediate feedback through transfer of heat and moisture exchanges with the atmosphere (ex. MacKay et al. 2009; Xiao et al. 2013; McGloin et al. 2014). While some studies have performed direct measurements of latent and sensible turbulent heat fluxes from eddy covariance systems over lakes and reservoirs (e.g. Blanken et al. 2000; Vesala et al. 2006; Blanken et al. 2011; Nordbo et al. 2011 McGloin et al., 2014a) these measurements can be difficult and expensive and as (ex. McGloin et al. 2014) such improved modelling approaches are necessary."

Tanentzap et al. (2008) did not consider the influence of variations in thermocline depth on fluxes to the atmosphere, thus I don't think this reference is appropriate.

Thank you for pointing out the error. – "and ignore the internal thermal structure of lakes, which influences surface temperatures and thus fluxes to the atmosphere (Mackay 2012).

'Rinke et al. (2010) illustrate the feedback between phytoplankton and thermal structure. . .' - There are other studies which you could also cite. For example, Mazumder et al. (1990) showed this over two decades ago. There are many other studies since then which I think the authors should read up on.

We could add a few more references to this (e.g. Tilzer, 1983; Tilzer 1988; Mazmuder et al. 1990)

Tilzer M.M. (1983) The importance of fractional light absorption by photosynthetic pigments for phytoplankton productivity in Lake Constance. Limnology and Oceanography, 28, 833–846.

Tilzer M.M. (1988) Secchi disk – chlorophyll relationships in a lake with highly variable phytoplankton biomass. Hydrobiologia, 162, 163–171.

It may also be worth mentioning that, on a regional scale, Samuelsson et al. (2010) found that the presence of lakes induces a warming on the European climate, and an observational study by Rouse et al. (2005) found that high-latitude lakes strongly enhance evapotranspiration when added to a landscape. A useful study, which I think the authors should cite, is Heiskanen et al. (2015). The authors should also look at the papers cited by Heiskanen et al. (2015) as these will be of direct relevant to this study. In addition, a paper by Rose et al. (2016) describes that water clarity can either amplify or suppress lake surface water temperatures, which in turn will influence their interaction with the atmosphere. Please read the Rose et al. (2016) paper and look at the references within.

A lake depth sensitivity analysis was undertaken by Balsamo et al. (2010) and might be worth mentioning also.

Thank you for the updated references. Please refer to our previous comments regarding the need to update the paper.

P2L26 - What is a small lake? How do you characterize a lake as small?

There is a lot of variation on this in the literature. It varies from on the order of ~ 10 km2 (Verpoorter et al. 2014) to on the order of ~1km2 (McGloin et al. 2014)

P3L24 - Water clarity can have numerous other influences on lake temperatures. I think this section needs to be expanded. A few examples include its influence on the thermal structure of lakes (e.g. Persson and Jones 2008), its influence on the absorption of heat during the day and greater release in the evening leading to larger diurnal cycles (Woolway et al. 2015b) and influencing the likelihood of diurnal stratification as well as seasonal stratification. Also, studies have shown that surface waters have been browning over the last few decades (Roulet and Moore 2006). All of these points should be included and expanded.

Please refer to our previous comments regarding these points.

Italics aren't needed for the description of all units.

Agreed.

P12L30 - The authors state that Kd is not often measured and measuring Kd for every lake might be a practical impossibility. In my opinion, this is one of the largest uncertainties in the inclusion of lakes in NWP. For example, in ECMWF's IFS Kd is assumed equal to 3 for all lakes, which could result in numerous biases in the turbulent heat fluxes. While I somewhat agree with the author's statements here, it may also be worth mentioning that satellites can estimate Kd, so there are possibilities in improving lake surface water temperature simulations. For more information, see Torbick et al. (2013) for information on how satellites can potentially be used to estimate secchi depth, which can be used as an indicator of Kd.

Thank you for the references.

P13L3 - 'this kind of monitoring has never been performed' - This isn't true. Lake monitoring stations now often have light sensors above and below the water surface and are thus used to determine water clarity and Secchi depth observations are traditionally recorded. I suggest the authors look through the literature to find examples of where they've been used. I'm almost certain that this information has not been used in NWP or climate modeling, but I hope in the future meteorologists and limnologists will work closer to address this and similar issues. A literature search on this topic is also needed in my opinion.

Our statement was untrue. It should however be fair to say that such measurements are not commonplace, especially when it comes to continuous measurements.

P13L9 - I don't think this can be a main conclusion as unfortunately it is not unknown. For example, see Heiskanen et al. (2015).

It can be instead expressed as confirming and building upon Heiskanen's results.

There doesn't appear to be much discussion in this paper. I would recommend restructuring the paper to include separate 'Results' and 'Discussion' sections and perhaps reduce the conclusion to one or two paragraphs. This, in my opinion, would make the paper easier to digest.

The conclusions should definitely be rewritten in light of the more recent literature.

I don't find many of the figures presented in the paper very informative. They seem to all show similar results. Much of this information could be shown in 1 or 2 figures, in my opinion.

It is one of the points of the paper that the results of the different SA methods are similar. This reinforces the importance of the light attenuation coefficient.

Figure 1 needs more information. For example, can the authors add a smaller inset map to show where the lake is? Also, the figure would need a 'scale ruler' so that the reader can easily interpret the size of the lake.

Please refer to our reply to a similar comment from the other anonymous reviewer.

At first glance, I don't particularly understand Fig. 8. It isn't clear what the grey regions represent as one would expect the grey area to be an envelope that surrounds the main (blue) line?

We could include a better description of the GLUE methodology and what the uncertainty bounds represent. An envelope around the main blue line should not be expected if the model fails to adequately reproduce the variable.

Fig. 9 - Isn't irradiance a term often used to describe solar irradiance and not the turbulent fluxes? Also, why isn't there an x-label on the bottom panels?

The x-label was placed on top instead. It is the parameter name.

The reviewer is right, although the units are the same, irradiance is often thought to refer solely to solar irradiance. The caption could read "heat-flux"

Fig. 10 - I'm not sure how to interpret this figure. Can you please provide a better description of what we're seeing? I think a more detailed discussion of this figure should be given in the text.

Reviewer one also suggested to make better use of this figures. It illustrates the trade-offs made by the model when simulating latent and sensible heat.

Table 1 - The square brackets appear the wrong way round in the fourth column.

The brackets were consciously placed as such to show that the set does not include its bounds.

References:

Balsamo G, et al (2010), Deriving an effective lake depth from satellite lake surface temperature data: a feasibility study with MODIS data. Boreal Environment Research 15:178-190.

Balsamo G, Salgado R, Dutra E, Boussetta S, Stockdale T, Potes M (2012), On the contribution of lakes in predicting near-surface temperature in a global weather forecasting model. Tellus A 64, 15829.

Cael BB, Seekell DA (2016), The size-distribution of Earth's lakes. Sci Rep 6, 29633.

Dutra E, Stepanenko VM, Balsamo G, Viterbo P, Miranda PM, et al. (2010), An offline study of the impact of lakes on the performance of the ECMWF surface scheme. Boreal Env. Res. 15:100–112.

Heiskanen JJ, et al. (2015), Effects of water clarity on lake stratification and lakeatmosphere heat exchange. J Geophys Res Atmos 120:7412-7428

Mazumder A, Taylor WD, McQueen DJ, Lean DR (1990), Effects of fish and plankton and lake temperature and mixing depth. Science 247:312–315

McDonald CP, et al. (2012), The regional abundance and size distribution of lakes and reservoirs in the United States and implications for estimates of global lake extent. Limnol. Oceanogr. 57:597-606.

Persson I, Jones ID (2008) The effect of water colour on lake hydrodynamics: a modeling study. Freshwater Biol 53:2345-2355

Rose KC, Winslow LA, Read JS, Hansen GJA (2016) Climate-induced warming of lakes can be either amplified or suppressed by trends in water clarity. Limnol Oceanogr Lett 1:44-53

Roulet N, Moore TR (2006) Environmental chemistry: Browning the waters. Nature 444:283–284.

Seekell DA, Pace ML (2011), Does the pareto distribution adequately describe the size-distribution of lakes? Limnol. Oceanogr. 56(1):350-356.

Torbick N, Hession S, Hagen S, Wiangwang N, Becker B, Qi J (2013) Mapping inland lake water quality across the Lower Peninsula of Michigan using Landsat TM imagery. Int J Remote Sens, 34:7607–7624.

Verpoorter C, Kutser T, Seekell DA, Tranvik LJ (2014), A global inventory of lakes based on high-resolution satellite imagery. Geophys Res Lett 41:6396-6402.

Woolway, R.I, Jones, I.D., Hamilton, D.P. et al. (2015a). Automated calculation of surface energy fluxes with high-frequency lake buoy data. Environmental Modelling & Software 70, 191-198.

Woolway, R.I., Jones, I.D., Feuchtmayr, H. et al. (2015b). A comparison of the diel variability in epilimnetic temperature for five lakes in the English Lake District. Inland Waters 5(2), 139-154.

Woolway, R.I., Jones, I.D., Maberly, S.C. et al. (2016). Diel surface temperature range scales with lake size. PLoS One 11(3): e0152466. doi: 10.1371/journal.pone.0152466