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Discussions

Interactive comment on "Lake surface water temperatures of European Alpine lakes (1989–2013) based on the Advanced Very High Resolution Radiometer (AVHRR) 1 km data set" *by* M. Riffler and S. Wunderle

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We would like to thank both reviewers for their useful and thoughtful comments which help to improve the manuscript. Below, we provide detailed answers and explanations to the questions raised by Reviewer 2.

GENERAL COMMENTS

Comment: This article presents a new Lake Surface Water Temperature (LSWT)

retrieval system using AVHRR data from 1989 to 2013 for all major European alpine lakes, including validation with in situ data.

The article is well presented and organized and will provide a significant contribution to the field of lake surface water temperature research once the following issues have been adequately addressed:

1) The climate justification of including smaller lakes (section 2.1) in a relatively small geographic region is not well supported. The authors need to explain how they think including more lakes will give a more stable or accurate climate trend, especially given the potential uncertainties that could arise due to pixel contamination from land effects (e.g. straylight) in small lakes with medium resolution imagery (~ 1 km). 2) The validation results and approach are questionable considering that much of the in situ data was not provided coincidentally with the satellite overpass and also on lake shorelines that were not spatially representative of the satellite pixel. The other concern is the issue of thermal signal from land pixels 'bleeding' into lake water pixels for the smaller lakes. Although visible data thresholds are used to account for this, the effects of thermal infrared straylight are quite complex, and I don't think can be completely and confidently mitigated, especially during daytime observations. I think what's needed in the paper is a more thorough discussion of all uncertainties involved (and their estimates) with the validation effort, and possibly better ways of reducing the uncertainties. For example, a more consistent and reliable validation method would be to use the Radiance-based method (e.g. see work done by Wan et al.). 3) The authors should provide more detail on the skin effect formulation used, uncertainties involved with its application to lake data, and consider applying a more representative formulation for lakes in the future. e.g. see recent work done by: Wilson, R. C., S. J. Hook, P. Schneider, and S. G. Schladow (2013), Skin and bulk temperature difference at Lake Tahoe: A case study on lake skin effect, J. Geophys. Res. Atmos., 118(18), doi:10.1002/jgrd.50786.

Answer:

Ad 1: Including lakes with various sizes and thus different morphological characteristics in a regional area could, for instance, be interesting for investigations on whether these lakes react in a similar way to the changing climate. We will express this more clearly in the revised manuscript.

Ad 2: As written in the answer to Reviewer 1, we made some changes to the section presenting the "validation". Firstly, we renamed the entire section into "Inter-comparison [...]", since it is not a validation in its strict sense. Secondly, we will extend the discussion of the various error sources included in this comparison.

Regarding the concerns of "the thermal signal of land pixels 'bleeding' into lake pixels" we assume that the reviewer is addressing the question of the "adjacency effect"? We did not consider this effect, since various publications (e.g. Richter et al., 2006: Influence of the Adjacency Effect on Ground Reflectance Measurements, Geoscience and Remote Sensing Letters, IEEE , 3(4), 565–569.) state that this effect mostly concerns high resolution (< 100 m) imagery within the visible and near-infrared spectrum, since the scattering efficiency is decreasing with increasing wavelength.

We do not include a radiance-based validation approach in the revised manuscript, due to the lack of coincident radiosonde measurements. We will, however, compare the proposed data set with the standard MODIS land surface temperature and emissivity data set.

Ad 3: We added more details about the skin-to-bulk conversion and included the study of Wilson et al. (2013) in our discussion.

SPECIFIC COMMENTS

p314, L16-20. What emissivity information was used in the simulations?
Answer: The emissivity is derived within RTTOV-10 based on viewing angle and 10m wind speed (cf. Saunders, R., Hocking, J., Rayer, P., Matricardi, M., Geer, A., Bormann, N., Brunel, P., Karbou, F., and Aires, F.: RTTOV-10 Science and Validation Report, Tech. rep., NWP SAF, EUMETSAT, 2012.).

p314, L29: What time of year were the 180 days represented of?

Answer: The time window of ± 180 days is a moving window, i.e. the split-window coefficients for a particular day were derived by choosing all simulated values within ± 180 days. Consequently, the data base of simulated values covers the entire period between 1989–2013. We reformulated the sentence into "Finally, we derived the split window coefficients for each day between 1989–2013 by applying a robust multiple linear regression analysis between the simulated satellite data and the LSWT including ± 180 days of simulations for the calculation of the coefficients." to maybe state that more clearly.

p315, L6: The parameterization for this equation should at least be shown. Also 'mostly reduces' is a vague statement. The effect should be quantitatively stated or shown in a plot.
Answer: We added the equation and a quantitative statement in terms of the reduction of the overall bias.

p316, L25: I would not consider this a pure validation due to the uncertainties involved with the in situ data, but rather more of an evaluation, or pseudo-validation.
Answer: In the answer to Reviewer 1, we mention that we renamed this section into "Inter-comparison with in situ and MODIS data", for which we now also include a comparison with the MOD11_L2 product. In addition, we replaced the word "validation" with "comparison" (or similar) in the entire manuscript.

p317, L25: This lake is only 2-3 km wide so I'm assuming the 3x3 pixel average was not possible, and neither the spatial homogeneity test?
Answer: Some of the pixels in the 3×3 pixel matrix for this location are classified as land and are therefore masked out. Nonetheless, the spatial averaging and homogeneity test were still possible.

TECHNICAL CORRECTIONS

p306, L3: Replace 'Thus' with either 'As a result' or 'Consequently'
Answer: Changed

p306, L11: '.. ten more years, offering a ...'
Answer: Changed

p307, L20: Also add reference: Schneider, P., Hook, S. J., Radocinski, R. G., Corlett, G. K., Hulley, G. C., Schladow, S. G., et al. (2009). Satellite observations indicate rapid warming trend for lakes in California and Nevada. Geophysical Research Letters, 36
Answer: Added

p307L22: Remove 'be done'
Answer: Removed

p311, L1: State why nighttime measurements should give better results.
Answer: This part has been removed, since both daytime and night-time data is included now.

p312, L3: Here and elsewhere: 'Kelvin' should be 'degrees Kelvin', or simply 'degrees'.
Answer: The physical correct notation is "Kelvin", thus we did not change it.

p312, L22: Replace 'has to be admitted', with 'should be noted'
Answer: This part has been reformulated into "It should be noted, however, that the data from NOAA-16 exhibits more of these spikes than other satellites which is maybe related to the problems with the scan motor this satellite had [...]".

p312, L29: 'were corrupted'
Answer: Changed

p313, L5-10: Consider showing the basic thermal infrared radiative transfer equation to better illustrate the atmospheric effects.
Answer: We added a short paragraph at the beginning of this section including a simplified version of the equation.

p316, L11: replace 'prolonged atmospheric pathway' with 'longer atmospheric pathlength'. The longer pathlength increases the uncertainty due to non- linear effects of the Planck function.

Answer: Changed

p317, L21: replace 'are apparent' with 'were found'
Answer: Changed

p318, L10: replace 'admitted' with 'stated'
Answer: Changed

p320, L16: '.. NOAA-12, which has a ...' also 'too cold' should be 'cooler'
Answer: Changed

p321, L5-10: I don't think you can rule out the effects of undetected cloud as an error source as well.
Answer: We reformulated this phrase into "Potential error sources are uncertainties in the spatio-temporal match-up between satellite and in situ measurements and undetected cloud pixels (especially thin cirrus clouds)."

p321, L19: What do you define as a significant warming trend? It's not obvious from the text.
Answer: We reformulated this part and added an example with the trend of Lake Constance.

Table 3: Show the total RMSE and Bias for each method and Lake.
Answer: We added these values to Table 3.

Fig. 4: Figure is difficult to read and should be much larger. Also it appears the last panel is an average of results for different satellites, this was not made clear in the caption.
Answer: We changed the figure caption accordingly. The size of the figure can easily be increased. We will clarify this issue with the production office of the journal.

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