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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

Anonymous Referee #1

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GENERAL COMMENTS

The authors provide a very interesting study about the use of multi-platform AVHRR data for retrieving lake surface water temperatures (LSWT) at several alpine lakes for the period 1989 through 2013. They describe in detail the retrieval algorithm including correction schemes for channel calibration and further demonstrate the feasibility of using an existing split-window retrieval with coefficients derived from a radiative transfer model by validating LSWT at several small to moderate-size lakes located in Switzer-

land. The manuscript in general is clearly written, well organized, and provides a coherent description of the performed work. Overall the manuscript is a much needed contribution to the field of lake temperature remote sensing and I highly recommend the paper to be published in ESSD after the remaining methodological and technical issues, which are outlined below, have been adequately addressed.

Firstly, my main concern is related to some of the validation methodology applied in demonstrating the quality of the developed data product. For example, the validation carried out using in situ sites with daily or even monthly samples (for which the exact observation time varies and is not even known exactly!) is in my opinion quite problematic and the results are difficult to interpret due to the large number of possible error sources involved (spatial distance, temporal distance, bulk/skin conversion using a model derived for the ocean, etc.). Furthermore, even for the other sites that do provide hourly sampling, the in situ temperature was observed either at or close to the shoreline, whereas the satellite time series were generally extracted many kilometers away in the center of the lake. Now, these are still interesting comparisons and the results (Fig. 4) are surprisingly good given the various error sources (although quantitatively they are more similar to general land surface temperature validation studies with their variable emissivity than other lake-specific LSWT validation studies with their know and homogeneous emissivity), but in my opinion they are not quite suitable as an actual "validation", which should ideally make use of radiometer-based skin temperatures measured in the center of the lake at high temporal frequency. Such data is relatively sparse and I do not believe it exists for the study sites used here, however I think the authors could significantly improve the quality of the manuscript if they a) discussed these issues and uncertainties of the current validation approach in more detail in the text and b) explored other ways to strengthen the quality of the validation. For example, in order to get around the lack of suitable validation data over their study sites, I encourage the authors to also explore other validation methods, which are not dependent on in situ data, such as radiance-based techniques (e.g. Wan and Li, 2008; Coll et al., 2009), or relative inter-comparisons with other LST products such as those

retrieved from MODIS or AATSR.

Secondly, another major concern relates to the orbital drift exhibited by most of the earlier AVHRR platforms (e.g. N11, N14, N16) and the associated shift of the local sampling time throughout the day. As the authors chose to study only daytime retrievals, diurnal variability of the lake surface temperature can be quite large and a shift in observation time will most definitely affect the usability of the resulting time series for trend analysis (which appears to be the primary intended application of the presented dataset). While the authors have provided in Table 3 a validation of the individual NOAA platforms, this does not address this issue sufficiently as the in situ observations were selected closest in time to the satellite observation, so the impact of observation time drift on trend analysis is not accounted for here. At a minimum, I believe a thorough discussion of this topic is required in the manuscript, explaining the phenomenon as well as its impact on the stability of the time series, and what can be done to mitigate this problem. I think it is also necessary to provide plots or data documenting this in more detail and quantifying the impact on the dataset, particularly given the fact that the authors are planning on using the same approach and dataset in a follow-up paper for European-wide trend analysis.

References

Coll, C., Z. Wan, and J. M. Galve (2009), Temperature-based and radiance-based validations of the V5 MODIS land surface temperature product, J. Geophys. Res., 114(D20), 1–15, doi:10.1029/2009JD012038.

Wan, Z., and Z.-L. Li (2008), Radiance-based validation of the V5 MODIS land-surface temperature product, Int. J. Remote Sens., 29(17-18), 5373–5395, doi:10.1080/01431160802036565.

SPECIFIC COMMENTS

P307L20: I think this list of references should also include Austin and Colman (2007)

who were the first to describe this phenomenon.

Austin, J. A., and S. M. Colman (2007), Lake Superior summer water temperatures are increasing more rapidly than regional air temperatures: A positive ice-albedo feedback, Geophys. Res. Lett., 34(L06604), 1–5, doi:10.1029/2006GL029021.

P309L14: What is the reason for limiting the dataset to lakes in or near the Alps? Is this purely a data volume/processing issue or is the retrieval algorithm specifically designed for such lakes and would not work as well for lakes in other parts of Europe? Please describe the reasoning behind this limitation briefly.

P309L17: ArcLake (http://www.geos.ed.ac.uk/arclake/) in Phase-3 now also looks at a lot more of the smaller lakes than previously

P311L1: I do not understand why the study limits itself to daytime data only which are much less useful for time series analysis than nighttime data. Does the additional information from the visible channels for cloud-masking really outweigh the substantial problems caused by diurnal heating/orbital drift, particularly as one of the major intended applications of this dataset appears to be trend analysis? Please discuss the reasoning behind this choice a bit more.

P314L21: Why not just use ERA-Interim which covers the entire study period as one homogeneous dataset?

P315L4: As the skin/bulk conversion is quite essential for this product and significantly affects the validation results, please provide some more information on the algorithm used here. Is it also based on the frequently used Fairall et al. (1996) method or is it a different approach? Wilson et al (2013) is probably also quite relevant for this section as they specifically described and modelled the skin effect over a lake.

Fairall, C., E. Bradley, J. Godfrey, G. Wick, J. Edson, and G. Young (1996), Cool-skin and warm-layer effects on sea surface temperature, J. Geophys. Res., 101(C1), 1295–1308.

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.

P316L12 What is the reasoning behind the angle threshold of 45 deg? Please comment briefly on how this was derived.

P317L5 I think this approach is quite questionable as it is a bit of an apple/orange comparison. I wonder if this should be called something more along the lines of "comparison" rather than "validation", given the huge uncertainty this time difference introduces.

P318L15 Same here. I think this can still provide interesting comparisons, but I would not want to call it a "validation" given the various sources of uncertainty.

P319L11: "as described in Sect. 3" - but it is not really described there? At minimum you should provide the underlying equation and the model coefficients. Also, Wilson et al (2013) is probably relevant for this section as well.

P320L1: "convinced that the EPFL in situ data are not reliable". This is a pretty bold statement given the amount of uncertainty in the "validation" for this site (spatial distance between observations, interpolation between hourly values, skin/bulk conversion). Would it be possible to check with the providers of the EPFL in situ data about this issue to see if this statement is indeed correct?

P321L14: "sufficient for climate related studies". I think this is worded too strongly since this statement is not backed up sufficiently by this study. First, the used validation methodology includes various sources of errors (spatial distance between observations, interpolation between hourly values, skin/bulk conversion) which are generally eliminated as much as possible for actual validation exercises, and which decrease the accuracy of the validation for the individual satellite platforms. Second, the potentially large impact of the orbital drift (particularly for daytime data!) is not taken into account, as far as I can tell. Third, other events such as aerosols from volcanic eruptions (e.g.

Pinatubo in 1991/1992) or similar, which have the potential to significantly alter satellite time series and associated trends, are not studied. The authors mention at the end of the manuscript that some of these issues will be investigated in a follow-up paper looking at Europe-wide climate trends, and this will be a truly excellent and welcome addition, building upon the current study. However, the submitted manuscript in my opinion does not yet sufficiently demonstrate the long-term stability of the AVHRR time series for climate applications. As such, I think the authors need to either weaken this statement considerably or (preferably) include additional material that actually demonstrates that the long-term stability of the time series truly is sufficient for climate applications.

Table 1 and 2: It would be very useful to include an additional Figure showing a map of the location of the study sites. This would be particularly helpful for the sites with in situ observations. Such a map would ideally indicate the locations of the AVHRR 3 x 3 pixel extraction as well as the location where in the lake the in situ observations were made. This would be very helpful for being able to better interpret the validation results.

TECHNICAL CORRECTIONS

P309L1: Better call it the "Radiative Transfer for TOVS (RTTOV) software package" or similar to make clear that it is code.

P309L11 better say "and provides a"

P309L12 better say "data used for deriving LSWT"

P309L14: "all major European alpine lakes" This can be misunderstood as the term "alpine" is often used to refer to other high mountain ranges besides the Alps (e.g. "alpine climate"). Maybe rewrite to something like "all major lakes located in or near the Alps" or similar?

P310L20 It is not clear to me why Metop-A is abbreviated as M02?

P312L22 This sentence is not clear (why "although"?), please consider rewriting.

P314L27: "Theta_vs": The plural "s" looks awkward, maybe better write "used eight different values of 'Theta_v'" or so to get around this notation

P314L28 I recommend writing "multiple linear regression analysis" or similar rather than "multi-linear fit". By the way, can you describe what makes it robust? Did you use a non-traditional regression approach?

P316L12: Kilpatrick and Oesch are cited twice in the same sentence.

Figure 3: Please add in the caption to this Figure again a brief statement that RT and NN stands for the RTTOV and NOAA NESDIS methods, respectively.

Figure 4: This Figure needs to be made considerable bigger. It is very difficult to read in the current extremely small size. Furthermore I highly recommend showing the validation statistics that are given in this figure also in a separate Table for clarity.

Interactive comment on Earth Syst. Sci. Data Discuss., 7, 305, 2014.