Earth Syst. Sci. Data Discuss., 7, C249–C257, 2014
www.earth-syst-sci-data-discuss.net/7/C249/2014/

Open Access Earth System Science Data 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

M. Riffler and S. Wunderle

michael.riffler@oeschger.unibe.ch

Received and published: 28 October 2014

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 too the questions raised by Reviewer 1.

GENERAL COMMENTS

Comment: 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.

Answer: We agree that the comparison between the in situ and satellite data included in our manuscript is not a validation in its strict sense, since two different physical properties (bulk and skin temperature) of a lake are compared. As a consequence, we decided to rename section 4 into "Inter-comparison with in situ and MODIS data". With this change we also introduce an inter-comparison with the standard MODIS/Terra

LST/E 5-Minute L2 Swath 1 kilometer data set (MOD11_L2). We did not apply the radiance-based techniques due to the lack of radiosonde data during the individual satellite overpasses. We could only use atmospheric profiles from numerical weather prediction data, which by itself would introduce new uncertainties.
Regarding the various error sources included in the inter-comparison between satellite and in situ data, we were maybe not clear enough to explain that the final data set based on pixels/areas which were extracted from the lake centre (Table 1) was not directly compared with the in situ locations. We rather compared in situ temperatures with satellite pixels which were extracted in the vicinity of the in situ locations (Table 2). We have changed the text in the manuscript accordingly. In addition, we discuss the error sources included in this inter-comparison in more detail than in the discussion paper.

Comment: 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.

Answer: In the meantime, we were able to process the night-time data as well and have created a combined data set including both daytime and night-time overpasses. We also address the issue of satellite drift in the revised manuscript. For this reason, we derived the seasonal trends for Lake Constance for both in situ and satellite data. It turned out that these trends are very similar and that the satellite data set is not showing any artificial trends from the strong drifts of the early satellites. We address this issue in the revised manuscript in more detail.

SPECIFIC COMMENTS

P307L20: I think this list of references should also include Austin and Colman (2007) who were the first to describe this phenomenon.
Answer: We have included this article at this position and in the reference list.

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.
Answer: The project was funded by the Swiss GCOS Office and the data set presented is a result of this funding contract. However, based on the results of this project, we were recently able to get the funding for an European-wide project and will therefore extend the data set in the near future. The retrieval method is not restricted to the Alps, it can be applied to any region, if the necessary satellite and atmospheric profile data are available.

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.
Answer: We added the ArcLake project in the revised manuscript.

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 substan-

tial 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.
Answer: As written under GENERAL COMMENTS, we processed the night-time data as well having now both day and night-time data included in the final data set.

P314L21: Why not just use ERA-Interim which covers the entire study period as one homogeneous dataset?
Answer: In the new version of the data set (including the night-time data), the radiative transfer modelling is now based on ERA-Interim data. We have changed the text accordingly.

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.
Answer: The algorithm for the skin-to-bulk-correction is the wind-dependent parametrization of Minnett et al. (2011). In addition to the information we have provided so far, we now include the equation for this parametrization and also mention the study of Wilson et al. (2013).

P316L12: What is the reasoning behind the angle threshold of 45 deg? Please comment briefly on how this was derived.
Answer: This test has been adopted from Kilpatrick et al. (2001) to account for the

increasing path length through the atmosphere which is associated with larger retrieval uncertainties. In addition, at a scan angle of 45° the pixel size already increases to ~ 2.2 km (along the scan direction), for 50° the size would be ~ 2.7 km. We decided to stick with the 45° to have a balance between still reasonable retrievals and loss of data points. This has already been explained in the manuscript, but we added the number for the pixel size for clarity in the revised manuscript.

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.
Answer: As mentioned under GENERAL COMMENTS, we changed the heading of this section into “Inter-comparison with in situ and MODIS data” and call it now a comparison with in situ stations. Since a high-quality validation site is not available in the study region, the comparison with in situ observations should give an estimate of the data reliability and stability. In addition, we added for one location a short trend analysis to demonstrate how well the satellite data compares with in situ observations.

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.
Answer: See answer above.

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.
Answer: We give more details about the parametrization and also refer to Wilson et al. (2013).

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 conver-

sion). 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?
Answer: We removed this statement in the revised manuscript.

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.
Answer: In the revised manuscript we discuss this in more detail and also include an example with the trend of Lake Constance demonstrating the stability of the proposed data set.

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.
Answer: We created a figure showing the locations of the in situ sites used for the comparison and the lakes and locations for which the final data set was extracted.

TECHNICAL CORRECTIONS

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

P309L11: better say "and provides a"
Answer: Changed

P309L12: better say "data used for deriving LSWT"
Answer: Changed

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?
Answer: Changed

P310L20: It is not clear to me why Metop-A is abbreviated as M02?
Answer: This is the official abbreviation for Metop-A and leads to consistent “short names” in Table 3.

P312L22: This sentence is not clear (why "although"?), please consider rewriting.
Answer: We changed this section into “It becomes obvious that periods with signal corruptions frequently occur and the different calibration techniques lead to temperature differences of several Kelvin. 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 [...]”

314L27: "Θ_vs": The plural "s" looks awkward, maybe better write "used eight different values of Θ_v" or so to get around this notation
Answer: Changed

314L28: 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?
Answer: Changed. The regression had been run two times. In the first run, we applied a standard multiple regression analysis and determined residuals of the individual values. In the second run, a weighted regression analysis based on the error information from the first run was performed.

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

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.
Answer: Changed

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.
Answer: The individual figures are in vector format and can easily be scaled to a larger size. Depending on the output format of the final manuscript we will either place each panel as a separate figure or increase the size of the individual plots. This will be clarified with the production office of the journal.
We also created a new table containing the statistical results of Fig. 4.

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