

## ***Interactive comment on “The water temperature characteristics of the Lena River at basin outlet in the summer period” by Vera Fofonova et al.***

### **Anonymous Referee #1**

Received and published: 24 June 2016

### **General comments**

The present paper concentrates on the study of stream temperature records collected at two sites in the lower part of the Lena River basin over the last 60 years. It mostly aims at explaining why temperature measured at the site which is farther north is warmer in summer than temperature measured at the other site, although the opposite would have been expected based on the fact that air temperature is colder at the first site. The authors succeed in identifying the probable causes explaining this phenomenon. Their work is certainly relevant for HESS, although the manuscript requires major revisions in my view. Below are some general points that should be considered by the authors when revising the manuscript.

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1. The article should focus more on its main message, i.e. the explanation of the positive temperature difference between Habarova and Kusur. In my opinion, the reason why the temperature difference is considered to be surprising should be explained more clearly and earlier in the manuscript. In addition, the analysis of the temperature trends at Kusur and Habarova over different time periods (Table 2 and lines 13–27 on page 5) divert the reader from the main findings of the paper. These trends are not used in the remaining of the discussion and also do not represent substantial new findings in comparison with the work of Liu et al. (2014). In order to clarify the manuscript, Sections 3 and 4 could be merged into one and subdivided into three subsections: (1) presentation of the data (Fig. 5), (2) presentation of the temperature “anomaly” (Figs. 6 and 7), and (3) discussion of the possible causes for the “anomaly”.
  
2. Regarding the numerical simulations, the use of a regular mesh grid with cells of size  $1\text{ m} \times 24\text{ m} \times 100\text{ m}$  appears to me as highly questionable. From my own experience, the mesh resolution is typically very fine at the walls (i.e. at the banks and over the river bed) and progressively coarsened towards the center of the simulation domain. According to the usual recommendations when using wall functions, the height  $z$  of the cells in contact with the walls should be chosen such that the non-dimensionalized height  $z^+$  lies approximately between 30 and 500 (Versteeg and Malalasekera, 2007). In the present case, this condition results in  $z$  having to be chosen between about 1 mm and 1 cm, i.e. much smaller than the value of 1 m used in the manuscript. Although the mesh used by the authors might be possibly sufficient to prove that a cold current forms along the right river bank, I strongly recommend to investigate the impact of the mesh resolution on the numerical solution (see e.g. Shen and Diplas, 2008). In order to spare computational resources, the domain length could be reduced from 20 km to a bit more than 5 km, since the distance between the tributary located further upstream and Kusur station is about 5 km (according to Fig. 1).

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3. More generally, additional information should be provided about the numerical simulations. In the first numerical experiment, it is not clear whether the Lena River is approximated as a channel with a rectangular cross-section or whether the real bed profile is used. Similarly, nothing is said about the way the lateral tributaries are modeled. Regarding the second numerical experiment, the simulation configuration is missing: which mesh grid is used? Is the river approximated as a rectangular channel? Is Tit-Ary island modeled or not? Figures showing the simulation domain with the numerical results would be a valuable addition to the manuscript.
  
4. Formulation of the sentences should be revised. Some words are used inappropriately, which makes the manuscript hard to understand. For example, “elevation” is used instead of “water depth” on the first line of page 9, which confused me for a long time. The authors also speak about a “frequency of optimization process” in line 16 of p. 12, which is highly cryptic. Other examples can be found in the Specific Comments below. More generally, it is sometimes hard to understand the point that the authors want to make: statements and counter-statements follow each other, without the authors saying what they think is right (see e.g. point 15 in Specific Comments below). Some facts are stated as if always true, before being proven wrong later in the text. This makes the explanations more difficult to follow and sometimes confuses the reader. Three examples follow:
  - p. 4, ll. 14–27: Based on the observation of vertical stream temperature profiles, the authors state that the “vertical temperature distribution is uniform within [the] cross-section at both Habarova and Kusur”. Later, in lines 21–34 on page 7, the authors discuss measurements which prove the exact opposite, namely that “surface water temperature measurements at Kusur GS lack representativeness”.
  - p. 13, ll. 5–28: The authors state that the regions of the river bed which

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are expected to contribute most to the stream warming are those located in shallow, tranquil flow areas. At the end of the paragraph, they actually prove that the shallow flow area around Island Tit-Ary is colder than expected due to cold water released from the island.

- p. 7, ll. 4–11: According to the authors, “it is obvious that during July–August the heat should be transferred from [the stream] water to the sediments”, whereas on p. 14 at ll. 29–31, they conclude that a substantial heat transfer must take place from the sediments to the stream.

## Specific comments

The following list of specific comments is admittedly long, but should be considered as a set of recommendations as to where the manuscript could be improved.

1. p. 2, ll. 8–9: “the existing analyses of stream temperature [...] are fragmentary and cannot provide the aggregate picture”. This sentence is probably overstated and should be moderated.
2. p. 2, l. 13: “These data are rarely used”. It is not clear which data have already been used by other authors, and which ones are presented here for the first time. This should be clarified in Sect. 2, where the data is described into more detail.
3. p. 2, ll. 14–17: I recommend to give more explanations on the positive temperature difference observed between Habarova and Kusur, namely briefly explain why this difference is surprising. I would avoid mentioning the station names since they have not been introduced yet: it would be sufficient to indicate that one station is located 200 km north from the other. There is no need to go into too much detail though, since the complete explanation should come later in the text (see General Comments above).

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4. p. 2, l. 22: “the water temperatures measured at Kusunur [...] reflect the thermal conditions of the Lena River in general”. This statement is certainly overstated, as temperature evolves continuously along the river (see Fig. 6). The measurements at Kusunur would be at most representative for the lower portion of the Lena basin, but even this fact is actually invalidated in the remaining of the article (see e.g. p. 5, ll. 26–27: “the measurements at Kusunur GS [should] be taken for analysis of water temperature changes in the delta head area with a great caution”).
5. p. 3, ll. 10–16: A reference to Table 1 is missing, so as to make clear that the temperature data which is described is the same one as in the table.
6. p. 4, l. 6: “fairway” (sic) should be replaced with “river bank” (I assume).
7. p. 4, l. 10: “inflow” should be replaced with “tributary”.
8. p. 4, ll. 14–27: This paragraph should be moved later in the text, when the possible causes for the temperature “anomaly” are discussed. As mentioned in the General Comments above, it should also be reformulated so as to be less categorical on the conclusions drawn. On line 25, the Reynolds number should be discussed into more detail: is this number high? What does it imply? Some citations would help readers who are unfamiliar with the Reynolds number.
9. p. 5, ll. 7–27: As mentioned in the General Comments, the authors might consider removing Table 2 and its corresponding paragraph in Sect. 3 so as to focus on the main message of the manuscript.
10. p. 6, ll. 2–3: “The correlation coefficient between maximum events at Habarovsk GS and Kusunur is  $\sim 0.6$ ”. It is not clear which quantities were correlated here: was it the magnitude of the events or their time of occurrence? It is also not clear what this correlation demonstrates.

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11. p. 6, ll. 5–14: In my opinion, this paragraph is not very clear and should be reformulated. In particular, it should be stated clearly what is meant by the “temperature inconsistency”.
12. p. 6, ll. 22–33: Based on Fig. 7, I would actually identify not only one, but three “inconsistencies”:
  - In June, the net heat flux is decreasing over time at both Kusun and Habarova, which should imply a decrease in observed stream temperature as well. However, temperature is observed to increase in time at both locations. This might be possibly explained by the upstream conditions, or by unaccounted heat sources.
  - In July and August, the net heat flux is decreasing over the entire reach between Kusun and Habarova, which would imply that the difference in temperature between Habarova and Kusun should decrease as well. Again, the opposite is observed: this “inconsistency” is the one discussed by the authors.
  - In September, the net heat flux is *negative* at both locations, which implies that the temperature at Habarova should be lower than the one at Kusun. Observations contradict this reasoning, which hints at the presence of unaccounted heat sources or measurement errors/non-representativeness (same reasons as those discussed by the authors).
13. p. 7, l. 3: The authors might want to clearly state that ice could be present in June.
14. p. 7, ll. 4–5: “it is obvious that during July – August the heat should be transferred from water to the sediments”. This is not obvious to me: either more explanations should be provided, or the sentence should be reformulated.
15. p. 7, ll. 4–17: In both paragraphs d) and e), it is hard to understand the point that the authors want to make. They mention arguments in favor and against each

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- possible explanation of the temperature difference, without clearly stating in the end whether the explanation could be valid or not. I recommend the authors to reformulate both paragraphs.
16. p. 7, l. 31: “there are no other inflows [...] which could affect the temperature measurements at the stations”. The authors could mention the possible presence subsurface water inflows, which cannot be ruled out and are later on shown to be most probably present.
  17. p. 8, l. 2: The authors might want to explain in a few words how they obtained the estimate of  $0.2 \text{ km}^3$  for the mean annual cumulated discharge of the other tributaries.
  18. p. 8, l. 13: The authors use a roughness height of 3.2 m for the stream bed, but they do not justify their choice. For comparison, Shen and Diplas (2008) and Constantinescu et al. (2014) use a roughness height of 0.01 m, which appears as much more physical to me.
  19. p. 8, l. 17 and p. 11, l. 13: Instead of the numbered points, I would make a separate subsection for each numerical experiment. This would clarify the text structure.
  20. p. 9, ll. 1–3: The point that the authors want to make is not really clear according to me.
  21. p. 9, l. 9: A separate subsection could be created for the analytical computation of mean stream temperature (between the two subsections presenting the numerical experiments, see point 19 above).
  22. p. 9, ll. 11:  $T_k$  in Eq. (3) is not defined. The authors might consider adding a schema to explain the different terms of the equation.
  23. p. 10, ll. 1–20: Figures 8 and 9 could be merged into a single one with two panels (a and b).

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24. p. 11, l. 5: It is not clear to me what the high correlation between the water temperature measured at Habarova and the one measured at Eremeyka is supposed to show.
25. p. 11, l. 13: The authors might want to clearly state the goal of the second numerical experiment.
26. p. 11, ll. 15–16: It is not clear to me how the penetration depth of short-wave radiation is used in the model. I encourage the authors to add detailed information regarding the simulation setup (see point 3 in the General Comments above).
27. p. 11, l. 25: It is not clear which “previous estimates of the total discharge” are referred to.
28. p. 12, ll. 1–3: The temperature differences discussed by the authors are difficult to see on the figure since the compared curves are not displayed on a same plot.
29. p. 12, ll. 4–6: The authors invoke the non-representativeness of the measured temperature at Kusur to explain the difference between the measured and modeled temperature curves. They however do not mention the fact that modeling errors could also be responsible for the discrepancy between the two curves. If the simulation setup is similar to the one of the first experiment (i.e. a three-dimensional flow and heat simulation), I would expect the model to be able to provide not only the mean stream temperature at Kusur, but also its actual value at the gauging site location.
30. p. 12, l. 16: The meaning of “larger frequency of optimization process” is not clear to me.
31. p. 12, l. 18: Should not “warming effect” be replaced with “cooling effect”?

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32. p. 12, ll. 19–20: The atmospheric forcing do actually not “cool the water from Kusus GS to Habarova in the middle of September”, since stream temperature at Habarova is higher than the one at Kusus. On the other hand, the difference between the two temperatures tends to decrease.
33. p. 12, l. 21: “There is an indication in favor of an unaccounted source of heat in the middle of September 2007”. The unaccounted heat source is certainly also present in all other years, except that it is more evident in year 2007.
34. p. 13, ll. 32–33: In my opinion, it is not necessary to invoke a “negative heat flux [ . . . ] in the area from Kusus to Tit-Ary” to explain the cold temperatures measured at Tit-Ary, since the cold water released by the island is certainly already sufficient enough an explanation.
35. p. 13, l. 32: The authors invoke a “large positive heat flux from the hyporheic zone in the delta head area” to explain the observed temperatures at Habarova. Could not also diffuse subsurface inflows of water be responsible for a warming of the Lena River?
36. p. 20: The lines indicating the respective locations of the gauging sites could be thicker in order to be more visible. The North arrow and the length scale are missing from the figure. I recommend adding an inset showing the entire portion of the Lena River between Kusus and Habarova, in which the locations of the three considered areas (Kusus, Tit-Ary Island and Habarova) would be indicated.
37. p. 21: In the column containing the date of first ice appearance in fall, it is not clear why the measurement period is split in two. For example, could not “1986–1999, 2000–2007” be simply replaced with “1986–2007”? Also, the last row of the table is not discussed in the text and should be removed.
38. p. 22: In my opinion, Fig. 2 is not really necessary and could be removed from the manuscript.

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39. p.24: As mentioned in point 1 in the General Comments, Table 2 could be removed.
40. p.25: In Fig. 5, the mean summer stream temperatures at Kusur in 1936 and 1940 seem to be anomalously high and low, respectively. I encourage the authors to double check the data in these years. Also, the legend of panel (b) is not clear: it took me a long time to figure out that the panel actually displays the observed probability distribution of the time of the year at which the maximum stream temperature is observed.
41. p. 26: The zero line could be indicated in Fig. 7 to improve readability. The authors might also want to remind in the legend that the data correspond to daily mean values, averaged on each summer day between 2002 and 2011.
42. pp.26–27: As mentioned above, Fig. 8 and Fig. 9 could be regrouped in a single figure. It should also be stated in the legend that the displayed data was computed (and not measured).
43. p.28: The top panel should be labeled as (a), and the bottom ones as (b) and (c).
44. p.29: The top panel should be labeled as (a) and the bottom one as (b).
45. p.30: I would indicate the months using their names instead of numbers 6 to 9.
46. p.32: The label is missing along the y-axis, and the data units are missing along both axes.

## Technical corrections

1. p. 1, l. 28: “Costard et al. (2007)”

2. p. 6, l. 8: “towardss”
3. p. 7, l. 4 and l. 12: points d) and e) have been inverted (e is before d)
4. p. 8, l. 5: “2002 to 2011”
5. p. 9, l. 17: Symbol ‘>’ should be replaced with ‘<’.
6. p. 13, l. 30: “is” should be removed between “it” and “becomes”.
7. p. 13, l. 14, l. 19 and l. 23: “mln” should be replaced with “ $\times 10^6$ ”.
8. p. 13, l. 29: “Boike et al. (2015)”
9. p. 28, l. 5 and l. 6: “observed” should be replaced with “modeled”.

## References

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-254, 2016.