RESPONSE TO COMMENTS FROM ANONYMOUS REVIEWER 2

We thank the reviewer for comments and suggestions that helped to improve the manuscript. Below, we reply to specific comments and describe changes made in the manuscript in response. The reviewer's comments are highlighted in blue, the excerptions from the revised text are italicized.

Comment 1: On page 3, line 101 (P3L101) it is said that measurements were taken at 75 stations and Figure 1 is introduced. In Fig. 1, however, there are not 75 stations. We can guess that 75 does not refer to the number of stations, but rather the number of times that profiling measurements were taken during the three campaigns. Please clarify this in the text.
Response to Comment 1: Corrected.
Action taken: The following sentences were added to the Data section and the Abstract.

During the campaigns, CTD profiling and water sampling were performed at 34 locations all over the lake. The total of 75 CTD profiles were obtained.

Comment 2: On P3L101 it is said that at each station CTD profiling was performed. However only physical data at the deepest location of the lake (and continuous measurements recorded at the Tyup and Jyrgalan estuaries) are presented. To prevent the reader having any false expectations, it would be better to advise them that the recorded CTD profiles will not be presented.
Response to Comment 2: Indeed, the individual vertical CTD profiles in Figure 2 are shown only for the deep stations, but the CTD data from several other stations are presented in the form of vertical-longitudinal sections (Figures 4a and 13). Besides, the CTD data collected at all stations were used to plot the horizontal distributions of salinity and temperature at surface (Figures 5-7). However, we agree that it is useful to identify the focus of the study from the beginning, so we added a respective sentence to the Data section.
Action taken: The following sentence was added to the Data section, immediately after mentioning the total of 75 CTD profiles:

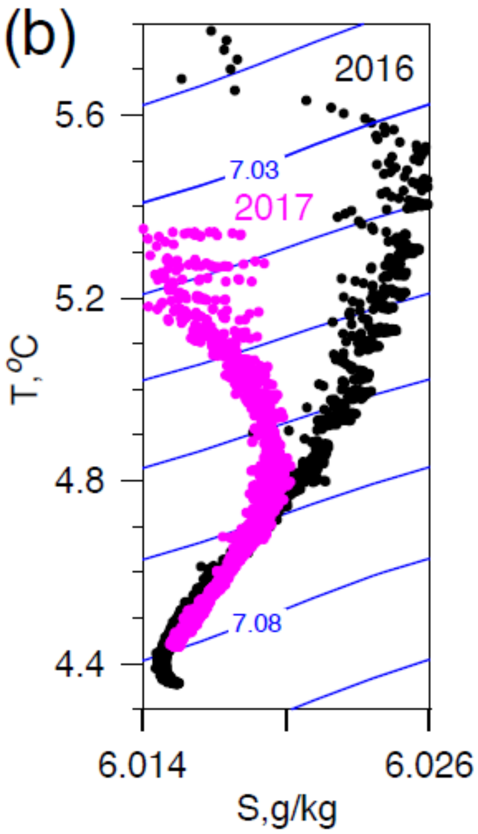
In this study, as far as the CTD data are concerned, we mainly focus on the deep stations, as well as those in the area adjacent to the river mouths in the eastern littoral region.

Comment 3: Authors emphasize the peak they found in the indirect measurements of salinity in 2016. The amplitude of this peak, less than 0.01 g/kg, is below the standard resolution of the salinity obtained by the well established UNESCO formula in most of the CTD profiles in the ocean. It is also said that such a peak does not affect the density profile (in Fig. 2b please plot the isopleths of constant density). Does this not mean that such a variation is within the noise level?
Response to Comment 3: We insist that the discovered salinity maximum in the intermediate layer is securely resolved by CTD measurements. The nominal resolution of the SBE19plus profiler is 0.0004 g/kg in salinity for ocean waters (see p.11 of the User's Manual), although, of course, this may not be applicable to the Issyk-Kul because of its different salt composition. To evaluate the performance of the instrument on the Issyk-Kul water, we took the accuracies of the conductivity sensor and the temperature sensor as specified by the manufacturer, namely, 0.0005 S/m and 0.005ºC, respectively, and used these values to infer that for the calculated salinity based on the Peeter's conversion formula. This procedure yielded the accuracy for the Issyk-Kul's salinity at about 0.00095 g/kg (note that this is the estimate for the accuracy, and not the resolution – the latter should be even better). Hence, the magnitude of the intermediate salinity maximum we report in this paper (i.e., from 0.004 to 0.011 g/kg) is much larger than the instrumental uncertainty of the measurements. The maximum is also reflected in Fig. 2b, where

we added the density isopleths as you suggested. It can be seen that this feature reduces the stability to a certain extent, while the column remains stably stratified.
Action taken: We added the following sentence to Section 3.1, page 5:

However, the magnitude (0.004 to 0.011 g/kg) is much larger than the instrumental uncertainty of CTD measurements, which can be estimated from instrument's specifications and Peeter's formula as about 0.001 g kg^{-1} or smaller.

We also added density isolines to Fig. 4b, see the new figure and caption below:



(b) – TS diagrams for water below the thermocline for October, 2016 (black) and June, 2017 (magenta), the blue lines are density (ρ – 1000 kg m^3) isopleths for pressure 40 bar calculated through the empirical formula by Peeters et al. (2003)

Comment 4: It is true that the peak is a coherent structure (Fig 2) but it is highly correlated with an abrupt change in the temperature profile which is not observed in 2017 when the salinity peak is not observed either. Authors use the formula provided by Peeter et al (2003) to obtain the salinity from CTD measurements for Issyk-Kul. Furthermore, they say that Peeter et al (2003) present a similar structure although they do not discuss it. Maybe this is because they are aware that these are below the resolution?
Response to Comment 4: The intermediate salinity maxima were observed both in 2016 and 2017 (see the inset panel in Fig. 2a), although the peak of 2016 was more pronounced. It is also true that in both cases, the salinity maximum coincided in depth with a certain inflection of the temperature profiles. This is a good point, thank you for bringing it to our attention. In fact, it is consistent with our hypothesis that these features originate from the admixture of water penetrating to the lower layers from the shelf in winter – these waters are not only fresher, but also colder than the ambient waters, so it is only natural that they leave their signature in not only salinity, but also temperature distributions. As to the structures reported by Peeters et al.

(2003), these authors focus on fine-scale features of salinity profiles as small as 0.001 g/kg (see, for example, their Figs. 2 and 3), which they hypothetically attribute to horizontal intrusions, and never question the sufficiency of the resolution of their CTD measurements. They also used a SeaBird's instrument, although a previous model.
Action taken: None.

Comment 5: Even if such a peak can be correlated with some signal in the chemical measurements and could be explained by the supposed circulation in the lake, the reliability of this peak should be clearly stated especially as the authors want to present it as the most important finding of their work i.e., this being the only idea included in the title of the manuscript. There are, however, other aspects which the data presented can support (i.e. bottom advection due to differential cooling).
Response to Comment 5: A statement regarding the measurability of the salinity maximum in conjunction with the accuracy and resolution of the profiler we used was added to the text, see our response to your Comment 3 above. We also added specifications of the instrument to the Data section:

In the surveys of 2016 and 2017, a SeaBird's SBE19plus Seacat instrument allowing for deep profiling was used. According to the manufacturer's specifications, the profiler has accuracy of 0.005^{o}C and 0.0005 S/m, and resolution of 0.0001^{o}C and 0.00005 S/m, in temperature and conductivity, respectively.

While the intermediate salinity maximum might be an important finding, we cannot agree that this is the only idea put forward by the title of the manuscript. The "unknown features of thermohaline structure" as claimed in the title refer also to the central area of elevated salinity an and its specific "bimodal" shape and other features discussed in Section 3.2; the "frontal zone" near the estuaries in the east; (Fig. 4a) and the vertical structure across the Jyrgalan River plume; and other details reported here for the first time. The details of winter cooling development on the eastern shelf as obtained from the thermistor chains also are, in a sense, previously unknown features of the Issyk-Kul's thermal structure.
Action taken: Sentences added to the text as specified above.

Comment 6: P5L175-177: If there are no sources or processes that can justify a wide range of salinity values, why does a low variability of salinity all over the lake indicate intense mixing?
Response to Comment 6: Actually, the Issyk-Kul is subject to a variety of processes and forcing factors that would usually have resulted in much stronger variability of salinity. Such factors include uneven distribution of freshwater sources along the shoreline; synoptic and seasonal variability of river runoffs translated by lake's circulation; spatial inhomogeneity of evaporation rates (especially on shelves and summer); possible submarine groundwater discharges; etc. Many other saline or brackish lakes and inland seas of the Earth, some of which are comparable with the Issyk-Kul in size and conditions, demonstrate much larger spans of salinity values, both in spatial and temporal dimensions. Very often, they also form vertical haline stratification, which, in many cases, is a major dynamical controller of the lake's regime. If this is not the case for the Issyk-Kul, this can only mean that the water mass mixes very intensely through some mechanisms that are yet to be identified. The sinking of waters from the shelf during winter cooling might be one of these mechanisms.
Action taken: None.

Comment 7: P5L180: Frontal zone or a small gradient and a frontal color artefact?
Response to Comment 7: As it can be not only seen from the image color scale, but also quantified by counting the isolines in Figure 3, the horizontal gradient of salinity in the area centered at approximately 78ºE is higher than that elsewhere over the lake by about an order of

magnitude. Whether or not this fact justifies the use of the term "front" is, indeed, arguable. Therefore, we removed this word and rephrased the sentence accordingly.
Action taken: We rearranged the respective sentence as follows:

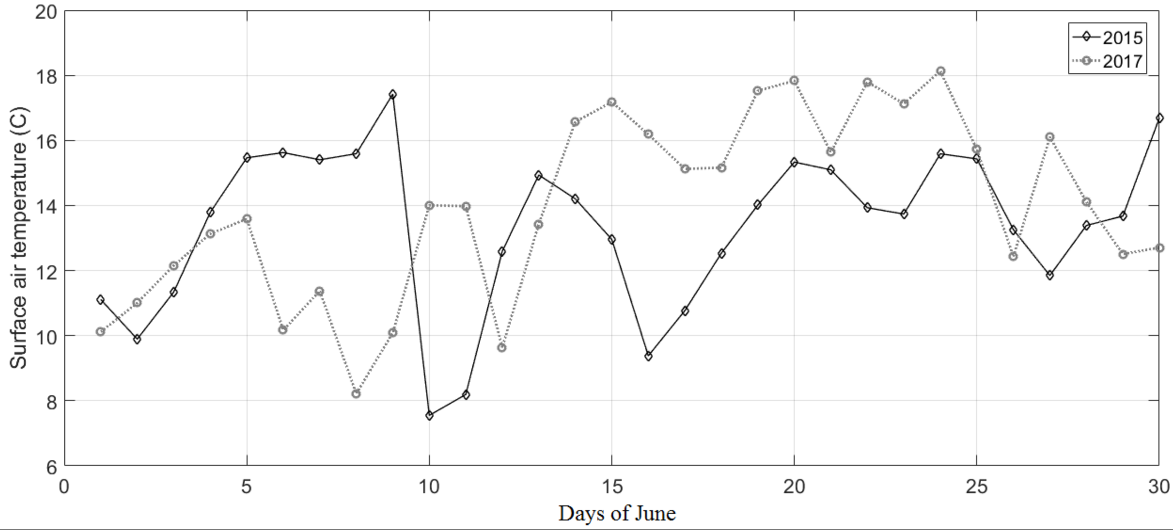
However, the discharges from the Jyrgalan and the Tyup rivers, bringing to the lake respective 28 m^3s^{-1} *and 12* m^3s^{-1} *of fresh water on long-term average (Romanovsky, 1990), do exhibit distinct signature in salinity on the eastern shelf, forming a zone of high salinity gradient centered at about* 78^oE.

Comment 8: P6 L 203: . . ."the interannual differences observed in the lake surface temperature were more significant than those in salinity". Please comment on your idea behind this, if any.
Response to Comment 8: We do understand your point. Is 0.9ºC of temperature change more significant than 0.02 g/kg of salinity change, or not? The units are different, so there is no way to compare. However, we can evaluate their contributions to the respective density changes. It is clear from the new Figure 4b that in the terms of density, the interannual changes of temperature are approximately 6 times as important as those of salinity, so, in this sense, they are indeed more significant.
Action taken: None.

Comment 9: P6L203: "All over the lake, the surface water temperature in June of 2017 was warmer than that in June 2015". Is there any reason for it not to be warmer or cooler or equal?
Response to Comment 9: One of the objectives of this study was to assess how variable the Issyk-Kul is at the interannual scales, given that this issue has not been fully addressed in the available literature, and we had hydrographic data for 3 consecutive years. This is why we explicitly emphasize the observed changes. The reason for these changes are different atmospheric forcing conditions - see our response to the next comment.
Action taken: None.

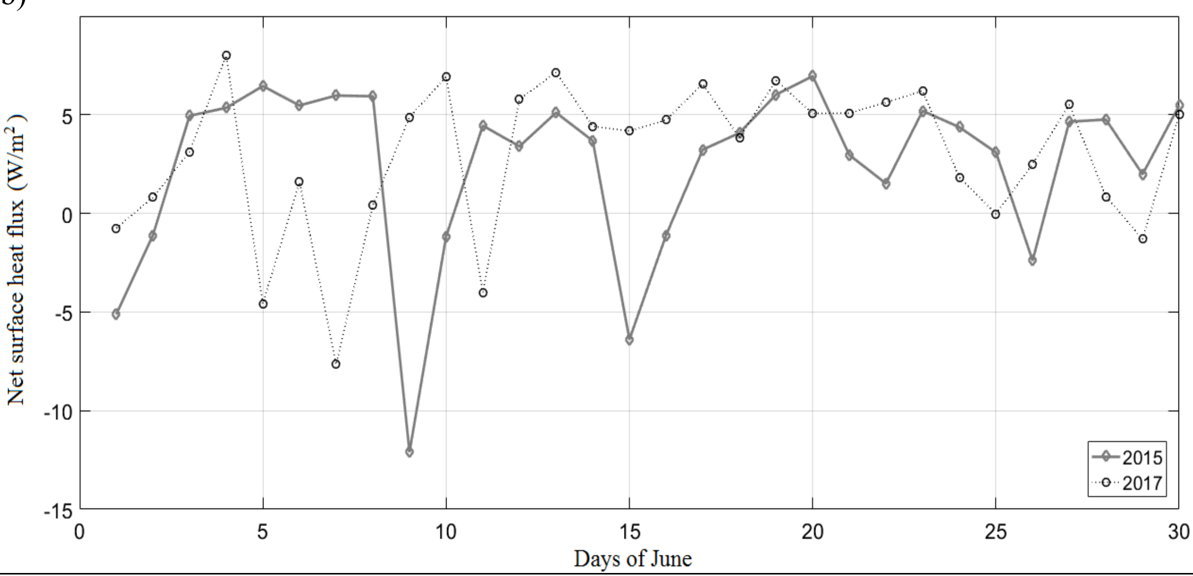
Comment 10: In Fig. 8 the air temperature is presented but the relevant variable is the total surface heat flux as the authors indicate in the text (P6L211-212). Please include the total heat flux in Fig. 8. This same comment applies also to Figure 13.
Response to Comment 10: Done. We added to Figure 8 not only the net surface heat flux, but also the winds from the reanalysis for both years.
Action taken: Figure 8 was redrawn as depicted below. Respective amendments were introduced to the text and the caption.

(a)



2015
2017
Surface air temperature (C)
20
18
16
14
12
10
8
6
0
5
10
15
20
25
30
Days of June

(b)



Net surface heat flux (W/m²)
5
0
-5
-10
-15
0
5
10
15
20
25
30
2015
2017
Days of June

(c)

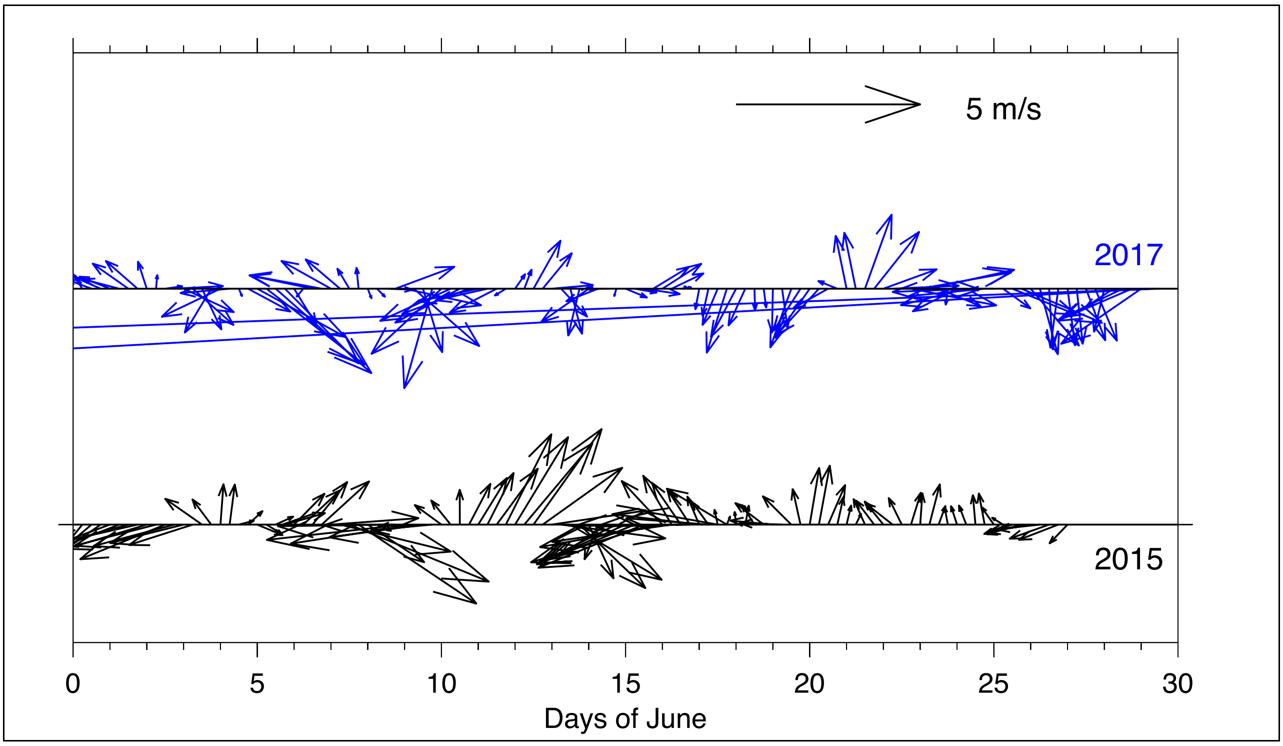


Figure 8: (a) - Air temperature at 2 m height in June, 2015, and June, 2017, from ERA-Interim reanalysis; (b) – Net surface heat lux in June, 2015, and June, 2017. Positive sign corresponds to gain of heat by the lake; (c) – Wind speed vectors at 2 m height in June, 2015 (black), and June, 2017 (blue), from ERA-Interim reanalysis. The data correspond to the point 42°20'N, 77°30'E in the central part of the lake.

Comment 11: In Figure 9, the vertical profiles of some chemical parameters are presented and compared to the values presented by Kadyrov 1986. Because this reference is in Russian and therefore the information may not be able to be easily accessed by many potentially interested readers, I recommend very briefly summarizing the data, which are presented by Kadyrov, in the text.

Response to Comment 11: Done.

Action taken: The following paragraph was added at the beginning of Section 3.3:

In this section, in order to evaluate the interdecadal variability, we repeatedly refer to the book by Kadyrov (1986). This monograph published in the Russian language presents hydrochemical data obtained by the author and his co-workers mainly during the period 1961-1985 (although some data reported by earlier researchers are also summarized in the book). Chemical parameters were determined in a certified laboratory of Water Management Institute (city of Frunze, USSR) using standard methods and techniques adopted at the time. Nutrients and pH were analysed colorometrically, and oxygen through the Winkler titration.

The historical variability ranges shown along with our recent data in Figs. 9-11 were derived from the material presented in Kadyrov's book. They correspond to at least 535 samples for each parameter, collected mainly at 24 standard locations distributed over the open part of the lake.

Comment 12: P6L228: It is said that a difference in the pH from 8.71 to 0.68 is not of importance. This means a reduction of 7% on [H+] if, due to methodology, the authors consider that this is below the measurement resolutions then please provide the pH values to a precision of one tenth.

Response to Comment 12: Some misunderstanding is taking place here. Difference in pH of 0.68 would have been of great importance, but, as reported in the manuscript (subsection 3.3.2), what we actually observed was the change from 8.71 to 8.68, that is, the decrease by only 0.03, or less than 0.4%. The nominal accuracy of measuring pH through the method used is 0.01.
Action taken: None.

Comment 13: P8 L285-290. Please move all methodological issues to the data sections. Explain better how the pigment index is computed.
Response to Comment 13: Done.
Action taken: All methodological issues were removed from this section and placed in the Data section. The latter was complemented by the following paragraph:

Along with the LiDAR measurements, water samples were collected at all stations and analyzed for concentrations of suspended matter and chlorophyll through the procedures described by Konovalov et al. (2014). The pigment Carotenoid-Chlorophyll index (CCI) showing essentially the "age" or the life stage of the phytoplankton was also determined photometrically in laboratory as the ratio of optical densities of pigments extracted from the samples at 430 nm and 665 nm wavelengths (e.g., Margalef, 1967, Wozniak and Dera, 2007).

Comment 14: P8L310: Bottom currents due to differential cooling is a nice hypothesis somewhat supported by the data presented which, here, is also related to the biological parameters. It would be good to emphasize such a relation later in the text.
Response to Comment 14: We agree.
Action taken: The following sentence was added to the discussion at the end of Section 3.4:

The sinking shelf waters from the Jyrgalan and the Tyup estuaries area are also rich in nutrients (see Fig. 10), so the mechanism discussed above may also contribute to biological productivity in the open part of the lake.

Comment 15: P9L32: In the text, advective and convective cooling evenes refer to bottom and surface cooling signals. Surface cooling events do not have to necessarily be only convective. I suggest using the classification of surface cooling and bottom advection instead.
Response to Comment 15: We do understand your point, and, in vast majority of cases, you are right – most of the cooling events that we call "convective" originate from the surface cooling. But, strictly speaking, they do not necessarily have to. In our definition, the event was "convective" if, at the beginning of it, the temperature at the bottom thermistor was higher than that at the next thermistor above it (not at the surface!). Provided that both temperature values were above the maximum density temperature (which was practically always the case in these measurements), such a situation implies convective instability. This is why we call it "convective".
Action taken: None.

Comment 16: Table 1. Bottom advection is a continuous event, although at a point there are velocity and temperature variabilities according to the variation of the differential cooling if this is its origin. Please explain why the durations of advective events are presented as some tens of minutes.
Response to Comment 16: The data revealed that both types of cooling – advective just as well as convective - occurred not as a continuous monotonic decrease of temperature, but, rather, as a series of relatively short individual events or pulses, each characterized by progressive temperature drop, separated by periods of "relaxation" during which the temperature remained constant or even slightly increased. We may hypothesize that the intermittent character of the advective flow reflects the fact that the latter originates from intermittent convection of cold

water in the upper reaches of the river canyons. However, confirming this requires additional measurements and is beyond the scope of the present paper.
Action taken: None (except that now the statistics for the duration of the cooling events in Table 1 is also given in the units of days, see also our response to RC1).

Comment 17: Figure 12. Please add the name of the variables and the units to the Table.
Response to Comment 17: The names of the variables and units were added to the caption.
Action taken: The caption to Figure 12 was modified as follows:

Figure 12: Chlorophyll-"a" concentration and CCI pigment index for June 2015 (blue bullets), November 2016 (green bullets), and June 2017 (red bullets). The straight lines represent linear regressions. The concentrations of chlorophyll (mg m^{-3}, "CHL" columns) and total suspended matter (g m^{-3}, "TSM" columns) are also exhibited in the inset table as the minimum and the maximum values and averages by 4 parts of the lake, delimited here by the 42.45^{o}N parallel and the 77.4^{o}E meridian.

Comment 18: Figure 4. In accordance with the text (P3L183), change the units of the longitude from degrees to metres. This same comment applies to Figure 13 as well.
Response to Comment 18: Done.
Action taken: Figures 4 and 13 were amended accordingly, see below.

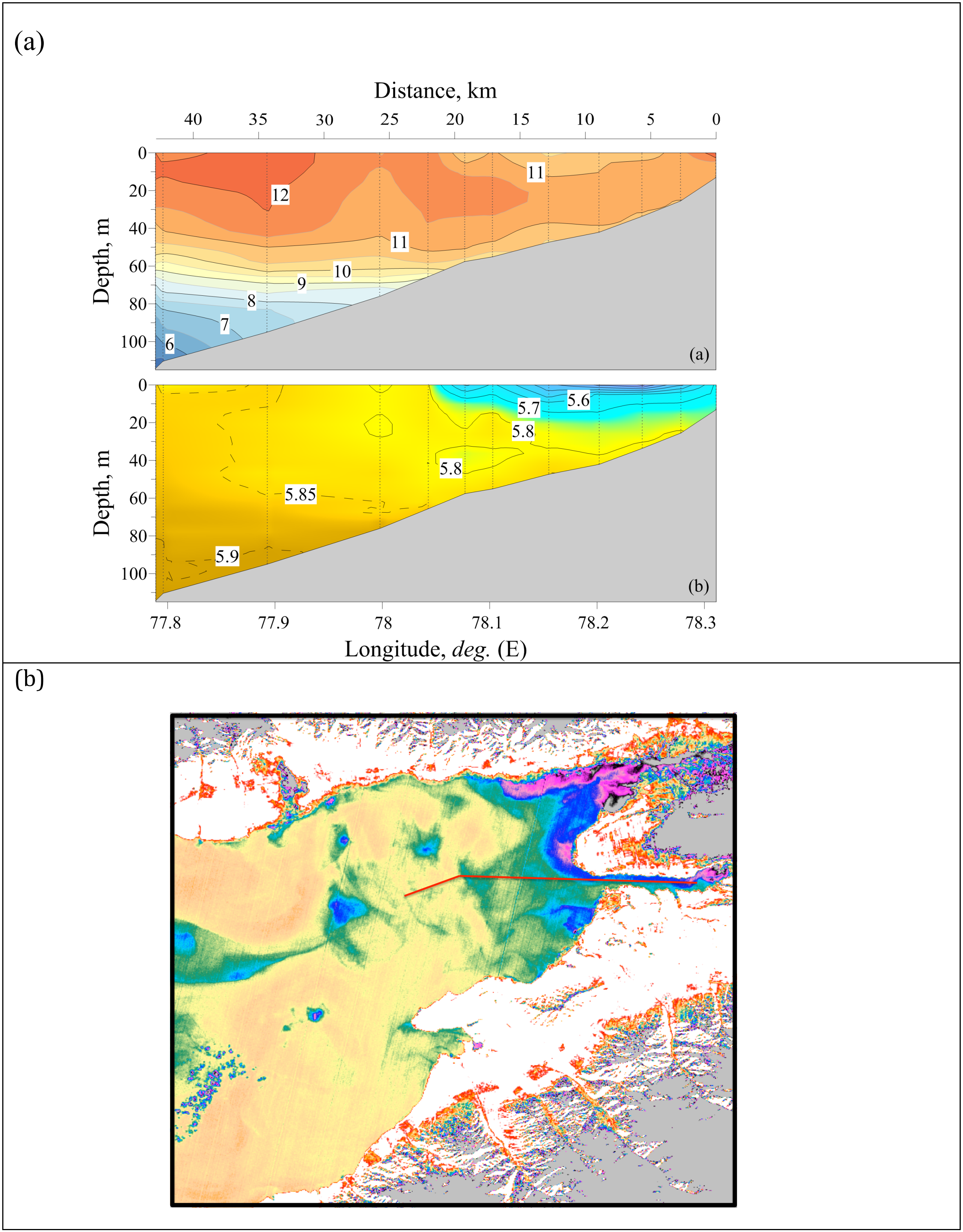


Figure 4: (a) - Distribution of temperature (^oC, upper panel) and salinity (g kg^{-1}, lower panel) at longitudinal section following the ancient bed of the Jyrgalan River on the eastern shelf (green bullets in Fig. 1). November 1-2, 2016. (b) – Landsat infrared satellite image at 60 m resolution obtained on the same day (courtesy from Alexei Kouraev, LEGOS, France). Red line corresponds to the section where the in situ measurements shown in Fig. 4a were taken.

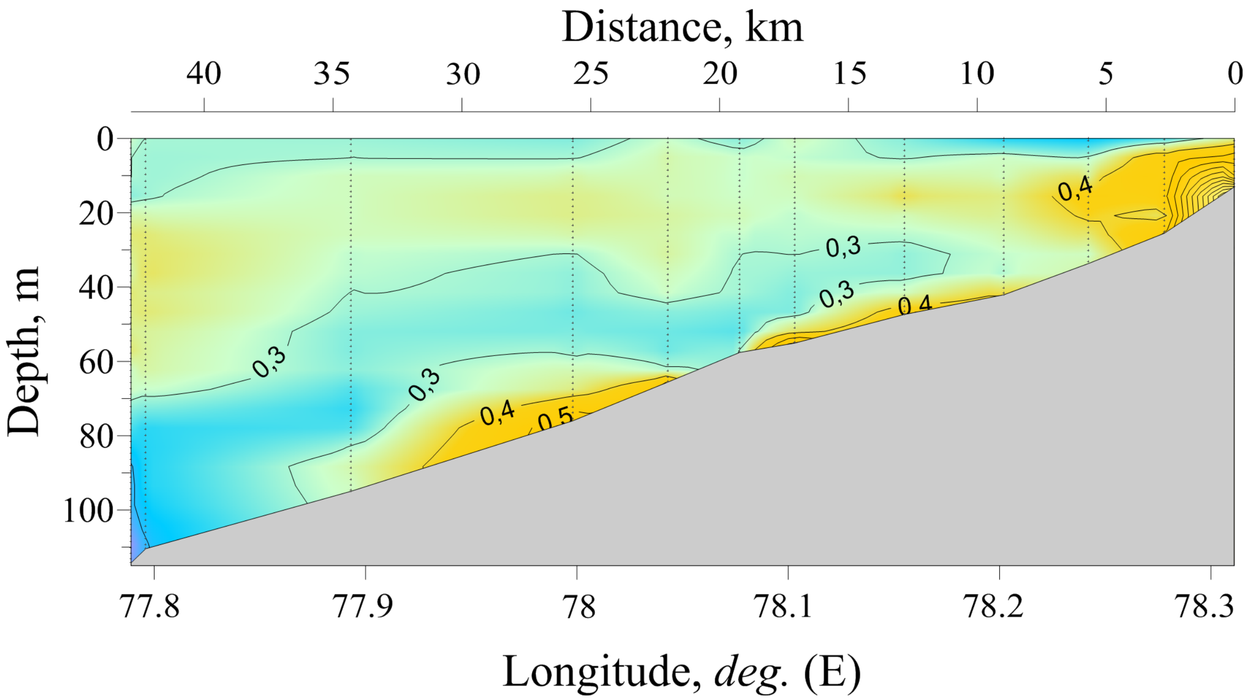
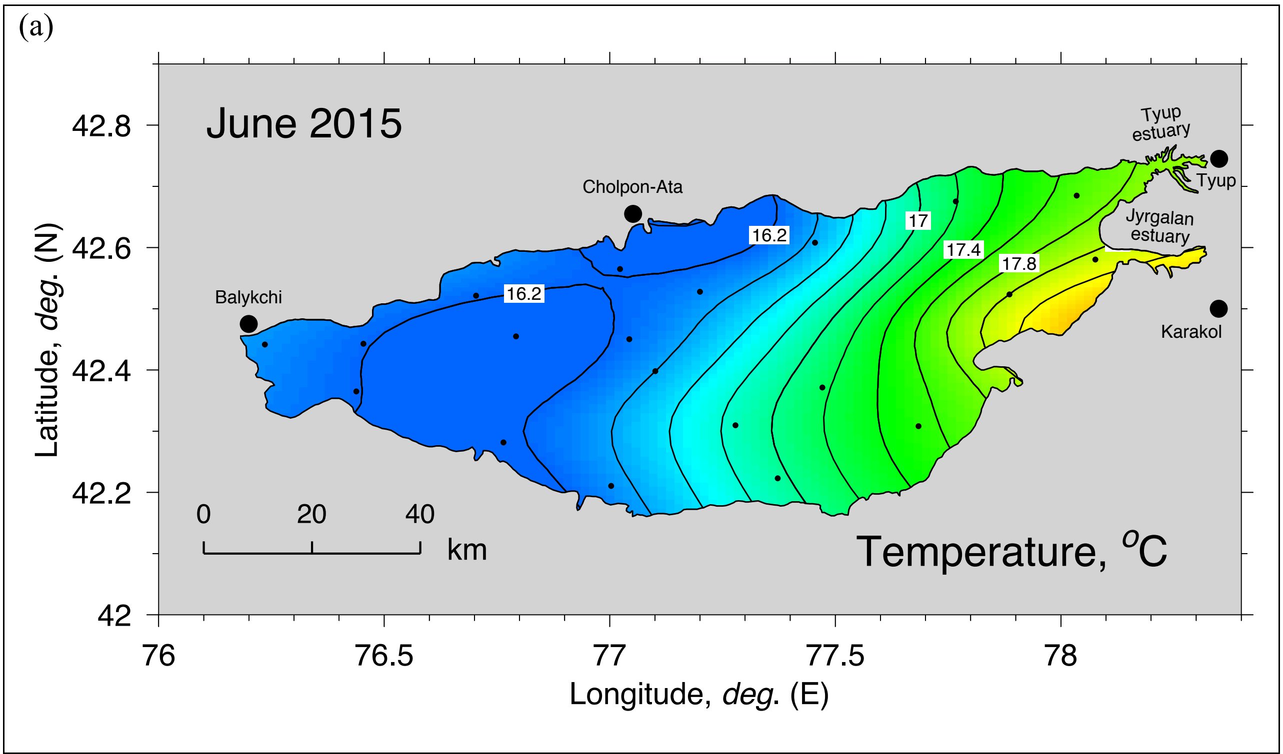


Figure 13: Vertical distribution of chlorophyll-"a" concentration (mg m^{-3}) at longitudinal section following the ancient bed of the Jyrgalan River on the eastern shelf. November 1, 2016.

Comment 19: Figure[s] 6 [and 7]. Is there any particular reason for presenting the differences in surface temperature and surface salinity between June 2017 and June 2015? The information is clearer if presented in absolute values, as it is done for 2016 (Figure 3).

Response to Comment 19: As to Figure 7 (temperature), we agree. This figure was redrawn completely and now represents the absolute values as you suggested. As to Figure 6 (salinity), we think that it is important to keep the plot of the interannual differences, given that they are unexpectedly small (see our response to Comment 6 above) and distributed in rather specific pattern. We believe that conservative character of salinity fields is one of noteworthy findings of this study.

Action taken: Figure 7 was substituted by a new one, see below:



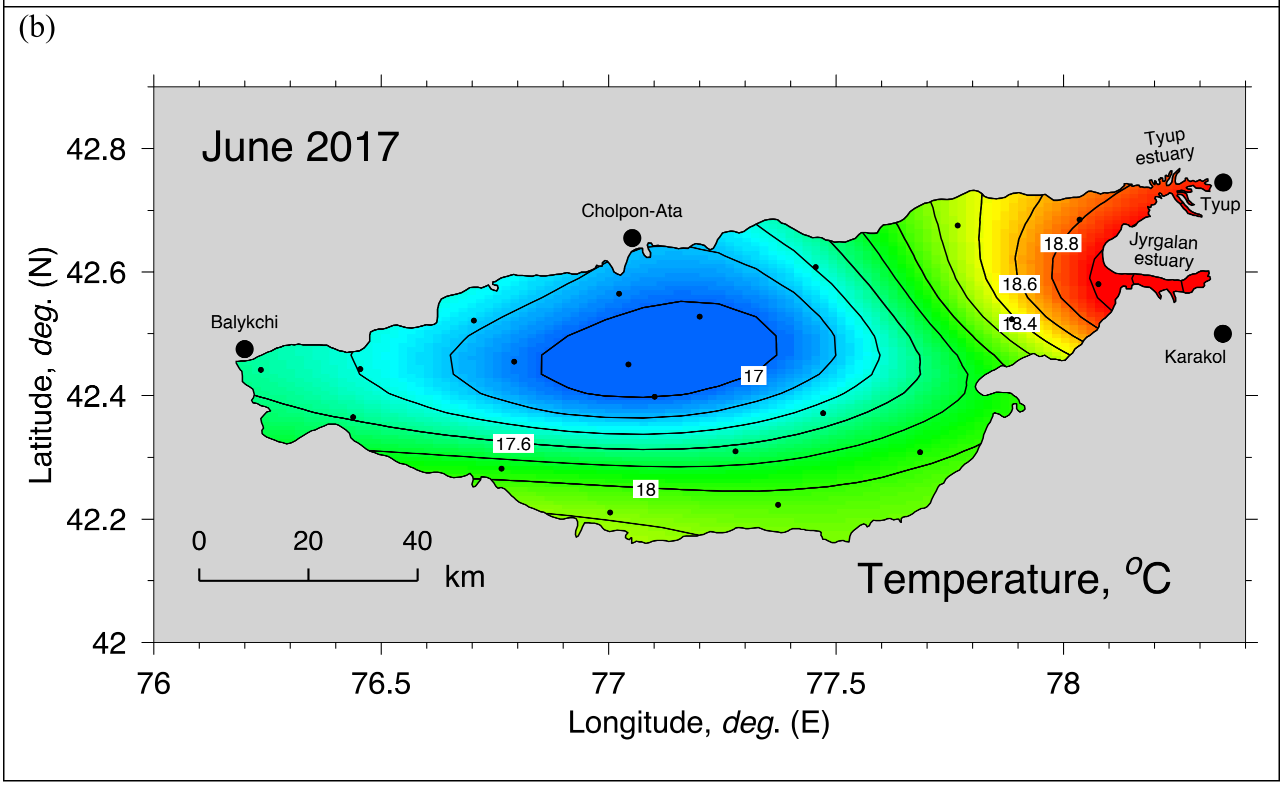


Figure 7: Distribution of lake surface temperature observed in June of 2015 (a) and June of 2017 (b), based on data from hydrographic stations indicated by bullets.