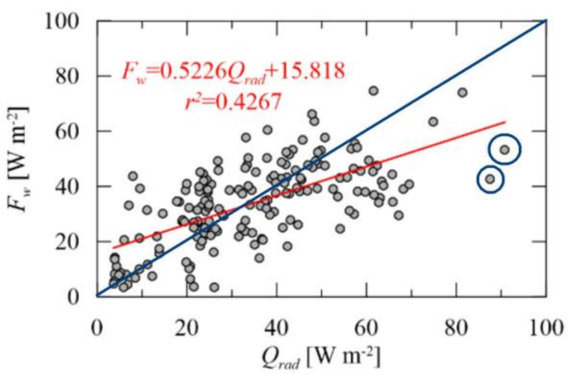
Reply to RC2

Note: the **comments** and **authors' replies** are in font color of **black** and **blue**, respectively.

The study is dedicated to the heat budget of ice-covered waters in Central Asia. This is a weakly investigated topic important for understanding the seasonal ice balance in large arid endorheic regions of strongly continental climates. The authors used four years long observations of temperature and radiation in a large shallow lake of Inner Mongolia. High temporal and vertical resolution of observations allowed estimation of the boundary fluxes in the ice-water-sediment system and their relationship to solar irradiance, and ice and snow thicknesses. Data from the ice-covered seasons from 2015 to 2019 provided estimates of inter-annual variability in the winter heat budget. The methods are generally correct and adequate to the posed research questions. The results are of interest for the ice research community and are suitable for publication in "The Cryosphere". The manuscript is well-organized. The presentation can be however improved by language and style editing. I have some remarks and questions on the analysis of the results, listed in the attached file.
I have some remarks and questions on the analysis of the results. In particular, the concluding part of Discussion, including Eq. 6 and Figure 9, is rather confusing. Why apply a least-square linear model to approximate the water-ice heat flux Fw as a function of solar radiation Qrad? It directly follows from your data that Fw ≈ Qrad (see the last sentence before Eq. 6). Hence, the coefficients a and b in your linear model have no physical meaning, unless you propose their interpretation. Moreover, looking at Fig. 9, one could suggest that a straight line Fw = Qrad would explain approximately the same amount of variance in the observations (see the blue line in the drawing below), especially if the outliers at very high under-ice radiation levels (blue circles in the drawing) are removed. Herewith, apart from being unjustified physically, the coefficients a and b introduce only additional uncertainty without any additional predictive power. This part of the analysis requires essential revising.



Reply: Thanks a lot for your deep consideration and constructive advice. Using a least-square linear model, we intended to only present the significant correlation

between water-to-ice flux (Fw) and transmitted solar radiation (Qrad), but as you suggest, we missed the physical background of the fitted function.
Your analysis makes our results clearer. We totally rephrased this section based on stronger physical discussion (Lines 460-483).

Below are remaining comments and questions arranged along the text flow.
• Line 104: Figure 1a needs some edits and(or) explanations. What do the colored areas mean? They are subscribed in Chinese only. If it is a classification of climatic zones, where it comes from? A reference to the source is needed.
Reply: Thanks for your good suggestion. The colored areas mean classification of climatic zones. But we replaced Figure 1a using a climate zone classification map of China, which gives a more accurate zonation. And this map is provided by the website of China Meteorological Administration (www.cma.gov.cn). Key information was added to the map.

• Lines 115-117: I have not found any information on water depths where 1 the irradiance sensors were installed in the water column.
Reply: Actually, we can see from Table 1 that the number of irradiance sensors deployed was different at four winters, so the sensor depths were different. Since the sensor depth gives important information, we added the sensor depths in Table 1.

• Line 137: Eq. 1 is valid if Tw is the water temperature averaged across the water column. It should be explicitly stated in the text.
Reply: Revised accordingly.

• Line 145: How the extinction coefficient was measured?
Reply: Actually, the extinction coefficient is calculated from the under-ice irradiance (R_d) at two depths at least, following $R_d(z_2) = R_d(z_1) \cdot \exp(-\kappa(z_2 - z_1))$, where $R_d(z_1)$ and $R_d(z_2)$ were observed irradiance at depth z_1 and z_2, respectively.

• Lines 152-158: Can you provide details on the “optimal control model”? How deep the temperature loggers were buried in the sediments? How the thermal conductivity of the sediment was estimated?
Reply: Optimal control model is one of the common methods to retrieve thermal diffusivity of medium if temperature profiles (>= 3 depths) within this medium are measured continuously. The description of this method can be found in Shi et al, 2014, where we used this method to determine the thermal diffusivity coefficient of lake ice cover. It is physically based on the classical one-dimensional heat conduction equation and is well applicable to temperature profiles with obvious temporal variation and with obvious temperature difference between depths.
In winter 2018, four thermistors were buried in the sediment (1 cm, 9cm, 17 cm, and 30 cm below the sediment surface). We used vertical temperature profiles to estimate the thermal diffusivity of the top sediment based on Optimal Control Model. Then thermal conductivity can be determined with measured density and specific heat

capacity of sediment. We added general information on this method to the revised manuscript (Lines 171-178), but detailed description needs long text and many equations. I suggest readers to refer to the following reference or some manuals on this mathematic model.
Shi, L., Li, Z., Niu, F., Huang, W., Lu, P., Feng, E., Han, H.: Thermal diffusivity of thermokarst lake ice in Beiluhe basin of the Qinghai-Tibet Plateau, Ann. Glaciol., 55(66), 153-158, 2014.

• Lines 163-165: Replace "first" with "second" and vice versa.
Reply: Revised accordingly.

• Lines 176-177: Why these certain thresholds were chosen for the irradiance? Can you compare them to typical seasonal radiation values under ice?
Reply: Thanks for your advice. Comments on this part from you and Reviewer #1 make us realize that grading the error is not necessary for the manuscript and just giving the error values is enough to evaluate the uncertainties in heat flux calculations. So, in the revised version, we removed the four categories and just kept the error values in this section.

• Line 192: Did absolute humidity change in the diurnal cycle, or was it just an effect of the air temperature variations?
Reply: After we estimated the absolute air humidity based on synchronous relative humidity and air temperature using formulas in Huang et al. (2016), we can also clearly see the diurnal cycle in absolute humidity (with peaks occurring on afternoon).
Huang, W., Li, R., Han, H., Niu, F., Wu, Q., Wang, W.: Ice processes and surface ablation in a shallow thermokarst lake in the central Qinghai-Tibet Plateau, Annals of Glaciology, 57(71): 20-28, 2016.
• Line 218: Replace "persist" with "persistent"
Reply: Modified accordingly.

• Lines 223-225: It would be more consistent to describe the phenomenon as a local temperature *minimum* created by vertical salinity gradient preventing downward heat transport from the upper waters. Cf. Mironov et al. [2002, Section 6 "Effect of salinity"].
Reply: Thanks for your reminding. Yes, this local temperature peak (actually a warmer layer than both overlying and underlying layer) was called temperature minimum in Mironov et al (2002), but was also called a temperature *dichotomy* or *dicothermal layer* in Kirillin et al 2011 [*Kirillin, G., & Terzhevik, A. (2011). Thermal instability in freshwater lakes under ice: Effect of salt gradients or solar radiation? Cold Regions Science and Technology, 65(2), 184–190*]. And we guess the terminology *temperature minimum* is easy to bring readers a misunderstanding, so we used temperature dichotomy instead and added direct description on its thickness and variability, reading "*This abnormal layer is sometimes called a local temperature minimum (Mironov et al., 2002) or a "temperature dichotomy" (i.e., a dicothermal*

layer used in oceanography) (e.g., Kirillin et al., 2011, 2021). Water temperature contours (not shown) revealed that both the bulk temperature and thickness of the dicothermal layer show significant diurnal cycles: its temperature and thickness take up and increase following the solar insolation cycle and decrease or even disappear during night. The developing and extending of this layer also increases the thermal gradient of the overlying interface layer."

• Lines 242-243: The temperature-salinity distribution described here inevitably suggest development of double-diffusive convection [Schmitt, 1994]. While the existing data do probably not allow direct estimations of double diffusion, its potential role in the vertical heat transport is worth mentioning here or in Discussion.

Reply: Thanks for your advice. Yes, the temperature-salinity profiles directly suggest double-diffusive convection, i.e., the temperature destabilizes while the salt stabilizes the water column. But staircases cannot possibly form due to its small lake depth and large heat gradient.

Generally, this diffusive regime is believed to apparently enhance the heat diffusivity compared to that of salt. But we could not estimate the double diffusion (such as the density ratio) due to lack of concurrent data of temperature and salinity. But we can assess its impact on heat diffusivity using dataset of winter 2017. The water-to-ice heat flux Fw gives the bulk effective heat diffusivity of the water column of 5-15 times (mean 10 times) larger than the molecular diffusivity, indicating the enhanced heat diffusivity due to double diffusion. We added this statement to the Discussion, reading "*Although we did not acquire concurrent salinity profiles to the water temperature, sampling results in winter of 2017 inevitably indicate the development of double diffusive convection as the temperature destabilizes while the salinity stabilizes the stratification (Schmitt, 1994; Schimid et al., 2010). The effective heat diffusivity of the bulk water column estimated from Fw derived by Eq. (4) was 5–16 (mean of approximately 10) times larger than the molecular diffusivity, indicating the significantly enhanced diffusivity of heat due to double diffusion.*"

• Lines 269-271: How the relative contribution of convection to Fw was estimated?

Reply: We used a relatively simple and course estimation, i.e., we compared the Fw values prior to the onset of convection and during the convection. For instance, if Fw increased when convection started, we think the convection accelerates the water-to-ice heat transfer.

• Lines 359-361: Eq. 3 requires temperature profiles within the ice cover and knowledge of the heat conduction coefficient. Neither of them are "routinely observed".

Reply: Thank you for your reminding. The thermal conductivity/diffusivity coefficient of ice is not often observed, but of clean freshwater ice, it falls in a narrow range of 2.1-2.2 W m^{-1} $°C^{-1}$. You don't have to measure it. Observations of temperature profiles within ice cover are often observed in lake thermodynamic and thermal stratification research, but maybe not in other researches. So, we modified this statement to "*actually these variables were often observed in lake thermal regime and ice programs*".

References
D. Mironov, A. Terzhevik, G. Kirillin, T. Jonas, J. Malm, and D. Farmer. Radiatively-driven convection in ice-covered lakes: Observations, scaling and mixed-layer model. Journal of Geophysical Research, 107(C4):7–1–7–16, 2002. doi: 10.1029/2001JC000892.
R. W. Schmitt. Double diffusion in oceanography. Annual Review of Fluid Mechanics, 26(1):255–285, 1994.
Reply: Thanks for providing related publications.

Other main changes:

[1] In this shallow brackish lake, we found almost all (97%) of the transmitted solar radiation returns back to the ice bottom. In *Discussion*, we added comparison with a 20-m deep freshwater lake (Kilpisjärvi) in northern Finland, where 1/3 of transmitted radiation returns to ice (Leppäranta et al. 2019). We think the lake depth is a key geometric factor influencing the ratio because larger depth means a thicker mixing layer, where more solar energy is needed to deepen and heat the mixing layer, so, the returning proportion of irradiance decreases.

[2] *Section 4.1*: After discussion among all authors, we realized that stage III in this brackish lake is of different regime to that in freshwater lakes in Kirillin scheme because of the salinity profile. We reworked on this and named it stage IIIb for brackish water lakes because the convective mixing in brackish lakers may be stopped by a dicothermal layer in the middle and full convection is possible only when the bottom water is warm enough.