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Interactive comment on "Numerical study on the response of the largest lake in China to climate change" *by* Dongsheng Su et al.

Anonymous Referee #1

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The paper is focused on Qinghai Lake, the largest of thousands of lakes situated on Tibetan Plateau, China. The lake is brackish with salinity about 12.5 g/L. The authors use the well-known one-dimensional model FLAKE forced with a local set of historical gridded meteorological data for the period 1979-2012 to simulate the thermal and ice regimes and their ongoing trends accompanying the global warming. Because the Qinghai, as well as all other Tibetan lakes, has been very sparsely covered by in situ measurements, and virtually no field monitoring data are available (except those from a single meteorological buoy used in this study), numerical simulation is the only mean capable of giving quantitative insights into the long-term variability of the Tibetan lakes. Therefore, in my opinion, the article presents interesting and useful information and should be published after moderate revision.

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Discussion paper

My only general comment about this manuscript is as follows: I think that the possible role of salinity and its changes in the estimated long-term variability of the lake regime should be evaluated and discussed more thoroughly. For instance, can the trends of the ice regime (Section 3.3) be associated not only with the air temperature increase, but also, at least partly, with salinity increase over the period 1961-2004? According to the information supplied in Section 2.1, the lake level dropped for about 3.3 m during this period, which, given the mean depth of 21 m and mean salinity about 10 g/L, implies salinity increase of about 2 g/L. This, in turn, may have affected the ice regime.

Generally, salinity may exercise influence on the issues addressed in the article through (1) salinity stratification, which is not accounted for in the FLAKE model, but may strongly affect vertical mixing; (2) temperature of maximum density, which is different from that of fresh water and may affect winter convection; and (3) freezing temperature, which is different from that of fresh water and may affect the onset and duration of the ice cover period. While the first of these mechanisms is difficult to be included in the model designed for freshwater lakes, the other two, probably, could be taken into account, if it is possible to replace the respective constants in the model (i.e., the freezing temperature and the maximum density temperature) by those appropriate for Qinghai Lake. I suspect that the exact values of either variable for the Qinghai are unknown because of the lack of direct measurements and because the ionic composition of the lake is different from that of the ocean. However, as a “first guess”, the oceanic values for the respective salinity 12.5 g/L can be considered - namely, about -0.65oC for freezing point, and about 1.6oC for TMD. If it is possible to repeat some of the experiments done using FLAKE with the settings modified accordingly, and then assess the differences in the outcomes of the “freshwater” and “salty” experiments, this would allow to evaluate the role of salinity vs air temperature and surface fluxes and hence strengthen the study. If this approach is technically not possible, potential role of salinity still should be discussed in the paper, perhaps based on literature and data from other similar lakes.

More specific comments:

The “Study area” Section: The elevation of the Tibetan Plateau is never mentioned in the paper. What is the absolute elevation of Qinghai Lake surface? This is an important piece of information, please specify.

P3 Lines 25-30: It follows from these numbers that the lake’s water budget has been shifting towards an increase of the incoming components since 1970, accompanied by the decrease of evaporation. Then why the lake kept shrinking until 2004? Was the rate of shrinking in the 1960s much higher than in the early 2000s? Please explain.

P4 L15: “rare abnormal values influenced probably by cloud cover” – if you are confident that these abnormal values are artifacts corresponding to low clouds, then why keep them? Just remove them from your data base and the plot.

P5 Section 2.2: More details about the FLAKE model would be useful. What is the form of the expression for the profile in the lower layer?

P5 L30 and thereafter: The adjustments introduced to the air temperature and wind speed through linear regressions seem to help very little in minimizing biases between the simulated and the observed LST, so what is the point of using them?

L11 P15: “Keeping in mind the cool skin effect, we can suggest that the model predictions of the bulk LST are even better than the satellite data suggest” – But your Figure 2 shows good agreement between the satellite and the buoy data, and the latter meadured bulk temperature. Therefore, it looks like the skin effect in this case did not affect much the satellite-derived temperatures.

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