Appendix 1

Review of: *Flow, Thermal Regime and Sediment Transport Studies in Lake Tanganyika,* edited by Timo Huttula, Kuopio University Publications, 1997; 173 pages + appendices

General comments

Timo Huttula's report summarises results from field and computer modelling studies undertaken by the University of Kuopio since 1995 on the physical limnology of Lake Tanganyika. Their work on mixing, circulation and sediment transport should provide valuable background information for studies of lake ecology and fish resources. The programme has been an ambitious one, and, as one would expect, not without problems and setbacks with instrumentation and data collection. The sheer size of the lake, as well as the logistical, social and political complications surrounding the work, have compounded the usual difficulties and challenges associated with any limnological or oceanographic field work. On balance I feel that the project team should be commended for what they have planned and achieved. My critical comments are given in the following section (*Specific Comments),* and are mostly at a technical level rather than in reference to the major project goals and achievements.

The project team has taken a whole-system perspective on physical processes in the lake. From the outset, the study has tried to incorporate all the important environmental factors - mainly climate, inflows and outflows - that cause the lake to behave as it does. This is in addition to observing the major physical processes within the lake itself - water temperatures, currents, light transmission, and sediment concentrations. The sampling programme has consisted of a combination of time series measurements at selected locations, together with shipboard profiling on three cruises. I recommended during the planning stages that it was necessary to take a whole-system perspective if one wanted to understand how the lake works, so it is not surprising that I agree with the general direction that the project has taken. Someone with a different philosophy might disagree. The only possible approach that I can think of that is missing from the report is the use of remote sensing. I seem to recall that, at the planning stages, remote sensing was to be included in the study. If it has been, perhaps the results are reported in another programme document. I think remote sensing has the potential to complement the kind of on-site measurements described in the report, just as the time-series from fixed moorings now complement the synoptic "snapshots" obtained from profiles during cruises.

A massive amount of data has already been collected. A limited sample of results are presented in the project report, together with a description of some of the statistical analyses used during the processing stages. I am happy to see that project time has been budgeted for data processing and archiving, necessary if the huge investment in fieldwork is to yield its potential payoff. In addition a large effort seems to have been

devoted to development and application of a hierarchy of numerical models that describe lake hydrodynamics and the meteorological forcing that forms the all-important surface boundary condition. The meteorological modelling seems to make use of existing (commercially or otherwise available) models, which is an efficient approach. It is not clear how much of the hydrodynamic modelling is based on existing models, and how much has had to be developed. While I have some questions about the technical details of the models, I certainly believe that such modelling is a necessary component of the data analysis. Such simulations provide the only way of combining the multiplicity of processes that impact on the lake with the response of the lake itself. The modelling complements the statistical and graphical analyses of data, which tend to focus on individual processes rather than the complete system. To me, a striking example of the success of modelling was the use of the three-dimensional meteorology model (HIRLAM), in combination with regional-scale hydrodynamic lake models, to explain the strong diurnal and local topographic effects seen in the observations.

I did find the report difficult to read, both because of problems with English and difficulties in interpreting some of the figures; details are given below. If future reports are to be published in English, I think their usefulness would be greatly enhanced if the authors could employ a copy editor, or someone with similar skills, to help with project presentation.

Specific comments

1. Numerical models and scaling

I was somewhat surprised at the conclusion on page 107 that the effects of stratification could be ignored in the numerical modelling. Details of calculations for the terms in Table 2 are not given, but surely the size of the baroclinic term in Eq. 3.9 depends on the depth over which the integral is evaluated. The baroclinic terms may be small in the shallow, coastal areas around the river mouths, where the more detailed regional models have their major focus. But I question whether the same conclusion would hold in the deeper, offshore areas. The same question applies to the use of a barotropic, basin-scale model to supply boundary conditions for the regional models.

I also wonder whether the advantages of using a bottom-following vertical co-ordinate system are great enough to offset problems that arise where bathymetry changes abruptly. The sides of the Tanganyika basin are extremely steep, and I would have thought that this could cause difficulties for a bottom-following vertical co-ordinate system. This would be especially true for resolving density stratification. But because density stratification was neglected, this latter difficulty did not occur.

I think that the incorporation of a sediment tracking module in the regional models is a good idea, and will enhance the models' usefulness as management and diagnostic tools. However, as shown in the results for suspended sediment (Figures 3.5.3/5 - /7),

because buoyancy effects have not been incorporated in the hydrodynamic model, it is not possible to reproduce the effects of plunging or overflowing inflows. Suspended sediment concentrations are now predicted to be vertically uniform (until they are affected by settling).

Finally, in the "Summary and conclusions" section, mention is made of the possibility (in future work) of simulating oxygen dynamics in the lake. Since oxygen distribution is strongly influenced by the effects of density stratification, I cannot see how oxygen simulations will be successful if the numerical models remain barotropic.

2. Page 15 - Location map.
This is a helpful map but it does not show all the sites referred to in the text, e.g. Kabogo (pp. 46,72,99), Rumonge (p.49), Mwela (p.99).

3. Page 18 - Flow cylinders.
I was not sure exactly what the "flow cylinders" were - I guessed they were some sort of drogues or submerged floats. The authors referred to them as if they were widely used and well known. Is there a reference that could be cited?

4. Pages 30,33 - wind gusts
What is the averaging time for the "wind gusts?"

5. Page 44
Figures have been interchanged - they do not match the captions.

6. Pages 46-51 - Isotherms
The figures showing isotherms are very colourful, and I appreciated having the same colour scale for each one, to make comparisons easier. However, I found the figures difficult to interpret in some other respects. The use of nautical miles for the distance scale is not consistent with other distance scales used in the report. The fact that each figure is exactly the same size, while the horizontal distances vary considerably, makes comparisons difficult. Why are there such marked discontinuities in Fig. 2.4.2/1? What do the dark areas along the sides and bottoms of some of the figures signify? In some figures they seem to indicate the lake bed, but not in others.

7. Pages 62-78 (& elsewhere) - graphs showing time series of currents
The time scales on these graphs are not very helpful or meaningful. The start times seem arbitrary. Each figure is exactly the same size but covers a different length of time, making comparisons difficult. The lack of any dates, subdivisions, or actual (local) times makes it practically impossible to relate the information in the graph to comments in the text.

8. Pages 64 & 67 - Figures showing the distribution of wind speed and direction.
What are the lines that cross the isolines of wind speed and direction? Depth contours? Isobars?

9. Page 64.
The text on this page is very confusing and the connection with the current time-series in Fig. 2.4.4/12 was not obvious. The water flow is said to be "a two-layer system", but "they were also fairly similar at all measured depths." If they are all similar, why are they said to form a two-layer system? What does "they" refer to in the sentence following the one just quoted? ("During the first night *they* were flowing W ...")

10. Page 66.
"The extremely high current speeds were recorded ..." How high?

11. Page 67.
Something seems to be missing at the bottom of page 67.

12. Page 69.
What is meant by "bed cable connections?"

13. Page 70 - final paragraph.
How were the conclusions linking the water current structure to the diurnal cycle of surface winds arrived at? On the basis of statistical analyses (not presented)? Visual comparison of graphs of wind and water velocities (not shown?)?

14. Page 76 - first sentence.
The first sentence does not make sense. "*This* measurement area ..." - which one? .."dominated by a threshold which between the South Basin and the Middle Basin." Do you mean that the two basins are separated by a submerged sill?

15. Page 82 - last paragraph
The first sentence seems to be saying that there is a distinctive coastal, near shore current pattern, but the second sentence says there is "no or minor coastal effect." I am not sure what message the authors are trying to convey.

16. Pages 86 & 87
"epiliminium" should be "epilimnion."

17. Page 97
"slops" should be "slopes."

18. Page 98
"restartification should be "restratification."

19. Page 98 - next to last paragraph
This paragraph contains some discussion of how currents measured in the field compare with hypothetical patterns that had been proposed in earlier publications, by previous investigators. These hypothesized patterns had been inferred from water temperature and wind patterns, not from direct measurements of water velocities. I thought that the assessment of such existing "theories," or hypotheses about the

general circulation patterns in the lake, was one of the main objectives of the project. At least the hypotheses could be used to provide a framework in which major results could be presented. This would give the presentation of results a bit more structure than they seem to have at present. The authors do state (page 97) that "The results will be compared here to our earlier results and results from previous researchers. It is especially interesting to compare the present results to the results of Coulter ..." I do not think the comparison is completely effective or clear. To me, this paragraph (on page 98) was a good example of a missed opportunity, where the authors could have addressed more fully how earlier hypotheses stand up in light of the authors' measurements. One of the overarching patterns hypothesized by Coulter, one that dominates the entire seasonal production pattern of the lake, is upwelling in the southern half of the lake during the "dry" season and associated northward transport of surface water, driven by southerly trade winds. In effect this paragraph (on page 98) seems to be saying that the temperature structure observed during the project does agree with that observed many times in the past, but the direction of the measured currents is not consistent with either the temperature structure or the hypothesized large-scale flow pattern! Is not this a significant finding, in need of a little explanation or interpretation? Are we to draw the conclusion that the basic hypothesis is flawed? Or do the results represent some local effect?

20. Page 100 - first paragraph
"The direction and even magnitude of the flow near Kungwe in May 1993 corresponded very well with present results from the dry season." Do you mean that similar current speeds and directions have been observed over a period of years, at least for the duration of the project? Do these possibly represent persistent patterns?

21. Page 103 - near the end of the page
"The reason for the shorter period during the dry season is the periodic forcing by winds." Does this mean that the oscillations observed in the dry season are primarily forced oscillations (determined by the frequency of oscillation of wind stress), as distinct from free modes (determined only by density stratification and basin shape) that are observed at other times?

22. Page 105
Eq. 3.1 - dot product has been reversed in term (2).
Eq 3.5 - what is the symbol between the bottom stress and the bottom velocity boundary conditions?

23. Page 106
Are not the bottom stress boundary conditions, given by either Eq. 3.7 or 3.8, inconsistent with the no-slip zero-velocity condition, given in the following sentence? Or are these just presented as two alternative approaches, depending on the resolution of the vertical computation grid?

24. Page 107 - first paragraph

"Observational data (Chapter 2) show that the south part of the lake is almost completely mixed during a dry season." My question has to do with what exactly is meant by the words "completely mixed." Are the authors just saying that the water column is nearly isothermal? Or are they saying that the water column circulates over its entire depth? Or both? I think the distinction between near isothermy and deep mixing is important here, and needs to be clarified. The depth over which circulation ("mixing?") occurs is a variable of considerable significance, and presumably the vertical velocities predicted by the model would have something to say about this. Without including density stratification in the numerical models, however, the results would still be inconclusive.

Concluding remarks

On page 97, the authors state: "The previous data collected before LTR have been uneven in spatial and temporal coverage, and the sampling carried out mainly around the few major ports of the lake.... The present report gives the first lake wide perspective on the circulation pattern and sediment transport within the lake using both best possible current measurement technique and modelling." Again, on page 104, "The results obtained now are unique when considering the Great Lakes of Africa. There is no data from any other African lakes about currents." I do not think the authors are overstating the significance of their achievements. They have made use of the best available technology, and have applied it in a challenging environment as successfully as could be expected under very difficult logistical, political and social conditions. I am not as sure, however, that their numerical "models were validated, mainly with the data collected during the last two expeditions" (page 165). I think the models have been successful in some respects, for example in explaining the strong diurnal component in currents that seems to prevail, to a surprising degree, around the entire basin and over the entire year. I also think the models have the potential to be useful in future pollution and ecological studies, as suggested by the authors (page 166.) However, I remain to be convinced that these future goals can be achieved without including density variation in the numerical models (see point 1 under *Specific comments*, above). Also, as mentioned earlier, I think there is room for improvement in communicating the valuable results the authors have obtained, hopefully in future publications.

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