Appendix 2

Authors notes on **'The Review of Flow, Thermal Regime and Sediment Transport Studies in Lake Tanganyika edited by Timo Huttula, Kuopio University Publications, 1997; 173 pages + appendices by Dr. B Spigel, October 1998'**

General

First of all, it is highly appreciated that Dr. Spigel has used his time to thoroughly review the concerned document. A review by a person, as Dr. Spigel, with his insight, experience and knowledge of substance, gives a new perspective for authors while working. Unfortunately, his review came only after the work had been published and not when concluding the achieved results. Therefore, not too much can be done this time. However, we mostly welcome his idea that communication could and should be improved when elaborating publications in the future.

Dr. Spigel is absolutely right when he mentions that only a small part of the collected data has been used in the report. As it is known, the political situation in the region and also during the study period (data collection) was unstable. Due to the full embargo of several months against Burundi, when the sampling was planned to take place, occurred several delays. A strict timetable, an intensive field programme and already changed deadline for the reporting did not allow a more comprehensive review and analysis of the collected information. The last expedition took place in August-September 1997 and the manuscript was sent to print in the end of January 1998. We are pleased to hear that Dr. Spigel agrees that the approach and methodology used was acceptable also for him. His only critical statement concerns the lack of remote sensing data. He states: 'The only possible approach that I can think of that is missing from the report is the use of remote sensing'. In fact the remote sensing data were used in a very limited way to validate the models and also to study the patchiness structure of the lake surface temperature. The results of the remote sensing sub-component will be seen in the LTR scientific summary, which is to be published in the end of this year. The editor of this work is Prof. Hannu Mölsä from the University of Kuopio. At the time of the finalizing the IAA-report we concentrated to the most important aspects of our work. The decision was made simply due to the time constraints. We fully share Dr. Spigel's opinion that the remote sensing data could have used as supportive for the publication. What comes to the quality of technical or linguistic part of the report it is also our wish to improve it already in the near future when publishing the further analysis of the data. Many aspects are still to be studied.

The numerical modelling work was partially based on the existing flow model of Lake Tanganyika developed during the execution of the Lake Tanganyika Research (LTR) Project. Regional flow models and sediment transport models were developed and applied during the execution of Inter-Agency Agreement between UNOPS and FAO. The meteorological modelling was conducted within a separate research project funded by the Academy of Finland. More information about models' characteristics and development history is provided in Table 1.1/2 and Figure 1.1/1 of the Introduction (page 8-11). More information about the stages of model development can be found in the publications given in the list of reference.

Answers to specific comments of the reviewer

1. Numerical modelling and scaling

Our conclusion that stratification effects can be excluded was not a general one. We tried to show only that for **specific hydrometeorological conditions of Lake Tanganyika and short-term synoptic scale simulations** the baroclinic component of the pressure gradient is smaller than the barotropic one. It is correct that the magnitude of the baroclinic forcing depends on the depth. It also depends on the horizontal gradients of water density. It is known from observations that strong quasi-regular winds and small range of water temperature variation are typical in the tropical conditions of Lake Tanganyika region. This led us to an idea to compare the magnitude of the baroclinic and barotropic terms in the momentum equations. Since the water temperature in Lake Tanganyika below 200 m is practically constant, the contribution of hypolimnion is small and only the fraction of the upper active layer is essential. The integrals in Table 2 (p.107) were calculated using Simpson quadratures on the basis of CTD data at depths ranging from 30 to 300 m. By neglecting the density stratification we introduced the error which on a short time scale is smaller than the error of reconstruction of the density distribution. This also helped to avoid difficulties with approximation of density gradients using sigma-co-ordinate transformation. Our preliminary numerical experiments with baroclinic version of the model showed that error caused by approximation of the integral density gradient term using a sigma-coordinate transformation and the same horizontal triangular mesh as for barotropic version was high. Undoubtedly long-term simulations of seasonal and annual scale require full baroclinic version of the flow model in Cartesian coordinates.

The suspended sediments concentrations are vertically uniform mainly due to extremely high depths in Lake Tanganyika. The great depth reduces the magnitude of wave and current induced shear stresses that could cause the resuspension of bottom sediments. In our opinion the impact of plunging and overflowing inflows on vertical profiles of suspended sediments concentrations in Lake Tanganyika is negligible. The horizontal and vertical wind induced mixing prevail in Lake Tanganyika conditions.

We agree with Dr. Spigel that any possible future ecological and water quality studies, like simulating the oxygen dynamics, should include the water temperature variations. It means that the present flow modelling system has to be revised considerably in order to take into account also the stratification effects.

2. Page 15 - Location map

The locations of the villages Kabogo, Rumonge are missing from the map. Their location can be estimated with minor effort from fig's on pages 46, and 49. When referred to Mwela-station it is done to get with Coulter 1964 where the location can be found.

3. Page 18 - Flow cylinders
The cylinders are described in the text. It refers to the earlier documents published (Huttula et al. 1993 and Kotilainen 1993) wherein the structure of a cylinder and the measurements carried out have been fully described.

4. Pages 30, 33 - Wind gusts
On page 14 we referred to earlier document by Huttula, Peltonen and Nieminen (1993): In this document technical specifications have been described in detail. The gust is the maximum speed during 2 seconds within an interval.

5. Figures have indeed been interchanged

6. Pages 46-51 - Isotherms
The software was new and all its' characteristics were not fully known. Variable or unjust scaling was indeed one of the problems and interpolation between distant points was another one (discontinuities). Bottom is marked as black whenever it is within the depth range. Due to the unjust scaling bottom is marked sometimes as sharp peaks.

7. Pages 62-78 - Graphs showing time series of currents
The time span of water current series are given in the figure text. Maximum amount of data are shown and it was not possible to scale the data display in an uniform manner. The software provided by the instrument manufacturer did not allow this.

8. Pages 64 and 67 - Figures showing the distribution of wind speed and direction
In the figures there are both the depth contours and the vertical line, which is a 'meridian' of 29º 42' E.

9. Page 64.
Explaining with few words the time variations of a current complex system is not easy. In the text it is said that the very surface layer (0-4.5 m) was different from the other water mass. This is consistent with 'the two layer system' statement in the beginning.

The sentence should read 'During the first night the surface currents (0-4.5 m) were flowing west, while the waters below that were flowing south'.

10. Page 66.
The highest current speeds were 30-40 cm/s.

11. Page 67.

The following text is missing:
......the depth of 15-17 m. The upper layer was flowing E and was probably induced by the W wind.

12. Page 69.
Correct: ‘bad cable connections’

13. Page 70
The conclusions are drawn on the basis of visual comparison of the wind and current records as explained and shown on the pages 55-70.

14. Page 76
We mean the strait between Kungwe Mountains and Kalemie. This is a shallow area and there is an underwater sill. The word ‘threshold’ maybe gives a wrong idea.

15. Page 82
Unclear sentence. Correct one should read: ‘Due to the coast it differs from the offshore flow in direction.’

16.-18.
Are miss prints like observed by Spigel.

19. Page 98
Spigel has observed the same thing from our observations like we did. The Coulter’s hypothesis about dry season upwelling in the south with major water transport is quite simplified. On these two pages (98 and 99) we are discussing on the basis of CTD and current data, which are quite limited. ADCP and the model results are discussed later. Maybe because of this we were quite moderate in our statements.

Concerning the discussion of our findings near Kigoma we can say that:

1) 'The upwelling' or better to say destratification of the epilimnion-hypolimnion off Kigoma was weaker in 1996-1997 (during the intensive period) than in 1993, i.e. the years varied already in 90's not compared only to the earlier studies (Beauchamp 1939, Coulter 1991) and therefore, wasn't pointed out that strongly.

2) The current measurements (Kotilainen 1995) were carried out fairly close to the shore and due to their relatively small number they were not considered to give strong enough evidence for authors to draw conclusions that large scale flow pattern in the region definitely differed from the earlier studies.

20. Page 100.
We mean the our results from the dry season expedition (Aug-Sep. 1997). It is a good reason to think that it is persistent for the dry season.

21. Page 103

Yes. We agree that during the wet season the oscillation is more free than during the dry season, when the periodic oscillation of the winds causes a short term periodic stress to the water body and reduces the free oscillation of the water body.

22. Page 105

The vector notation was used to write down the equations more compactly.

The dot product of the gradient operator

V = (d/dx.d/dy)

(this equation transcribed from hard copy and should be confirmed with T. Huttula)

and horizonal velocity vector V=(u,v) gives for x - momentum equation

(u.d/dx + v.d/dy)u = u.d/dx + v.d/dy

(this equation transcribed from hard copy and should be confirmed with T. Huttula)

and for y-momentum equation :

(u.d/dx + v.d/dy).v = u.d/dx + v.d/dy

(this equation transcribed from hard copy and should be confirmed with T. Huttula)

The symbol V - logical OR was used.

23. Page 106

Equations 3.7 and 3.8 present two alternatives for approximating bottom shear stresses in the Neumann type boundary condition (first part of Eq. 3.5). The 'no-slip' or zero-velocity boundary condition is another option.

24. Page 107

We mean an isothermal condition by the words 'completely mixed'.

Concluding remarks

We agree that the validation process of the models can be extended. At the same time the ability of the circulation model to explain the diurnal variations of the flow due to strong variations of the wind stresses was very encouraging. There is no doubt that there is a lot of room for further improvements of the modelling system to ensure the highest quality of the results in possible future studies. We share the opinion of Dr. Spigel that future numerical modelling should take into consideration the water temperature dynamics as one of the most important physical variable for pollution and ecological studies.

These comments were produced by Timo Huttula, Pekka Kotilainen, Anu Peltonen and Victor Podsetchine.

On the behalf of the team
Timo Huttula