

"From the fruit of his lips a man is filled with good things as surely as the work of his hands rewards him."

..... Proverbs 12:14

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To: David Briggins, Manager, Water & Wastewater Branch, NSDEL
From: S. M. Mandaville B.E., Post-Grad Dip. Professional Lake Manage.
Chairman and Scientific Director
Date: May 22, 2002
Subject: Some informal thoughts on your modelling/water quality workshop report

Good morning David, and all recipients: This submission is as a result of very interesting comments made in your all day December, 2000 workshop summaries. There may be several typos/grammar, but the content will not be lacking in facts!

[!] I think that NSDEL's idea of accepting and/or developing one single model for all of Nova Scotia is not (r) not a sensible methodology as I already stated to Darrell Taylor not once but scores of times; further it is NOT true that all models arrive at the same results. Did anyone ever check in a detailed manner into this or were they just philosophic statements made by whoever at the workshop?

The limnologist/researcher has to select a model depending upon the questions that need answering and the lakes that he/she wants to model. This is NOT something that an unqualified person, e.g., an engineer or planner, should get involved in if it has legal relevance. The engineer and/or planner must work in partnership with an authentic limnologist and/or a researcher eminently versatile in limnology.

If not, the development industry will challenge them legally, and the industry would be correct in challenging sloppy science as has already occurred in the HRM and its previous municipal districts on more than one occasion, and no elected Municipal Council or Minister would approve pragmatic controls on development; past urban councils had already rejected such controls, and all they accepted were studies on a philosophical level only! The legal ramifications are way too serious to be taken lightly though ofcourse there is no hindrance to continued endless research at universities/institutes as such research has no legal relevance!

My experience now with nine hundred and thirty five (935) lakes/ponds proved to me beyond a reasonable doubt that it is **foolhardy** to stick to just one model. Even the OECD model (Vollenweider & Kerekes, 1982) which I had promoted for years, as Darrell

Taylor knows, has let me down badly on several occasions! Though it was quite useful in terms of displaying neatly in a graphical format the progression on a trophic scale, but NOT in developing actual predictions in terms of modelled Cha values in many cases, and TP in some cases!

The same applies with the various Ontario models (Dillon *et al*) though they were quite accurate in predicting the TP values in the low range (ultra-oligotrophy to low mesotrophy). But they have failed me in predicting Cha values even in high oligotrophic lakes, not just in high mesotrophic to eutrophic lakes.

I have applied several models, not just OECD or Dillon *et al*, and I am getting contradictory, sometimes severe, results especially for mesotrophic and eutrophic lakes in the HRM area though the results are not that much different for low oligotrophic to mesotrophic lakes. I surmise here that since the TP values are low for such lakes, there is not much one can go wrong in predictability in terms of statistical variations!

I again surmise that the aforementioned is the principal reason as well in the case of the Dillon *et al* model used for the trophic component in the 5-lake, \$120,000 SEDA-funded Kings County model where there was good correspondence between the modelled and the field values for both TP as well as Cha! Decadal-time scales will only tell if the Kings County's application of the Dillon *et al* model will uphold, say 50-100 years from now, if and when those lakes become high mesotrophic to eutrophic, if that occurs at all.

During the mid-1990s, I had compared the results of several regression relationships from Ontario (Dillon & Rigler, 1975; Dillon *et al*, 1986 & 1994; Hutchinson *et al*, 1991; and Molot & Dillon, 1991), and the OECD (Vollenweider, 1968; and Vollenweider & Kerekes, 1982), and arrived at considerably contradictory values in the modelled and field data for **Cha**, especially in lakes undergoing current nutrient stresses not only due to watershed effects but also due to the **recently enhanced deleterious effects of both UV-A as well as UV-B penetration**.

For the latter, *cf.* Schindler *et al* (1990, 1996); the current extensive research at ELA w.r.t. the influence of ultraviolet radiation on alkaline phosphatase activity (APA); and Wetzel (2000). I don't think it is possible now to factor in all these variables into a single model though the USDA and USEPA are attempting to do so (as I hear); time will only tell if they will be successful.

Indeed, at the presentations I made to your Department on **December 15, 1994** and on **March 08, 1995**, I not only showed detailed overheads proving some of this but also submitted a 20-page or so of actual comparisons to Andy Cameron. I am not sure what Andy Cameron did with my submissions and where they ended up as I had made other submissions with 'proofs' as well! As you may know, I made several other presentations to NSDE subsequently, both written as well as with overheads at NSDEL.

[II] Further, none of the models discussed in your workshop summary address the paralimnetic component, they just deal with the pelagic (planktonic) component!

As commonly used, trophic state indices refer to the level of planktonic autotrophy. In lakes dominated by paralimnetic or littoral organic sources, the TSI will be low because autochthonous (pelagic, phytoplanktonic) production is low, e.g., dystrophic lakes.

On the other hand, respiration-based trophic indices include littoral production as autotrophy: Holdren *et al* (2001- official handbook of the USEPA, latest revision); Nürnberg & Peters (1984); Nürnberg (1984, 1985, 1987, 1991, 1994, & 1995a,b); Nürnberg *et al* (1986); and many others (*cf.* <http://chebucto.ca/Science/SWCS/referenc.html>)- (all our URLs are case-sensitive)!

[III] Also, the models discussed in your workshop report do not address shallow lakes since the vast majority of shallow lakes may be 'macrophyte-driven' and not 'algal-driven'.

As an example of severe erroneous conclusions, a major engineering consulting firm for the HRM made several erroneous statements in their study of Cranberry Lake, Dartmouth quite recently. Indeed, they claimed that Cranberry Lake was even **oligotrophic based on their Cha values** which no authentic limnologist would claim so.

The consulting firm failed to take into account the biological component represented by the vast number of macrophytes and totally ignored the **concept of allelopathy (allelochemical autotoxicity)**.

Suitable references for shallow lakes, allelopathy, and the recommended assessment/indices are well illustrated in the following **leading references**: Ervin & Wetzel (2000, 2001); Mann & Wetzel (2000a,b); Moss (1998); Porcella *et al* (1979-USEPA); Scheffer (1998); Wetzel (Limnology, 3rd Ed., 2001); Wetzel (2000a,b).

[IV] Re adoption of trophic criteria as the official guidelines for our beloved Province, the three Probability Distribution diagrams (mean yearly TP, mean yearly Cha, and peak yearly Cha) of the 18-country OECD (Vollenweider & Kerekes, 1982) are preferred based on my several international discussions and local experience! And the sampling frequency as well should be as per the recces in the said OECD study. The strength of the said diagrams is a reflection of what 100 limnologists may conclude as, and not of one limnologist!

cf. <http://chebucto.ca/Science/SWCS/TPMODELS/OECD/oecd.html>

An example of what I did in this aspect for 27 HRM lakes can be seen at <http://chebucto.ca/Science/SWCS/TPMODELS/OECD/trophic.html#example1>

Trophic states may be averages of mean-TP, mean-Cha, and peak-Cha under each trophic category! SD values may have to be ignored!