EXHIBIT 22 (M.21)

Review of: Numeric Nutrient Criteria for the Great Bay Estuary, in light of comments made by John C. Hall and Thomas Gallagher (2010)

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Background

NH DES published Numeric Nutrient Criteria for the Great Bay Estuary in June 2009.[1] In response to requests by states, EPA published additional guidance to develop nutrient criteria based on stressor-response relationships.[2] The EPA Science Advisory Board published its review of the EPA stressor-response guidance.[3] Hall and Associates, assisted by Hydroqual, published a review of the NH DES Great Bay nutrient criteria document based on the findings of the SAB review.[4] The NH DES criteria document was reviewed by two independent reviewers in 2010 through EPA's N-Steps program.

NHDES developed the Great Bay estuary using multiple lines of evidence, including deriving criteria to protect designated uses related to swimming (based on the 90th percentile of chlorophyll concentrations) and aquatic life use. For aquatic life use, the endpoints included dissolved oxygen levels, eelgrass extent (based on water clarity and conversion to macroalgal beds), and extent of phytoplankton blooms (e.g. 90th percentile of measured concentrations). Most of the approaches were based on statistical relationships between causal (total nitrogen) and response variables (e.g. chlorophyll *a* concentrations).

The SAB review criticized the EPA stressor-response guidance for inadequate attention to highlighting the need for conceptual models to provide a foundation for the expected stressor-response relationships. The SAB stated that purported stressor-response relationships based on statistical associations are not sufficient to prove cause and effect unless supplemented by additional analyses, such as multiple regressions or classification to eliminate the effects of potentially confounding, or co-varying variables. In addition, the SAB emphasized that the strength of the stressor-response relationship and levels of uncertainty should be quantified. Hall and Gallagher emphasize these points in their review of the Great Bay estuary nutrient criteria.

Thus, I reviewed the Great Bay nutrient criteria to determine whether the authors of the NH DES criteria document provided enough information to establish a scientifically defensible cause and effect relationship. To be defensible and consistent with the concerns raised by the SAB and Hall and Gallagher, I looked at whether:

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[1] Numeric Nutrient Criteria for the Great Bay Estuary. June 2009. Prepared by Philip Trowbridge, P.E. State of New Hampshire Department Of Environmental Services. R-WD-09-12.
[2] Empirical Approaches for Nutrient Criteria Derivation. Prepared by: United States Environmental Protection Agency, Office of Water, Office of Science and Technology. Science Advisory Board Review Draft August 17, 2009
[3] SAB Ecological Processes and Effects Committee Review of Empirical Approaches for Nutrient Criteria Derivation. April 27, 2010.
[4] Evaluation of Proposed Numeric Nutrient Water Quality Criteria for the Great Bay Estuary. John C. Hall (Hall and Associates) and Thomas Gallagher (Hydroqual, Inc.). DRAFT. June 30, 2010.

Was a reasonable conceptual model described to explain functional relationships and established based on both literature and site-specific data or models?
Were confounding variables eliminated as potential explanations of observed relationships"?
Was the level of uncertainty evaluated?

Overall, the document meets these conditions, but could be improved in some areas. Below I make some suggestions of additional data or analyses that could be emphasized to improve the confidence of the stressor-response relationships described in the NH DES criteria document.

Conceptual models

I think the document could do a better job of explaining the connections between nutrient enrichment and biological responses in a conceptual model. Instead, these connections are interspersed throughout the document, or incomplete. They rely on literature and only sparingly rely on established results from the estuary itself. It would be better to document some of the connections within the estuary itself.

Algal blooms

For example, on page 30, it is stated that median nitrogen concentrations are the best explanatory variable for peak chlorophyll *a* concentrations. The conceptual model should state more clearly why median concentrations of TN are associated with the *90th percentile (rather than a median concentration)* in chlorophyll *a* measurements. Perhaps the conceptual model should be clarified as follows: nitrogen is the major limiting nutrient throughout the Great Bay estuary (or in salinities greater than 10 psu?) and increases in TN result in increases in primary production resulting in increases in algal biomass (as represented by chlorophyll *a*). The probability of algal blooms, as represented by the 90th percentile of chlorophyll *a*, is increased when the average concentrations of chlorophyll *a* increase.

The evidence for nitrogen limitation is presented, and there is good supporting evidence that on a seasonal basis, when bioavailable nitrogen (and phosphorus) is depleted, chlorophyll *a* levels increase.

The correlations between total nitrogen and 90th percentile chlorophyll *a* levels by assessment unit or by trend monitoring station are strong, but does this discount other factors, such as salinity and wind, or stratification? Was as strong a relationship found between median nitrogen and median chlorophyll? Is there supporting information to suggest that the chlorophyll *a* levels observed in the estuary are consistent with a response from the measured or estimated nutrient loading to the estuary? Was primary production ever measured, and if so, would the production rates result in chlorophyll biomass or bloom conditions observed in the data? When were the bloom conditions found? Are they primarily in the spring before stratification sets up, or during mixing events? Related to this, why wasn't a shorter index period used, rather than the full year? Why would the full year provide a better statistical relationship? If so, how does that figure into the conceptual model? My understanding of the growth period of eelgrass in New England is April to October, yet year round data are used. Similarly, why is year round data used when dissolved oxygen problems are manifested only in summer months?

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Macroalgae

On page 37, in the discussion on macroalgae, it is stated that the macroalgae mats have now replaced areas formerly occupied by eelgrass. The conceptual model is that as TN increases, eelgrass is replaced by macroalgae, but the actual mechanism is not sufficiently explained. Are macroalgae better able to utilize nutrients in enriched conditions and thus outcompete eelgrass? Are there any literature or mesocosm experiments in Great Bay that document this? There is literature from Waquoit Bay, but is this area similar enough to Great Bay to explain the process?

Although there does seem to be supporting evidence of this replacement based on one aerial surveys, there is insufficient documentation of the loss of eelgrass and coincident replacement by macroalgae. There are two years of observations (1996 and 2007) for eelgrass, and only one year for macroalgae. Are there other observations that would support this model of replacement of eelgrass by macroalgae?

Light extinction

The section titled Conceptual Model on page 4 doesn't mention light extinction, although this is addressed later on. On page 15, the authors state that eelgrass is sensitive to water clarity without citing the specific experimental evidence in the Great Bay estuary. Fred Short and colleagues have conducted experiments in mesocosms and in the field (I think) showing that phytoplankton shade and intercept light, affecting eelgrass growth. For example, do the mesocosm experiments show the effects of increasing nitrogen enrichment on eelgrass in terms of light attenuation, or lengthening of blades, or loss of carbohydrate stores, or epiphytic growth? Are these loadings similar to loadings into Great Bay and are the responses in Great Bay expected based on the mesocosm experiments?

Page 55 has a nice summary of the conceptual model of eutrophication and light extinction that affects eelgrass. And, the model for light extinction[5] is corroborated by the data on presence and absence of eelgrass in the estuary. In areas of more light extinction, there is less eelgrass. So, this is corroboration of the model, but also a good example of a weight of evidence approach.

Confounding factors

Chlorophyll a

The authors did not sufficiently evaluate whether salinity is more important than nitrogen in controlling phytoplankton abundance. The data presented clearly shows that nitrogen tracks salinity (see Figure 6; there is higher nitrogen in the upstream, less saline tributaries). Does chlorophyll *a* track salinity as well? It does seem that there is also a gradient from upstream to downstream in chlorophyll *a* levels (see Figures 13 and 14). It would be nice to figure out what kind of suspended algae, i.e. phytoplankton, are contributing to the blooms -- are they marine or

[5] It would be good to explain how light extinction was calculated. Is it based on percent of light at 1 meter below the surface?

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freshwater algae? This would provide supporting material to document that the chlorophyll *a* response is controlled primarily by nutrients, rather than habitat changes (i.e. low salinity vs. higher salinity zones).

Benthic indicators

In contrast, the authors in some cases considered confounding factors to explain the benthic indicator data. For example, the discussion of whether organic matter derived from phytoplankton blooms contributes to organic enrichment and benthic community changes in sediments on page 40 (Benthic invertebrates and sediment quality) is evaluated in the context of salinity changes, in addition to nutrient enrichment. Here they evaluated the effect of nutrient enrichment on an Index of Biotic Integrity (IBI), and found that salinity may be the controlling factor. This is based on the original work to develop the IBI, but also on reasonableness. In this case, salinity is a confounding factor and one that has been shown in the literature to be a major influence of biological communities as well.

The authors state (on page 40) that organic matter comes from primary producers, but they don't evaluate the effect of organic matter from terrestrial sources, especially in the upper parts of the estuary. On page 41, they state that the regressions prove that total organic carbon in sediments is associated with nitrogen and chlorophyll *a* concentrations in the water column, but they don't say that they are caused by them.[6] I suspect that terrestrial sources from nonpoint and sewage treatment effluent are more important than autotrophic sources of organic matter.

Dissolved oxygen

The dissolved oxygen section on page 45 presents an incomplete conceptual model, because they do not address other sources of organic matter, including sewage treatment effluent, and terrestrial runoff. Although the graphs are good, they don't really get at the actual dissolved oxygen response, which could be daily dissolved oxygen swings, or a lag, or very low dissolved oxygen in the mornings in the summer. In addition, the relationships could be confounded by salinity stratification, or flushing, rather than nitrogen. The sonde data sources for low dissolved oxygen are all in the tributaries, which are really different than the Great Bay areas, and therefore the low dissolved oxygen could be partly related to poor circulation and salinity wedges and other sources of organic matter (e.g. terrestrial organic matter). Additional information should be presented to discount these other factors.

The discussion about determining an appropriate criterion related to dissolved oxygen on page 51 should be graphed, rather than shown in text. Then we would be able to see the confidence intervals described there.

[6] So I think they should soften the language a little, eliminating the expression of "proof".

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Light extinction

The authors make an excellent effort to determine whether light extinction is caused by algal material or non-algal material, and they conclude based on a multiple regression, that algal material is an important source of controllable light extinction.

On page 63 and in Figure 34[7] the authors suggest that the particulate organic matter in the water column expressed as turbidity is caused by nitrogen and that this particulate matter is autochthonous (i.e. derived from phytoplankton). But, there should be supplemental evidence that discounts the possibility that this organic matter is related to the salinity gradient and is from upstream sources of terrestrial runoff.

Level of uncertainty:

Uncertainty was addressed throughout the document (with a few exceptions) by characterizing the confidence intervals around the regressions. In addition, the authors sought to meet strict levels of variability and did not extrapolate beyond the regression lines.

[7] By the way, the two lines in Figure 34 are not fully explained.

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