Washington State Department of Ecology

Response to Comments on Draft Report:

Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen (April 2003)

Original Response to Comments (October 03)

Addendum

Response to City of Spokane Comments (January 04)

DEPARTMENT OF ECOLOGY

October 24, 2003

TO: Ken Merrill, David T. Knight, and James Bellatty
Water Quality Program, Eastern Regional Office

FROM: Bob Cusimano, Watershed Studies Unit
Environmental Assessment Program

THROUGH: Will Kendra, Manager, Watershed Ecology Section
Environmental Assessment Program

Karol Erickson, Unit Supervisor, Watershed Studies Unit
Environmental Assessment Program

SUBJECT: RESPONSE TO REVIEW COMMENTS ON THE SPOKANE RIVER AND LAKE SPOKANE (LONG LAKE) POLLUTANT ASSESSMENT FOR DISSOLVED OXYGEN STUDY AND MODEL DEVELOPMENT

Ecology received review comments on the Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment study reports and associated CEQUALW2 model development and calibration reports from Limno-Tech, Inc., Esvelt Environmental Engineering, and the Environmental Protection Agency. The documents we received that contain the comments are included in this memorandum with our responses. In addition, Attachment 1 contains responses to the Esvelt Engineering comments by Tom Cole, Corps of Engineers. Tom Cole's comments were sent to Ecology via an e-mail dated July 21, 2003. Below are general responses to the overall comments, followed by the documents from Limno-Tech, Esvelt Engineering, and the Environmental Protection Agency (as italics text) with our responses following each specific comment.

Ecology General Responses

Ecology General Responses

1. The responses to comments on the draft model development and calibration reports we provided in our May 1, 2002, memorandum should be referenced because some of the comments we received from Esvelt Environmental Engineering on the current documents were similar to those we have already addressed. The May 1, 2002, memorandum general responses two through four addressed Model Selection, Model Calibration, and Model Uncertainty. These topics were also discussed in Ecology's draft project report.

2. We are reviewing the current model calibration and will be proposing changes to the model that may improve the model's performance, particularly for the year 2001. Currently, there are four major changes that we are considering: (1) lowering groundwater dissolved oxygen input concentrations, (2) reducing periphyton growth rate, (3) modifying CBODu phosphorus stoichiometry to better represent the average total phosphorus concentrations measured at the model boundaries (i.e., State Line, tributaries, and point sources), and (4) using more algal groups to simulate algal growth in Lake Spokane. However, Portland State University will need to evaluate the proposed changes with respect to the overall calibration of the model and provide Ecology with a final model that can be used to run loading scenarios. The draft study report for this project will not be finalized until any model calibration changes are approved by PSU.

3. Although model calibration refinements are planned as described above, the current model (as calibrated) can be used to determine the overall impact of point and nonpoint source loading of BOD and phosphorus to Lake Spokane. Specifically, the model results show that the current point source NPDES BOD5 permit limits are too high. In addition, the model shows that there is little assimilative capacity for BOD (internal and external BOD) and phosphorus in Lake Spokane based on allowing a 0.2 mg/L dissolved oxygen deficit below natural conditions. If required to meet the current 0.2 mg/L criterion, there would be little or no allocation of these parameters to the point sources. Model calibration improvements will not change these conclusions.

4. The existing phosphorus TMDL allocations are not protective of the water quality in Lake Spokane. The major reason it is not protective is that the loading allocations are based on using an estimated median June-October river of 2,970 cfs at the outlet of Lake Coeur d'Alene without a Margin-of-Safety (MOS). Until final TMDLs for BOD and phosphorus can be established under the current study and modeling, we believe the existing TMDL allocations should be modified to include a MOS based on the original URS (1981) study proposal to use a one-in-ten year seasonal flow. The TMDL phosphorus allocations would then be reduced from 259 kg/day to 163 kg/day for the June through October period based on the historical work (i.e., using a one-in-ten year flow of 1,537 cfs to determine loading allocations). We calculated a one-in-ten year flow for the June-October period of 1540 cfs using 1968-2002 which is very close to the 1537 cfs presented in Patmont et al., (1987).

5. We recently received continuous dissolved oxygen monitoring data collected just downstream of Lake Spokane (Long Lake) dam during July-September 2000 and 2001 by

USGS.   The data show that August and September dissolved oxygen concentrations are consistently below the water quality criterion of 8 mg/L and can be less than 5 mg/L. In order to protect downstream uses, the effects of the reservoir's water quality on dissolved oxygen concentrations below the dam will need to be considered when determining allowable phosphorus and BOD loading to the Spokane River system.

Limno-Tech, Inc. Documents
Including Ecology Responses

Limno-Tech, Inc.
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Memorandum

DATE: February 2, 2004
PROJECT: **SPOCFP**

TO: **Ken Merrill**
Department of Ecology

FROM: **Dave Dilks**
CC: **Bruce Rawls**
Bruce Willey

SUBJECT: **DRAFT: Review Comments on Spokane River/Long Lake TMDL Documents**

SUMMARY

On behalf of Spokane County, Limno-Tech, Inc. (LTI) has reviewed the following Draft documents related to the Washington State Department of Ecology's water quality modeling of the Spokane River and Long Lake:

- *Upper Spokane River Model: Model Calibration, 1991 and 2000*
- *Upper Spokane River Model: Model Calibration, 2001*
- *Upper Spokane River Model: Boundary Conditions and Model Setup, 1991 and 2000*
- *Upper Spokane River Model: Boundary Conditions and Model Setup, 2001*
- *Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen*

In general, the reports document sound scientific work, and the authors are to be commended. Some specific details, however, need to be addressed. Specific comments on these documents are listed below, divided into categories corresponding to the model calibration reports and the Loading Assessment report.

Calibration Documents

- *The primary failing of the model in its current form is the inability to accurately simulate algal productivity during 2001 conditions. The purpose of the model for the TMDL is to link nutrient loads to algal productivity to dissolved oxygen for low flow periods; it is therefore essential that the model be capable of simulating 2001 algal productivity. Addition of a second algal group as provided in the Loading Assessment report is a step in the right direction, but insufficient to correct the problem. Every comparison between modeled and observed epilimnetic DO and pH at every Lake station indicates an underestimate of productivity, even with the addition of a second algal group. Once this problem is better resolved, the model should be a useful tool for TMDL development.*

Response: Please reference general response #2, Attachment 1, and our response to Esvelt Engineering's comment #4 on the model calibration documents. We believe the model as currently calibrated can be used to establish loading limits for Lake Spokane. However, as noted in general response #4 we are working to improve the 2001 calibration.

Historical studies have shown that phosphorus and BOD loadings reduce metalimnetic and hypolimnetic dissolved oxygen concentrations in Lake Spokane. We know there is a reduction in dissolved oxygen due to these pollutants since historical lake data show an improvement in dissolved oxygen when these pollutants are decreased. We are only uncertain about the exact amount of reduction associated with a given pollutant load. The CE-QUAL-W2 model is currently providing us with a best case estimate, from the dischargers point of view, of the magnitude of the reductions due to current and possibly future pollutant loading.

Even if model calibration does not exactly match the dissolved oxygen profiles in the lake for the days that data were collected, the model can still be used to set loading limits based on meeting the 0.2 mg/L allowable change because model comparisons to determine the allowable loading are made based on the difference between nearly identical loading scenarios and predicted responses by changing known sources (i.e., all model forcing factors are the same for each model run except for specific loading changes from one or more known sources). The current model predicts that phosphorus and BOD loading would cause greater than a 2 mg/L deficit in dissolved oxygen in the hypolimnion of Lake Spokane. It is not possible that increasing the model productivity, which increases metalimnetic and hypolimnetic BOD loadings, would decrease this value, such that the current model estimate should represent the minimum reduction in Lake Spokane dissolved oxygen due to known pollutant loading.

Given the large difference between the predicted and allowable dissolved oxygen deficit (>2 versus 0.2 mg/L), we believe TMDLs can be developed using the current model because they would require that point sources not add any additional phosphorus or BOD to the estimated natural conditions for the lake (i.e., they could only discharge estimated naturally occurring concentrations of phosphorus and BOD). Nonpoint sources would also have to be reduced to near natural conditions. It is clear that if the model calibration is improved with respect to phytoplankton productivity, the conclusions regarding allowable loading of phosphorus and BOD to Lake Spokane would be even more restrictive.

- *The reported error statistics should be modified to include a measure of model bias, i.e. whether the model is consistently above or below the data. The two error statistics presently reported provide essentially the same information (average error) without any indication of the direction of the error.*

Response: Error statistics can be recalculated. However, it will require a significant effort and PSU would need additional funding to revise all of the reported error statistics.

- *The modification of the reaeration rate between Upper Falls Dam and Seven Mile Bridge appears arbitrary, with the sole purpose of masking problems that the model may have in describing periphyton productivity. If the "surfactant-induced decreased reaeration" theory is to be maintained (and I strongly recommend against this), significant additional justification would be required. This justification would need to include description of what the originally estimated reaeration rate was, documentation of other sites where this phenomenon was observed and the extent to which reaeration was reduced, and evidence of the site-specific factors that would cause this phenomenon to be observed in the Spokane River. It is noted that the draft calibration report attributed the supersaturated dissolved oxygen concentrations in the Spokane River to excess reaearation. Accurate prediction of seasonal periphyton dynamics, especially with the relatively simple periphyton framework in CE-QUAL-W2, is extremely difficult. My belief is that it is more forthright to admit that the model cannot capture all of the observed periphyton phenomena, rather than arbitrarily adjust coefficients to improve model comparison to data*

Response: See response to John Yearsley's comment # 1 on the model calibration report.

Loading Assessment Document

- *The report provides a compelling argument that the nutrient targets used for the previous Long Lake TP TMDL are under-protective. No discussion is provided on the selection of more appropriate nutrient targets. Selection of new nutrient targets will be a time-consuming task, requiring public involvement. If new nutrient targets are to be defined, this process should begin immediately. If the presumption is that no new nutrient targets will be defined because the DO TMDL will be expected to prevent nuisance algal growth, that presumption should be stated explicitly.*

Response: Our current water quality criteria provide guidelines for establishing nutrient criteria for specific categories of lakes in different Ecoregions of the state. For example, Lake Spokane is in the Columbia Basin Ecoregion and the recommended ambient TP criteria for a lower mesotrophic lake would be 20 ug/L or less (range 10-20 ug/L). Given the Lake Coeur d'Alene source water is <10 ug/L (1995-2002 average 0-30 meter data for all samples collected in the northern part of Lake Coeur d'Alene from 1995-2002 was 7 ug/L), it would be appropriate to set the range for Lake Spokane <20 ug/L with some MOS (i.e., the current

TP TMDL set the target at 25 ug/L with no MOS). However, we believe that meeting the current dissolved oxygen criteria for the lake and river critical reaches will prevent nuisance algal growth. At this time we are uncertain what the outcome of the proposed Use Attainability Analysis will be with respect to modifying uses or economic impacts and the potential for meeting some alternative dissolved oxygen criteria such that we may change our view that the aesthetic quality of the lake also can be protected. Hopefully, the UAA analysis will include an evaluation of the aesthetic issues for the lake and the dissolved oxygen criteria.

It should be noted that regardless of our concern about how the lake TP TMDL target concentration was established, the major *problem* identified with the current phosphorus TMDL in the draft report was that there is no MOS. An appropriate MOS would account for the uncertainty associated with the methods and assumptions used to set the lake target. See general response #3 for further discussion.

- *The sediment oxygen demand (SOD) to be assumed in Long Lake for defining "natural conditions" for future allocation scenarios needs to be explicitly stated. At prior public meetings on the TMDL, I believe that it was mentioned that the existing calibrated SOD would be used for future scenarios. At the June 26, 2003 meeting, it was indicated that this may not be the case. This issue needs to be resolved.*

Response: The SOD scenario was not defined in the report as natural conditions. We included model run results using a literature value for an oligotrophic lake and noted in the report:

"Although it is probably not possible to determine exactly what level of sediment oxygen demand would be in the system without point and nonpoint sources of pollution, the predicted profile probably represents the "best possible" dissolved oxygen profile that could be attained over time given the time of year, location, and flushing rate.

….the SOD scenario results can only be used as a possible best case condition for the lake and should not be used as the reference condition for establishing pollutant loading allocations relative to an allowable change. Pollutant allocations should be established using the NO-SOURCE scenario as the reference condition to determine allowable dissolved oxygen deficits, because the pollutant loads that cause dissolved oxygen deficits of 0.2 mg/L should be the same for either scenario."

Although this is still our recommendation for establishing pollutant allocations, improvements in dissolved oxygen based on the change from the *no-source* scenario alone cannot be used to evaluate the environmental *value* or the social and financial *cost* for any incremental change without considering the potential changes represented by the SOD scenario results. As mentioned in the report, Lake Coeur d'Alene minimum dissolved oxygen concentrations in the northern part of the lake were found in 1989 (a low river flow year) to be between 6-7 mg/L. More current Lake Coeur d'Alene profile data collected from 1995-2002 also show that minimum dissolved oxygen concentrations are between 6-7 mg/L. We believe that Lake Spokane would have similar minimum dissolved oxygen concentrations if algal productivity and BOD loading were reduced, because over time this would lead to lower SOD.

Esvelt Environmental Engineering Document Including Ecology Responses

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July 18, 2003

MEMORANDUM: COMMENTS ON WASHINGTON DEPARTMENT OF ECOLOGY (DOE) REPORTS:

1. *Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen, April 2003 – Draft, Review Draft –5-23-03.*
2. *Data Summary: Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen, April 2003, Review Draft 5-23-03.*
3. *Upper Spokane River Model: Model Calibration, 2001, Portland State University, Chris J. Berger, Robert L. Annear Jr., Scott A. Wells, January 2003*

BY: Larry A. Esvelt PhD PE DEE, Mark H. Esvelt MS PE

Submitted on behalf of City of Spokane, Inland Empire Paper Co., Liberty Lake Sewer and Water District.

INTRODUCTION

The publications that are the subject of this memorandum were issued by the Washington Department of Ecology as a portion of the documentation intended to lead to a Total Maximum Daily Load (TMDL) for oxygen demanding substances to the Spokane River, intended to protect and enhance the water quality of Lake Spokane (Long Lake). The further limitation of oxygen demanding substances, which include substances with potential indirect as well as potential direct impact on the dissolved oxygen resources of the river, may significantly impact the wastewater treatment practices, and costs, for dischargers to the Spokane River. It is essential that those expected to bear those costs be intimately involved in the TMDL process to assure that any resulting recommendations are technically and scientifically sound, and that expenditures result in actual, not hypothetical, improvements to the water quality that will benefit aquatic life and residents of the region.

UPPER SPOKANE RIVER MODEL: MODEL CALIBRATION, 2001, January 2003
Chris J. Berger, Robert L. Annear Jr., Scott A. Wells

Calibration of the CE-QUAL-W2 model for the Spokane River for the 2001 data collected by the dischargers to the river was performed in 2002 with the report made available in 2003. It is this calibrated model that is used by the Department of Ecology for development of DOE Report No. 1 from data presented in DOE Report No. 2. Consequently review of the DOE reports inherently includes reference to the calibration report, and some of the comments may reflect on the model, and / or its calibration.

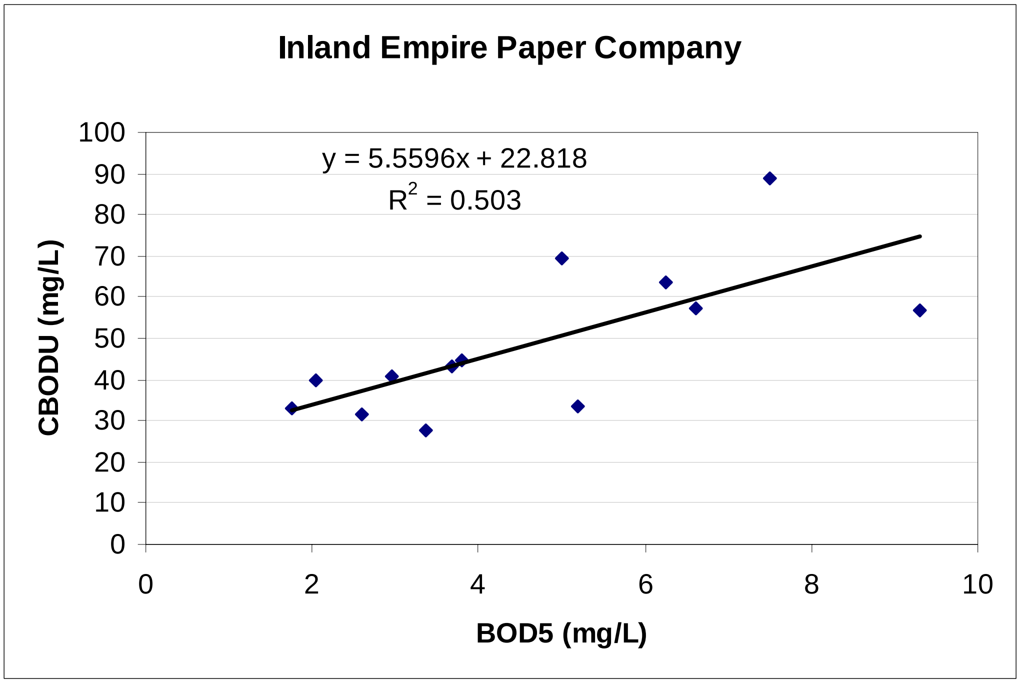
1. *In review of the model input files, for 2001the use of daily monitoring report (DMR) data from the dischargers applied to the model for calibration had two apparent adjustments:*

1) *Five-day biochemical oxygen demand (BOD5) from the DMRs was adjusted to reflect ultimate carbonaceous biochemical oxygen demand (CBODu) by factors apparently determined through the discharger sponsored testing at WSU Department of Civil and Environmental Engineering. Review of the input file data and comparison with DMR data, shows that the average ± the standard deviation, and range of adjustments (factors) used was as follows:*

Liberty Lake Sewer and Water District: Mean = 4.5 ± 2.34; Range = 1.55 – 13.96.
Inland Empire Paper Co: Mean = 10.81 ± 3.73; Range = 6.12 – 26.31.
Spokane AWTP: 3.25 ± 0.93; Range = 3.18 – 4.52.

There is no explanation as to why there was such a disparity in the adjustments for BOD5 to CBODu for the model input files.

Response: We are not sure what monitoring data you are comparing, but the data in the files we used match the multipliers listed in the calibration report for all of the dischargers except Inland Empire Paper Co (IEPC). I believe Esvelt Engineering sent Portland State University (PSU) the 2001 point source data. (The 2001 point source data that we used are available on Ecology's web site—linked to the data report.) I recommended to PSU that we change the single CBODu:BOD5 multiplier for IEPC to a regression estimate (see graph below) because their CBODu test data showed a concentration relationship between CBODu and BOD5 (i.e., lower multiplier at higher CBODu values). Using the regression relationship reduced the CBODu estimates for higher BOD5 values (especially for those values >10 mg/L). Unfortunately, we did not update the reports with this change before they were finalized.



2) *Total phosphorus (TP) from the DMRs was adjusted in the model input files, according to personal correspondence with Bob Cusimano, to reflect soluble reactive phosphorus (SRP) believed to be the readily available form of phosphorus to biological organisms, including algae, for assimilation and growth. The adjustment factors were reported by Mr. Cusimano to be the data collected by the dischargers in testing done at Columbia Analytical Services (CAS). Review of the input file data and comparison with the DMR data, shows that the average ± the standard deviation, and range of adjustments (factors) used was as follows:*

   Liberty Lake SWD: Mean = 0.935 ± 0.935; Range = 0.517 – 3.01
   Inland Empire Paper Co.: Mean = 1.226 ± 0.950; Range = 0.094 – 3.889
   Spokane AWTP: Mean = 0.364 ± 0.001; Range = 0.360 – 0.368

The input files for groundwater also used SRP instead of TP. This apparently resulted in the input files containing concentrations from 40% to 60% of the TP concentrations and loading. See discussion under comments regarding Pollutant Loading Assessment Report.

The input file for the State Line boundary condition (statec01.npt) contains PO4 values that appear to represent SRP concentrations instead of TP also

There is no explanation of the discrepancy among factors applied to the reported data in development of the input files. The model, without the remainder of the phosphorus as input may not have been calibrated accurately, as algae growth in Long Lake is highly sensitive to phosphorus input. It appears that the model input files assume that all of the TP not identified as SRP is not available for biological growth or stimulation, and that it is an inert substance without effect. There is no attempt to track that portion of the phosphorus through the river-lake system.

Previous work, including that directed by Dr. Ray Soltero has used TP as the independent variable in determining algae growth response in Long Lake. This is consistent with lake and reservoir modeling practice initiated by Dr. Volenweider in correlation of algae response to total phosphorus loading. Improvements in the water quality of Long Lake were correlated with reduction in TP input in development of the phosphorus TMDL approved by EPA in the early 1990s. Use of SRP and ignoring the non-SRP portion of TP in this model does not allow comparison of water quality results from the model with previous work.

Total phosphorus, TP, discharged to the environment can consist of a range of compounds, including:

- *Soluble orthophosphate (PO_4^{-3}, HPO_4^{-2}, $H_2PO_4^{-}$, depending on pH);*
- *Poly phosphates (polymerized orthophosphate, as used in detergent buffering agent formulations); and*
- *Organic phosphorus compounds (phosphorus bound in cellular structure of biological organisms).*

The soluble reactive phosphate (SRP) is readily available for biological uptake and promotion of cell growth according to most biologists and researchers in water quality. However the other forms of phosphorus can also become available:

- *Poly phosphate compounds are made available by hydrolysis to orthophosphate. This is readily promoted by biological action in receiving water, and possibly by light and other factors.*
- *Organic phosphorus is made available by biological degradation (decomposition) of the organics, freeing the inorganic portions of the organic molecules, including phosphorus. This degradation occurs readily in the environment with copious availability of organisms to perform the degradation and rapidly at the temperatures found in the Spokane River and Long Lake during the spring, summer, and fall seasons.*

There is no apparent use of the TP data during model calibration, which places the entire calibration in question, as phosphorus is acknowledged to be a critical consideration in assessment of the response of Long Lake to discharges from point source discharges and to non-point sources of pollutants.

Consideration of all sources of oxygen demand sources is important in the assessment of alternatives for remediation of oxygen deficits in Long Lake, if the low DO can be addressed. Phosphorus, when converted to biological organisms, such as algae cells, can result in the generation of over 100 pounds of oxygen demanding substances per pound of P used in the cell synthesis. Calibrated model parameters ALGP-A1 (algae), EP-E1 (periphyton), and BIOP of 0.005 vs. 0.011 "typical" appear to indicate that the organic matter equivalent of P consumed is about 200. The value representing "typical" indicates only approximately 90 mg organic matter per mg P, which could indicate that during calibration of the model it was identified that there is more organic material being generated than the model input P should provide for.

The calibrated half-saturation constant of P for algal growth is 1/3 of the "typical value". This adjustment during calibration would also indicate a higher growth response of algae than would have been anticipated, potentially due to the lower P availability indicated by the input files.

If P is the limiting nutrient, as it has been determined to be in the Spokane River and Long Lake during earlier studies by Soltero and others, then this source of oxygen demand needs to be fully accounted during model calibration and in the model as it is used to assess remediation alternatives.

Since the input files were provided to the model calibrators, there would have been no way for them to know that the P input to the system was reduced from the actual input. The calibrators then responded by adjusting the parameters to more closely represent actual conditions based on the data collected, with the model only realizing the modified P in the input files. It appears that this has resulted in a model that is less than representative of real conditions during calibration, and potentially unable to as accurately respond to changes in input conditions that may be explored. In other words, the model as now calibrated does not appear to be usable for simulation of river water quality with changes in discharge and non-point input conditions.

[NOTE: Personal communication with Dr. Ray Soltero, director of many studies of the Spokane River and Long Lake, obtained his opinion that the use of only that portion of the TP load characterized as SRP as model input is highly risky, and potentially could make the model unusable as not accurately representing the river/lake system.]

Response: Please reference response in Attachment 1. As noted in the attachment, the model does currently include phosphorus in the CBODu inputs and, therefore, provides an estimate of *total* phosphorus (i.e., organic and inorganic phosphorus). However, as noted in general response #4, we are in the process of re-evaluating the boundary CBODu values and associated phosphorus stoichiometry to improve the models overall estimate of total phosphorus.

2. *Error statistics presented in the model report do not show the bias in the errors, in that both methods of presenting the statistics utilize square of the error. It is fairly apparent from observation of the comparison of model output with the data collected in the river and Long Lake that the errors could include bias, differing by depth and location. Alternative methods of presentation of the error statistics should be considered, and statistics presented on the basis of segments of the model and water bodies as well as overall to accurately reflect that there may be significant errors in the model that could be a factor in use of the model in evaluation of alternatives for water quality improvement.*

A compilation of error statistics from other models, as well as other applications of this model would be helpful in assessment of the model suitability and its calibration for the Spokane River system. Error statistics for testing model acceptability would also be helpful.

Response: Reference response to Limo-Tech bullet #2. We will provide a list of typical error statistics for other model applications.

3. *Conductivity correlations between the calibrated model and actual data appear to be poor. Examination of the ground water input quality from the Spokane Rathdrum aquifer, which assumes consistency through the year, could contribute to this.*

Response: Please reference response in Attachment 1. We are reviewing the groundwater model input files and ranges of conductivity data measured in wells along the river. If necessary, we will modify the groundwater input files based on the measured ranges to better represent the river conductivity during 2001.

4. *Dissolved oxygen (DO) simulation of the model compared to actual data is not sufficiently accurate to give confidence in use of the model to predict changes as small as 0.2 mg/l with accuracy. The error statistics indicate errors, by these measures to average nearly 2 mg/l in the profiles (Long Lake) and nearly 1.5 mg/l based on time-series error statistics. This indicates that accurate prediction of when the DO will be in violation of the water quality standards will be highly uncertain, and prediction of when the oxygen deficit exceeds the 0.2 mg/l non-reduction criteria for non-degradation will also be uncertain.*

It appears that the model consistently under predicts the DO in the surface layer (epilimnion) of Long Lake, while consistently over-predicting the DO in the metalimnion and below. These apparently consistent errors may lead to erroneous conclusions regarding the effectiveness of various alternatives for improvement of water quality, and make it difficult to predict actual benefits for what could be significant expenses of the dischargers (public).

It appears that the model over predicts running average DO concentrations and the diurnal variation on DO in the river when compared to actual data. This could be due to over prediction of algal and periphyton productivity due to the calibration procedure that resulted in the half-saturation constant being lower than expected and biomass production being higher than expected per unit of P in the model input files. The discharge of P at the Spokane AWTP is the most potentially under included in the input files, whereas P input from the upstream dischargers is more nearly the same as reported as TP. This could lead to erroneous conclusions regarding the affect of alternatives intended to improve water quality. In the upper portions of the river it appears that the violations of the DO standard occur due to the diurnal low DO concentration.

Response: Please reference response in Attachment 1 (page 3, paragraphs 4-6). The current and historical data for Lake Spokane show that Lake Spokane dissolved oxygen concentrations violate the water quality criterion. (Note that if the new state water quality criteria are approved by EPA, the lake will still violate the dissolved oxygen criterion.)

In the early 1990s, we recognized that in some cases a significant amount of loading to a water body could cause a *non-measurable* change in dissolved oxygen, which we defined as 0.2 mg/L (Ecology, 1996). We recognized that this amount could only be estimated through modeling (i.e., the difference between model runs with and without loading). In applying this criterion, we understood that model uncertainty may be greater than the 0.2 mg/L allowable deficit but that allowing the 0.2 mg/L deficit would provide some relief to NPDES permittees, such that in many cases discharges would not have to be removed from impaired waterbodies. For the Lake Spokane, current loading from point and nonpoint sources are predicted to cause greater than a 2 mg/L decrease in dissolved oxygen in model segments that represent the hypolimnion, which is much greater than the allowable 0.2 mg/L deficit in Lake Spokane. In addition, the SOD scenario shows that DOs could improve over time much more than 2 mg/L if pollutant sources are reduced.

As noted in general response #3, we are working to improve the model calibration for the river. It is clear that the model is over predicting dissolved oxygen in the river. However, the reason is that we used the *average* measured groundwater dissolved oxygen concentrations for specific river reaches to represent the groundwater inputs along different model branches. Adjusting the groundwater input values to better match the river data would be appropriate and is one of the aspects of the model calibration that we are reviewing.

Ecology, 1996. Total Maximum Daily Load Development Guidelines. Publication No. 97-315. Washington State Department of Ecology, Environmental Assessments Program, Olympia, Washington.

DATA SUMMARY: SPOKANE RIVER AND LAKE SPOKANE (LONG LAKE) POLLUTANT LOADING ASSESSMENT FOR PROTECTING DISSOLVED OXYGEN
Washington State Department of Ecology, Publication No. 03-03-023, April 2003, Revised May 2003, Review Draft 5-23-03.

1. *Data compilation for the river and dischargers is well presented. Notable data is that from the Stateline sampling point (RM 96.0) where violation of the Washington State water quality standards for DO during August is documented during diurnal low DO periods. The diurnal variations are potentially attributable to the effect of algal and periphyton activity, but whether these are occurring in the flowing river or in Lake Coeur d Alene Spokane river arm is not certain. In addition the high temperature of the river, most certainly affected by Lake Coeur d Alene are a factor, as the saturation of DO in the river may limit the diurnal high concentration, causing the diurnal low to be depressed downward.*

Response: Changed the text to read "…are mainly due to photosynthesis and respiration of floating and attached algae."

2. *There is no indication in this report, or in the attached letter to Scott Wells transmitting the input file data, that total phosphorus loads from the treatment plants, as reported in the DMRs, would be factored for the input files for model calibration.*

Response: Please see response to your comment #1-2.

SPOKANE RIVER AND LAKE SPOKANE (LONG LAKE) POLLUTANT LOADING ASSESSMENT FOR PROTECTING DISSOLVED OXYGEN, Washington State Department of Ecology, Publication No. 03-03-0??, April 2003 – DRAFT, Review Draft – 5-23-03.

1. *Abstract – p. vi: Fourth paragraph: diurnally low dissolved oxygen is due in part to other factors as well: temperature, low re-aeration due to backwater caused by hydro-electric facilities, influence of groundwater.*

Response: We agree that there are other factors that contribute to changes in dissolved oxygen. However, the main cause is from periphyton. We modified the sentence to read "….mainly due to periphyton growth…."

2. *Abstract – p. vii: Second paragraph: These statements are not universally true. Only in select portions of the river/reservoir system does the model predict reductions to point-source discharges will improve water quality. The report does not actually include any results of model scenarios where point source discharges are reduced to verify that reductions would result in improvements.*

Response: We agree with the first sentence of your comment and changed the text to read "….in some areas of the Spokane River and Lake Spokane …." However, Figures 37 and 47 show that eliminating point sources would improve dissolved oxygen from predicted to be below the criteria to above the criteria. The changes could be as much as 0.5 to 0.6 mg/L from the current (2001) loading condition. In the River Results section, we explain that some segments were predicted to have values <8 mg/L under the *current* scenario (i.e., 2001 conditions). It should be noted that the current calibration of the model is over predicting dissolved oxygen in the Spokane River. As discussed during the June 26, 2003, public meeting, we will be working to improve the calibration of the model and expect that more areas of the river will be predicted to violate the criteria if lower groundwater dissolved oxygen is input into the model (e.g., model segments that represent the Upriver Dam and Upper Falls Dam pools).

3. *Introduction – p. 9: first paragraph: 1981 and 1987 reports are cited as identifying bottom waters of Lake Spokane as being "impaired". The definition of impairment may have changed over time, and with evolution of the current water quality standards. The 0.2 mg/l dissolved oxygen depression as the standard for impairment was not established until after 1987. In addition, it is accepted that conditions have changed considerably since 1987.*

Response: Anoxic/hypoxic conditions have been identified as a problem in Lake Spokane since the 1960s. The 1981 URS and 1987 Patmont et al. reports have a number of references to the impairment of dissolved oxygen in Long Lake (Lake Spokane) and the potential for improvement in dissolved oxygen if pollutants were removed. For example:

"Comparison of existing conditions to criteria indicates that several current and projected problems are present: 1) algal growth in Long Lake, 2) low DO levels in and below the lake,…." (Summary URS, 1981)

"…., several additional investigations were performed to verify the occurrence of hypolimnetic anoxia and algal blooms, and examine potential control strategies….EWU studies examined nutrient loading dynamics, algal biomass, and hypolimnetic anoxia….their work supported positions of the EPA and Ecology that phosphorus removal at the City of Spokane Wastewater Treatment Plant would substantially improve water quality conditions in Long Lake." (Introduction: Patmont et.al., 1987)

Allowing a 0.2 mg/L dissolved oxygen deficit does not pertain to determining that the water body is impaired today or has been in the past.

Although there appears to have been improvements in hypolimnetic dissolved oxygen in Lake Spokane over the last 15-20 years because of pollutant loading reductions, the dissolved oxygen concentrations are still depressed compared to estimated natural conditions and when compared to current conditions in Lake Coeur d'Alene. (As mentioned, profile data collected from 1995-2002 in Lake Coeur d'Alene show that the northern part of the lake has minimum dissolved oxygen concentrations of 6-7 mg/L.) Ecology is concerned that the current permitted BOD5 effluent limits and the total phosphorus TMDL allocations are too high. If the current allowable loads for these parameters are discharged, they will likely reverse any improvements and significantly decrease the summer/fall seasonal dissolved oxygen in the hypolimnion (and metalimnion) of the lake.

4. *Introduction – p. 10: fourth paragraph: This paragraph contains a conclusion that the study will require allocations for both BOD and nutrients to mitigate the impact of these pollutants on dissolved oxygen. Due to current shortcomings in the model, and the preliminary nature of analysis of potential remediation alternatives and the outcome of the UAA currently being initiated, this conclusion is premature.*

Response: We believe that the current analysis and discussion should be based on the current water quality standards not on the potential UAA. See our general response #2 and response to comment #7 for further discussion.

5. *Figure 1 – p. 12: This figure appears to omit portions of the St. Joe River watershed and portions of the North and South Forks of the Coeur D'Alene River watersheds.*

Response: The watershed map was designed to show the reader the Spokane River watershed including the headwater source Lake Coeur d'Alene. In the figure heading we will note the portions that are not included.

6. *Sources of Oxygen Consuming Substances and Nutrients – p. 14: Clarifications suggested: Only a portion of the Cheney POTW effluent actually discharges to a tributary of Latah Creek. No discharge occurs during the summer period when evaporation in their wetland effluent polishing season exceeds inflow. Also: Only a portion of the Medical Lake Effluent is discharged to a tributary of Deep Creek during the growing season, consisting of the amount required by Ecology in the City's NPDES permit.*

Response: We agree and changed the text to reflect your recommendation.

7. *Classification and Water Quality Criteria – p. 15: Update to reflect newly adopted water quality criteria for Washington State. Addendum suggested, but probably not extensive, as this report does not contain recommendations for WLAs or LAs. Indication that the new standards contain provision for UAA studies and site specific water quality criteria could be included.*

Response: As noted in response to comment #4, we believe the current water quality standards apply. However, we will add the following paragraph to the Water Quality Criteria section discussion of the report:

```
Ecology has recently revised the surface water quality standards
(effective August 1, 2003).  The class-based system of organizing the
standards was changed to a use-base system.  However, the changes are
not effective for federal Clean Water Act programs (i.e., the TMDL
program) until they are approved by EPA.  It is not anticipated that
the new aquatic life dissolved oxygen criteria will change the
discussion presented in this document.  However, if site-specific
criteria are developed or uses changed under a use attainability
analysis in future rule changes, then these actions may change the
interpretation of the data and modeling results presented.
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8. *Project Goals – p. 16: The City of Spokane has now completed and had approved its Facilities Plan for upgrading and increasing the capacity of the Spokane Advanced Wastewater Treatment Plant. Implementation of the plan is underway. In addition Spokane County has completed and had approved its Facilities Plan for serving larger populations within its service area by construction of a new wastewater treatment facility serving the Spokane Valley area that would include a new discharge to the Spokane river. Also, the Liberty Lake Sewer and Water District has completed and had approved a Facilities Plan for upgrading and increasing the capacity of its treatment plant serving the Liberty Lake area. Implementation of that plan has begun.*

Response: The project began in 1999 and the project goals reflect the expectation and understanding of the project at that time. Given the planning and upgrades that have already occurred, it is important that Ecology complete the TMDL project and set appropriate limits for the discharge of pollutants to protect the Spokane River system.

9. *Hydrology – Spokane River and Major Tributaries: - p. 21 discussion: A review of Spokane river flows conducted in about 1995 found intermediate-term (1-3 month summer season) precipitation in the upper parts of the Spokane river drainage area (St Maries and Wallace) showed decreasing trend similar to the trend observed in the Spokane River 7-day low flows.*

Response: Okay.

10. *Hydrology – p. 19: The first paragraph indicates differences in flows at the various gauging stations. It should be better emphasized that the average flows discussed were obtained for different periods of records. If there are long term hydrologic cycles that affect the river flow (see previous comment) these values are not directly comparable.*

Response: The data statistics and discussion presented is a simple summary of river flow based on historical USGS gauging data and is consistent with how others would summarize flow data for any basin. The text discusses the differences between gauging points to provide the reader information about the changes from one gauge to the next. Although the downward trend of low flows in the Spokane River is significant, there is no significant trend for the annual flows that would make comparing the relative differences between gauging stations based on mean flows unacceptable. However, we would agree that the statistically based selection of low flow recurrence intervals is compromised and the long-term data record should not be used to set low flow statistics, especially for the data prior to 1968 which was used to calculate flow statistics for the phosphorus TMDL.

11. *Figures 5 and 6 – p. 22: The trend line in Figure 6 may be misleading. Figure 5 clearly shows a trend through about 1950, then an increase to a more stable average after the mid '60s. This is when the irrigation diversion from the river at Post Falls was discontinued. Examination of Figure 6 likewise shows a decreasing trend through about 1950, after which the low flows appear to be more stable, at least decreasing at a lower rate. The net increase at Post Falls is not reflected at Spokane since the irrigation water previously diverted from the river was replaced with groundwater withdrawals in the area just upstream of the State line.*

Response: In the description of Figure 6 on page 21, it is noted that the trend was also significant for the period 1968-2001. Although the text states that a "more comprehensive hydrologic analysis" should be conducted, it appears that the low flows are getting lower. Given 2003 will be another low flow year that will likely be similar to 2001, five of the six lowest seven-day low flows on record will have occurred since 1989.

12. *p. 30, ¶ 4: TP range shown from 0.010 to 0.126 mg/L. SRP range shown from 0.003 to 0.016 mg/L. PO4 values in input files for model following JDAY 72 ranged from 0.0030 to 0.0080. Apparently SRP values were used for model. See discussion under Model Calibration report.*

Response: See response to your comment number 1-2 on the model calibration report. All boundary files contain input values for SRP. It should be noted that the summary statistics for the variables reported were for the period 1990-2002 and are not directly comparable to statistics or values for the calibration data sets.

13. *p. 32, ¶ following Table 6: should reference Table 6, not Table 5. Discussion should indicate that other treatment plants upstream have reduced phosphorus loads during that period (Coeur d Alene, Post Falls, Inland Empire Paper Co.) and two discharges have been eliminated (Spokane Industrial Park, Millwood). In addition, wastewater flows from Coeur d Alene and Post Falls have increased, and the Liberty Lake Sewer and Water District and Hayden treatment plants were brought on line. Actually it appears from the Table that loads have increased substantially, unless numbers are wrong.*

Response: I corrected the table number reference and added text describing loading changes. Table six shows that the metric tons of TP have been reduced in the river.

14. *Water Quality – Spokane River, Latah Creek, and Little Spokane River – p. 33: bottom of first paragraph – decimal points in listed total phosphorus concentrations are mixed up.*

Response: I corrected the decimal point error.

15. *Water Quality – Spokane River, Latah Creek, and Little Spokane River – p. 33: center of paragraph shows mix of mg/L and ug/L units. Bottom of second paragraph: implies uptake of TP by periphyton is a TP sink during August, and dilution from groundwater inflow also acts to reduce TP concentration. Based on mass balance, what is the relative contribution of each to the observed decrease in TP concentration?*

Response: I corrected all to be mg/L. I'm not sure why you are asking in the second part of the comment? If you are interested in mass balance estimates, you have access to all of the project data and can perform the calculations.

16. *Water Quality – Spokane River, Latah Creek, and Little Spokane River – p. 36: bottom of first paragraph: States sampling times during 2001 not recorded. Times may be available for Ecology if sampling notes are reviewed.*

Response: It is our understanding that many sample times were not recorded.

17. *p. 36, last ¶: The operation of the Post Falls dam is complex, with variations in flow diurnally. The quality of water from the Spokane arm of Lake Coeur d Alene varies diurnally too. It seems that a conclusion that all diurnal changes in dissolved oxygen concentrations and pH are a result of periphyton is unwarranted without better delineation of the various potential causes.*

Response: The text states that periphyton growth "can increase diurnal changes in dissolved oxygen and pH." We believe the text is accurate. We also believe it is accurate to assume that the dominate effect on diurnal dissolved oxygen concentrations in the Spokane River during the summer is from periphyton growth and respiration.

18. *Water Quality – Spokane River, Latah Creek, and Little Spokane River – p. 37: middle of paragraph: "The main sources of phosphorus loading to the River during the summer growing season were found to be the point source discharges." Based on Table 6 on p. 32, can it now be extrapolated that point sources are no longer the "main sources" of TP to Lake Spokane?*

Response: The sources of phosphorus loading to the river during the summer are mainly the point sources. Table 6 shows that the city of Spokane AWTP in 1991 contributed twice as much as the Little Spokane River and Latah Creek combined. (It appears you are miss-interpreting the data presented in Table 6.) Plus, given that Latah Creek flows approach zero during the summer, the only continuous surface sources of P to the Spokane River in Washington (and Idaho) upstream of Lake Spokane are the point sources.

19. *Water Quality – Spokane River, Latah Creek, and Little Spokane River – p. 38: first paragraph: Wording in this paragraph implies that reducing the magnitude of diurnal DO fluctuations will improve (increase) diurnal minimum dissolved oxygen concentrations. This may not be true as graphs of model output on p. 91 – 96 indicate that reduction of loads lowers not only the diurnal fluctuation, but also lowers the centroid of the data to where the minimum concentrations appear to be about the same. The wording could contribute to unrealistic expectations for minimum diurnal DO if efforts are made to reduce the magnitude of the diurnal fluctuations.*

Response: The criterion is based on the daily minimum not the daily average. Reducing periphyton growth will increase the minimum dissolved oxygen in the river because there will be less biological respiration.

20. *p. 39, Table 7: TP and SRP values are presented for ground water. It is uncertain how this data was used to generate the branch input water quality in the calibrated model.*

Table 7. Page 39				
	TP, mg/l		*SRP, mg/l*	
	Mean	*Range*	*Mean*	*Range*

Sullivan Rd well 1999	*0,015*	*0,004-0.026*	*0.006*	*0.005-0.007*
Sullivan Rd well 2001	*0.014*	*0.005-0.033*	*0.009*	*0.002-0.033*
Downstream to Upper Falls Dam – 2001	*0.014*	*0.005-0.061*	*0.009*	*0.003-0.032*
Values in Calibrated Model Input Files – (File cdt-br_.npt)				
Branches 1-4	*State Line to Islands Footbridge*			*0.006*
Branch 5	*Footbridge to Upriver Dam*			*0.0226*
Branch 6	*Upriver Dam to Green Street*			*0.0074*
Branches 7-11	*Green Street to Nine Mile Dam*			*0.00897*
Branch 12	*Nine Mile Dam to Long Lake Dam*			*0.025*

The rational for establishing the input file numbers is not evident. It appears that SRP values rather than TP values were used in the input files. See discussion above under the Model Calibration Report comments.

Response: See response to your comment #1-2 on the model calibration reports for additional response to this comment. SRP was input for groundwater (i.e., we did not use TP for groundwater inputs). PSU reviewed the groundwater data and tried to match well data with branch groundwater inputs for SRP which accounts for the difference in branch inputs. PSU discusses this in the calibration report. We believe that using SRP values best represents the phosphorus that might enter the river from groundwater. Unlike the surface water boundaries, the non-soluble fraction of P is not represented in the model because our assumption was that only the soluble fraction would be mobile and move through the aquifer to the river, and/or only the soluble fraction would mix with the water column. The oxic conditions found in the aquifer would be expected to favor absorption and retention of phosphorus. In addition, materials found in bed and bank sediments have strong affinities for phosphorus and oxic conditions in the Spokane River hyporheic zone and would also favor retention of phosphorus from groundwater entering the river.

21. *Point Source Discharges – p. 40: NPDES Permits do not include wastewater flow under the effluent limitations, Section S1. Rather the design flow is listed in Section S4, A. Design Loading.*

Response: Changed the text to state that the flows listed are design flows and not permit limits.

22. *Point Source Discharges – p. 42: First paragraph below Table 8: reference to Table 8 should be to Table 9.*

Response: Corrected Table reference.

23. *p. 46, ¶ 1: The studies referenced concluded that P was responsible for low DO in Long Lake hypolimnion. There were other studies that concluded that BOD input had a significant role as well.*

Response: Agree, and changed sentence to read "….identified that phosphorus and BOD loading…."

24. *Comments on Total Phosphorus TMDL – p. 63: This summary has the appearance of putting forth new conclusions, that to date may be difficult to substantiate. The discussion appears to omit portions of the previous findings, for example the amount of public input in determining lake criterion and beneficial uses. The public involvement was briefly reference earlier, but lack of documentation does not necessarily mean that public involvement was inadequate.*

The summary includes examples of missing target criteria (dependent variables), but does not note corresponding TP loading or TP concentration (controllable variable) in all of these cases. The referenced apparent violation of the definition for mesotrophic conditions applies to portions of the reservoir, while the objectives of the TMDL (based in part on public input) were to maintain mesotrophic conditions on the average during low flow years (1 year in 10 low flow conditions).

The discussion regarding previous decisions affecting selection of criteria focuses on trophic status, probabilities of missing target criteria, and margin of safety (lack of), but omits discussion of "reasonable goals" and economically achievable targets. While the discussion is useful in pointing out how ecology would approach water quality goals differently in today's TMDL process, it fails to compare how today's approach to economic evaluation differs from the way it was considered for the phosphorus TMDL.

The summary on page 66 is in the form of a conclusion, and the conclusion appears to be that the TMDL being violated. The conclusion is based on an interpretation that was not placed in the TMDL, but rather an updated, and unofficial, and un-adopted, interpretation of what "should-have-been" in the eyes of the author.

Response: The summary does make conclusions about the existing phosphorus TMDL based on reviewing available documentation that discussed the studies, assumptions, and process that lead to the recommended TMDL. However, the summary does not conclude "that the TMDL is being violated," but rather that the TMDL is inadequate to protect water quality in the lake. The conclusions are Ecology's conclusions not just the author's. Patmont et al., (1987) was the only document that discussed any economic issues. The only text in the document about economic issues was on page 55 of the report:

> "In consideration of the potential environmental benefits and additional treatment costs associated with alternative design flow conditions, Ecology determined that the proposed 25 ug/L euphotic zone TP standard should be applied to the median flow event…."

Your comment is incorrect in that it says the existing TP TMDL was established "*to maintain mesotrophic conditions on the average during low flow years (1 year in 10 low flow conditions).*" **The TMDL actually "assumed" that mesotrophic conditions would be maintained on average during a median flow year without a margin-of-safety (MOS).** It is our conclusion that the decision to use a median flow event to define allowable phosphorus loading without a MOS cannot be justified, either based on today's TMDL standards or those known when the decision was made. If the one-in-ten year low flow was used instead of the median flow, then the loading that existed when the TMDL was determined would have needed to be significantly reduced. However, by using a median flow event with no MOS, no reductions were needed to meet the TMDL.

25. *CE-QUAL-W2 Model Selection, Calibration, and Uncertainty – p. 70, ¶ 4: The reference to water quality data and algal productivity underestimation may be a result of the omission of a significant portion of the "limiting nutrient" from the model – the non-SRP TP load.*

Mention at this point regarding the added algal condition included in Appendix D may be appropriate. Also how the model will be maintained in an ongoing, but coordinated, periodic calibration update process.

Response: See General Response #1.

26. *p. 70, ¶ 5: This discussion, and the resulting model calibration discussion, should be modified reflecting the correction of the mis-reported chl* ***a*** *data from CAS.*

Response: We received the final corrected CAS chlorophyll *a* results in July 2003. Given the need to update the calibration comparisons and possibly make changes to the model discussed in General Response #4, we will not be including any data or model changes in the final model development documents; instead, we will prepare a short technical addendum discussing any model changes and the basis for those changes later this year.

27. *p. 71, ¶ 1: The comment that "the rates and constants act collectively and were set specifically to provide the best calibration of the model" appears to be inaccurate based on our understanding of the calibration process. Our understanding is that the rates and constants were mostly assigned default values, with any "calibration" done with only a portion of the variables (ref. Model Calibration Report, Table 16).*

Response: The rates and constants were set to provide the best calibration of the model. The fact that only a few rates/constants were changed from default values suggest that the Spokane model can be modeled using values that have been found to represent many other systems.

28. *CE-QUAL-W2 Model Selection, Calibration, and Uncertainty - p. 71, Fourth paragraph: While the model has been made available, the resources to run the model and assess the results in a timely manner were not included. Therefore, during this review period it has been impossible to conduct further analysis of model uncertainty or sensitivity for variables or parameters of specific interest. Consequently few, if any specific analyses can be reported at this time. Interested parties will continue to address these issues but it will take time. Ecology is asked to recognize that interested parties in most cases are subject to resource and time limitations similar to Ecology's, and therefore allow additional time for the parties to conduct their own analysis of model uncertainty or sensitivity.*

Response: As noted in General Response #1, we will be proposing additional changes to the model over the next few months and expect that you and other interested parties will review those changes and the model performance in general.

29. *CE-QUAL-W2 Model Selection, Calibration, and Uncertainty - p. 71-72. Ecology correctly points out that margin of safety needs to take into account the lack of knowledge concerning the relationship between effluent limitations and water quality. It should also be pointed out here that there does not need to be additional MOS added for variability in effluent quality parameters because the variability of effluent parameters is well established and predictable, and is included, or should be included, in the NPDES permits for the discharges.*

Response: At this time, Ecology is not proposing any specific MOS beyond using the approximate lower 10^{th} percentile flow conditions present during 2001. However, as noted in the report, an additional MOS may need to be considered based on those items listed in the Margin of Safety section. During the public process, the public needs to determine what, if any, additional MOS is needed.

30. *Application of Water Quality Criteria – p. 73, first paragraph. It should be noted that the 0.2 mg/l degradation due to human impacts allowed by Ecology in other TMDLs is applicable only when the dissolved oxygen concentration is below the numeric criteria of 8.0 mg/l.*

Response: Corrected the text to read "*….when the dissolved oxygen concentration is below (or near) the criteria.*"

31. *Application of Water Quality Criteria – p.73, last paragraph. The proposed application of Lake Class water quality criteria of 0.2 mg/l deficit from reference conditions estimated by CE-QUAL-W2 with no point source discharges or nonpoint source pollutants ignores the fact that the CE-QUAL-W2 model is unable to predict dissolved oxygen to within 0.2 mg/l for existing conditions.*

Response: See response to your comment #4 on the model calibration report.

32. *Application of Water Quality Criteria – p.74. The proposed application of Class A water quality criteria of 0.2 mg/l deficit from reference conditions estimated by CE-QUAL-W2 with no point source discharges or nonpoint source pollutants ignores the fact that the*

CE-QUAL-W2 model is unable to predict dissolved oxygen to within 0.2 mg/l for existing conditions.

Response: See response to your comment #4 on the model calibration report.

33. *Application of Water Quality Criteria – p.74. The proposed application of Class A water quality criteria of 8.0 mg/l by relying on the CE-QUAL-W2 model output puts too much confidence in the capability of the model to accurately predict dissolved oxygen to an exact numeric criteria. As discussed in the Ecology report (page 69), the model provides a good* ***approximation*** *of major forcing processes and features of the system that affect water quality such as the hydrodynamics of Lake Spokane, pools associated with the dams, periphyton growth, and pollutant loading. It is evident from the data comparisons to model output, however, (which is not included or summarized in this report) that the model does not accurately predict when actual violations in water quality criteria occur. It is suggested that the model be used not to predict violations in water quality, but to be used as a tool predict the vector of water quality response (and perhaps the relative probability of water quality violations) in response to a variety of pollutant reduction strategies. This approach would also allow more definitive input from the community when put in terms of level of risk versus incremental additional costs for different implementation strategies.*

Response: Ecology believes that current water quality regulations require that pollutant loading sources bear the burden of that uncertainty and not the environment. (See discussion in the report on Model Uncertainty pages 71-72.) However, as noted in General Response #4 and the response to your comment #4 on the model calibration report, we believe the model's predictive capabilities of dissolved oxygen in the river can be improved by making minor changes (within the range of measured values) to inputs from groundwater.

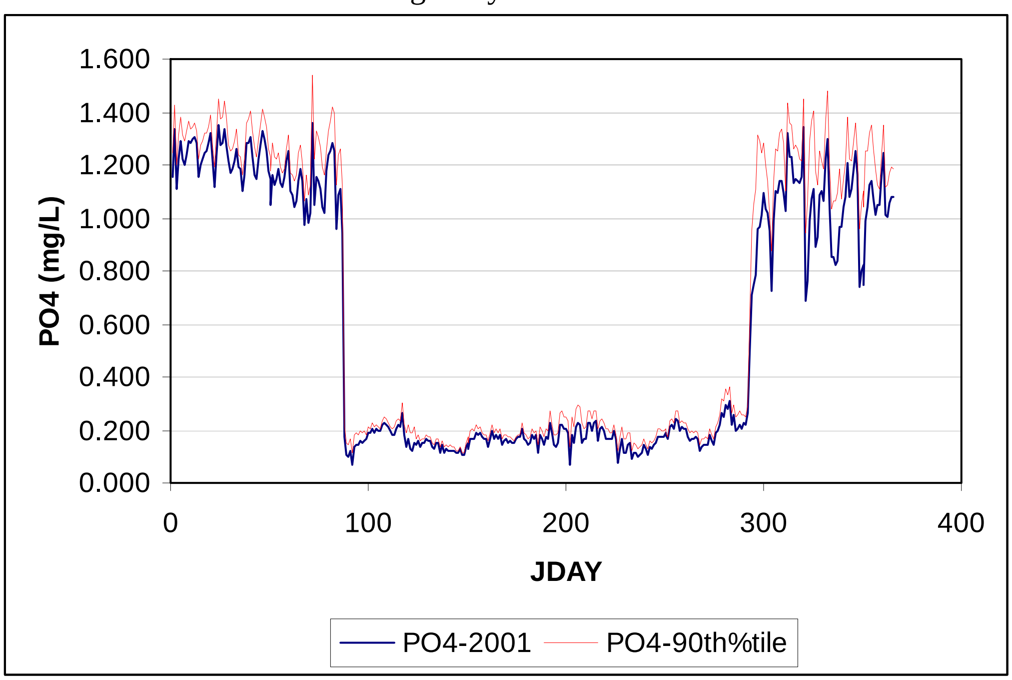
34. *Design Conditions – p. 77, ¶ 1: Reference to Table 15 should be to Table 16.*

Response: Corrected reference to Table 16.

35. *p. 78, Item 5., PERMIT: The rational of how 10^{th} percentile for soluble reactive phosphorus, ammonia and nitrate was selected for the "PERMIT" scenario is not presented. It is not clear how the 10^{th} percentile for these constituents compares to the maximum monthly permit limits (consistent with how BOD_5 is used in the scenario). It is not explained which data set was used to find upper 10^{th} percentile (June-October or all year) – and what is sensitivity when doing it the other way. This is a significant issue because preliminary modeling results appear to indicate that nutrient loading has a much stronger influence on dissolved oxygen concentrations than BOD_5. In addition, recalibration of the model using TP instead of SRP may have an affect on the outcome of this exercise.*

Response: We modified the text to note that we used the 2001 model input data files provided by PSU to calculate the PERMIT scenario conditions. The graph below is an example that shows the PO4 concentration data listed in the model input file that represents the city of Spokane AWTP discharge during 2001 and the estimated upper 10th percentile used for the *permit* condition. (Again, I believe your company provided the data to PSU.) We estimated the upper 10th percentile using the mean and standard deviation for the data listed in the model input file for 2001 conditions. In the example shown in the graph below, the running weekly average and standard deviation was used as follows for the city of Spokane AWTP daily record: calculated the weekly average P04 concentration, estimated the upper 10th percentile value, and then added the difference between the estimated upper 10th percentile and the mean to the daily input data. However, since the other point sources do not have daily estimates, the upper 10th percentile was based on the whole record (i.e., not the weekly input data used to estimate the city of Spokane AWTP effluent characteristics). For CBODu, again using the city of Spokane AWTP as an example, the daily model input value (calculated from BOD5 data using the estimated CBODu:BOD5 multiplier of 3.25 determined from the CBODu analytical test, e.g., BOD5 on 1/1/2001 was 12 mg/L and CBODU was estimated to be 39 mg/L) was used to determine a monthly average CBODu value. The daily CBODu input data were adjusted using the monthly average and daily value such that the *permit* scenario monthly average would equal 97.5 mg/L (i.e., 30 mg/L*3.25).

Over the last few years, Ecology has held a number of public meetings to discuss the Spokane study and modeling efforts. During those meetings, we have emphasized that Ecology would accept alternative estimates for input concentrations if they can be shown to better represent the system (i.e., upstream boundary, point sources, and tributary parameter concentrations). If you or your clients believe the input estimates for SRP and CBODu are not correct, can be improved, or you just want to make sure that they are correct to your satisfaction, please provide us with your estimates and an explanation for setting alternative values for any input parameters and we can include them in our modeling analysis.



36. *Margin of Safety – p.78: The need for an explicit margin of safety in addition to the 2001 design year is not apparent. The selection of design year 2001 inherently uses matched data for conditions other than low flow (temperature, groundwater dissolved oxygen, etc.), as explained earlier in this report. As such, it is a better approximation of critical design conditions than would be achieved through the use of an arbitrary additional explicit margin of safety. In fact, the margin of safety inherent in selecting the 2001 design year already far exceeds what would be comparable MOS if the 7Q10 were used. It appears from the presentation in the Ecology report (p. 76) that not only did 2001 see the occurrence of a 7Q10 (actually closer to 7Q20) flow, but also 10-year (or more) recurrence interval low flows for longer episodes (14-day, 30-day, etc., and for the entire season.) These continual low-flow episodes effectively compounds the margin of safety for pollutants and conditions which effect DO both seasonally (nutrients) and short-term (temperature, effluent DO, ammonia, BOD).*

A determination of the 10 years in 100 condition for all of the applicable parameters, and combination to determine an overall 10 years in 100 conditions for all parameters combined would be a rigorous exercise. Some discussion would appear to be appropriate as to how this would be established in the light of the comments regarding MOS and the Ecology tendency toward adding margins of safety on top of margins of safety.

Response: As stated in response to comment #29, we are not proposing an additional MOS other than applying 2001 conditions. However, any additional or alternative MOS considerations should be decided during the TMDL public process. Our analysis, presented in the critical conditions section of the report, shows that 2001 is the best year to represent critical conditions because the spring and summer flows were very close to the lower 10^{th} percentile.

37. *Margin of Safety – p.78: Last sentence: Using only post-1968 data actually increases MOS rather than decreases it. A downward trend in 7-day low flows appears to disappear, or at least ameliorate after that year. Observed trends in 7-day low flows may or may not be long term, which is why normally all available, reliable data is used instead of only the most recent several years. The premise being that climate fluctuations are short-term (some climate fluctuations have been observed to oscillate on decadal to thirty year cycles), and more data will allow better approximations of longer-term, climate-related data. See comment above regarding presumed reasons for the observed trend in 7-day low flows.*

Response: The Spokane River flow record is one of the longest records available in Washington State. See response to comment #11. The analysis we presented shows a decreasing trend in low flows over the last 100 years. Until a more comprehensive analysis of the river flow, groundwater flow, precipitation, and land-use change data is conducted that might explain the trend better or we collect 20-30 more years of data, it would seem reasonable to assume that there is a potential for low flows to continue to decrease or stabilize at very low levels such that it should be considered when discussing setting a MOS for loading pollutants to the river.

38. *Lake Results – p.79: This discussion, along with discussion in the section recounting the current phosphorus TMDL, indicates that the most significant contributions to DO depletion may be from non-point sources. There has already made significant progress, at great expense, toward reducing their contributions to DO depletion in Lake Spokane by point source dischargers. It appears that the next logical course of action may be to attempt to reduce non-point source contribution to the same order of magnitude reduction as has been achieved by the point source dischargers. This example model scenario was not included.*

Response: Our current modeling assessment shows that the greatest potential impact to dissolved oxygen depletion in Lake Spokane would be if the point sources discharged their allowable permitted BOD5 loading. In addition, an increase in 2001 total phosphorus loading of about 35% would be allowed under the current phosphorus TMD, although we agree that nonpoint sources need to be addressed. Point source discharges also need to be addressed because under the current water quality criteria there will be little or no assimilative capacity in the Spokane River system for BOD or phosphorus. As you point out in your comment #8, the facilities that discharge to the river have been proceeding with plans to increase discharge to the river, which demonstrates the need to implement TMDLs that protect dissolved oxygen concentrations in the river and lake.

39. *Lake Results – p.81: If the model appears to be under-estimating algal productivity, it may be a result of under inclusion of available phosphorus compounds. The organics yield rate of the model is double literature values and the half-saturation constant is 1/3 literature values, which place the "productivity" per unit of SRP at extreme high levels. It is probable that use of TP instead of SRP will allow "productivity" per unit of P to be within reasonable values. The low productivity referenced in the model may have been a function of the model calibrator's reluctance to extend the yield and rate parameters further than provided in the model report.*

It is somewhat subjective to conclude that the results therefore represent "minimum impacts" of pollutants, because the model also appears to underestimate the dissolved oxygen in the euphotic zone in Lake Spokane (due to respiration). If better simulation of algal productivity could lead to management strategies aimed at reducing algal blooms, it is expected that would be well received, but it may produce mixed results regarding achievement of the dissolved oxygen criteria. (Appendix D is incomplete in not comparing the results between the alternate algal productivity parameters and the current model algal productivity parameters.)

Response: See response to comment #1 on the 2001 model calibration report by PSU. We believe the model does represent the minimum impacts on dissolved oxygen because if the model better predicted algal productivity, not only would epilimnetic dissolved oxygen be higher but internal loading of organic material and associated decay in the meta- and hypolimnion would also be higher. Therefore, the growing season/stratification period impact on dissolved oxygen would be predicted to be greater as shown in the two algal group example in Appendix D.

Appendix D was presented as an example of the model with more productivity, not a complete calibration equal to the current 2001 calibration. As such, it would not be appropriate to make 1:1 comparisons using two algal groups with the current model until complete calibration checks are completed (we expect to make recommendations for model changes before the end of 2003). However, the results with the second algal group and more productivity show that there is a predicted increase impact on dissolved oxygen from the point sources, especially in the interflow zone of the lake in the area represented by lake station LL3. The profile data collected at LL3 during the end of August 2001 show a distinct metalimnetic (or interflow zone) dissolved oxygen minimum that is better reproduced with two algal groups.

40. *Lake Results – p.82 - 88: None of the graphs include sample data (when and where available). Although included in the calibration report, the scope of this report warrants that the "calibration" information be included.*

Response: The data/model comparisons are presented in the calibration reports. The goal in the subject report was to show scenario results that represented different times and locations that would be considered *critical* areas with respect to the model predictions and water quality criteria in the river and lake. The calibration and model set-up reports for the 1991/2000 and 2001 models are a combined 786 pages of descriptive information including model/data comparisons. I believe this amount of documentation plus the 130+ pages of the Ecology report meets any requirement based on the scope of the project.

41. *Lake Results – p.82 - 88: No profiles are presented for the upper portions of the reservoir, where the model predictions are quite different in terms of dissolved oxygen concentration. The omission of the results for this portion of the reservoir distorts the overall picture of the effects of the dischargers on the dissolved oxygen concentration in Lake Spokane. This is especially critical considering the degree of discussion that was included in this report regarding the inadequacy of the Phosphorus TMDL in the upper lake areas.*

Response: No profiles were presented for the upper end of the lake because the dissolved oxygen concentrations were both measured and predicted by the model to be higher than the criterion. Higher productivity at the upper end of the lake actually leads to super-saturation of dissolved oxygen. The impact of high productivity in the upper end of the lake does depress the dissolved oxygen in the interflow zone and deeper parts of the lake downstream (i.e., organic matter produced in the upper end of the lake is transported downstream). See response to comment 39 for more discussion.

Please also note that most of our discussion presented in the report about the existing phosphorus TMDL was focused on the methods for determining the allowable loading to protect the *aesthetic* quality of the upper end of the lake based on the productivity *criteria* defined in the TMDL analysis documents. A minimum mean hypolimnetic dissolved oxygen criterion for the whole lake was defined to be >4.0 mg/L, but predicted to be 3.3 ± 1.3 mg/L for the median flow and 2.4 ± 1.0 for a one-in-ten year flow based on the adopted euphotic zone total phosphorus criterion of 25 ug/L. The modeling results presented in the phosphorus TMDL development document showed that dissolved oxygen would not meet even the lower quality criterion they proposed for maintaining a mesotrophic lake. In addition, the modeling showed that the mean and peak chlorophyll *a* criteria would also not be met with a high level of statistical confidence.

Environmental Protection Agency Document

Including Ecology Responses

John Yearsley, EPA Environmental Scientist

Review of Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen

I have reviewed the documents, " Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen" prepared by Bob Cusimano of the Washington State Department of Ecology, and the "Upper Spokane River Model: Model Calibration, 2001" by Berger et al. My overall impression of the collected work is that it meets or exceeds levels of technical effort generally given to studies of this kind. The collected works use accepted methods for data collection and analysis and follow standard practice in applying the data to the development of a mathematical model for use in supporting decisions for water quality management. The mathematical modeling framework is based on the two-dimensional model, CEQUAL-W2 version 3.1 The methodology has been peer-reviewed and widely accepted to support quality planning and decision-making. All of the work is well documented, results of monitoring program are provided and the files required to run the model are available in electronic form.

I believe the collected work meets accepted standards for environmental regulatory modeling. However, there are also some areas in which I would recommend additional discussion or where I would interpret results or policies for applying the model differently than did DOE or its contractors.

Upper Spokane River Model: Model Calibration, 2001, Berger, C.J., R.L. Annear, Jr., S.A. Wells, Technical Report EWR-1-03, Portland State University

1. *Page 67. The fixed reaeration rate of 0.05 days^{-} seems low for a riverine segment. Discussion of the way in which this parameter was estimated could be expanded.*

Response: (The discussion of the fixed reaeration rate is on page 42 in the document.) We agree that the 0.05 day^{-1} is at the low end of the range of measured reaeration coefficients. Choosing this value was based on matching supersaturation conditions of periphyton downstream of WWTP discharges. In this case, choosing a low reaeration coefficient allowed reasonable model-data agreement over several years of data. Because of this, we kept the lower reaeration value realizing that it could be a result of the WWTPs discharging surfactants or other films that may interfere with surface gas transfer. Also, we realize that this could be a result of not accurately simulating periphyton productivity. But since this seemed to work for several time periods, we kept this in the model. This area could be investigated further to understand the real reason for this effect.

2. *The basis for setting the various values of sediment oxygen demand and their impact on the dissolved oxygen (DO) simulations should be discussed more thoroughly. The numbers themselves are of the order of those observed in relatively clean sediments. (Bowie et al, 1985).*

Response: Setting SOD rates are truly a back calculation of the amount of oxygen lost in the hypolimnion between oxygen profile dates. In the model, this rate was used to achieve reasonable model-data agreement in the hypolimnion. As processes in the system are better understood (such as particulate organic matter inflows into Lake Spokane), this value can be adjusted to reflect other sources/sinks that become better known.

3. *Figures 186 and 187 have incorrect labels.*

Response: They are incorrect.

4. *Page 231. The authors conclude the "model is well suited for evaluating the impacts of management strategies ----". The authors do not say if this conclusion is based on technical analysis (hypothesis testing, for example) or if it is based on policy considerations. DOE's Bob Cusimano's (pp. 68-72) review of model uncertainty concludes (correctly) there are no EPA regulations or guidelines defining a quantitative basis for model acceptance. My own experience has been that a rigorous quantitative basis for model acceptance is not necessary for the application of models for supporting environmental decision-making. Of course, that doesn't mean scientists should not do rigorous analysis. In a review of EPA's success in convincing the courts of the validity of its science, Schroeder and Glicksman (2001) concluded that EPA fails in this regard only "when the Agency provides no evidence at all to support its technical determinations, relies on evidence that conflicts with the stated views of its own experts, employs technical models or methodologies that are obviously ill-suited to assessing the impact of the regulated activity on the environment …..". Similarly, Circuit Judge Gee, in Marathon Oil Company v. EPA (U.S. Court of Appeals, 1987) stated in a case involving EPA science: "Marathon has not made a direct attack (<u>usually futile, of course</u>) on the quality of EPA science.' Assuming the court would respond similarly to the way in which other regulatory agencies like Washington' s DOE apply their science, it probably isn't that difficult to make the case that CEQUAL-W2 can be used for water quality planning purposes. However, I believe the statement, by itself without explanation and by those who have developed the model, is self-serving and invites the question of how the authors reached this conclusion. Furthermore, the same could no doubt be said (and probably was) about all the previous models of phosphorus loading to Lake Spokane.*

Response: This is an important philosophical and technical issue that plagues all model studies. We should probably reword the text to include the following:

“Based on the model-data agreement and errors, we conclude that the model represents the system’s basic hydrodynamics, transport and water quality. Since this tool is currently the best tool for integrating the physics, biology, and chemistry of the Spokane River - Lake Spokane system, we recommend using the model to evaluate management alternatives. We recognize that further research on some of the complexities of the system may be resolved in the future and that this will lead to yet a better model. The model is a living tool that hopefully will be refined over time to provide an integrated basis for evaluating water quality in the Spokane River system.”

Also, we should start developing a list of references of similar studies and similar model-data comparisons where we can say that *our* errors are better than, or similar to, other studies. But again this still does not *prove* a model is *correct*. We need to think more about this issue of what makes a model acceptable to use for management applications.

The key question to ask when attempting to determine if a given model can be successfully used to address management issues is--“Are we confident that the model is correctly representing the most important processes affecting the system upon which we will be basing management strategies?”--before concluding whether or not “a model is well suited for evaluating the impacts of management strategies…”

For example, the impacts of point source discharges on dissolved oxygen in Lake Spokane are immediately impacted by the ability of the model to simulate the transport and decay of point source oxygen demanding materials that enter Lake Spokane from the Spokane River. The subsequent exertion of oxygen demand resulting in reduced DO in Lake Spokane that violates water quality standards is the most important mechanism that must be correctly portrayed in the model in order to address the impacts of point source dischargers in Lake Spokane. If there is strong evidence that the model is accurately portraying this mechanism, then it can be concluded that the model will be useful for addressing management decisions affecting point source discharges into the Spokane River and the *immediate* impacts on DO in Lake Spokane.

From the conductivity simulations, it is clear that the model is accurately portraying the movement of Spokane River water into Lake Spokane. By inference, the model is also accurately portraying the transport of point source discharges into Lake Spokane. Since the model is using laboratory measurements for point source discharge CBOD decay rates, they were thus removed from being a calibration *knob* during the simulation. Therefore, it is clear that the model is immediately useful in addressing the impacts of point source discharges on DO in Lake Spokane, regardless of whether or not all other mechanisms impacting DO in Lake Spokane are accurately represented or even included in the model.

Furthermore, the impacts of whether or not a given model is, or is not, accurately reproducing all observed DO measurements in a system also needs to be taken into account in order to address the effectiveness of a model to address management alternatives. For example, what are the impacts of the lack of a sediment diagenesis model on the model’s ability to address management alternatives in Lake Spokane? In this case, the lack of a sediment diagenesis model would impact the model’s ability to accurately portray the long-term (decades long) impacts of either increasing or decreasing point source discharges.

Thus, the lack of a sediment diagenesis model would result in underestimating the long-term *improvement* in DO in Lake Spokane if point source discharges were decreased, and the long-term *reduction* in DO if point source discharges were increased. However, the lack of a sediment diagenesis model would have no impact on the ability of the model to simulate the immediate impacts of the effects of increasing/decreasing point source discharges into Lake Spokane.

Additionally, the lack of a sediment diagenesis model does not preclude using it to address long-term impacts of point source discharge increases/reductions, but one must be aware that the model represents the best case/worst case scenarios, respectively. Thus, the model is still useful for addressing management strategies regardless of the impact of not accurately portraying sediment phosphorus recycling.

In summary, improving the model's portrayal of certain mechanisms affecting DO or algal productivity will allow the model to address more accurately a wider range of management issues involving DO, but the fact that a given model does not accurately portray all mechanisms impacting DO in the system or all observed DO measurements does not necessarily preclude its use in addressing management issues.

Although we could do a better job explaining how we arrived at the conclusion that the " model is well suited for evaluating the impacts of management strategies…", we do not believe that this is a self-serving statement but rather accurately reflects the usefulness of the model to address management strategies on the Spokane River/Lake Spokane system.

Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen by Bob Cusimano, Washington Department of Ecology (draft)

1. *One of the stated objectives of the project (page 16) was "evaluate and update" the P-attenuation model developed in the previous TMDL effort. I assumed this meant something more than the 1993 work done be Soltero et al. Although the present report discusses the P-attenuation model in some detail, it always seems to be more in the evaluation mode. I could not find a specific place where an attempt had been made to update it. It may be in there somewhere, but it was not obvious to me. Perhaps there could be some additional discussion that would make it more clear how DOE updated the P-attenuation model.*

Response: We have not yet updated the P-attenuation model because of our concerns about how the TMDL was determined (especially because there is no Margin-of-Safety), and also because the application of the current dissolved oxygen criteria using the CEQUALW2 model will require phosphorus (and BOD) reductions from current levels which are already less than the existing phosphorus TMDL. Maybe the P-attenuation could still be used to address aesthetic issues for the lake if the TMDL allocations are based on the one-in-ten year flow instead of the current median flow.

2. *The word "suggest" is used in this report, by my count, nearly 40 times. If the model is truly "suited for evaluating the impacts of management strategies", and I have little doubt that it can be, I would think a stronger word than "suggest" should be used. Another option would be to make the statements in a neutral way and avoid use of the passive voice. For example, on page 24, the statement:*

"Flow data collected by Spokane Community College during 1998 and 1999 at the Plantes Ferry Park Footbridge (RM 84.7) suggest that at low river flows the inflow zone could extend between Barker Rd and the Footbridge (i.e., RM 90.4 – 84.7)"

could be rewritten as:

"Based on flow data collected by the Spokane Community College during ------, we estimate/calculate with specified uncertainty that at low river flows the inflow zone extends --------."

Response: We will review the use of "suggest" in the text and rewrite sentences where appropriate.

3. *Based on an analysis of the chlorophyll a data and previous studies, the DOE report concludes that hydrodynamic effects result in a longitudinal gradient in phytoplankton concentrations. The conceptual model DOE develops from this conclusion is that diatom blooms occur in the early spring in the downstream segment of Lake Spokane and blue-green blooms occur in the upper segments in the late summer and fall. Presumably, the simulations from CEQUAL-W2 support this conclusion since the model simulates hydrodynamics as well as water quality constituents. I would recommend that additional discussion of the model, focusing specifically on this issue, be included. It also has relevancy for the TMDL discussion. The TMDL discussion considers the June-Oct period as critical. However, the data presented in the document (Figure 25 and Table 11) show that levels of chlorophyll a greater than 10 ug/l occurred at the downstream end of the reservoir during the April-June period.*

Response: We will add more discussion about the CEQUALW2 model algal bloom predictions. In order to simulate the overall historical pattern of algal blooms we will need to add algal groups to the current model.

The TMDL discussion and the June-October period was in reference to the current phosphorus TMDL and the critical period that was used to develop the TMDL. We agree that the historical productivity data indicate that April-May was also high but this period was not addressed in the historical documents or the TMDL. Our current modeling includes internal and external loading from the late winter/spring that may influence summer/fall water quality in the lake.

4. *Should the reference to Table 13 on page 64 be a reference to Table 14 instead?*

Response: Corrected the text to refer to Table 14.

5. *I find the discussion of the application of water quality criteria (pp 73-74) troubling primarily due to the potential conflict it has with our interpretation of Washington's water quality standards for the Columbia/Snake water temperature TMDL. In the case of the Columbia/Snake TMDL, we are not proposing to remove any of the dams in the system. The TMDL will, however, provide a framework for assessing the impact of human development and stimulate discussion regarding the best way to meet water quality standards. Building dams and reservoirs change a water body from a freely flowing river to an impounded lake. A good part of the water quality problems in the Spokane River (and the major problem in the Columbia/Snake) system is a result of that. Of course, DOE may not wish to address the Spokane issues in the same way we addressed water temperature standards in Columbia/Snake system. However, there should be some effort to reconcile the two approaches. I don't believe the wording in the present document does*

Response: We will add text to better explain our rational for defining natural conditions with the dams in place.

References

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Schroeder, C. H. and R. L. Glicksman. 2001. *Chevron, State Farm,* and EPA in the Courts of Appeals During the 1990s. Environmental Law Report, News & Analysis. 4-2001. 10371-10412.

United States Court of Appeals, Fifth Circuit. 1987. Marathon Oil Company, Petitioner, v. Environmental Protection Agency, etl., Respondents. No. 86-4739.

This is the end of the response to specific comments. The following attachment is a general response to the Esvelt Environmental Engineering document.

BC:cn
Attachment

Attachment 1

The following is a response to comments 1-(2) through 4 on the *Upper Spokane River Model Calibration* report from Larry Esvelt, Esvelt Environmental Engineering. The response was prepared by Tom Cole, Corps of Engineers, and sent via e-mail to Ken Merrill (ERO) and Bob Cusimano (HQ), Washington State Department of Ecology.

Ken and Bob,

I have reviewed Dr. Esvelt's comments concerning the latest calibration report and have the following comments.

Regarding Dr. Esvelt's TP comments, for mechanistic models that keep track of phosphorus in its various forms and transformations thereof, TP should not and is not used as a state variable for modeling algal/nutrient interactions. TP in the real world consists of inorganic P and organic P, the latter of which is composed of detritus and living algal cells and this is what is measured when determining TP. This is also how it is portrayed in the model. For the Spokane River, organic P is included in CBODu for all time varying boundary condition. The concentrations of labile and refractory DOM and POM are set to zero at the model boundaries to ensure that the model does not "double dip" regarding organic matter and associated nutrients at the boundaries. Labile and refractory DOM and POM are included in the model as state variables making up TP that are involved in organic matter recycling by autochthonous algal/epiphyton production.

Thus, all components of TP are included in the model, so Dr. Esvelt's statements that only SRP is included in the model is erroneous and indicates a fundamental lack of knowledge with regards to mechanistic water quality modeling in general and CE-QUAL-W2 in particular. TP is used as an independent variable only in regression models (e.g., Vollenweider types) between TP and DO or TP and chl *a* concentrations because of an inability to mechanistically portray algal/nutrient interactions in regression models including kinetic transformations. As stated previously, TP should not be and is therefore not used as the independent variable in mechanistic modeling of algal/nutrient interactions. If TP were used as the state variable, then many mechanisms that occur in the real world would have to be eliminated from the model, opening it up to numerous valid criticisms by knowledgeable mechanistic modelers.

For example, autochthonously produced organic P and subsequent mineralization to bioavailable P is included in the model through decay of labile and refractory DOM and POM. Thus, organic P can eventually be made available for algal growth, but only after it has been transported in the model and subsequently decayed. Using TP as the independent variable would completely miss the transport of P that is initially unavailable for algal growth (and may never be available if advected out of the system, which certainly occurs and is an important variable in most regression based water quality models), but may eventually become available at another time and place.

Additionally, CBOD decay and mineralization to bioavailable P is also included in the model. So, all components of TP are included in the model formulations and can ultimately impact algal production at the appropriate time and place if transported and decayed properly. Indeed, TP in the model was calculated, output, and compared to observed data in the report. It should have been clear from this comparison that the model included all components of TP. This information is also included in the User's Manual and an equation is actually given for calculating and outputting TP based on model state variables.

Concerning the algal phosphorus half-saturation coefficient being one-third of the typical value, the report is in error for this and several other "typical" calibration parameters. A half-saturation value of 0.003 is the default value and can be verified as a good, default value by other, knowledgeable reviewers. The "typical" value of 0.009 in the report is in error and is indeed quite high. Other values indicated as typical values that are erroneously stated in the report include the stoichiometric equivalent between organic matter and phosphorus, phosphorus partitioning coefficient, extinction due to suspended solids, and algal/periphyton mortality, excretion, and respiration rates.

The values used during calibration for all of these coefficients are actually recommended model defaults or "typical" values. Other knowledgeable reviewers can verify this. Additionally, the temperature rate multipliers for algae are also default values for diatoms, which are the dominant algal group during the spring algal bloom. The "typical" values listed for these coefficients are representative of greens, which are not a dominant component of the spring bloom. The fact that the model could not reproduce the fall cyanobacteria bloom using one algal compartment is strong evidence that indiscriminate turning of calibration "knobs" cannot accomplish an acceptable curve-fit of calibration data, which in turn is strong evidence that the model is reproducing observed data for the right reasons. The following are the only calibration parameters that deviate from their recommended default values.

algal light saturation	default = 75,	value = 40
algal/periphyton growth rate	default = 2,	value = 1.5
labile DOM decay rate	default = 0.12,	value = 0.08
ammonium decay rate	default 0.1,	value = 0.4

None of these calibrated values deviate significantly from the range of measured values determined in a number of literature studies as reported in the User's Manual. All of the other > 70 calibration coefficients were set to default values (those which have default values - some, like sediment temperatures, don't have default values but require measured data instead). The fact that only four calibration parameters were adjusted from default values is, in and of itself, quite remarkable for mechanistic water quality modeling and gives powerful evidence that the model is reproducing very complex real world behavior for the right reasons and, as a result, is quite appropriate for use on the Spokane River TMDL.

I am at a complete loss as to how the following statement could be made by an objective reviewer - "Conductivity correlations between the calibrated model and actual data appear to be poor." With few exceptions, the plots of conductivity in Lake Spokane show an amazing degree of agreement between model predictions and observed data given the uncertainty of the magnitude of groundwater flows, frequency and accuracy of conductivity measurements, location and extent of groundwater inflow, accurate inflow temperature predictions along with accurate water column temperature predictions, and the requirement to accurately simulate the complex hydrodynamics in Lake Spokane. The close match of conductivity profiles for the majority of observed dates at all stations significantly reinforces the conclusion that inflow and water column temperatures and Lake Spokane hydrodynamics are accurately portrayed by the model.

Regarding Dr. Esvelt's DO comments, the fact that the model does not closely agree with every piece of observed DO data does not obviate its use for the Spokane River TMDL. If this were the case, mechanistic models could never be used for water resource management. It is impossible to exactly reproduce DO in Lake Spokane with any model or in any complicated system for that matter. First, there are measurement errors (as was made clear by the 2001 data), there are discrepancies generated between observed and computed values because they are not compared at the exact time (because there is no recorded time for observed data), there are errors generated by inaccurate meteorological data that impact hydrodynamics that in turn impact water quality, there are errors generated because zooplankton and macrophytes are not included in the simulation, there are data gaps in the boundary inflow temperatures and concentrations, and there are other sources of error as a result of hydrodynamic and water quality model assumptions and numerical solution techniques. All mechanistic models make assumptions that result in errors between computed and observed data. However, this fact does not invalidate their usefulness when applied appropriately by a knowledgeable modeler if all of these potential problems are taken into account when drawing conclusions based on model results.

Additionally, it is important to point out that the model/data comparisons included in the report are the most restrictive way of making these comparisons and, by design, are included so as to expose reviewers to not only model strengths, but also model weaknesses in order to determine what the model can and cannot be used for. In contrast to the model/data comparisons in the report in which the comparisons are made at specific points in time and space, it is common practice in modeling studies to average results over space and/or time in order to determine the appropriate spatial/temporal scales that the model can be used for. The reasoning is that it is not only appropriate but desirable to average out so called "random" fluctuations that may possibly obscure appropriate interpretation of model results. Although this practice is generally accepted as a valid method of data manipulation for mechanistic water quality modeling (all Chesapeake Bay model/data comparisons were averaged over significant portions of time and areas of space), in reality, it is generally done in order to make poor model/data comparisons and statistics look better. One man's random fluctuation is another man's mechanism.

Assuming my previous comments to have some relevance, then, in my opinion, the important questions for reviewers of and involved parties in the Spokane River TMDL are "are the inevitable disagreements between computed and observed measurements sufficient to show that the model is fundamentally missing real world system behavior and is therefore an inappropriate tool for basing management decisions on, or are the disagreements most likely a result of boundary condition inaccuracies which, if corrected, would result in better model/data comparisons? Furthermore, if recommendations based on the model are followed, will there be an acceptable level of improvement of water quality in the system without wasting precious resources?" An analysis of error statistics alone cannot answer these questions. One must look at the plots of computed and observed data and ask oneself, "is the model missing a fundamental process occurring in the real world that prevents it from addressing the important water quality related management issues?" For example, if the nitrate decay rate were twice the default decay rate, would this have any impact on model usefulness to address algal/nutrient/DO issues in a phosphorus limited system?

What is clear from the conductivity comparisons is that the model is correctly placing the Spokane River inflows over the proper water column depths in Lake Spokane, and, by inference, the point source CBOD loadings from the dischargers. Since the CBOD coefficients used in the model were developed from laboratory studies and are not a calibration "knob", then the impact on DO in Lake Spokane from the decay of organic matter released from the point source dischargers is correct regardless of how accurate other mechanisms are that affect DO in the model, what the values for other model coefficients are that impact DO, or whether or not the model closely reproduces all observed DO data or not. I would argue the model would be clearly appropriate for management decisions that impact the point source dischargers and the subsequent impact in increasing/reducing DO in Lake Spokane even if this mechanism affecting DO were the only thing included in the model simulation.

It should be pointed out that all the other mechanisms included in the model are included only to determine if the model can reproduce the important spatial and temporal changes in DO, algae, and nutrients, thus giving one confidence that the real world is being modeled "correctly". In essence, the inclusion of other mechanisms is an attempt to make folks feel "warm and fuzzy" regarding the use of the model in making management decisions. As stated previously, all of the other mechanisms could be left out of the model resulting in very little relationship between computed and observed DO, but the model could still be used in a meaningful manner. This is because it clearly reproduces the placement of point source discharges into Lake Spokane thus reproducing the immediate impacts of their organic matter loadings on Lake Spokane DO, assuming that the laboratory determined CBOD decay rates are correct.

Furthermore, if some thought is given to interpretation of model results, it is clear that the above scenario is, in actuality, a best case scenario (from the discharger’s viewpoint) for determining the impacts of point source discharges on Lake Spokane DO. This is because the mineralization of organic phosphorus and subsequent impact on autochthonous organic matter production and decay is not included. For a valid model review, it is not sufficient to point out that this or that mechanism is lacking ,or this or that parameter is not accurately modeled, or this or that calibration parameter is not within an acceptable value. This appears to be Dr. Esvelt's approach, and it is misleading at best and wrong at worst. One must go beyond this sort of superficial review (even if the comments were valid) and determine if the lack of an important mechanism in the model, lack of reproduction of a certain parameter, or inappropriateness of a calibration parameter is sufficient to conclude that the model is not useful. If all of the errors in computed versus observed data can be attributed to errors in boundary conditions, then a given model may be very inaccurate, but still very useful. In other words, a comment/criticism can be valid, but irrelevant. Determining this is the real purpose of the review process.

It is my opinion that all the other issues Dr. Svelte has raised regarding the models ability to reproduce observed DO are red herrings when it comes to determining if the model is a suitable tool for making management decisions regarding point source impacts on DO in Lake Spokane. It is clear that there is no other model currently available now or in the near future that can model the complex hydrodynamics of the Spokane River and Lake Spokane as accurately as CE-QUAL-W2, there are no fundamental errors or mechanisms lacking in the algorithms describing algal/nutrient/DO interactions in the model, and the model is reproducing a wide range of very complex prototype behavior for, I would argue, the "right" reason, Dr. Esvelt's TP argument not withstanding. In summary, Dr. Esvelt's argument that the model is an inappropriate tool for the Spokane River is, in essence, arguing that mechanistic modeling cannot be used as a tool in the

TMDL process. If so, then we might as well throw a dart at a dart board to determine loadings to a system that will not result in impaired use of the waterbody and save everyone a lot of time and money.

I can state with certainty that everyone involved in the Spokane River/Lake Spokane model development welcome valid criticisms of the model and the modeling effort provided that the comments are relevant and result in a more useful modeling tool. Many of the points I raised above regarding model/data comparisons are subtle and can easily be overlooked by a reviewer and would need to be pointed out during the review process. However, it is clear that Dr. Esvelt's comments regarding the way TP is handled in the model, the supposed poor reproduction of conductivity, and of the applicability of the general modeling effort again show a basic and fundamental lack of understanding of mechanistic eutrophication modeling and should be so viewed by the modelers, other reviewers, and involved parties.

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Addendum

Response to comments from the City of Spokane

Dated July 18, 2003

Response to General Comments - City of Spokane's Cover Letter

<u>General Comment 1</u>:
Non-point source impacts continue to be a significant concern to the City of Spokane. Successful and equitable management of water quality in the River and Lake requires that non-point contributors participate in the solution. If phosphorus control is included with the DO TMDL, all sources should be considered, including septic tanks.

<u>General Response 1</u>:
Ecology agrees that controlling nonpoint sources of pollution including well-constructed septic systems is essential to protecting Spokane River water quality. Those controls are being addressed developing and implementing future watershed plans to meet TMDLs to protect dissolved oxygen in the Hangman Creek and Little Spokane River Basins. Since nutrients also contribute to dissolved oxygen consumption in by stimulating primary productivity, it is common for best management practices (BMPs) to include elements that help control nonpoint nutrient inputs in these basins thus reducing these loads to the mainstream of the Spokane River.

Evaluations of phosphorus loading to Lake Spokane show that point source loads of BOD and nutrients during the most critical summer season are very significant (see Figure). Previous evaluations performed by Eastern Washington University determined that shoreline septic systems contributed a very small fraction to the Lake Spokane nutrient loading. The importance of implementing known and affordable treatment for point sources, especially from the largest sources will result in large and measurable pollution reductions that must not be under emphasized.

<u>General Comment 2</u>:
Central to the issue of model applicability is that its accuracy must be commensurate with the anticipated level of expenditure by the dischargers to abide by the resulting TMDL(s). Uncertainty and sensitivity issues pose ongoing questions about its effectiveness as a management tool.

<u>General Response 2</u>:
The level of uncertainty in the model does not prevent it from being useful tool to develop target loadings for the TMDL. Uncertainty in a model is usually addressed as the TMDL is established by incorporating a margin of safety so that waste load allocations given to the pollution sources prevent repeated and prolonged periods of impaired water quality. As stated previously, the reliance of pollution reductions from all sources will be important for improving water quality. The current model and ones previously completed all agree that the pollutant loading needs to be significantly reduced to maintain acceptable dissolved oxygen. The emphasis for pollution controls must be placed on the largest and most controllable sources while continuing to pursue reductions from the non-point sources. The use of tertiary chemical removal with filtration to control BOD and phosphorus, and/or provide options for reuse from treatment plants is not new or unaffordable. The uncertainty argument does not negate the overall conclusion nor should it be used any longer to delay improving water quality.

General Comment 3:
"In terms of adaptive management, Spokane requests that Ecology consider run-of-the-river operation during critical periods. Such operation could prove more effective and far less costly to the region than strict effluent limitations".

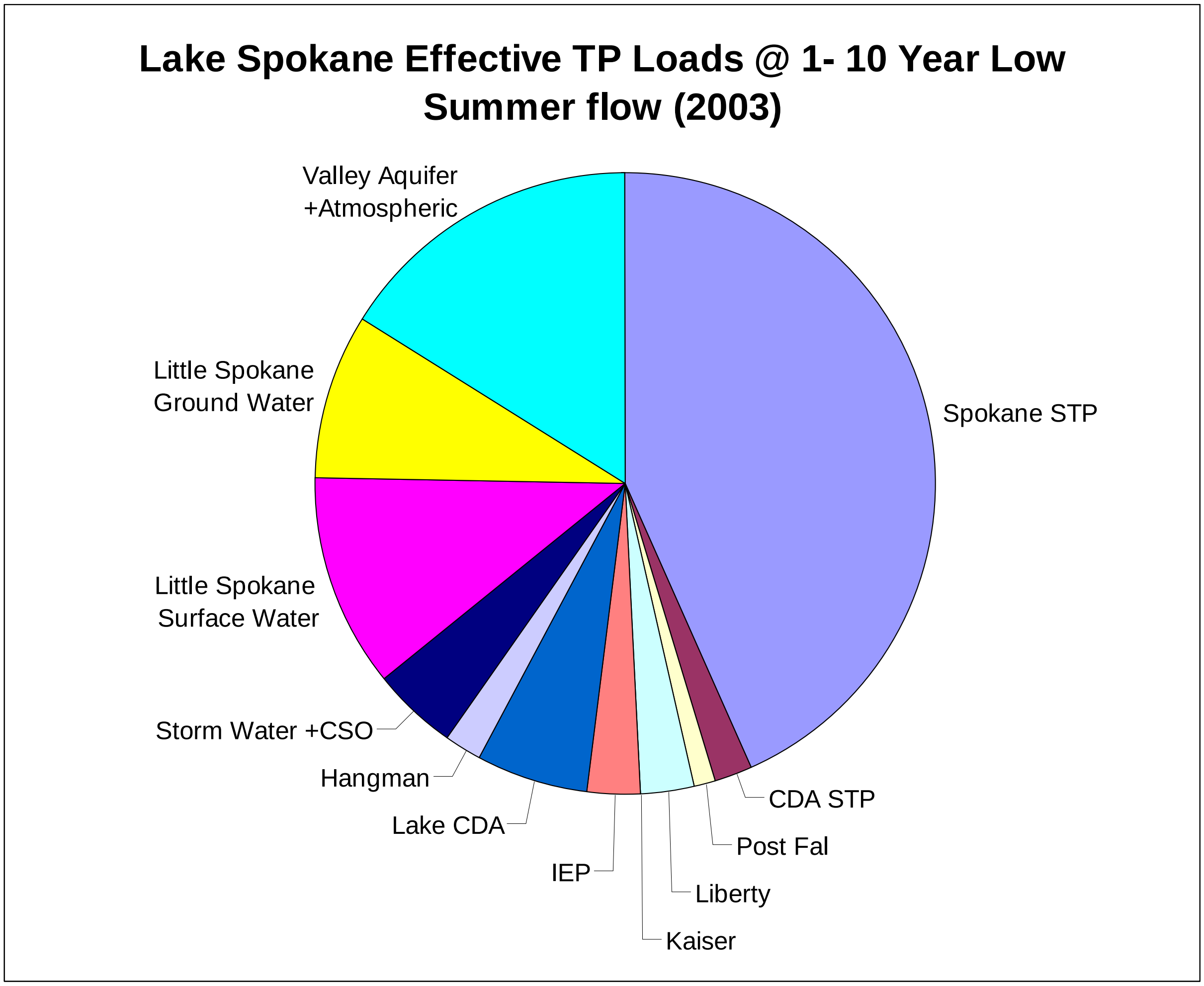
General Response 3:
It is assumed that run-of-the-river operation refers to using *in situ* (in-river) methods to mitigate the pollutants causing oxygen demand such as artificial reaeration and/or stream flow adjustments. As has previously been discussed with City staff and consultants, *in situ* mitigation can be a valid means of reversing negative environmental impacts that would not normally respond to conventional pollution controls. However, all known and reasonable methods to eliminate the sources of pollution must also be implemented. In this case, phosphorus and BOD are the limiting parameters nutrient and by using better removal technology, it can be controlled much better than treatment levels currently achieved by Spokane dischargers.

New treatment technologies can result in TP concentrations 10 fold less than existing discharges. At these lower concentrations, most of the existing DO problems associated with excessive algae growth would likely disappear. If removal of excessive algae is not enough to adequately control oxygen demand, *in situ* mitigation might be a good potential tool to add to the total implementation strategy. Items such as restoring lost reaeration due to dams, maintaining higher minimum flows during critical seasons, and hypolimnetic reaeration in Lake Spokane may all provide partial mitigation and, with input from the advisory group, could become parts of the implementation strategy developed for meeting the TMDL.

General Comment 4:
City of Spokane requests that Ecology consider that accuracy tolerances are inherent in measurement equipment and methods when determining compliance standards and sampling frequency for verifying compliance with the TMDL.

General Response 4:
Establishment of a TMDL sets the target for which a Detailed Implementation Plan will prescribe the actions necessary to meet load allocations for nonpoint sources (LAs) and waste load allocations for point sources (WLAs). Compliance for point sources will be measured by compliance with each of their individual permit conditions. Follow-up monitoring of the river/lake would also be used to measure the long-term success of the TMDL and implementation plan to ultimately achieve water quality standards. With the help from the advisory group, the long-term monitoring plan for assessing critical conditions will be developed and included with the detailed implementation plan.

Estimate of effective growing season phosphorus loading to Lake Spokane during 2003 using the P-Attenuation Model developed by Harper-Owes (Report for Spokane River Phosphorus TAC - December 2003)



DEPARTMENT OF ECOLOGY

January 15, 2004

TO: Ken Merrill, David T. Knight, and James Bellatty
Water Quality Program, Eastern Regional Office

FROM: Bob Cusimano, Water Quality Studies Unit
Environmental Assessment Program

THROUGH: Will Kendra, Manager, Watershed Ecology Section
Environmental Assessment Program

Karol Erickson, Unit Supervisor, Water Quality Studies Unit
Environmental Assessment Program

SUBJECT: **RESPONSE TO REVIEW COMMENTS ON THE SPOKANE RIVER AND LAKE SPOKANE (LONG LAKE) POLLUTANT ASSESSMENT FOR DISSOLVED OXYGEN STUDY AND MODEL DEVELOPMENT**

Our October 24, 2003, memorandum contained responses to comments we received on the Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment study reports and associated CEQUALW2 model development and calibration reports. The comments we addressed were from Limno-Tech, Inc., Esvelt Environmental Engineering, and the Environmental Protection Agency. However, you notified us in November that comments from the city of Spokane and their consultant (CTE Engineers) did not get included. This memorandum addresses the specific technical comments listed in the CTE Engineers memorandum. Please note that our October 24 memorandum addresses many of the comments submitted by the city and is cited in response to some of CTE Engineers specific comments.

We did not address Dale Arnold's general comments concerning managing pollutant loading to the river in the cover letter from the city because these comments can be more appropriately addressed by your program. In addition, there are a few references in the CTE Engineers memorandum related to using the model to make "prohibitively costly" or "costly" decisions that might affect the wastewater treatment facilities and their comment on Page 9 of the memorandum under "Compliance" that ask a question about developing solutions. Again, these are subjects best addressed by your program.

Attachment 1 is a copy of the letter from the city of Spokane, Dale Arnold, Director, Wastewater Management and the CTE Engineers review comment memorandum. Attachment 2 is a copy of an email from Tom Cole, Corps of Engineers. Attachment 3 is a memorandum from Scott Wells and Chris Berger, Portland State University (PSU), Department of Engineering. These documents contain responses to most of the comments in the CTE Engineers memorandum.

We believe the historical studies and our current project show that the river system (especially Lake Spokane) is very sensitive to pollutant loading that either directly or indirectly affects dissolved oxygen concentrations. The additional analysis presented in the Scott Wells and Chris Berger memorandum (i.e., Attachment 2, *Issues With the Algae-P-Oxygen Relationships*) shows that the major pollutant of concern under 2001 loading levels is phosphorus, which is consistent with the results of historical water quality studies summarized in our draft report. In addition, the modeling demonstrates that the current BOD5 permit limits would result in severe additional degradation of dissolved oxygen levels in Lake Spokane. As discussed in our October 24 response memorandum, Portland State University is reviewing the model calibration and will be making some changes. The final model will be available for distribution by the end of January 2004.

We intend to complete our final report after PSU completes their model review. However, we do not anticipate that PSU's review will significantly affect the conclusions presented in the draft report.

Overall, many of CTE Engineers' comments focus on issues that we have addressed in the past. In addition, many of the comments say, or imply, what "Ecology should have done…." with respect to model development and analysis. As you know, the 2001 data collection effort was initiated and managed by the dischargers (i.e., not Ecology) such that they could have collected data they wanted in order to satisfy their questions or concerns about the system. It seems unreasonable today to question Ecology's sampling effort given the additional time we added to the project in order to allow the dischargers to collect data. In addition, over the last few years, Ecology has held a number of public meetings to discuss the Spokane study and modeling efforts. During those meetings, we have emphasized that Ecology would accept alternative estimates for model input values if they could be shown to better represent the system (i.e., upstream boundary, point sources, tributary parameter concentrations, and coefficients/rates). To date, we have not received any specific request for changes in our modeling assumptions or inputs based on CTE Engineers or any other consultant's professional assessment.

Most of the comments are addressed in the attached Cole and Wells/Berger documents. The following are responses to comments either not addressed, or only partially addressed, by these documents:

Additional Response to CTE Engineers Comment #1 on Page 1

We disagree with the overall conclusion of the reviewer's comment that the CEQUALW2 model is not capable of being used in decision making for controlling point and nonpoint loads to achieve current dissolved oxygen criteria. We defined how the current model can be used in our October 24 memorandum, General Response Number 3 and Response to Limno-Tech Bullet Number 1.

We are not proposing to set periphyton and chlorophyll *a* targets. As stated in our responses to Limo-Tech Bullet Number 4, we believe that meeting the current DO criteria will prevent nuisance algal growth, because point and nonpoint sources of BOD and phosphorus would have to be reduced to much lower levels than currently added to the river system.

On Page 2, Paragraph 1, the reviewer's intention is to argue that the dischargers cause less impact than nonpoint sources such that "restricting discharges from existing wastewater treatment facilities may not provide as much improvement as previously anticipated, while being prohibitively costly." First, since we have not even begun to talk about what possible mitigation strategies may be needed, it seems unreasonable to say that any change in the current wastewater discharges to an impaired river and lake is too costly. Second, it also seems unreasonable to argue that restricting existing discharges may not provide "as much improvement" based on the relative difference between the potential effects of nonpoint and point sources because nonpoint sources have a greater effect than point sources. Any comparison(s) of the effects of pollutant loading on DO should be made with respect to the DO criteria not how much bigger or smaller the effects are from different sources. However, currently, the DO depletion predicted by the model due to the point sources is far in excess of the allowable 0.2 mg/L for the current and permitted loads, not to mention the possible impacts of proposed future increased loading from the point sources.

We generally support the "adaptive implementation" strategy discussed in Paragraphs 2-5 on Page 4. However, until either the UAA process is completed or a decision is made to pursue the TMDL(s) based on the current water quality criteria, we cannot make a determination as to the adequacy of the model to make final wasteload and load allocations because different criteria definitions may require different levels of confidence in the model predictions. We also believe that historical studies and our current study show that, at a minimum, further reduction in point and nonpoint source loads are necessary to improve water quality in the Spokane River and Lake Spokane.

As discussed in our October 24 memorandum, we are pursuing with PSU possible changes that will improve the CEQUALW2 model calibration which may improve the model calibration (reference General Response 2).

Response to CTE Engineers Comment #2 on Page 4-5

Monitoring of the river at the Idaho/Washington border has demonstrated that dissolved oxygen levels fall below the Washington criteria during critical conditions. Federal NPDES regulations require that permits are protective of downstream water quality. Since EPA is the regulatory authority for the Idaho dischargers, it is EPAs responsibility to evaluate both near and far-field effects of the discharges from the Coeur D'Alene, Post Falls, and Hayden Lake WWTPs when these permits are reissued. If that evaluation determines these discharges are impacting water quality at the border, then appropriate water quality based effluent limitations will be established.

Model* scenarios have been conducted in which discharges from the Idaho facilities into the river were excluded. These runs predict dissolved oxygen levels might improve without these discharges to just meeting Washington criteria at the border. However, this would not create capacity for existing or increased loading from Washington dischargers. In the meantime, the known water quality problem(s) to which the Washington discharges contribute need to be addressed.

* Although the CEQUAL model for Idaho portion of the river produces results that correlate well with measurements at the border, the model has not yet been calibrated.

Additional Response to CTE Engineers Previous "Unanswered" Comments on Pages 5-9

First, we have responded to many of these comments (or similar comments) presented in this section in past responses or public meetings. Overall, we believe we followed accepted practices for developing the model by using available data as noted by the EPA Environmental Scientist reviewer, John Yearsley, that was included in our October 24 response memorandum.

Model Development

Comment #1 on Page 6
We agree with the assessment of collecting SOD measurements in the Cole and Wells/Berger responses. In addition, the discharger's consultants discussed sampling
SOD in Lake Spokane for the 2001 sampling plan and they decided not to collect the data apparently because of the expected poor accuracy and precision of SOD measurements.

Comment #3 and #4 on Page 6
Ideally, we would have collected on-site wind speed and solar radiation data. However, it is also not an uncommon practice to use regional meteorological data for water quality modeling. Given the length and topography of the waterbody, it is unlikely that one, or even two, meteorological stations would be representative of the on-site conditions. One of the advantages of using regional meteorological data is that the data are collected in open locations that are not affected by the topography such that the maximum potential wind and solar radiation are better represented than site data that may not represent the whole waterbody. Although the wind direction may not be the same as that found on the lake, it would be difficult to measure the actual wind direction along the entire length of the lake.

One of the major influences of these variables is on water column temperature and how temperature represents the physical structure of the water column. It is unlikely that site specific data would significantly improve an already good lake model temperature calibration.

Model Calibration

Comment #3 on Page 7
Is this comment in reference to assessing instrument error or model uncertainty analysis? The comment is somewhat confusing because "instrument error" itself is usually assessed with respect to the manufacturer's specifications through calibration and post-calibration and is not generally part of the uncertainty analysis associated with water quality modeling (i.e., if the instrument calibration falls within specifications, then the instrument readings are considered accurate.) AVISTA and USGS provided us with water level data and we expect that they calibrated their instruments within specifications.

We agree that total sampling variability (i.e., laboratory and field sampling variation) is much greater than instrument error. Ecology estimates total sample variation, or total precision, by collecting replicate samples as discussed in our report "Data Summary, Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen" (http://www.ecy.wa.gov/biblio/0303023.html). Please note that we did not receive a data quality analysis report for the 2001 data collected by the dischargers.

If this comment is in reference to model uncertainty analysis, then see the discussion in the draft project report section "CE-QUAL-W2 Model Selection, Calibration, and Uncertainty." As we have explained in past meetings, responses to comments, and the draft report, if it was possible to conduct a complete model uncertainty analysis for a dynamic model, then that uncertainty would have to be included in the Margin-of-Safety (MOS) for establishing loading limits (e.g., the lower confidence limit for any model estimate would be used to assess water quality criteria and loading limits). Using a low flow year as part of the MOS helps account for our lack of knowledge about the system and possible variation in model input values. However, we may want to consider some additional MOS to address variation in model input values.

Model Interpretation

Comment #2 on Page 16
Increased flow rates were not included in the PERMIT scenario (i.e., just increased loading). If the dischargers or their representatives have questions about what the model would predict under different conditions, they should set up the model to run those scenarios. However, for our final report, we are only including the results from the five scenarios defined in the draft report which we believe represent the major questions about the estimated impact of point and nonpoint pollutant loading.

Comment #7 on Page 16
Yes, we agree that there are impacts from agriculture and other human activities (including small point source discharges) in the Little Spokane and Latah Creek drainages, which is why we defined the NO-SOURCE scenario the way we did (i.e., without human impacts, we would expect the tributaries to have much better water quality).

Comment #8 on Page 17
The PERMIT scenario is defined in the draft report section "Design Conditions" on Page 78. We will put the worksheets that we used to establish the point source concentrations on the Spokane TMDL web site.

Model Enforcement

Comment #1 on Page 17
See our response to this issue in the October 24 memorandum, Esvelt Environmental Engineering Comment #4.

BC:cn
Attachments: 3

Attachment 1

Letter From Dale Arnold, Director, Wastewater Management, City of Spokane

and

CTE Engineers Review Comment Memorandum

July 18, 2003

Ken Merrill
Washington State Department of Ecology
4601 N. Monroe, Suite 202
Spokane, WA 99205

Re: "Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen" draft report

Dear Mr. Merrill:

The City of Spokane Wastewater Management Department acknowledges Ecology's effort to date in developing a comprehensive model to simulate dissolved oxygen levels in the Spokane River and Long Lake and for hosting the June 26 workshop regarding the model development. We also appreciate the opportunity to comment on the referenced draft report. Key comments are highlighted in this letter and detailed in the attached copy of a memorandum from CTE Engineers.

The impact of non-point sources continues to be a significant concern to the City of Spokane. It is our opinion that successful and equitable management of water quality in the River and Lake requires that non-point contributors participate in the solution.

Further, if the Phosphorous TMDL is revisited as this process moves forward, we request that <u>all</u> sources of phosphorous be analyzed, including septic tanks at Lake Spokane.

Regarding the model, uncertainty and sensitivity issues pose ongoing questions about its effectiveness as a management tool. Several parameters appear to need additional calibration, or at least confirmation of their validity, to ensure accurate model simulation. Central to the issue of model applicability is that its accuracy must be commensurate with the anticipated level of expenditure by the dischargers to abide by the resulting TMDL(s).

In terms of adaptive management, Spokane requests that Ecology consider run-of-the-river operation during critical periods. Such operation could prove more effective and far less costly to the region than strict effluent limitations.

The concentration of dissolved oxygen at any point in the water body varies with time. In addition, accuracy tolerances are inherent in measurement equipment and methods. Consequently, with regard to monitoring for compliance, the City of Spokane requests that Ecology seek weekly, monthly, and/or seasonal compliance standards and take a reasonable number of samples when verifying compliance.

Finally, the City of Spokane requests Ecology work with Spokane to incorporate our comments and resolve outstanding questions concerning the model. The City supports the need for clean water as one aspect of quality of life for Spokane residents and believes it is in our mutual best interest to manage water quality in an effective manner.

Thank you again for the opportunity to comment on the draft report. We look forward to working with Ecology toward resolution of these issues regarding assimilative capacity of the Spokane River and our ability to meet water quality standards. If you have any questions regarding our comments, please call Lars Hendron at 625-7929.

Sincerely,

Dale E. Arnold
Director – Wastewater Management

DEA/lhh

cc w/o attachment:
Jack Lynch; City Administrator
Roger Flint; Public Works & Utilities

cc w/ attachment:
Tim Pelton; AWWTP
Lars Hendron; Sewer Maintenance
Gerry Shrope; CSO-PMO
Jim Correll; TP-PMO
Larry Esvelt; Esvelt Environmental Engineering

CTE ENGINEERS
CONSOER TOWNSEND ENVIRODYNE ENGINEERS, INC.

MEMORANDUM

140 SOUTH ARTHUR STREET • SUITE 500 • SPOKANE, WA. 99202 • (509) 535-5454 • FAX (509) 535-5725

TO: Lars Hendron, PE **cc:** Russell Mau, file

FROM: Gerry Shrope, PE

DATE: July 11, 2002 **PROJECT #:** **71240-1000**

SUBJECT: Comments on the "Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen" review draft report

The purpose of this memorandum is to provide comments regarding the Dissolved Oxygen (DO) TMDL being conducted on the Spokane River and Long Lake.

This memorandum is separated into two components, comments or issues regarding the "Review Draft" report "Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen" dated 5-23-03 and unanswered comments from previous documentation provided regarding the DO TMDL.

Comments on the "Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen" review draft report

Comment #1. Use of the Current Form of the CE-QUAL-W2 Model for Defining Waste Load Allocations.

The current form of the CE-QUAL-W2 model is not thought to be capable of being used in making decisions on controlling point and nonpoint pollutant loads to achieve current dissolved oxygen standards in the Spokane River and Long Lake and as yet unspecified targets for river periphyton levels and Long Lake chlorophyll-a concentrations.

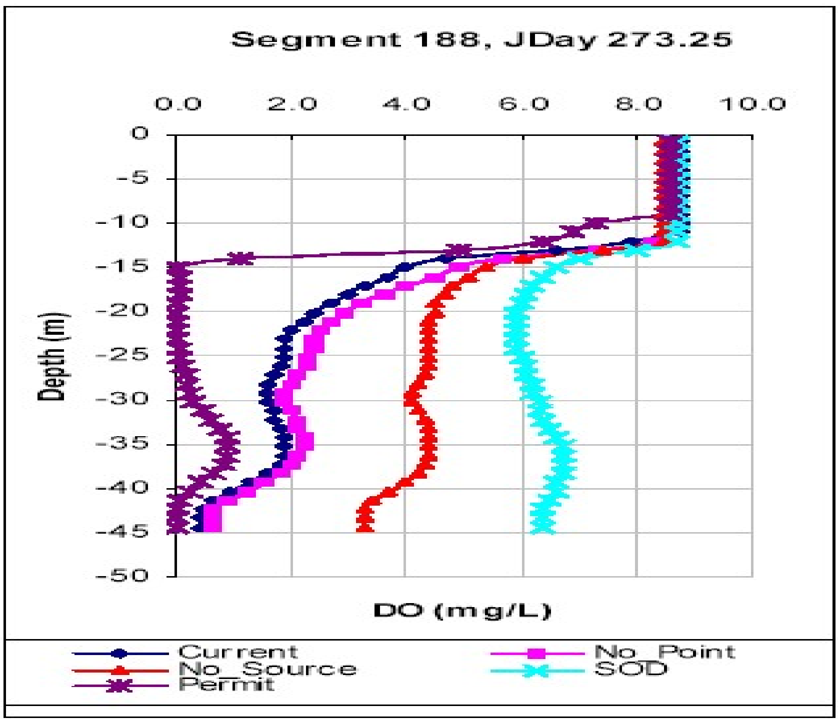
As evidence supporting this comment, the following statement is quoted directly from page 68 of Ecology's review draft report:

> *"Although Ecology recognizes the need to apply good scientific principles, we also recognize that determining a level of treatment for a point source discharge or recommending BMPs in a watershed to mitigate nonpoint sources should not require an exhaustive scientific data collection and model parameterization process. Rather, the objective was to collect enough data to develop a scientifically based model application that provides a good approximation of the system."*

It appears that Ecology believes a proper balance has been achieved between data collection and a good approximation of the Spokane River and Long Lake system and that extreme pollution control measures such as removing Spokane and other treated wastewater from the river basin is justified. The following observations indicate that the data collection efforts on the Spokane River system were extensive, but not exhaustive, and that the model approximation of the system is not good enough to make certain pollution abatement decisions.

1. Sediment oxygen demand (SOD). SOD is a major component of the dissolved oxygen balance in Long Lake, yet there are no measurements to support the SOD in the model. Although it is recognized that it is difficult to measure SOD and collect enough samples to completely characterize the sediments of Long Lake, it is not technically justified to consider a model well calibrated when there are no SOD measurements in a system where SOD is such an important component of the dissolved oxygen balance.

The current model has shown that the DO in the Spokane River and Lake system is potentially out of compliance for DO in Long Lake, where a DO concentration deficit of up to 3 or 4 mg/L may result in Long Lake under future loading conditions. These analyses, completed by Ecology, also showed that the DO concentrations only improved on the order of 0.5 mg/L when point source loads were removed from the modeled system; however, DO concentrations improved by 2 to 3 mg/L (or more in some instances), respectively, for a total of 4+ mg/L, when nonpoint sources and the sediment oxygen demand in the lake were removed from the system. This is illustrated in the accompanying figure taken directly from the Review Draft report "Spokane River and Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen", Figure C1.



Such results show that point sources may not be as impactive as previously thought and restricting discharges from existing wastewater treatment facilities may not provide as much improvement as previously anticipated, while still being prohibitively costly.

2. <u>Model weakness in computing chlorophyll-a levels in Long Lake.</u> One of the principal intended uses of the water quality model is to develop new point and nonpoint phosphorus controls to reduce late summer/early fall algal blooms in upper Long Lake; yet this is perhaps the weakest component of the water quality model. During August 29-30, 2001 peak surface chlorophyll-a levels of 70 μg/l were measured and a much more dense blue-green bloom was observed during the middle of September. During this same period, the calibrated CE-QUAL-W2 model computed chlorophyll-a levels of 3-4 μg/l. Although some improvement is achieved when a low settling velocity is assigned to algae during this period, it is reasonable to conclude that the current water quality model is not adequately

calibrated to justify phosphorus reduction strategies that may include diversion of municipal and industrial effluent out of the basin.

3. <u>Uncertainty in properly representing nonpoint source inputs of phosphorus and organics.</u> Ecology indicates that Latah Creek and the Little Spokane River typically have high TSS and total phosphorus loads during the high flow period of January through April. In addition, Latah Creek has a high organic loading during this period. It is not clear if the current model calibration properly represents these nonpoint source inputs to Long Lake and subsequent settling to the sediment which in turn produces SOD and sediment nutrient fluxes. This question could be more completely addressed if the sediment submodel contained in the Chesapeake Bay model were included in this modeling framework. The advantage of a sediment submodel is that SOD and nutrient fluxes are not "assigned" but rather computed from properly tracking the point and nonpoint source inputs of organic carbon and nutrients. The sediment submodel calculates the SOD and sediment nutrient fluxes associated with the decay of nonpoint and point source organics and algae that settle to the bottom of Long Lake. The calculation of SOD and nutrient fluxes that generally match measured values provides further support that all point and nonpoint source loads have been represented in the model.

4. <u>Uncertainty in model coefficients that relate phosphorus to algal growth and the oxygen demand of algae in the subsurface layer of Long Lake.</u> Because phosphorus limits algal growth in Long Lake, the amount of phosphorus required to produce one gram of algal biomass is very important (P/C ratio). In this study the algal P/C ratio was set at approximately one half the Redfield ratio, a common estimate of algal cell stoichiometry. However, this ratio varies as the algal cell experiences nutrient stress associated with low water phosphorus concentrations. One method of estimating the algal P/C ratio is to plot particulate organic phosphorus versus algal carbon (derived from the algal carbon to chlorophyll-a ratio). However data is not available for this study and therefore leads to some uncertainty in the link between phosphorus and algal levels in Long Lake.

The algal carbon to chlorophyll (or biomass) ratio assigned in the model has a significant impact on the oxygen depletion associated with respiring and decaying algae in the lower layer of Long Lake. An equivalent carbon to chlorophyll-a ratio of approximately 60 was used in the model calibration. Limited data from the increase in particulate organic carbon (TOC minus DOC) and chlorophyll in the upper part of Long lake during the August 15-16, 2001 and September 26-27, 2001 surveys suggest that this ratio could be closer to 30. If this is the case, the current model would be overstating the oxygen consumption of computed algal concentrations by a factor of two.

5. <u>Uncertainty in the proper calculation of algal growth limitations by light and nutrients.</u> The difficulties in the calibrated model computing algal levels in the upper part of Long Lake may be partly related to the calculation of available light in the photic zone. In the 1991 model calibration, the measured light extinction coefficient during the summer and fall at Station LL4 averages about 0.6/m whereas the model computes an average extinction coefficient of approximately 1.2/m, which means that in the model the algae in the photic zone are exposed to approximately half the actual light. This may explain the assignment of a light

saturation value of 40 W/m^2 versus the more common 150 W/^{2} that would partially compensate for underestimating the available light. For the 2000 model calibration the problem is reversed with the model computing approximately twice the actual light in the photic zone. In this case, the use of a light saturation value of 40 W/m^2 versus 150 W/m^2 would further contribute to an underestimation of the actual limiting effect of light on algal growth.

One other technical point that affects the limitation of light and nutrients on algal growth in the CE-QUAL-W2 is that algal growth is limited by the minimum limitation of light and nutrients as opposed to the more common approach in most models of limiting algal growth by light and nutrients concurrently. It is not clear how important this difference is but the CE-QUAL-W2 model representation would produce a lesser limitation to algal growth by light and nutrients that most other models.

The purpose of pointing out data limitations and uncertainties in the model calibration is not to discredit the entire modeling effort to justify no further point and nonpoint source phosphorus reduction, but rather to indicate that the level and cost of proposed phosphorus controls should be commensurate with our understanding of the factors controlling water quality in the Spokane River and Long Lake. It is clear that some further phosphorus reduction should be implemented to reduce algal blooms in Long Lake and that the modeling effort conducted by Ecology is a vast improvement over the prior technology used to develop the current phosphorus TMDL for Long Lake. However, it is also equally clear that the current understanding of the point source phosphorus-algae-dissolved oxygen link does not justify the recommendation of such costly alternatives as diverting point sources from the river and lake system.

This concept of achieving a balance between good science and decision making based on the available science is described as adaptive implementation in the National Research Council report, "Assessing the TMDL Approach to Water Quality Management." In this report it is stated that

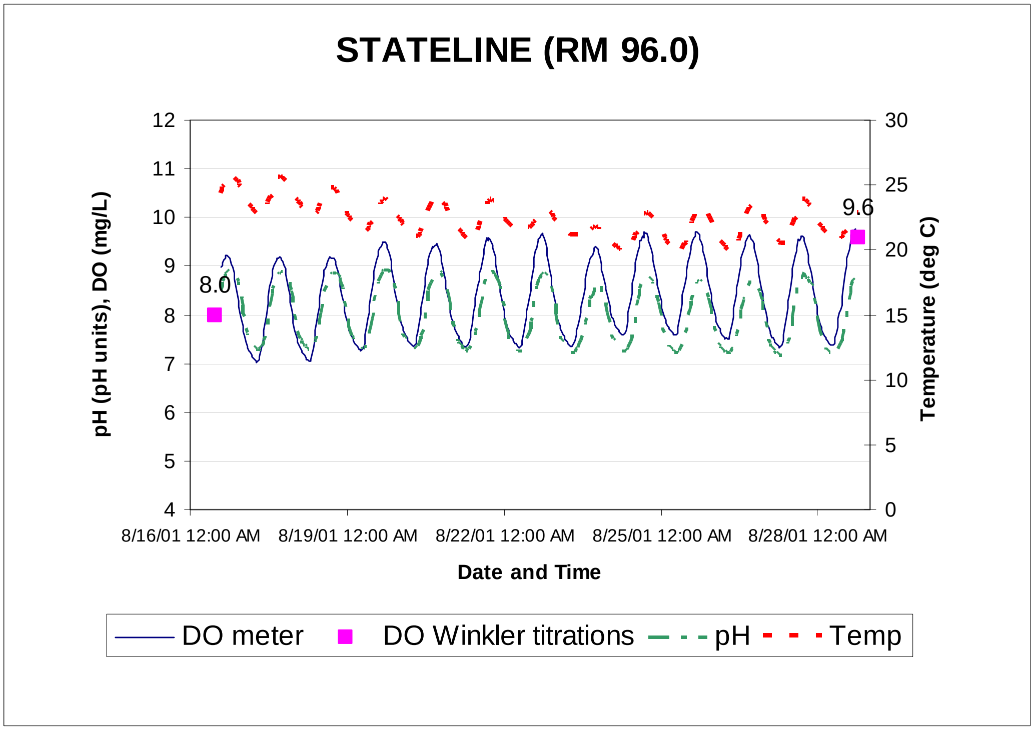
"Adaptive implementation is, in fact, the application of the scientific method to decision-making. It is a process of taking actions of limited scope commensurate with available data and information to continuously improve our understanding of a problem and its solutions, while at the same time making progress toward attaining a water quality standard."

On the basis of this principle of adaptive implementation, it is incumbent on Ecology to recognize the uncertainty in the data and modeling analysis and set pollution control targets that reflect this uncertainty but yet make considerable progress in improving water quality with the concurrent commitment to continuously monitor water quality and improve the science to justify further reduction in point and nonpoint source loads if necessary.

Comment #2. DO in Spokane River at Stateline

Currently, the DO in the Spokane River at Stateline does not meet the water quality criterion established by Ecology for the State of Washington. This is shown in the accompanying figure taken directly from the Review Draft report "Spokane River and

Lake Spokane (Long Lake) Pollutant Loading Assessment for Protecting Dissolved Oxygen", Figure 15.



This figure clearly illustrates that the DO in the Spokane River is deficient by approximately 1.0 mg/L of DO at Stateline. Our understanding of federal law is that whenever a difference in water quality criteria exists between delegated entities, the upstream delegated entity is required to meet downstream water quality requirements.

As addressed by Federal statute, such a deficit should be analyzed and mitigated, or planned for mitigation, prior to addressing downstream dischargers.

Previous Comments on the Overall DO TMDL MODELING AND STUDY PROCESS AND RESULTS (unanswered). These comments are also directed at information reiterated at the June 26, 2003 Workshop.

Process

The expectation for the TMDL Modeling and Study was to reach agreement on each of the following areas prior to moving on to the next:

- Develop – Sampling
- Verify – Calibration
- Interpret – Determine waste loads impacts and determine if noncompliance exists
- Enforce – Define waste loads (define the TMDL)
- Compliance – Evaluate ongoing compliance

All subsequent comments are categorized according to this framework.

Model Development

1. SOD has the greatest single impact on the modeled DO concentrations (see Figure 6 of the summary report dated June 18, 2002), yet no measurements of either SOD rate nor sediment transport were included in the model development.

Although it has been stated that the SOD rate is too sensitive to measure and provide meaningful information, the net effect of SOD on oxygen depletion is on the order of 3 mg/l out of a total depletion of 6 mg/L, at depth, in Long Lake. This supports measuring or researching sediment impacts on SOD.

Examples of web sites and programs that address the increase in soil erosion and lake sedimentation resulting from agricultural practices and BMP methods to reduce same are listed below:

- Great Lakes Basin Program for Soil Erosion and Sediment Control (Regional) (www.glc.org/basin/glbp.html)
- Management Measures for Agricultural Sources (EPA) (www.epa.gov/OWOW/NPS/MMGI/Chapter2/ch2-2a.html)
- Seafriends (information is referenced from the USDA) (www.seafriends.org.nz/enviro/soil/erosion/htm) (Particularly applicable, this site identifies the level of soil erosion occurring throughout the United State due to farming practices – of which, southeast Washington is one area with high erosion

Several advanced, predictive sediment transport models are available to define natural bed load transport, including Brownlie (Mau presented the model in comparison to other models), Van Rijn, others. The Brownlie-Mau model is fully predictive in terms of flow regime and sediment transport. Sediment sampling and characterization for organic content and other contaminants can augment these transport models. Such sampling and characterization would provide a method to distinguish oxygen demand of "normal" sediments versus agricultural-induced sediments. Furthermore, such sampling would need to be conducted in the winter and spring time during high flows when the vast majority of the runoff and sediment transport occur. Also, such sampling would need to be conducted at or near the bottom of the river, rather than the water column, because the sediment transport is concentrated near the bed of the river.

2. No measurements of electron activity for water chemistry were conducted. In addition, none of the descriptions and presentations of the model have indicated whether the model

is balanced with respect to electron activity. Also, this model does not appear to provide electroneutrality.

3. D.O. is shown to be sensitive to wind speed, yet no local weather data was collected (see Comment 4. under Model Interpretation).

4. Solar radiation was not available from the Spokane International Airport, so solar data from Odessa, Washington was utilized. In addition, the measured cloud cover at the Spokane International Airport was considered inaccurate, so cloud cover was theoretically calculated from Odessa, Washington solar radiation. Sensitivity analyses showed that a difference in modeled D.O. of 0.16 mg/L resulted between using cloud cover data directly from the Spokane International Airport versus calculated cloud cover from Odessa, Washington solar radiation. It may make sense to collect additional, local meteorological data because this sensitivity is within the level of DO required for compliance.

5. Is the model capable of simulating the drawoff and layered dynamics of stratified flow in impoundments (i.e., without appropriate energy input to disrupt the stratification, stratified layers will drawoff basically completely prior to the next layer withdrawing, rather than the layers mixing during drawoff)?

Model Calibration

1. Page 40 of the Model Calibration report mentions that "with few exceptions, the same model coefficients were used for 1991 and 2000". In terms of using a model to predict modified conditions, the same parameters must be utilized and the calibration process should be continued until the exact same model meets pre-determined accuracy conditions. Has this been updated since the release of the Model Calibration report?

2. Need to also define the AME and RMS errors as relative or percentage error based on a realistic baseline, where the average measurement over the time period for each parameter could be used, such for water level use average depth as, as follows:

$$\boldsymbol{AME}\% = \frac{\boldsymbol{AME}}{\boldsymbol{AverageValue}} \times 100$$

(Or calculate a scaled value first and then find the AME for the scaled value)

The AME value should also indicate whether it is plus or minus the measured data.

Also, level of confidence with respect to the AME or RMS should be calculated because of the relatively few data points used to calculate the various errors. In many cases, small values on the order of micrograms per liter were cited for AME values; however, these represented difference factors of ½ and greater.

3. Need to define the instrument error for each measured and modeled parameter. For example, what is the depth gage (or water surface) device accuracy and then what was the reproducibility of the measurement in the River or Lake. Most likely, for all parameters, the instrument accuracy is far greater than the local randomness at a point in the River or Lake.

4. Table 19 in the Calibration Report needs to be expanded to include typical or observed value ranges. Calibration needs to be within ranges and similar parameters should be calibrated on the same side of the range.

5. The model's accuracy, citing AME values, with respect to Dissolved Oxygen are the following:

Data Set	**Average DO AME (mg/L)**
1991 Profile	0.88
1991 Time Series	0.94
2000 Profile	1.18
2000 Time Series	0.54

What is the instrument accuracy for measuring DO? What possible reasons are there for the major differences between each type of DO data set (0.54 to 1.18 mg/L)?

6. pH data error statistical analyses needs to be recalculated for actual value (i.e., concentration or activity of hydrogen ions) not log value. The average AME for the four sites in 1991 for pH is 0.452. This error in pH reflects a **factor of 8** difference in actual hydrogen concentrations versus simulated hydrogen concentrations.

7. In Figures 124 and 125 of the Calibration Report (NOx time series data), no sample data were available to confirm peak NOx concentrations. Also, the same figures list the ordinate as NO_3-N rather than NOx. Which label is correct? Table 33 (and Table 35) for the time series NOx data does not list the number of data comparisons. Figure 126 does not show any sample data for comparison for Julian Day 257.7, same for Figure 127, 128, 129, 130, and 131. In Figures 132 and 133, no sample data were available to confirm peak NOx concentrations, while the model underpredicted NOx concentrations by 30% for Segment 154 for Julian Days 200 to 280.

8. It is especially imperative to scale the ammonia-nitrogen data because the AME is approximately 0.03 mg/L (for 1991), which is the same order as the data. In Figures 140 and 141, only a couple data points confirmed the model's predictions for peak concentration.

9. Similar comments as Comments 7. and 8. for phosphorus (and others).

10. Table 49 in the Calibration Report is not complete.

11. Data shown in Figures 189 and 190 in the Calibration Report do not represent "calibrated" figures because the model always over-predicts the sample data.

12. Table 51 in the Calibration Report is not complete.

13. The Model Calibration report does not have a summary or listing of the level of calibration, i.e., listing of model versus measured parameter and degree of error between the two.

Model Interpretation

1. Is it being assumed that all sediment transport is "natural". This is counter to an extensive list of studies that have shown that uncontrolled agricultural practices results in significantly greater erosion/sediment transport, and this type of sediment is most likely to be associated with oxygen depleting chemicals. As mentioned above, a number of quality sediment transport models are available that define "natural" sediment tranport that could be used in conjunction with a bed load sampling study to determine if "extra" sediment is being transported through the Spokane River system.

2. Have the increased flow rates associated with ultimate permit loadings been considered?

3. How has DO in the effluent from the dischargers been modeled? Have any scenarios been considered where the DO in the effluent was raised to saturation levels at discharge?

4. Under Sensitivity Analysis, the three different re-aeration formulae resulted in a mean D.O. difference of greater than 0.30 mg/L, which is greater than the prescribed 0.20 mg/L deficit proposed by Ecology. In addition, profile data differed by "approximately 0.5 mg/L" for D.O., where this difference is ascribed to uncertainties in wind data, yet no local wind data was collected during the recent sampling periods (it is noted that the largest profile difference was in the upper 10 meters of the lake, which may not be the most critical portion of the lake).

5. Under sensitivity analysis for algal growth rate, no explanation is provided for nearly all sensitivities being negative, whether algal growth rates were increased or decreased by 50%.

6. The sensitivity coefficient shown on page 172 of the Calibration Report did not appear to be utilized in the report.

7. Are Eloika Lake and Lake Coeur d'Alene (and the respective No Source studies) representative of "No Source" loads to the Spokane River and Long Lake system? Both lakes are more alpine versus the significant agricultural impacts and inputs to both the Little Spokane River and Lattah Creek.

8. It is not clear from where the data for loads from the various permitted dischargers for ultimate permit conditions are developed. Can the exact data in terms of permitted discharge concentrations and flow rates for CBOD, etc. be provided?

Model Enforcement

1. How can a model be used to set a standard that is more precise than the model's capability to simulate? For example, for DO, as shown above, the model could only be calibrated within 0.54 to 1.18 mg/L of measured DO, yet a standard of 0.2 mg/L DO deficit is being proposed. In addition, it is not clear whether the instrument error plus randomness in the natural system is less than 0.2 mg/L.

Strictly speaking, the predictive accuracy of a mathematical construct cannot exceed (i.e., be more precise) than the basis of the mathematical construct or the ability to measure or monitor. Alternatively, one can speak of averaging to meet a deficit, where the randomness in the natural system and instrument error would be mathematically eliminated.

Compliance

1. If noncompliance is determined to occur within the Lake, how will organizations such as AVISTA be approached to provide assistance in developing ultimate solutions, because the existence of the dams provide conditions conducive for D.O. depressions?

Attachment 2

Email from

Tom Cole, Corps of Engineers

-----Original Message-----
From: Thomas.M.Cole@erdc.usace.army.mil [mailto:Thomas.M.Cole@erdc.usace.army.mil]
Sent: Tuesday, November 18, 2003 10:42 AM
To: Cusimano, Bob; scott@eas.pdx.edu; berger@me.pdx.edu; annearr@cecs.pdx.edu
Subject: RE: Scanned Info you requested

Bob,

Here are my responses concerning CTE's comments on the modeling effort and the applicability of the model to address management issues in the Spokane River and Long Lake.

1. Lack of SOD measurements

It is my professional opinion (and others as well) that the model is the most accurate measurement "instrument" of the actual SOD in Long lake. Reservoir SOD measurements are essentially meaningless for providing SOD rates for W2 because 1) SOD measurements are notoriously unreliable, inaccurate, unreproducable, and generally unrepresentative of SODs in the prototype under real world conditions, primarily because SOD measurements are temporally and longitudinally/laterally varying, so it is highly unlikely that an SOD measurement has any relationship to the actual SOD exerted at any point in time and space; additionally, the laboratory SOD measurements themselves can vary by an order of magnitude depending upon the stirring rate, which generally has no relationship to the actual time varying velocities near the sediment/water column interface that the prototype experienced; additionally, cores are always disturbed; 2) even if an SOD measurement method could be made accurate and reliable, based on time and cost constraints it is impossible to take sufficient SOD measurements over time (since they vary temporally) and over the longitudinal extent of the reservoir to provide accurate measurements for setting SOD rates in the model.

Bottom line, back calibration of the model SODs to match observed DO depletion rates is the most reliable and accurate measurement of actual SODs. Increasing or decreasing the point source discharges for any of the calibration years will not have an immediate impact on the SOD rates used in the calibration years, although there clearly is an impact on the long term behavior of SOD. There is no question that there would be a feedback of point source discharges and SOD if we had calibrated to10 years of continuous data, but this is not really relevant to the conclusions that you have drawn from the model as to the impact of the point source dischargers on Long Lake DO.

I will let you address the comments concerning where the effort for DO improvement should be concentrated between point and non-point dischargers. Regardless, the point source dischargers violate the state standards for DO in Long Lake. No improvement in the inclusion of non-point source impacts will alter that conclusion.

2. Model weakness in computing chlorophyll-a levels in Long Lake

As I have mentioned previously, the fact that the model does not exactly reproduce the upstream algal bloom is, in terms of point source impacts on Long Lake DO, a best case scenario for the point source dischargers. The fact that it does not exactly reproduce algal blooms, in terms of point source impacts on Long Lake algal concentrations, is a matter that may be needed to be addressed in more detail in the future, but Ecology currently is not making any management decisions based on algal concentrations in Long Lake, only on DO concentrations.

3. Uncertainty in properly representing nonpoint source inputs of phosphorus and organics

The reviewers need to understand that a sediment diagenesis submodel is of questionable improvement for studies such as these. A sediment diagenesis submodel is important, first and foremost, for trying to address *long term* impacts of changes in nutrient loadings (decades or longer) such as was necessary for Chesapeake Bay. For this study, the model is not being used for long term management strategies. It is being used to assess the seasonal impact of point source discharges on DO in Long Lake. As a result, a complex sediment diagenesis model is not needed for this study.

In reality, attempting to include a complex, sediment diagenesis model introduces a whole new set of problems, the most important of which is how does one specify initial conditions for the sediment compartment for organic matter and nutrients? Sediment diagenesis, as mentioned previously, is a long term process in which initial conditions dominate the solution. There currently is no accurate method for specifying initial conditions for an entire waterbody (same as measuring SODs). What is normally done is to run the model with either "representative" loadings and meteorology for multiple decades, and use the ending conditions as initial conditions. This procedure, although sounding reasonable (and is what was done in the Chesapeake Bay study) probably has very little correspondence to reality.

Additionally, based on the tact that most of the reviewers have taken regarding the review of the modeling effort, including a sediment diagenesis model would only serve to provide more red herrings regarding model calibration that would certainly be raised in an attempt to invalidate the use of the model (or any model for that matter) on this system. With regards to sediment diagenesis, KISS (Keep it Simple) is currently the correct method to start from, going to a complex sediment diagenesis model only if absolutely necessary to address the relevant issues. It is not necessary for addressing DO issues in Long Lake for this study.

4. Uncertainty in model coefficients that relate phosphorus to algal growth and the oxygen demand of algae in the subsurface layer of Long Lake

The reviewers state that the model is using 1/2 of the C/P Redfield ratio. The model is using the Redfield ratio, but the number used in the control file is the P/OM ratio, which is about 1/2 of the P/C ratio, assuming a C/OM ratio of 0.45, which is what the model is using. Regardless, as I have stated before, it is not sufficient in the review process "to point out that this or that mechanism is lacking, or this or that parameter is not accurately modeled, or this or that calibration parameter is not within an acceptable value." Since we can never specify all of the mechanisms accurately in a mechanistic water quality model, we have to determine if model inaccuracies have a critical impact on the use of the model that could render the model useless for addressing the important management issues.

Regarding the C/chl a ratio, TOC minus DOC can be an indicator of C/chl a ratios, but it can also be very misleading because the error associated with attempting to use this data can easily be greater than the current difference in the model C/chl a ratio and TOC-DOC. Trying to infer that this is a problem in the current model formulation affecting Ecology's conclusions is problematic at best.

5. Uncertainty in the proper calculation of algal growth limitations by light and nutrients

Again, what is the proper method for calculation of algal growth limitation is a moot point when it comes to the model's immediate prediction of point source discharge impacts on DO in Long Lake, since the scenarios showing the impact of point source discharges on DO will be the same regardless of the values used for any other model coefficients affecting DO. The current formulation has worked quite well for over 100 studies involving algal dynamics, and I see no reason to change it just because there are alternate formulations that have not been applied on near the number of systems as the one in W2.

However, I do agree that if the management issues eventually encompass nuisance algal growth, that more thought needs to be given as to what the impacts are of uncertainties in the model that affect algal biomass. It should be up to the reviewers to provide this information if they deem it relevant, not the modeling team.

6. DO in Spokane River at Stateline

You are much better equipped to address this issue, but it should have no impact on conclusions as to whether point source discharges violate state water quality regulations for DO in Long Lake.

Model Development

All of the comments fall under the heading of "it is insufficient to point out model deficiencies without pointing out their impacts on management decisions based on model results". None of the reviewer's comments point out how insufficiencies in model calibration parameters, model formulations, or boundary conditions invalidate the conclusion that the impacts of point source dischargers on Long Lake DO violate state DO standards.

Model Calibration

The exact same parameters were used for 1991 and 2000, with the exception of measured CBOD decay rates. Regardless, the statement that the same parameters must ***always*** be used for different data sets is wrong. Certain calibration parameters change over time, and model simulations must oftentimes reflect this. For example, if the dominant algal group in 1991 was diatoms and the dominant algal group in 2000 was cyanobacteria, the growth, respiration, mortality, and settling rates, C/chl a ratios, and a number of other parameters would be totally inappropriate to keep the same between the two years and would fly in the face of reality.

Ideally, a great deal of confidence could be placed in a model that was able to simulate the progression from a diatom to a cyanobacteria dominated system, but the lack of sufficient data for simulating the intervening years would preclude any model from reproducing the eutrophication process, even if the model was capable of reproducing the changes. Having to use different CBOD decay rates between 1991 and 2000 clearly shows the necessity of having to vary some kinetic parameters for different calibration data sets. Using the same rates would allow for valid criticism of the modeling effort by the reviewers. The requirement that model calibration parameters be the same for two different data sets is a common misconception in the modeling community.

Regarding additional statistical analyses, if these are important for the reviewers to evaluate model performance, then they should undertake the effort to compute them. It is unreasonable to expect any further effort on this part by Scott, Chris, and Rob, or Ecology, just because the reviewers want to see them. Their inclusion will not change any of the conclusions regarding point source discharge impacts on Long Lake DO. Regardless, the request for a + or - AME value is meaningless. This is the absolute mean error, the calculation of which is meant to remove the directional bias that the mean error computes.

The recommendation that pH comparisons use actual hydrogen ion concentrations is, to put it succinctly, silly. We measure pH and compare pH, period. Most of the other comments in this section fall under the category of being irrelevant for what the model is being used for, although not quite as badly as this one.

Model Interpretation

1. I am at a total loss as to the argument being presented here. Regardless, as has been pointed out throughout the course of this project and in numerous responses to reviewer's comments, how the model currently represents SOD has no impact on the conclusion that point source discharges into Long Lake result in a violation of state standards.

2. The reviewers must make some effort as to how the inclusion of this or that mechanism, choice of model coefficients, and lack of accurate boundary conditions might impact conclusions based on the model before raising them. This is the scattergun

approach to model review that just tends to obfuscate the relevant issues. The model doesn't explicitly include bacterial, zooplankton, or fish dynamics either, all of which impact DO in Long Lake, but that doesn't invalidate the usefulness of the model for this study, since their inclusion would not alter the conclusions based on current model results.

Indeed, the reviewers note in a backhand way that perceived problems with reaeration formula and DO predictions only impact DO concentrations in the epilimnion, which is irrelevant to where the DO violations in Long Lake exist that the model is being used to address. A lot more thought like this needs to go into review comments by all parties before concluding there are problems with the modeling effort that you need to address, before the model is considered a useful tool for addressing management issues.

3. Questions such as these can be relevant, but I consider issues such as these should be addressed by the reviewers, which is why the model application was provided to the reviewers. Again, Ecology has clearly shown that point source discharges cause a violation of state DO standards in Long Lake.

The above comments are applicable to most of the other comments.

Model Enforcement

The reviewers are wrong. Models can be used to address issues in which model accuracy is less than a standard. In this case, the model clearly places the point source discharge CBODs in the appropriate vertical location within Long Lake. Given the accuracy of the laboratory decay rates, the model clearly shows that point source discharges violate state DO standards. Errors associated with the model's underprediction of supersaturated epilimnetic DO, although included in the statistics, are totally irrelevant as to the accuracy of model predictions of DO standards violations in the metalimnion/hypolimnion. Model statistics that ignore epilimnetic DO comparisons would result in a much lower AME. Regardless, statistics alone cannot be used to address whether a given model is appropriate for addressing management issues.

I would like to again point out all of the preceding comments regarding the model's usefulness for computing the impact on point source discharges on Long Lake DO are red herrings, that, if appropriately addressed to the satisfaction of the reviewers (some of which would be impossible to do), would have no impact on the conclusion that current point source discharges violate state water quality DO standards.

Attachment 3

Memorandum

from

Scott Wells and Chris Berger

Portland State University

Memorandum

November 24, 2003

To: Bob Cusimano, Ecology

From: Scott Wells and Chris Berger

Re: Review comments on the Spokane modeling effort

I have included a few comments that may or may not add to what Tom has already stated. Please use what you wish freely. We still are looking at the algae-DO-nutrient issue in Long Lake.

<u>SOD</u>

This study is not compromised by the lack of SOD measurements for the following reasons:

- Oxygen depletion from the sediments as measured from SOD measurements must agree in aggregate with measured dissolved oxygen profiles data through back calculation. Hence, SOD measurements should only corroborate the measured profile data. If SOD measurements do not agree with back-calculated SOD, then there are potentially fatal issues with the test itself that must be evaluated:
  - SOD measurements are notoriously inaccurate.
  - SOD results are a function of the how the test was run and mimicked real-world conditions. Not having the correct velocity in the SOD chamber, or stirring up the sediment by the recirculation process invalidates the test. In SOD tests there must be an accurate measurement of the velocity field from where the sample was taken to use for the sediment chamber velocity.
  - SOD spatial variability is an issue where there is never a statistically assured expectation that the samples chosen represent the system in aggregate.

<u>Chlorophyll a levels in Long Lake</u>

The model's determination of chlorophyll a levels in the late summer in the recirculation region is not connected, in the short term, to the WWTP discharges in the Spokane River. The algal bloom occurs in the epilimnetic recirculation region. All summer inflows usually enter as an interflow below the epilimnion. By not reproducing this bloom in the epilimnion, the model is being conservative in not adding an additional particulate organic load to the bottom waters as a result of settling algae. Whether the model reproduces this bloom, which may be from fast recycling of nutrients or from another P source in the epilimnion, should have little effect on the dissolved oxygen predicted by the model in the interflow region. This though should be run as a model sensitivity, which we will try to do in order to illustrate this point.

Need for a Sediment Model

A complex sediment digenesis model, even though desirable from a research perspective, will not affect short-term management issues associated with the WWTP dischargers in the Spokane River. In fact by effectively decoupling the sediment processes from the inflow by using a zero order SOD model as background, the full historical impact of the WWTP dischargers may not have been addressed by the current modeling effort.

Issues with the Algae-P-Oxygen relationships

We turned on the kinetic flux algorithm and decided to determine the primary sources and sinks of DO over the summer of 2001 just for Long Lake. Figure 1 illustrates the dissolved oxygen sources. The sinks were shown in Figure 2.

The largest dissolved oxygen sinks appear to be largely driven by phytoplankton growth and mortality. Decay of particulate organic matter was the largest sink, followed by first order sediment decay. POM and sediment in Long Lake mostly originates from algae. Dissolved oxygen consumption due to zeroth-order SOD and CBODu decay was relatively smaller.

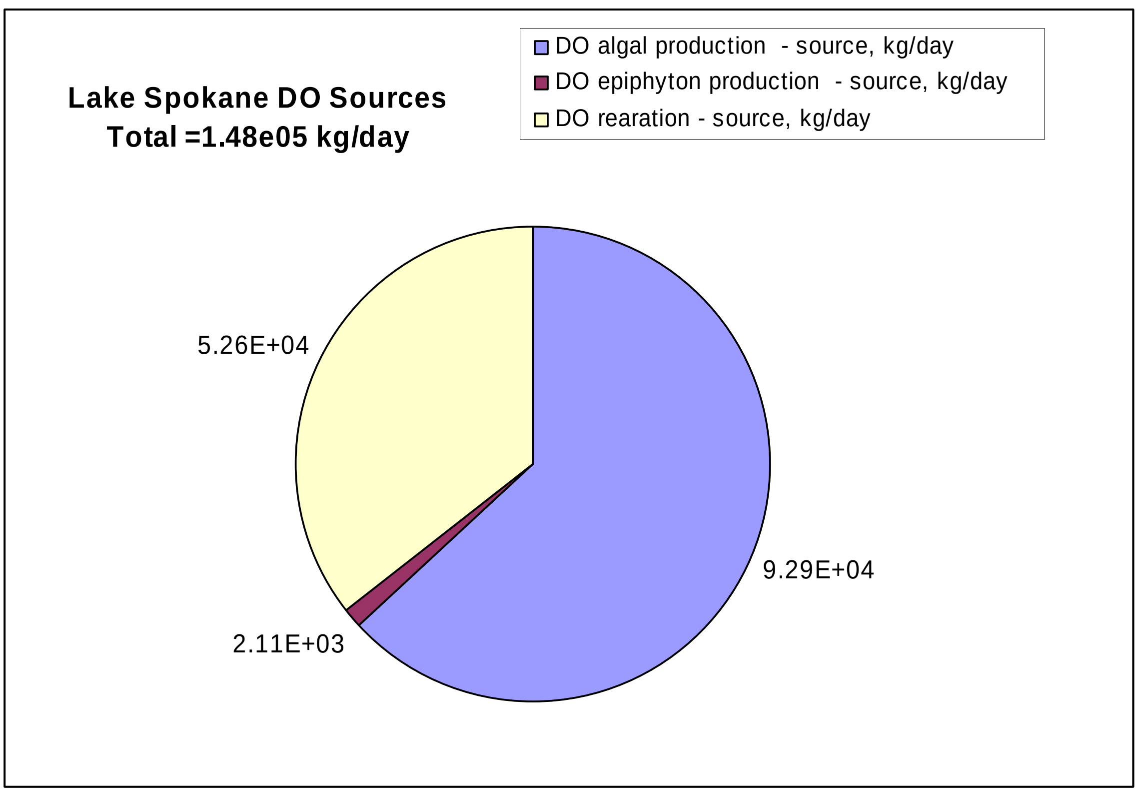


Figure 1. Dissolved Oxygen sources for Long Lake (Lake Spokane).

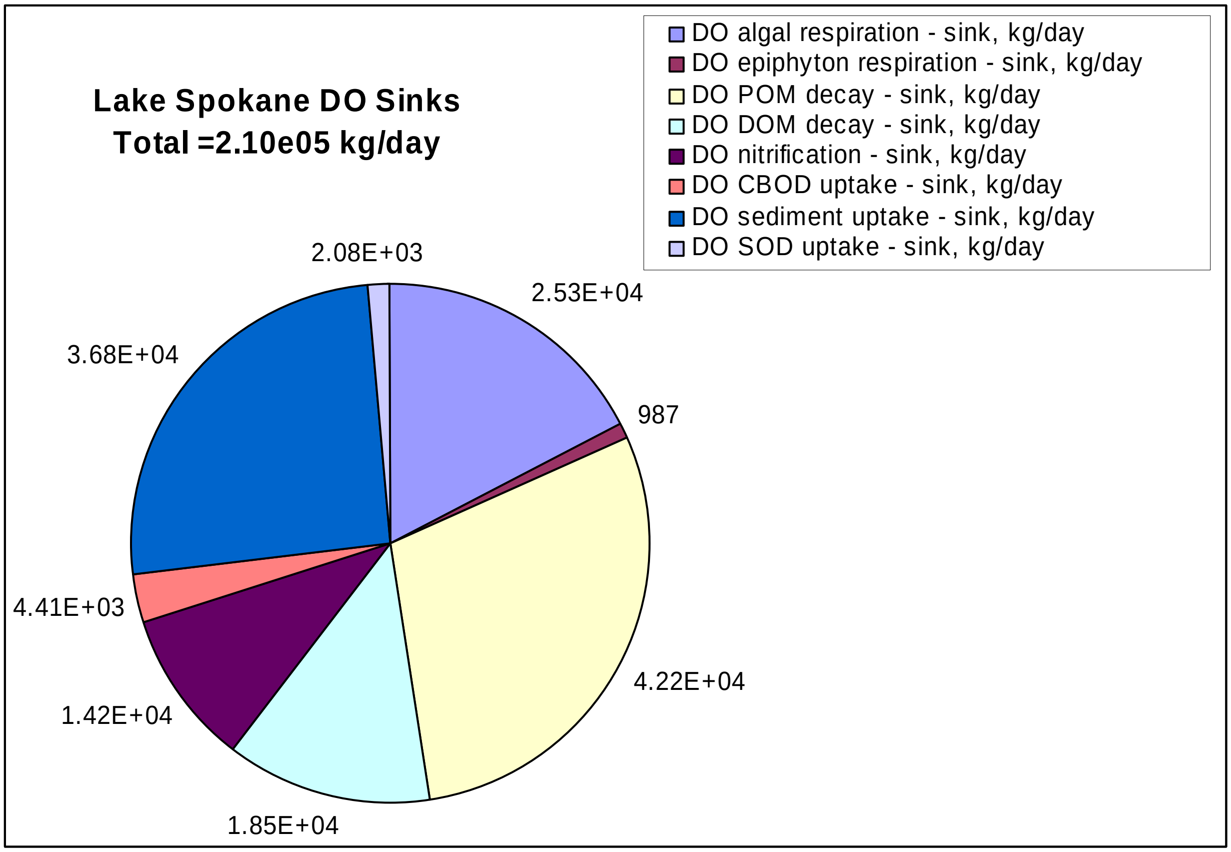


Figure 2. Dissolved Oxygen sinks for Long Lake (Lake Spokane).

We will be looking also at the inflow POM and trying to see if it is internally generated POM or if it is coming from upstream. We will look at this in more detail soon.

Light extinction

The measured light extinction at LL4 during 1991 varied from 0.5 to 2.3 m^{-1} even though the model prediction of extinction was on average 1.2 m^{-1}. It was unclear from Solerto's report how precise his estimates of light extinction were and whether he computed the light extinction using a 'beta' concept or not. Hence, there may be some small differences in approach to computing light extinction coefficients from the data to the W2 model. W2's predicted light extinction was based on summing the light extinction due to pure water with the contributions to light extinction caused by model predicted algae, POM and inorganic suspended solids. The model is not perfect in its ability to match all measured data everywhere. The proof though that in the aggregate the light extinction is reasonable has to do with model-data comparisons of temperature profiles. It is obvious looking at Soltero's light extinction data that at times there is great spatial variability.

A light saturation value of 40 W/m^2 is not uncommon. The use of 150 W/m^2 is extremely high in the range of literature values for phytoplankton. Belay (1981) assumes a value of 36 W/m^2 for a mixed assemblage of phytoplankton – see CE-QUAL-W2 manual.

Electron Activity

W2 is not a chemical equilibrium model except in the computation of the pH-carbonate cycle and its use of Alkalinity and total inorganic carbon. W2 obviously uses the electroneutrality condition in these computations.

Comment 5 on top of p. 7: W2 accounts for the shear induced mixing associated with a shear layer and accounts for selective withdrawal processes. The W2 User Manual Appendix A discusses the hydrodynamic formulations used in the model.

Comment 6 on bottom of p. 7: The measurement is pH and the model prediction is pH. There is no reason to compare anything else. We assume that those who understand pH can also understand what pH represents.