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California Regional Water Quality Control Board
North Coast Region
5550 Skylane Blvd., Suite A
Santa Rosa, CA 95403

Peer Review of Proposed Revision to Dissolved Oxygen Water Quality Objectives for the California Regional Water Quality Control Board, North Coast Region

Here I provide my assessment of the scientific basis of the "Proposed revision to dissolved oxygen water quality objectives for the California Regional Water Quality Control Board, North Coast Region. I will refer to the document itself as the "Staff Report" for brevity.

Overall, I was extremely impressed with the quality and thoroughness of the Staff Report. Establishing oxygen standards to protect fish and other wildlife is far from a simple task and the Staff Report dealt with nearly every possible complication that I could see. The key issue that the Staff Report recognized is that oxygen dynamics in streams are extremely heterogeneous across both time and space, at both fine and coarse scales. Recent technological development of reliable data loggers (datasondes) allow continuous oxygen and temperature monitoring to account for fine-scale variation in oxygen conditions (as is expected for relatively productive systems). This finer-scale monitoring allows for development of more specific oxygen objectives than could be established based on older sampling regimes involving grab sampling and Winkler titrations.

I believe that the proposed revisions are based on the best scientific information in hand. In particular, prioritizing life cycle objectives over background objectives will provide stronger and more justifiable protection for many fish species. Improved monitoring technologies also enable expansion of oxygen objectives to include weekly average limits. This will provide better protection to organisms from chronic low oxygen concentrations that may only become problematic over longer time periods. Updating water quality objectives in streams where natural conditions lead to relatively low oxygen concentrations is clearly justified. Many streams have low oxygen concentrations due simply to 'natural' physical and chemical conditions. The Staff Report represents a very strong synthesis of the state of knowledge concerning stream oxygen dynamics and the requirements of stream dwelling fishes.

The increased complexity of the water quality objectives proposed here will likely require more staff and funding to implement and monitor compared to the old objectives. Although I was asked to comment only on the science, I feel compelled to say that developing an intricate set of objectives is only a worthwhile enterprise if the appropriate research and implementation funding are provided to put them into action. One important aspect of this will be in more intensive data management demands. Moving to a more spatially and temporally explicit monitoring scheme will produce orders of magnitude more data than the old monitoring methods based on grab samples and titrations. To ensure that these new data streams are useful for management, adequate additional funding must likely be directed towards updating the data management systems as well.

The summary of oxygen requirements of all the major fish species in this region of California was strong and thoroughly justified the focus of developing oxygen criteria that protect salmonids. All other species in this region can reasonably be considered less sensitive to low oxygen conditions compared to the salmonids. So, although the focus on salmonids is clearly a bias, it is a scientifically justified bias.

In the section below I provide my responses to the specific questions posed by Holly A. Lundborg regarding four components of the proposed revisions to the oxygen criteria. I have paraphrased the questions.

1) *Is a 3 mg/L correction factor to account for oxygenation of gravel for egg and alevin requirements overprotective?*

The depression of intragravel oxygen concentrations relative to overlying water is expected to vary widely among streams and reaches depending on factors such as gravel porosity, sediment-based oxygen consumption, stream gradient, sediment organic matter content, and temperature. Thus, applying a <u>constant</u> correction factor to account for reduced intragravel oxygen concentrations compared the overlying stream is certainly a simplifying assumption that is not well substantiated. It is somewhat surprising that there does not exist more scientific information about gravel and flow characteristics that determine the oxygen depletion in stream gravels. Although applying a correction factor of 3 mg/L to overlying water concentrations to ensure that eggs and alevins receive adequate oxygen concentrations for development will provide strong protection, this constant correction factor does not seem to be exceptionally well-supported from the scientific literature. I would not say that this correction factor is 'over-protective' but it might be unnecessary and in some cases unattainable given the hydrologic, biological, and thermal conditions of streams. For instance, a moderately productive stream may not sustain 11 mg/L DO under natural conditions throughout a daily light cycle because of high organic matter or nutrient inputs that would fuel high oxygen consumption rates at night. It might be worth investing in the science to determine how much intragravel oxygen concentrations deviate from overlying water sources as a function of a variety of physical, chemical, and biological features of streams. Without further information on how much gravel oxygen concentrations can be depressed in a variety of stream types, I can not provide concrete advice on how to proceed. I do believe that an 11 mg/L objective will be difficult to achieve under natural conditions in many streams.

2) *Is 85% oxygen saturation at natural receiving water temperatures a scientifically supported criterion for assessing background DO conditions?*

I am not entirely clear on this question. My impression is that you are asking whether 85% saturation based on natural temperatures is adequate for fish protection, or whether it should be 90%. Given the relatively wide range of the degree of saturation in natural streams, it is a difficult judgment call to state that 90% is substantially better than 85%, or that 85% is not adequate. In a cold stream, 85% may provide exceptional protection for fish. In a warm stream, 85% may not be adequate because of the compounded effects of a decline in overall oxygen concentration in the warmer water <u>and</u> the increased metabolic rates of fishes in warm waters. I think it is safe to say that at temperatures below 20C that the 85% saturation is a reasonable criterion for establishing baseline conditions that are not stressful to fish. (Although this may not achieve the intragravel requirements) The key point is to embrace the fact that there will be wide variation in what constitutes 'natural' oxygen conditions among streams. An 85% criterion will capture more of the natural variation than will the 90% criterion.

3) *Are the methods for determining natural receiving water temperatures adequate?*

The Staff Report offers several alternatives for estimating the natural temperature regime of streams. The first of these methods is to use reference streams that are know to not have been impacted to any large extent. It is proposed that headwater streams are most appropriate for this type of comparison because their water source is ground water which has a relatively constant seasonal temperature. Although I agree that headwater streams may have the most simple suite of perturbations to them, I do not necessarily agree that ground water can be assumed to be their ultimate water source. If the reference stream method is used to assess natural temperature regimes of another stream, extra care should be taken to ensure that the reference and the study streams have similar hydrologic properties because it can not be assumed that they will both be dominated by ground water sources and, therefore, have predictable flow and temperature conditions. Although this method may appear the easiest to use, it is critical that appropriate reference streams are chosen to establish ‘natural’ conditions. The other methods suggested are probably more robust.

When using the simple mixing equation to determine reach temperatures, care must be taken to ensure that groundwater is not a large source to the downstream reach. If a reach of interest has a finite set of well-defined inputs, then the simple mixing equation should be adequate for a relatively short (or short residence time) downstream reach. Of course, this method only works for establishing ‘natural’ conditions if all of the upstream tributaries are also in a ‘natural’ condition.

I believe that the computer models suggested as the more complicated ways to establish the ‘natural’ temperature regimes are the most appropriate way to establish baselines. Staff should (and may have already done this) compare the model predictions to observed temperature regimes in set of streams of various sizes and geomorphic and hydrologic features that are deemed ‘natural’. If the models capture the key aspects of the seasonal changes in thermal conditions, then this is probably the easiest justified approach to establish natural thermal regimes.

4) *Is the procedure for determining if natural conditions prevent attainment of life cycle-based DO objectives adequate?*

The Staff Report suggests three steps to use in order to determine whether ‘natural’ conditions prevent the attainment of life cycle objectives. Recognition of this fact demonstrates the thoroughness of the report as it will likely be common that near pristine streams may often not meet life cycle DO requirements because of a suite of factors such as naturally high levels of nutrients and organic matter, or due to low flow regimes. The first proposed step is to use continuous monitoring to establish that non-compliance is an issue. Clearly this step will require careful Quality Assurance/Quality Control protocols (something that actually was not discussed much at all in the Staff Report) to ensure that datasondes are not fouled and properly calibrated. The second step is to produce a conceptual model that specifies all of the anthropogenic and natural conditions that control oxygen conditions in a specific site. This model, as proposed, qualitatively lays out a range of potential drivers of local oxygen conditions. This seems like a logical step in this assessment. The final step is to use a more formal mathematical model (equation) to estimate what the natural oxygen conditions at a site should be given the conceptual model specified in step two. This also seems like a logical step in this process but the specifics of the models to be used are not given so it is difficult to determine whether this final step is achievable. Producing a conceptual model that allows partitioning of the various natural and anthropogenic sources and sinks of oxygen is a relatively straightforward exercise. However, developing a quantitative model that will allow partitioning of anthropogenic from natural sources and sinks is not a trivial task. Although such a model could certainly be developed for any stream in California, these models are generally quite data-intensive and will also require considerable technical expertise to develop and use. Without more details on what the model to be used will look like, or specifically which data will be used to calibrate and parameterize such models, I really can not comment on whether this procedure will actually achieve what it intends to.

My final comment pertains to the monitoring plan. Although not stated explicitly, I am assuming that hydrologic flows are monitored simultaneously with temperature and oxygen concentrations. If not, this would be a serious oversight. Given the expected changes in hydrologic patterns in response to ongoing climate change, it will be critical to account for changes in hydrology and how these are associated with observed changes in oxygen and temperature. Data describing oxygen and temperature alone will be difficult to interpret as changes could be driven by internal features such as changes in biological productivity, or driven by external features and in particular changes in flow regimes.

In summary, I believe that the Staff Report provides scientifically reasonable justification for changing the oxygen water quality objectives for the North Coast Region of California. I do have some minor reservations as outlined above. However, I think these reservations reflect a lack of scientific information in some particular areas and believe that the proposed changes make sense as a way to increase protection of populations that are currently threatened.

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