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June 28, 2013

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Via email: Naomi.Feger@waterboards.ca.gov

Subject: State and Federal Contractors Water Agency Comments on the Draft SanFrancisco Bay Nutrient Conceptual Model, Draft External Nutrient Loads to San Francisco Bay, and Draft Review of Scientific Approaches Supporting NNE Assessment Framework Development for San Francisco Bay

Dear Ms. Feger and Dr. Senn:

The State and Federal Contractors Water Agency (SFCWA) appreciates the opportunity to provide comments on the three draft San Francisco Bay (SFB) nutrient reports – Draft San Francisco Bay Nutrient Conceptual Model, Draft External Nutrient Loads to San Francisco Bay, and Draft Review of Scientific Approaches Supporting NNE Assessment Framework Development for San Francisco Bay. We recognize that these three reports represent a great deal of work by the San Francisco Estuary Institute, Southern California Coastal Water Research Project and their technical teams, and we are pleased to see this level of effort addressing nutrient impacts in the San Francisco Bay Estuary. These reports are important elements of the Regional Water Quality Control Board's Nutrient Management Strategy, and will form the basis for moving forward. As such, it is critical that the reports incorporate all relevant scientific literature and address all potential nutrient impairments.

Our overall comments on the reports are described below, and our detailed comments on each report are attached. We previously provided you with a copy of the Nutrient Science Summary Technical Memorandum that summarizes and analyzes relevant nutrient literature. We are providing it again with this set of comments for your convenience, as it includes information and citations that will be helpful for addressing some of the information gaps in the draft reports.

Draft San Francisco Bay Nutrient Conceptual Model (10 pages of comments)

Our comments on the draft Conceptual Model report address important issues that are missing from the report, and places in the document where the scientific information is not, in our opinion, presented in a balanced manner. Several parts of the document imply that impairment is not currently occurring. While we all might agree that San Francisco Bay (SFB) has not shown widespread classic signs of nutrient over-enrichment, this report should not predetermine that SFB has been resistant to nutrient impacts. One of the key questions this paper and the larger NNE process are designed to assess is whether the estuary is impaired. Further, we are concerned that some sections of the report appear to give more significance to some published scientific studies and analyses over others. It is appropriate for the report to acknowledge uncertainties, and different scientific viewpoints and hypotheses, and to

recognize that our understanding of these complex issues is evolving. Finally, the draft report does a good job of describing the way forward for the SFB Nutrient Strategy and the recommendations capture the major issues.

Draft External Nutrient Loads to San Francisco Bay (1 page of comments)

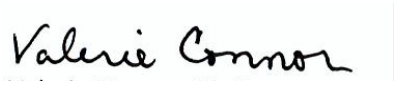
The draft Nutrient Loads report does a good job analyzing the available nutrient data for discharges and surface waters to develop a comprehensive picture of nutrient loads to SFB over time and season. The report identifies important gaps in the data collected past and present, and provides recommendations as to what is required to adequately monitor nutrient loads moving forward. Our comments mainly address some of the introductory text for the report.

Draft Review of Scientific Approaches Supporting NNE Assessment Framework Development for San Francisco Bay (4 pages of comments)

Overall, the draft Scientific Approaches for NNE Assessment Framework report provides a good summary of the various approaches being used throughout the United States and Europe to develop nutrient criteria or numeric endpoints. In the next version of this report it would be useful to include some assessment of the different approaches and their limitations and/or applicability to the SFB Estuary. One significant limitation of the report is that the discussion of assessment frameworks is focused on classic views of eutrophication (i.e., phytoplankton biomass and dissolved oxygen, etc.), and does not address the other adverse effects of nutrient over-enrichment, such as changes in species composition and reduced primary production. We recognize, however, that this is a general failing of all of the eutrophication assessment approaches and therefore lack of inclusion of these effects is a more general problem than a specific problem with this report.

We would be happy to meet with you to discuss or provide clarification for any of these comments. We also look forward to continuing to work with you as the SFB Nutrient Strategy is developed.

Sincerely,



Valerie Connor, Ph.D.
Science Program Manager

cc: Dr. David Senn, SFEI, davids@sfei.org
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SFCWA Comments on "*San Francisco Bay Nutrient Conceptual Model*," Draft May 1, 2013

The State and Federal Contractors Water Agency appreciates the opportunity to provide comments on the draft San Francisco Bay Nutrient Conceptual Model report. We recognize that this draft represents a great deal of work by SFEI staff and the technical team. We also understand that this report is still a draft, and that further substantial editing will occur. We hope the comments below are helpful in identifying some of the issues that may need or should have further development. The comments are mainly focused on big issues and on suggested text revisions where corrections are needed, rather than on areas where editing or polishing may be necessary.

In this regard, we would note that the document as a whole is unevenly referenced. Some places have clear and suitable citations; in many other places the text has no references; in some sections the authors have leaned heavily on a single reference only. We recommend that a more thorough referencing and development of the literature be part of the polishing effort. This would make the report a much stronger document.

The draft report has done a good job of describing the way forward, and the recommendations are well done. While we recognize there is ongoing debate as to the relative weight to place on one or another of the recommendations, as a whole the major issues are captured.

In our detailed comments below, we note several places where the presentation of the published work on nutrient impacts that are not classic signs of nutrient over-enrichment, such as ammonium inhibition of nitrate uptake by phytoplankton and the potential role of nutrient stoichiometry in shaping community composition, are incomplete. It is important for the report to include complete descriptions and referenced studies for these potential nutrient impacts so that they may receive appropriate consideration in the conceptual model and SFB Nutrient Strategy. We refer you to the Nutrient Science Summary Technical Memorandum that we provided previously for a summary of the published studies on these topics.

We also note that the issue of the effects of nutrient discharges have been considered in some detail and at some length by the State Water Resources Control Board in connection with the permit proceeding for Sacramento Regional County Sanitation District. For example, after discussing the extensive literature in the record before it, the State Board, among other findings, concluded that "the District's discharge of ammonia affects designated critical habitat" for listed species and that "ammonia toxicity to copepods is likely a factor adversely affecting candidate, threatened or endangered species populations (sometimes referred to as pelagic organism decline…" Order WQ 2012-0013 In the Matter of Own Motion Review of Waste Discharge Requirements Order No. R5-2010-0114 [NPDES No. CA0077682] For Sacramento Regional Wastewater Treatment Plant (Dec. 4, 2012) at 22 and 23. This regional effort should take into account the scientific findings and direction taken by the State Board.

Several parts of the draft report imply that impairment is not currently occurring. While we all might agree that San Francisco Bay (SFB) has not shown widespread classic signs of nutrient over-enrichment, this report should not predetermine that SFB has been resistant to nutrient impacts. One of the key questions this paper and the larger NNE process are designed to assess is whether the estuary is impaired. Suggested text revisions to make the report more balanced in its presentation of the issues are included throughout these comments.

Synopsis, page i, First Paragraph, suggested text revisions: "San Francisco Bay (SFB) has long been recognized as a nutrient-enriched estuary, but one that has exhibited resistance to some of the classic symptoms of nutrient overenrichment, such as high phytoplankton biomass and low dissolved oxygen. However, SFB may be exhibiting symptoms of nutrient overenrichment according to our newer understanding of nutrient responses, such as changes in species composition and suppression of beneficial productivity. SFB receives high nutrient loads from treated wastewater effluent, agricultural runoff, and stormwater. Dissolved inorganic nitrogen (DIN) and phosphorus (DIP) are essential nutrients for primary production that supports SFB food webs. However DIN and DIP concentrations in SFB greatly exceed those in other US estuaries where ~~water quality has been~~ impaired~~ment~~ by nutrient pollution has been recognized."

Synopsis, page i, Second Paragraph, suggested text revisions: "Scientific studies and monitoring over the last 40 years have played a critical role in discovering and characterizing the factors that have historically given SFB resistance to the classically identified adverse impacts of high nutrient loads: high turbidity, strong tidal mixing, and abundant filter-feeding clam populations, all of which have limited the efficiency with which DIN and DIP are

converted into phytoplankton biomass and severely limited food web productivity in the northern estuary."

Synopsis, page i, Third Paragraph, suggested text revisions: "However, recent observations indicate that SFB's resistance to high nutrient loads is weakening, and that SFB has been experiencing more subtle, though perhaps no less serious, symptoms of overenrichment for decades. ~~and that~~ ~~c~~Conditions are trending toward increased productivity and potential impairment along multiple pathways. These observations include: a 3-fold increase in summer-fall phytoplankton biomass in South Bay since 1999; frequent detections of algal species that have been shown in other nutrient-rich estuaries to form harmful blooms; an unprecedented red tide bloom in Fall 2004; and studies suggesting that the chemical forms of nitrogen can influence phytoplankton productivity and composition."

Synopsis, page i, second bullet: The report should not try to qualify how plausible each scenario is given the current debate. Some might think this scenario is more plausible. Revise text as follows: "A second, ~~equally~~ plausible scenario is that SFB's resilience will continue to decline until moderate to severe impairment occurs in some subembayments."

Page 1, First paragraph, suggested text revisions: "San Francisco Bay (SFB) has long been recognized as a nutrient-enriched estuary, but one that has exhibited resistance to some of the classic symptoms of nutrient overenrichment, such as high phytoplankton biomass and low dissolved oxygen. However, recent observations suggest that SFB's resistance to high nutrient loads is weakening and that SFB has been experiencing more subtle, though perhaps no less serious, symptoms of overenrichment for decades. The combination of high nutrient concentrations and changes in environmental factors that regulate SFB's response to nutrients has generated concern about whether the Bay is trending toward, or may already be experiencing, nutrient-related impairment."

Page 1, Line 33-34: The way this sentence is currently constructed makes it sound like SFB is not impaired. Revise text as follows: "However DIN and DIP concentrations in SFB greatly exceed those in other US estuaries where ~~water quality has been~~ impair~~ed~~ment by nutrient pollution has been recognized (Cloern and Jassby, 2012)."

Page 1, line 39-41, suggested text revisions: "Scientific studies and monitoring over the last 40 years have played a critical role in discovering and characterizing the factors that have given SFB its resistance to the classically identified adverse impacts of high nutrient loads."

Page 2, lines 45-46, suggested text revisions: "However, recent studies indicate that the response to nutrients in SFB's subembayments is changing and that SFB has been experiencing more subtle, though perhaps no less serious, symptoms of overenrichment for decades."

Page 2. The concern is not just about the estuaries changing response, but also due to our changing understanding of SFB's response to nutrients. Revise text as follows:

> Page 2, Line 52-55: "To address growing concerns about SFB's ~~changing~~ response to nutrient loads, the San Francisco Bay Regional Water Quality Control Board (Water Board) worked collaboratively with stakeholders to develop the San Francisco Bay Nutrient Management Strategy[1], which lays out an approach for gathering and applying information to inform management decisions."
>
> Page 2, Line 64-65: "The indications of ~~changing~~ SFB's response to nutrients have come to the fore at a time when the availability of resources to continue assessing the Bay's condition is uncertain."

Page 2, line 64. The report should explain how the continuation of these resources is uncertain. Is USGS no longer going to conduct this monitoring? More specificity in this discussion will lead to more specific recommendations.

Page 5, First paragraph: This paragraph does a much better job capturing the important distinction between classic impairment and the impairment SFB may be experiencing.

Page 6, first paragraph: Glibert 2010 and Glibert et al 2011 observed changes in phytoplankton community composition prior to the establishment of *Potamocorbula*, and delta smelt declined significantly in early 80s. Revise text as follows: "In Suisun Bay, extremely low phytoplankton biomass (Figure 3.7a) and highly-altered phytoplankton community composition (Figure 3.7b) have defined the system since as early as the late 70s ~~since 1987 when the invasive clam Corubula Potamocorbula amurensis became widely established.~~ The low primary production rates and the current species composition provide insufficient support for the food web and may be contributing to the dramatic decline in sentinel fish species in the Bay/Delta ~~since 2000~~ (pelagic organism decline, POD; Baxter et al 2010). Recent studies have argued that elevated levels of NH_4^+, high nutrient concentrations in general, and altered N:P play an important role in creating this low-biomass (Dugdale et al., 2012; Parker et al. 2012a,b; Wilkerson et al., 2006) and poor-quality (Glibert et al., ~~2012~~2011) food supply situation."

Page 6, second paragraph: The 2004 bloom and the salt pond bloom described below qualify as major blooms. In addition, Peggy Lehman, Department of Water Resources, has detected major blooms in the Delta. Revise line 227

as follows: "Although their abundance has not regularly reached levels that would constitute a major bloom or impairment, the fact that they are present and that nutrients are abundant means that HABs could readily develop should appropriate conditions present themselves."
Page 7, top paragraph: It should be noted that many shallow systems develop hypoxia on a diel basis. When sampling is carried on midday such a phenomena may be missed. In situ sensors are necessary to capture such dynamics.

Page 7, line 269, suggested text revisions: "Thus, both the severity of events and whether they are natural vs. anthropogenically-induced or anthropogenically exascerbated need to be considered."
Page 7, lines 289-291, suggested text revisions: "Finally, other nutrient-related effects on SFB food webs, have been proposed, such as high phytoplankton cellular N:P that adversely affects copepod populations feeding on those cells (Glibert et al., ~~2012~~2011), direct NH_4^+ toxicity to copepods (Teh et al., 2011), and altered nutrients ratios encouraging the spread of invasive macrophytes and invasive bivalves (Glibert et al 2011)~~(???)~~."

Page 7, line 290: The text indicates "These latter issues do not receive in depth treatment in this report, but may need to do in future iterations." These latter issues (the effect of high phytoplankton cellular N:P, direct ammonium toxicity, and the relationship between high nutrients and the spread of invasives) should not keep getting deferred for consideration down the road. They are equally important as the issues mentioned before them. In addition, ammonium has been linked to die back of native sea grasses in other estuaries. A journal query will return numerous references. That should be an area of investigation in SFB, especially since there is low areal coverage by native sea grasses.

Page 14, Section on stratification: It should be noted that little is known of fine scale stratification in SFB. For example, thin layer development can be an important factor in the vertical structure of phytoplankton accumulation in some systems.

Page 15. Resuspension is important not just from the perspective of suspended sediment, but may also be important in resuspending any phytoplankton cysts or benthic primary producers.

Lines 701 and 714, Correction: change figure 7.1 to figure 6.1.

Lines 702-704. Statement about dissolved organic nitrogen needs to be changed. Dissolved and particulate organic nitrogen are abundant in virtually all aquatic systems—not just pristine. Only in eutrophic systems are DON concentrations exceeded by those of DIN.

Page 17-18. The section on N transformations does not address the very important process of dissimilatory nitrate reduction to ammonium (DNRA). It is reasonable to say that at the current time there are no data on such rates in SFB (but studies are underway); however, it is increasingly recognized as an important source of NH4 in coastal lagoonal systems and therefore must be mentioned. It would similarly be appropriate to mention the process of anammox. It is also important to note that abiotic exchange of NH4 can occur as a function of changes in salinity and pH.

Page 18-19. The abiotic exchange of P must be further described- salinity, pH, etc. exchanges should be developed in more detail.

With regard to the previous two comments, the work of Gao et al. (2012) should be consulted.

> Gao et al. 2012. Effects of cyanobacterial-driven pH increased on sediment nutrient fluxes and coupled nitrification-denitrification in a shallow freshwater estuary. Biogeosciences 9: 2697-2710.

Line 817. Change species to functional groups.

Page 20. The document should mention atmospheric sources for the sake of completion. There may be no available data, but this should at least be acknowledged.

Page 21-22. DNRA should be mentioned here. While nitrification and denitrification may be the major fluxes, the potential importance of DNRA should not be ignored.

Line 932-33. There is no evidence provided that spring and fall are times of higher phytoplankton uptake. This statement should be supported with references or removed if it cannot be documented.

Page 23, Line 980, etc. – In "Section 7, Primary Production and biomass accumulation" it would be useful to recognize the overall relative productivity of the San Francisco Bay, as compared to other large estuaries, which are far more productive.

Lines 1025-1027. Please note that not all POC is phytoplankton – even if it is "phytoplankton derived". Some rewording would help to clarify here.

Page 24, Temperature. Rates are temperature sensitive, but it is worth noting that some have a temperature maximum—the way the text reads it implies that rates inevitably go up as temperatures rise. Shallow waters can heat up. For some species, this may be positive, for others this may be a negative effect.

Page 24-25, Light. This section could be improved by incorporation of some of our new insight into biological behavior and light. The early part of the document does an excellent job of discussing the interactivity of light with various other factors. There is no doubt that light is a major controlling factor- and that many P vs I curves and modeling support this. This is backed by considerable data. However, there are several aspects of biology or physiology that are not well captured – and not well understood when we think about light control. These include mixotrophy- the ability of "phytoplankton" to obtain some/much of their carbon by grazing. We know now that virtually all flagellates have this capability. This is an important notion because of the species compositional shift away from diatoms (the last 2 years excepted) that has occurred in the Bay Delta over the past several decades. Mixotrophic nutrition may be synergistic with primary production, not just additive. What would the addition of a mixotrophic component in the model of light control look like? Also, flagellates are swimmers. Their movement through the water column may be regulated in a number of complex ways. This has consequences for phototaxis, and/or aggregation. Some species (esp. dinoflagellates) aggregate in thin layers (often at comparatively low light). Mixing time has other important implications for regulation by light—short-term excursions to the surface have physiological consequences. These responses are also highly variable depending on the nutritional status of the cells. Many of these relationships have not been well characterized for the SFE or Bay Delta, but are worth recognizing in the broader context. As we attempt to conceptualize how ecosystems may change with future scenarios, capturing the biological complexity of these increasingly understood nutritional patterns and the range of dynamic regulation of photosynthetic responses may be important. We are also learning that light may play a role in regulation of toxin production of some HAB species. Thus, in addition to our classic understanding of light limitation, there may be a more complex and nuanced perspective of light as a controlling factor when viewed from the perspective of changing species composition.

Lines 1054 and following. Phytoplankton have a very large capacity to adjust to, or adapt, to fluctuating light and reduced irradiance levels. Not having high light is not necessarily a bad thing for some phytoplankton. Just like the case of temperature, the text should reflect the fact that more is not necessarily better. Many types of phytoplankton do not do well under elevated irradiance levels.

Lines 1082 and following. Same comment as above. Here also it is important to note that many phytoplankton (dinoflagellates especially) have the ability to thrive under extended periods of darkness. Mixotrophy is one strategy but there are others. Also, biology affects the extent to which stratification matters to the cells: sinkers and swimmers. Those with swimming capability can move. Some cells need turbulence; others fare far better under more quiescent conditions; turbulence is not even reflected as an important factor here.

Lines 1090-1091 and figure 7.3A. This text is conceptually too simplistic. It is strongly recommended that the authors review the literature on dynamic balance of photosynthesis.

Line 1140. C:chl ratios are not known to be constant, and should not be assumed as constant.

Page 27. It is important to note that excess nutrients can be stressful. One does not only have to be at the low end of the curve for physiological stressful responses to occur.

Page 27, Line 1156. The discussion of "low production rates due to elevated ammonium" does not give sufficient weight to the peer reviewed studies conducted by Drs. Dugdale, Wilkerson, and Parker that have shown the relationship between primary production and reductions in nutrients. The work does more than argue or hypothesize or suggest, as the draft implies, but has documented the relationship. Their work has been outlined in

our supporting Nutrient Science Summary Technical Memorandum – and is among the analyses that were cited and relied upon by the State Board in adopting the final Sacramento Regional County Sanitation District permit.

Page 27- Ammonium inhibition. Note that it is only the "regional" (line 1171) scientific community that does not have a consensus on the paradoxical role of ammonium. The role of ammonium in the repression and inhibition of nitrate uptake is exceptionally well characterized in the broader plant and physiological literature, which is referenced in our supporting Technical Memorandum and in the literature cited there. While we understand there is a debate about the role of ammonium in ecosystem structure, there is no lack of understanding of the biological mechanism at the cellular and physiological levels.

Page 28, line 1198: There are numerous potential reasons why phytoplankton community composition has changed, including changes in nutrients. Revise text as follows: "This size-dependent filtration efficiency may ~~play be the reason behind~~ contribute to the observed shifts in the proportions of phytoplankton biomass toward smaller size-classes in Suisun Bay since the 1980s (Kimmerer et al., 2012), and may have similarly contributed to shifts in phytoplankton community composition, as discussed in Section 9."

Page 28, lines 1218-1220 states, "…researchers are exploring the potential for higher freshwater flows in the Delta to drive *Corbula* out of Suisun (Greene et al., 2011)." Using freshwater flows to "drive *Corbula* out of Suisun" is not a viable management option. Among other reasons, there is not enough freshwater in this system to effectively control them. It would take sustained flood type flows every year. Refer to Thompson, JK and F Parchaso. 2010. *Corbula amurensis* Conceptual Model. In addition, strong evidence indicates that it would not address the issue of invasives, as not only would the freshwater *Corbicula* distribution expand into the fresher areas, but also, unless very high freshwater flows are maintained in every year, *Potamocorbula* would merely move downstream in high flow years only to recolonize, possibly at even greater densities, in subsequent lower flow years.

Page 28, Line 1230-1238. The discussion of this analysis as having "determined" (in contrast to the way in which the Dugdale and Glibert work is presented) highlights a concern about the emphasis and bias of the presentation. The interpretation reported by this researcher in this article is certainly open to debate.

Page 30- There is no mention of groundwater nutrients and MPB.

Page 31, line 1336- It should be noted that not only persistent, but episodic hypoxia can also be stressful.

Page 32, lines 1387 etc. Discussion of DNRA should be included here also.

Page 32: The section on oxygen production and consumption makes very little mention of bacteria or bacterial rates. This is a significant omission. The section on oxygen should also make reference to the potential for carbon limitation, pH changes and the like.

Page 35- Section on Phytoplankton Community Composition. We recommend that this section be reviewed with particular care as we believe it needs considerable editing. There are numerous significant factual errors which are highlighted in our comments that follow.

Line 1531- This statement about only cells >5 µm available as food for copepods is not true. Some small cyclopoids can graze on picoplankton. Much is being learned about small copepods and their juvenile stages with respect to size selection (which also may vary depending on whether they are autotrophs vs heterotrophs); picoplankton may be important for copepod nauplii.

Line 1536. Good to see mixotrophy mentioned here.

Lines 1550 and following. Do the functional size categories include picoplankton? The report has very little reference to them.

Page 36, line 1583 mentions a Phytoplankton Composition working group, but does not specify who is part of the working group or other details about its work, such as what is their purpose, when it was formed, who formed it, who participates, etc.

Page 39 Line 1677 and following, and line 1689: We agree that the simple light limitation model should be updated to include not only PFT response functions but it may be important to include a mixotrophy term as well.

Page 39 Lines 1707 and following: The comment on biphasic and complicated kinetics is fine—but importantly, this is backed up by a substantial body of literature (not just Collos). Also, it must be underscored that kinetics of N uptake are not fixed entities—cells adjust their kinetic parameters as a function of many factors. Their physiological kinetic flexibility is analogous to their photosynthetic flexibility—conceptualizing these as attributes that can be compared across species, times and places is erroneous. The same species growing under nutrient limited conditions and nutrient saturated conditions will have different kinetic responses.

Page 39, Section 9.2.6, Physiological Factors. The Nutrient Conceptual Model should consider the role of contaminants. We recommend associating this conceptual model with the DRERIP Chemical Stressors, and Aquatic Foodweb conceptual models (http://www.dfg.ca.gov/erp/cm_list.asp). While there are areas in the report where contaminants are discussed, the document appears to solely consider "direct" toxicity of contaminants, even though it then goes on to state "or some combination of these stressors" (lines 2059-2068). Both sublethal toxicity and contaminant mixtures are somewhat overlooked. Interactions of contaminants with ammonia toxic modes of action can be appreciated in the statement on lines 1758- 1760. Any effect of ammonium on cell membrane transport and integrity can directly affect the uptake and elimination of other contaminants.

Page 40- top 2 paragraphs. It is unlikely that these factors may have any significant role in this system.

Page 40, line 1749: The ratio of ammonium to nitrate also may regulate biomass and composition. Revise text as follows: "Two other nutrient relationships have been proposed as regulators of both total biomass and community composition. Dugdale et al. (2007) have proposed that elevated ammonium concentrations from wastewater discharge is suppressing diatom productivity, while Glibert (2010) and Glibert et al. (2011) have argued that N:P and NH4:NO3 ratios are indirectly controlling community composition."

Page 41, last paragraph. This is a much too simplistic description of nutrient stoichiometry and its potential role in shaping community composition at all trophic levels in the SFB. This section needs to mention the wealth of literature from other estuaries on the role of nutrient stoichiometry in community composition. Dr. Glibert is not the only one, and certainly not the first one to describe this. We urge the authors to refer to the Nutrient Science Summary Technical Memorandum for further consideration of these issues and would be pleased to discuss them further.

Page 41, line 1811-1814. This statement is in error. Cloern et al. did not object to CUSUM as a method to evaluate time series data. There has never been a critique of that statistical approach. It was a very limited interpretation of that approach that was the subject of the critique by Cloern. In fact, CUSUM as a technique has been promoted in many time series analyses. In addition, Glibert et al. 2011 came to the same conclusion using different statistical methods, and thus the debate over the use of CUSUMs in Dr. Glibert's 2010 work is moot. Accordingly, the following text should be deleted: . ~~This analysis is based largely on a statistical metric called cumulative sum analysis, and has been criticized by others as flawed (Cloern and Jassby et al. 2012; but also see Lancelot et al. 2012).~~

Page 42, lines 1820-1821. The idea that nutrient ratios can't control if neither is limiting has been debunked by numerous researchers. Refer to Glibert et al 2012 and references therein. Glibert, PM, TM Kana, and K Brown. 2012. From limitation to excess: the consequences of substrate excess and stoichiometry for phytoplankton physiology, trophodynamics and biogeochemistry, and the implications for modeling. Journal of Marine Systems, http://dx.doi.org/10.1016/j.jmarsys.2012.10.004. Delete text as follows: "The authors argue that decadal changes in DIN:DIP ratios correlate with declines in diatoms and chlorophytes, and increases in dinoflagellates, because diatoms and dinoflagellates also exhibit different intrinsic N:P ratios. ~~There are two~~ One potential issue~~s~~ with this argument~~. First, so long as N and P are saturating, the ratio should have no direct impact on species composition, other than by selecting for the organism with optimal growth (loss). Second,~~ is that chlorophytes have a higher N:P ratio than either diatoms or dinoflagellates, suggesting that chlorophytes should be dominant under these conditions (Figure 9.10)."

Page 42, lines 1831-1833 states, "While Glibert et al. (2011) conclude that the invasion of *Corbula* was triggered by gradual shifts in ecosystem nutrient stoichiometry, others have pointed to the invasion as coincident with the rapid decline of diatoms in San Francisco Bay (Figure 9.5)." These ideas are not mutually exclusive and should not be presented as such.

Page 42, section 9.2.7. This section misinterprets the conclusions of Glibert. Presenting the ideas of Glibert et al. (2011) as "strikingly different" is a mischaracterization of their paper. Instead, the text should be adjusted to

recognize this, as noted by Glibert et al. (2011): "This interpretation does not negate the importance of ecological invasions, habitat changes, multiple stressors and food web complexities, but adds a mechanism to those interpretations through biogeochemistry and organismal stoichiometry. Ecological stoichiometry affects systems by setting elemental constraints on the growth of organisms. This, in turn, affects food quality and the relationships between predators and prey. The biota modify the environment through nutrient regeneration which differs from organism to organism based on their elemental requirements and nutrient sources. .. In the Bay Delta, evidence is clear that top-down grazing of phytoplankton by *Corbula* exerts a strong control on phytoplankton biomass… The ecological stoichiometric interpretation does not preclude strong top-down control of selected component organisms, nor ballast water exchange as the mechanism of introduction. The distinction is that, at the overall ecosystem level, the structuring of species is affected by alterations in nutrients and ecosystem biogeochemistry."

Page 42, line 1851-1855, Revise text as follows: "The calanoid copepod *Limnoithona tetraspina* increased rapidly in the 1990s to become the numerically abundant zooplankter, presumably due to predator avoidance, low respiration, and a dietary preference for bacteria and mixotrophic ciliates, which were in turn stimulated by the shift from diatoms to flagellates and cyanobacteria (Figure 9. 11) which may in turn have been stimulated by a shift in nutrient forms and ratios."

Page 43, Line 1866. It is good to see these complex interactions between top down and bottom up underscored.

Page 43, lines 1873-1875. Glibert et al 2011 are not the only one, and certainly not the first one to propose stoichiometric controls on community composition. There is a large body of literature from other systems that also describe this. Just as the draft report appropriately relies on literature from other systems for trace metal and vitamin influences, it would likewise be appropriate to recognize in presenting stoichiometry those other researchers of other systems, in addition to the work done by Dr. Glibert in the SFB-Delta (and elsewhere). Accordingly, we suggest the draft draw upon the references from the Technical Memorandum provided with these comments and that the text be revised as follows: ~~The e~~Ecological stoichiometry ~~hypothesis proposed by Glibert et al. (2011) assumes~~ also describes a series of interactive effects, ultimately stemming from changes in nutrient forms and ratios.

Page 43, lines 1880-1882: Statements like this predetermine the relative importance of light and nutrients. That is the fundamental question this paper and the NNE process is asking. Revise text as follows: "A specific example of the potential for interactive effects focuses on light-nutrient-photosynthesis interactions. ~~There is clear evidence for light limitation of phytoplankton productivity in San Francisco Bay, while it is generally accepted that macronutrients are not limiting to productivity.~~"

Page 43, lines 1888 and following. While the authors have attempted to incorporate some physiological regulation in their explanation here, this text is an example of over-reaching. Diatoms do reduce nitrate to ammonium, and the ammonium may be released. However, there is no evidence that "much of this N" is released—some yes, much no. The scenario on lines 1890-1895 should be deleted as this is not based in fact, at least for diatom efflux.

Page 43, lines 1901 and following. This paragraph is incorrect. Kudela made the conclusion of urea uptake based on direct measurements of urea and based on 15N uptake rates. It was not inferred.

Page 44, line 1912. The list should include the physiological state of the cells, since kinetics are not fixed entities.

Page 44, Section 9.2.10. Either this section or section 9.2.6 needs to include a more complete discussion of the literature relating nutrient form to HAB formation, competitive advantage over other species, and production of algal toxins. A small subset of this literature is described in the Nutrient Science Summary Technical Memorandum that we previously provided.

Page 45, Summary section. Line 1978 must be revised. San Francisco Bay is not unique or "somewhat" unique in terms of having long term data – many systems do.

Lines 1982 and following. The comment about complexity only reflects that the underlying mechanisms have not been well understood. Here again the dynamic balance models of Kana, Geider and MacInyre are useful—the multiple photosynthetic regulatory models that many invoked to explain the complexity of photoadaptation were discarded once the underlying single regulatory process came to light.

Line 1993 and following. This section should be revised. The models of Follows and others do not incorporate evolution or natural selection—this is a common misconception. They were termed the "Darwin" models colloquially, but it is a bad name and a bad reflection of what these models do. These models do allow for self-assembly and interactions, based on a series of set criteria. There is no question that the development of numerical biological models is needed. Many new mechanistic models are advancing and the new data that are emerging will enable a new generation of models.

Page 47, section on Invasive Plants: We suggest that this section be revised to include only what can be supported by the scientific literature. As drafted, the text includes a number of conclusions about what is regulating these plants and what effects they have with no references to support the stated conclusions. While there may be sufficient nutrient quantity to dismiss total nutrients as a contributing factor, studies do suggest that the form and ratio of nutrients may play a significant role. For example, the existing ammonium and N rich water may give these plants a competitive advantage over native species. For example see,

> Feijoo, C., M.E. Garcia, F. Momo, and J. Tpja. 2002. Nutrient absorption by the submerged macrophyte Egeria dense Planch: Effect of ammonium and phosphorus availability in the water column on growth and nutrient uptake. Limnetica 21: 93-104
>
> Glibert, P.M., D. Fullerton, J.M. Burkholder, J.C. Cornwell, and T.M. Kana. 2011. Ecological stoichiometry, biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and comparative systems. Reviews in Fisheries Science, 19(4):1-60
>
> Reddy, K.R., J.C. Tucker, and W.F. Debusk. 1987. The role of Egeria in removing nitrogen and phosphorus from nutrient enriched waters. J. Aquat. Plant Management 25: 14-19.

Page 47, Section on Direct NH4+ toxicity to copepods. If this section is going to list the hypotheses for low zooplankton abundance other than the subject heading of direct ammonium toxicity, then nutrient stoichiometry effects should also be included in the list. However, the value of the first paragraph of this section is questionable. Direct toxicity to copepods does not need to be linked to the POD to be relevant, or to be considered an impairment. Also, the discussion of Dr. Teh's work should recognize that the State Water Board has reviewed his work, and found it sufficiently credible to rely on the analysis in issuing the Sacramento Regional County Sanitation District permit. In fact, in discussing his work, the State Board concluded that "[t]he available scientific evidence indicates that ammonia toxicity to copepods is one of the contributing factors compromising the integrity of the entire waterbody." State Board Order at 22.

Page 47, line 2083, revise text as follows: "Even 26 µM is still substantially higher than typical ambient concentrations in SFB (Figure 6.4); although it is commonly observed in the lower Sacramento River."

Page 48, First paragraph, suggested text revisions: "The future trajectory of SFB's response to nutrients is uncertain. One plausible trajectory is that SFB maintains its current ~~level of resistance to~~ response to high nutrient loads and no further degradation occurs. A second, ~~equally plausible~~ scenario is that ~~SFB's resilience continues to decline~~ nutrient impacts increase until moderate to severe impairment occurs along one or more pathways in some subembayments. The SFB may currently be impaired by nutrients and the highly elevated DIN and DIP concentrations Bay-wide provide the potential for ~~future impairment~~ more negative responses to develop."

Page 48, Line 2107-2113, revise text as follows: "However, primary production rates ~~are~~ may be ~~strongly~~ limited by light availability throughout most of the year due to high turbidity and strong tidal mixing of the water column. In some subembayments and habitats, filter feeding clams exert a further strong control that limits phytoplankton biomass accumulation. In addition, algal species that have formed harmful or nuisance blooms in other estuaries are regularly detected in SFB. There are ample nutrients to support their proliferation."

Page 48, line 2118. Phytoplankton assemblages have already shifted to suboptimal. Lehman, Cloern, Winder, Jassby, Glibert have all reported this shift. Perhaps a more appropriate question is how do we know when this shift is an impairment? Revise text at lines 2116-2119 and 2126-2129 as follows: "What would have to change to sufficiently relax physical or biological controls and allow current nutrient loads to be more efficiently turned into phytoplankton biomass, HABs and NABs to become common or widespread occurrences, or phytoplankton composition to further shift toward suboptimal assemblages?"

Page 48, Line 2131, suggested text revisions: "Addressing this question, using the conceptual model as a guide, pointed us toward several categories of change that have the potential to ~~move areas of SFB toward impairment~~ change how areas of SFB respond to nutrients:"

Page 49, Lines 2132-2137: A change in the form, ratio or total load of nutrients discharged to the estuary could also change the estuaries response. Add a bullet point to the list of categories of change: Changed form, ratio or total load of nutrients.

Page 49, Lines 2146-2156, suggested text revisions: "We considered current trends in indicators that have generated concern that ~~SFB is on a path toward nutrient-related impairment~~ SFB's response to nutrients is changing."

Page 51, Lines 2256-2257: This statement assumes we know the cause of these NABS. Unless 2004 is the only time we have had clear, calm days, factors other than clear, calm days must play a role otherwise there would be more NABs reported than the one in 2004. Revise text as follows: "If sufficiently long-term and reliable climate/weather records exist for the Bay area they could be examined to determine if the conditions that allowed this bloom to develop ~~(multiple clear, calm days in a row)~~ have been occurring more frequently during periods of low tidal energy in the Fall."

Page 51, Line 2260. The suggestion that there remains a lack of consensus in the scientific community should again be clarified as a lack of consensus among those in this region. Elsewhere, the scientific consensus is clear, as reflected in the literature summarized in the Nutrient Science Summary Technical Memorandum.

Page 52, line 2306: Changes in nutrient form, ratio and total load could also change the following responses and should be included in the list of factors under each:

- *High biomass and Low DO in deep subtidal areas:*
- *HABs and NABs:*

Page 53, Section on low phytoplankton biomass due to elevated NH_4^+: Shifts in rainfall patterns could also change dilution and transformation and loss rates. In addition, changes in nutrient form, ratio and total load could also change this response (e.g. with population growth, or changes in treatment processes).

Page 53, Line 2357. The phrase "albeit at considerable cost, in particular at higher removal efficiencies" should be deleted. The reference to the cost associated to the removal of nutrients (presumably through nitrification and denitrification, although that is not specified) is not appropriate for and well beyond the scope of this document and not founded on any specific data or analyses. Consideration of the cost and cost effectiveness of new discharge technology requires data and analysis specific to the particular POTW, as well as the overall potential economic activity associated with the investment in new infrastructure. Moreover, what is meant by "higher removal efficiencies" is not defined and to start to define that in this context is well beyond the scope of this analysis.

Page 54, Lines 2375-2376: Revise text as follows: "While under current conditions phytoplankton growth and biomass accumulation are limited ~~much of the time by light, and biomass accumulation is further controlled by clam grazing~~ by several factors, if those constraints on growth were relaxed, higher biomass production and accumulation would ensue."

Page 54, Line 2384. The direct contribution to Suisun Bay from POTWs does present a material contribution to the loading of nutrients, as detailed in the Draft Nutrient Loading Study. Thus the draft goes too far to suggest that the effectiveness may depend to a "large extent" on whether reductions are made elsewhere as there are no data or other analyses used to support that assertion. The text later suggests that the seasonal contributions from the Delta "need to be considered" (line 2412) which is a more reasonable characterization. Moreover, aside from the general contribution to nutrient loadings, the direct discharge can have significant localized impacts which must be considered, such as the direct impacts on the known habitat of threatened smelt in the channel into which the Vallejo POTW discharges.

Page 57, Lines 2522-2524: Revise text as follows: "Based on recent permit requirements, Sac Regional ~~will likely be~~ is required to both nitrify and carry out biological nitrate removal before discharge, with upgrades implemented by ~~around 2022~~mid 2021."

Page 58, Line 2572, suggested text revision: “Continuation of current trends (CT) ~~leading to an impaired state~~”

Page 58, Lines 2580-2584, suggested text revisions: “Change Scenarios (CS) that may lead to ~~impairment~~ a change in response**.** CS.1 High phytoplankton biomass and low DO in LSB and South Bay due to decreased sediments and longer stratification or further changes in nutrient loads, forms or ratios (climate change and wetland/salt pond restoration). CS.2 Increased frequency of HABs or NABs in all subembayments due to longer stratification and reconnection with salt ponds and wetlands or further changes in nutrient loads, forms or ratios.**”**

Page 59, Lines 2608-2612, suggested text revisions: "The future trajectory of SFB’s response to nutrients is uncertain. One plausible trajectory is that SFB maintains its current ~~level of resistance~~ response to high nutrient loads and no further degradation occurs. A second, ~~equally plausible~~ scenario is that SFB’s ~~resilience continues to decline~~ response to nutrients changes until moderate to severe impairment occurs along one or more pathways in some subembayments. The highly elevated DIN and DIP concentrations Bay-wide provide the potential for ~~future impairment to develop~~negative responses."

Page 59, Line 2618, suggested text revisions: "Although evidence is consistent with conditions in SFB moving toward a critical juncture, ~~widespread impairment due to nutrients is~~the classic signs of eutrophication are not currently occurring."

SFCWA Comments on "*External Nutrient Loads to San Francisco Bay*," Draft, April 9, 2013

This document summarizes the existing nutrient data and uses this to analyze and quantify nutrient loads to the SFB Region. It has identified valuable gaps in data collected past and present, and provides recommendations as to what is required to adequately monitor nutrient loads from 2012 onwards. The analysis is well done and will be an important document going forward. It would be interesting in the next iteration of this document to compare to the watershed analysis of Sobota et al.

> Sobota et al. 2009. Influences of climate, hydrology and land use on input and export of nitrogen in California watersheds. Biogeochemistry doi: 10.1007/s10533-009-9307-y.
>
> Sobota et al. 2011. Linking dissolved and particulate phosphorus export in rivers draining California'a Central Valley with anthropogenic sources at the regional scale. J. Envir. Quality 40: 1290-1302.

While this report focuses on the technical analysis of nutrient loading data, there is some introductory discussion about the role of nutrients in the SFB Estuary. It is important that this discussion be balanced in its presentation of potential nutrient impacts and the published studies that address nutrient impacts. For example, on page 2, lines 50-53, the report states that "other recent studies have argued" about the nexus between nutrients and productivity and composition. There is a long history of studies well documented in the literature regarding water bodies from around the U.S. and the world that "demonstrate" the relationship between nutrients and declines in productivity and changes in species abundance, and many of these studies are summarized in the Nutrient Science Summary Technical Memorandum provided again with these comments.

The report states on page 5, that the stormwater load estimates are "highly uncertain." Later in the report the authors state the analysis suggests stormwater loads have the potential to "contribute substantially" to DIN and PO4 loads in some locations and at some times during the year. The approach used to estimate stormwater loads appears appropriate given the lack of data. However, given the uncertainty in the stormwater load estimates, the report should focus on recommendations to address the data gap rather than potential stormwater loads.

SFCWA Comments on "*A Review of Scientific Approaches Supporting NNE Assessment Framework Development for San Francisco Bay*," Draft May 2013

The draft provides a summary of the various approaches being used throughout the United States and Europe to develop nutrient criteria or numeric endpoints. Overall, the summary is well done and captures the basic elements of all of the various approaches. There are several efforts for water quality limits also in use in China and various other Asian countries, but most of these have at least some similar elements.

The document does a good job identifying and describing a lot of different approaches to assessment frameworks. However, much of the information that is needed for the SF Bay community to select ones for further consideration is missing. It would be helpful to include some assessment of each of the approaches. What are their strengths and weaknesses? Are they all equally applicable to San Francisco Bay? Many are listed under the heading of "non-regulatory assessment frameworks." How have these been used/applied? In regions where they have been applied, has there been any progress toward achieving the targets or thresholds? Has there been any documentation of ecosystem response? The Introduction states that the document will provide recommendations for further consideration; however, these recommendations are not immediately apparent.

Eutrophication is defined in the document as "the acceleration of the delivery, in situ production of organic matter, and accumulation of organic matter." However, the term is used throughout the document as <u>the</u> adverse effect of nutrient over-enrichment. There are other adverse effects of nutrient over-enrichment that are not included in this definition, such as changes in species composition and reduced primary production. A global search for the term should be done and corrections made to not limit what is considered an adverse effect to acceleration of organic matter. Some of these misuses are identified in the specific comments below.

The section on indicators under further consideration for the SF Bay NNE Assessment Framework is missing a significant amount of information on the role of nutrient concentrations and/or ratios on ecological condition and beneficial use impairment.

It should be noted in the section in which various analytical methods are described (pp 14-15) for chlorophyll, productivity or taxonomic assemblages that where there are several techniques in use, resulting values are not always interchangeable. Newer approaches for assessing primary productivity may not equate to older 14C values. When making comparisons between data derived from several analytical approaches these differences must be understood. Such differences may have important implications for reference data.

All of these various approaches are based on more or less classic views of eutrophication: chlorophyll, dissolved oxygen, phytoplankton assemblage changes, seagrass, etc. It has proven exceptionally difficult to capture the more subtle ecosystem stress responses in such approaches (although some of the approaches do take nonlinearities into account). Some of the stress responses that are exceptionally difficult to capture are those that are highly dynamic. For example, systems that may develop hypoxia on a diel basis may not be well captured, nor will toxicity events that are uncoupled from phytoplankton abundance (for many HAB species the cells may not be the most toxic when they are the most abundant). Also, while residence time is captured to some degree in some of these approaches, differential response by system typology still has a long way to go before being fully incorporated into existing approaches. This is just a general comment- but worth keeping on mind especially for systems that do not have classic eutrophication characteristics.

One concern we have is that neither contaminants, nor invertebrates are included in the NNE framework. Both of these play an important role in determining the quality of a waterbody, the latter being direct ecological responses affected by contaminant and nutrient, and resulting primary production interactions. Many contaminants, for example Contaminants of Emerging Concern (CECs) and Pharmaceutical and Personal Care Products (PPCPs), are sourced from wastewater treatment effluent, and refineries and stormwater runoff, along with the nutrients in question. Assessing one without the other, while somewhat informative, loses validity because of this. Even at magnitudes below recognized toxicity ammonium and nitrate can significantly affect the impact of contaminant mixtures (additively, synergistically or antagonistically). Toxic pollutants are solely referred to toxins, which can also interact with anthropogenic contaminants. For a more realistic cause-effect approach, contaminants and invertebrates (and respective seasonality) should be considered as key factors to the nutrient assessment framework.

Specific Comments:

- The document contains a lot of tables that are cut and pasted from supporting documents. In many cases, further explanation of the contents of the tables is needed either in the text or caption for the table in order for the reader to understand the information presented.
- The document has numerous incomplete or improper sentences and would benefit from a careful proofreading.
- Page 1, ¶1, Revise sentence, "First, it would establish a suite of numeric regulatory endpoints based on the ecological response of an aquatic waterbody to nutrient over-enrichment ~~(eutrophication, e.g., algal biomass, dissolved oxygen)~~." There are other responses to nutrient over-enrichment. Either eliminate the bracketed text, or move the "e.g." to in front of "eutrophication."
- Page 1, ¶4, states that the document will recommend a suite of options for further exploration. If the document does this, the recommendations are not apparent. Suggest highlighting them, perhaps in their own section.
- Page 2, near bottom, "Philosophically, each step requires the review and input of the stakeholder advisory group." Why is stakeholder input only "philosophically" required? Suggest revising to "~~Philosophically, each step requires the review and input of~~ The stakeholder advisory group will be provided an opportunity for input at each step." Also, recommend calling out when input from the expert panel will be sought.
- Page 3, ¶2, It's not clear what is meant by the term "alternative states." Suggested revision, "Analyses will focus on identifying how data on indicators or combinations of indicators can be used to identify ~~alternative states~~ ecological condition and/or beneficial use support and how decisions on data aggregation across temporal and spatial scales affects the results of the assessment."
- Page 3, ¶3, Incomplete sentence, "Workgroup participants will to develop the scientific foundation for the assessment framework, specifying to the degree possible:"
- Page 4, ¶4, Eutrophication: Revise last sentence as follows: "These co-factors can include hydrologic residence times, mixing characteristics, water temperature, light climate, grazing pressure ~~and~~, in some cases, coastal upwelling, etc."
- Page 9, bullet 2. Revise as follows: "*A weight of evidence approach with multiple indicators will produce a more robust assessment of ~~eutrophication~~ impairment.*"
- Page 10, ¶4. The report states that "The review found four types of indicators met all evaluation criteria and are designated as primary: dissolved oxygen, phytoplankton biomass, productivity, and assemblage, and cyanobacterial abundance and toxin concentration (all subtidal habitats), macroalgal biomass and cover…" This is currently confusing and needs more specificity as to which the four types are.
- Page 10, ¶2. Revise as follows: "4) has an acceptable signal: noise ratio to assess ~~eutrophication~~ impairment."
- Page 11, 5th row, Second column designation of "co-factor" is not previously described. 3rd column use of term eutrophication should be replaced with "impairment".
- Table 2.1. The ammonium and urea row in the table says the action is to formulate a working group of "SF Bay scientists". We recommend that the working group be comprised of scientists with expertise in the role and effects of ammonium in estuarine ecosystems, not simply from the SF Bay region.
- Table 2.1 row on seagrass habitat should also acknowledge the role of nitrogen form (e.g. ammonium) on seagrass beds. There have been several studies linking declines in native seagrasses with increases in ammonium-nitrogen.
- Page 14, ¶1, Revise, "Because of their direct link and rapid response to nutrient additions, phytoplankton are considered a primary symptom of ~~eutrophication~~ nutrient over-enrichment and have been used extensively as a gauge of ecological condition and change"
- Page 14, ¶3, revise, "Chlorophyll-a is measured as a way to estimate the active phytoplankton biomass and is used extensively as an indicator of ~~eutrophic condition~~ impairment for estuarine waters."
- Page 14, last¶, "Phytoplankton blooms are expected to increase in frequency, magnitude, duration and spatial extent as water bodies continue to experience nutrient over enrichment [*Bricker et al.*, 2003]. Bloom duration can be directly quantified using continuous monitoring data. Frequency, magnitude, and spatial extent are typically assessed heuristically in the field and binned into groups (periodic versus

episodic for frequency, and high, moderate, low and very low for spatial coverage and magnitude) [*Bricker et al.*, 2003]."

- Page 15, ¶2, "Production in marine waters is influenced by numerous factors, including the supply of nutrients, light, temperature, flow regime, turbidity, zooplankton grazing and toxic substances."
- Page 15, ¶4, "Changes in phytoplankton community composition are expected to occur ~~as eutrophication develops~~ in response to changes in nutrient loading to estuarine environments."
- Page 16, the discussion of nutrient concentrations and/or ratios is missing a description of effects of nitrogen and phosphorus on primary production even when neither nutrient is limiting. It is also missing a description of how the ratio of nitrogen to phosphorus and the form of each (e.g. nitrate-N versus ammonium-N) can lead to undesirable shifts in community composition and total productivity. All of these have been discussed in depth in the Nutrient Science Summary Technical Memorandum that we previously submitted.
- Page 16, ¶3, Revise, "Relatively recent shifts in our conceptual understanding of ~~eutrophication~~ the effects of nutrients [*Cloern*, 2001; *Devlin et al.*, 2007a; *S J Painting et al.*, 2007] indicate that estuaries can have complex responses to nutrient inputs,"
- Page 17, ¶2, "Although relevant for nutrient-response modeling of SF Bay, we choose not to include a synthesis of this work in our review." It is not clear what "this work" refers to in this sentence. Is it the work related to dissolved oxygen endpoints?
- Table 3.1, 3rd row, not aware of a Lake Mead in Hawaii.
- Table 3.1, 5th row, both bullets describe lakes which do not thermally stratify. Should one of these describe lakes that do stratify?
- Page 21, ¶4, references Table 3.5 for studies comparing status results for the same system using difference assessment frameworks. Table 3.5 only summarizes the procedures used in the different frameworks. It would be informative to selection of a framework for SF Bay to provide more detail on how the different frameworks resulted in different findings.
- Table 3.3 needs additional explanation either in the text or the table caption. For example, what do "nutrient load related to impairments" and "integrated final rating" columns refer to in Table 3.3?
- Page 30, ¶2, states, "TRIX is based on the assumption that eutrophication processes are mainly reflected by changes in the phytoplankton community, which is typically only true for coastal waters and estuaries dominated by deep subtidal habitat." Do *Giovanardi and Vollenweider*, 2004 or *Vollenweider et al.*, 1998 state that this is typically only true for coastal waters and estuaries dominated by deep subtidal habitat, or is that an assessment by Sutula et al? What evidence supports that statement? Also, the equation and description of TRIX do not support the statement that TRIX is based on this assumption since phytoplankton community is represented equally with oxygen saturation and nutrient concentrations.
- Page 31, Figure 3.1 should be moved closer to where it is referenced in the text (page38).
- Page 32, bottom references Table 3.3. I believe this should be Table 3.9.
- Page 36 states, "Use of nuisance and toxic blooms has not seen wide-spread use because of uncertainty in linkage to anthropogenic nutrients." Is this assertion from OSPAR 2005, or from Sutula et al? There is certainly plenty of literature linking HABs and HAB toxins to anthropogenic nutrient loading, much of which is cited in the Nutrient Science Summary Technical Memorandum that we previously provided.
- Table 3.12 only appears as a box of different colors in the file I downloaded; there is no supporting text and several blank pages following.
- Table 3.13 does not appear at all in the draft report.