Review of

The Lake Tahoe Watershed Total Maximum Daily Load (TMDL) for Sediment and Nutrients

Prepared for the California Regional Water Quality Control Board

TMDL/Lahontan Basin Planning Unit

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This review is designed to meet the requirements described in a memorandum prepared by Doug Smith, Chief of the TMDL/Lahontan Basin Planning Unit, California Regional Water Quality Control Board, Lahontan Basin, dated 12 November 2008 and revised 4 June 2009. The purpose of the review, as given on page 3 of the memorandum, is to determine whether the scientific portion of the proposed basin plan amendment is based upon sound scientific knowledge, methods, and practices. The memorandum specifies eight issues that are to serve as the focus of the review, and directs the reviewers to specific sections of the draft TMDL report, the TMDL technical document, and supporting documents for information to be reviewed. This review is organized around the eight issues identified in the memorandum.

I) Fine sediment particles as the primary cause for impairment of clarity.

a. Draft TMDL report: comments.

1. The TMDL text of special interest here (Section 3) is poorly crafted in that it is awkwardly presented and in some places confusing or factually incorrect. This defect does not invalidate the section as a contribution to the TMDL, but it would be better if the text were revised so that it can be understood more easily and be free of misleading or incorrect statements (see below).

2. The opening statement, on page 3-1 contains a number of errors. Nutrients are not examples of particles, contrary to the text. The reference to "floating" algae is off the mark; the main concern for Lake Tahoe would be suspended algae (phytoplankton) in open water and attached algae (periphyton) near shore. Also, it is unlikely that leaves would be among the organic particles found in Lake Tahoe; breakdown products of leaves might appear in small amounts.

3. Conventions set by the regulatory agencies appear to distinguish between transparency

and clarity. This distinction, however, is not common knowledge and should be explained in the text. The report should state that, for purposes of this TMDL, transparency will be understood to refer to the secchi depth measurement and clarity will be assumed to refer to the extinction coefficient, as estimated by measurements of irradiance in the water column. The two are quite closely related, but the effect of particles on transparency is somewhat more drastic than it is on extinction coefficient, in that particles cause a cloudiness in water that interferes with the perception of objects even where there is enough light for vision.

4. The text associated with Figure 3-1 is erroneous, as is the figure itself. The text states that water does not absorb light. This is patently incorrect (see TMDL technical report). Pure water absorbs light and also scatters light. The proportion of light absorbed or scattered depends on wavelength. Particles also both absorb and scatter light, and do so differentially with respect to wavelength. Although the diagram in Figure 3-1 comes from a reputable study (PhD dissertation), it apparently misled the author of the TMDL draft, and should be either corrected or eliminated.

5. The opening page of Section 3 identifies pure water and particulate matter as factors that explain the decline of light with depth in the lake (although the relative mechanisms of decline caused by scattering vs. absorption are not explained). A key omission here is the role of dissolved organic matter, which has an additional effect on the absorption of light in water. This effect is most pronounced where humic and fulvic acids are present in water. These materials are derived from watersheds (soils) primarily. They are highly chromatic in that they cause rapid light extinction when present. They are present in all waters, but obviously are not abundant in Lake Tahoe,

which otherwise would not have such high transparency (see TMDL technical report). Mention of this occurs as an aside later in the Section, but a reader who is unaware of the CDOM effect may be confused.

6. Figure 3.3 is difficult to interpret. What is the assumed abundance or mass per unit volume of particles upon which this graph is based? The graph is meaningless without a more complete explanation of the underlying assumptions or of the observations that are portrayed here.
7. Figure 3-4 also cannot be easily interpreted based on the labels (see also TMDL technical report). The scattering effect of pure water is not labeled on the graph. Inorganic particles are labeled "sediment" although sediment is the name for all particles and not just inorganic particles. Organic particles are termed "algae" although it has already been stated that organic particles include other items.
8. On page 3-4, a reference is made to phytoplankton primary production before 1850. The wording of the sentence suggests that researchers were studying primary production before 1850. The author means to say that researchers have estimated production that occurred prior to 1850, but without measuring it (see the TMDL technical document).
9. On page 3-4, the box explanation of primary production is not very clear. The organisms in question need to be capable of photosynthesis. The byproduct is organic matter (a better term than "food" in this context).
10. On page 3-7, the last sentence in paragraph two could be a bit misleading. "Mixing" is used in two ways here: with reference to the seasonal mixing, which does not always reach the bottom of the lake, and with reference to mixing of the entire water

column, which occurs at multiyear intervals. The last sentence seems to say, but does not intend to say, that seasonal mixing occurs on an irregular basis. It would be better to state that Lake Tahoe shows an annual deep mixing that has seasonal regularity, but that mixing of the entire lake volume occurs on an irregular basis at multiyear intervals.

11. Page 3-8. At the bottom of page 3-8, periphyton is defined as “attached filamentous algae.” Periphyton includes all attached algae, not just filamentous species. References to “excessive” algae and “extra” nitrogen or phosphorus are a bit difficult to interpret. It would be better to say that the amount of periphyton in a given environment may increase if concentrations of phosphorus and nitrogen increase.
12. Section 8 comes through more clearly than Section 3, although it does raise a number of questions, as explained below.
13. On page 8-1, the first of a number items refers to the simulation of “secchi depth clarity.” Because Section 3 made a distinction between transparency (secchi depth) and clarity (extinction coefficient), the reversion to use of secchi depth as an index of clarity in this chapter is confusing and inconsistent.
14. In Figure 8-1, the output of the upper part of the flow diagram is shown as total pollutant load. Actually, this load is more correctly referred to as total load. Only a portion of this total is traceable to pollution. We cannot count every ounce of phosphorus, nitrogen, or suspended solids as pollution. Also, in the same diagram, there is a reference to CDOM, which comes in from the watershed mostly. It is good to have this component in the model, but the means of estimating it is not given in the text, nor is any information given on the treatment of CDOM in the model.

Presumably it is trivial, but some explanation is required.

15. Table 8-2 is given as proof of validation for the lake clarity model. The model predicts secchi depths within a very narrow range (23.1-23.9) whereas the observations fall in a considerably broader range (20.5-23.8). The model shows a consistent directional bias, which is problematic for any model. Furthermore, the observed and the modeled values are not significantly correlated with each other, i.e., the model is not capturing the causes of variation, which is its main purpose (Figure 1).

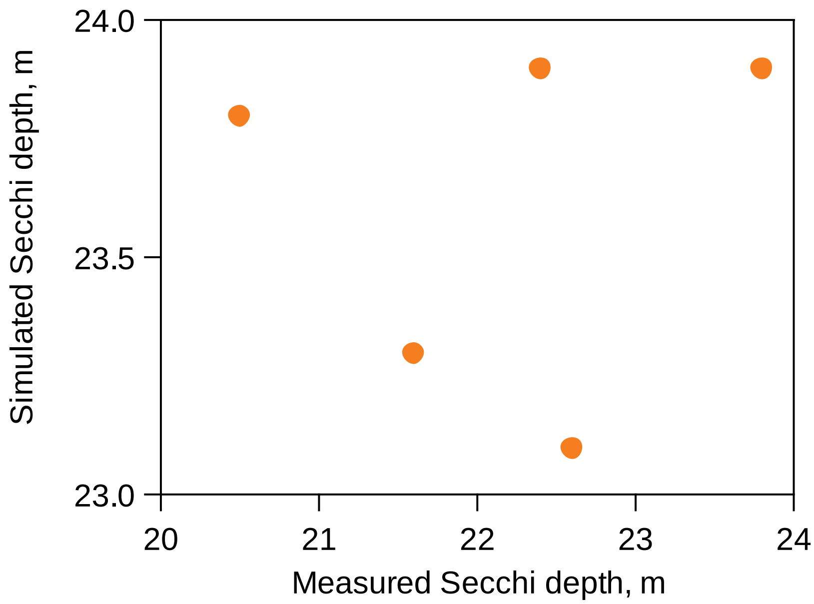

Figure 1. Plot of secchi depth measurements predicted from TMDL Report Section 8. R^2= 0.01; relationship not significant (p>> 0.05).

16. Figure 8.2 also poses some problems. Years 2000-2005 are reported to show good agreement, but there are some reasons to question this conclusion, as mentioned above. More troubling is the very wide variation of predicted secchi depths after 2005. The range of variation seen here for predictions is not found anywhere in the previous record of observed secchi depths. Certainly secchi depth observations must

be available now for years 2006-2008. How do the predicted large variations over this span of years compare with the observations for these years?

17. On page 8.6, it is mentioned that phosphorus and nitrogen control are more effective than phosphorus control alone in eliminating phytoplankton biomass. Some explanation should be added, particularly since Section 3 makes the argument that the lake is under substantial phosphorus control at present due to an increase in atmospheric loading of nitrogen. In fact, the two nutrients are nearly co-limiting in that addition of phosphorus is predicted to cause a phytoplankton biomass response, but this response has substantial limits because of depletion of inorganic nitrogen when phytoplankton biomass is increased by increasing phosphorus.

b. TMDL Technical Support Document. A number of the comments given above on the TMDL apply also to the TMDL support document, and need not be repeated here.

1. It seems strange that particulate phosphorus, mentioned on page 3-13, shows a sedimentation rate 1/40 of the sedimentation rate for fine particulate matter, mentioned on page 3-14. Perhaps some explanation should be offered.

2. On page 3-16, first full paragraph, the text seems to say that phosphorus and nitrogen nutrient limitation can be diagnosed accurately form the ratio of total N to total P in the water column of a lake. This is patently untrue. Total nitrogen and total phosphorus consist of mixtures of particulate, dissolved organic, and dissolved inorganic forms of nitrogen and phosphorus. These forms vary greatly in their availability to phytoplankton, and the ratio of available nitrogen to available phosphorus does not follow the ratio of total nitrogen total phosphorus. Furthermore, the picture is complicated by the ability of algae to store phosphorus and nitrogen

beyond their immediate needs. The text that follows the opening paragraph gives a more realistic view of the many qualifications that one must attach to the ratios of total nitrogen to total phosphorus.

3. Page 3-17 paragraph 4. There is a problem with the units that are given in this paragraph. The author seems to be equating chlorophyll a with carbon, which is incorrect. Chlorophyll makes up about one percent of algal dry mass, whereas carbon makes up about fifty percent of algal dry mass. This needs to be straightened out.

4. Page 3-24. Somewhat contrary to what one might expect from the text, there seems to have been no significant change in periphyton abundance between 1982 and 2003. There is a contrast here with phytoplankton.

5. Chapter 5, page 5-1, third paragraph. It is surprising that the TMDL technical support document relies here on pure speculation as to how much of the particle load is organic and how much is inorganic. There probably is some relevant literature on this matter, and certainly a few measurements would help.

6. Page 5-3 to 5-7. The method used for estimating the source strength for particles coming from the watershed follows a logical path but it mostly unpublished (partly because it is new) and therefore has not been as much scrutinized as the work on Lake Tahoe.

c. Summary of opinion on question 1: Fine sediment particles are the primary cause of clarity impairment.

The TMDL document and the parallel text of the technical support document summarize the evidence in support of the conclusion that fine sediment particles are the main cause for impairment of clarity in Lake Tahoe. The text of both documents contains a number of errors and misleading statements, which can be easily revised, but the underlying information is very sound scientifically. The key discovery, published by Jassby et al. in 1999, is that attenuation of light in the upper portion of Lake Tahoe by fine particles is more important than attenuation of light by phytoplankton biomass, which had earlier been considered the main cause for declining clarity of Lake Tahoe. The study was followed by additional studies of particle size distribution, seasonality, and proportionate contribution of other factors contributing to light attenuation. Publication of the Jassby paper and some of the other research in peer review outlets adds to the credibility of the analyses and interpretations.

A logical final step leading to the use of information on light attenuation factors as part of the TMDL is the development of a lake clarity model, as presented, by Swift and others. While there is no reason to doubt the predominant importance of particles in causing increased light attenuation through time in Lake Tahoe, as shown by empirical relationships derived from lake sampling, evidence for the soundness of the lake clarity model is still mixed. As indicated above, lake clarity model produces an accurate estimate of the mean clarity across years based on contributing factors, including fine particles, but fails to capture interannual variation. The concern here is that a secular change in mean might not be captured for the same reason that interannual variation is not captured by the model. The handicap for the modeler is that the range of variation is not very great, and the model simply may not be sensitive enough to depict interannual variation, but this matter needs attention.

Even if the model cannot be made to capture more variation interannually, there can be little doubt that measures taken through the TMDL process to reduce the loading of fine particles to Lake Tahoe would improve its clarity, provided that the presently substantial efforts to control nutrient loading are maintained.

II) Sources of Nutrients and Particles.

a. TMDL report.

Section 7 of the TMDL Report gives a clear overview of the results of studies contributing to quantitative partitioning of nutrients and particles for Lake Tahoe.

b. TMDL Technical Support Document.

1. Apparently no quantitative error estimates have been made.

c. Answer to question 2: Identification of the six sources of pollution affecting lake clarity.

The methods for estimation of sources of pollution (nitrogen, phosphorus, particles) as described in the TMDL Report reflect the state of the art, and incorporate both modeling and empirical analysis of sampling data. Although at least some of the modeling components were calibrated with empirical data, there is no clear presentation of the expected error for each of the estimates. Even so, the great observed difference between mean concentrations of particles emanating from upland urban areas and other areas insures that the final conclusion is quite secure qualitatively. Thus, for TMDL purposes, a strong focus on particle release from upland urban areas is warranted.

Overall, the partitioning work was done very conscientiously and should be viewed as reliable for TMDL purposes.

III) Lake Tahoe watershed model.

a. TMDL report.

1. The TMDL report contains only a sketch of the water quality modeling. The validity of the modeling must be judged entirely from the technical support document and

modeling report.

b. TMDL Technical Report.

1. Tetra Tech, which did the modeling, chose LSPC, an EPA approved watershed model for application to the Lake Tahoe basin. Because this model is approved by USEPA for TMDL applications, it seems likely that the model is appropriate for use. As is the case for widely used models of this type, LSPC is quite flexible with respect to number of watershed components and other features that are specific to any given basin.

2. The LSPC model apparently was customized for the Lake Tahoe project because of the specific importance of particles less than 63 μm for Lake Tahoe. Apparently, as explained on page 4-25, the model is able to produce predictions of total suspended solids, and it was assumed that the observed fractionation of total suspended solids in the watershed, as shown by monitoring, could be applied to the predicted TSS. This seems reasonable, although it means that there are no mechanistic components of the model that specifically deal with fine particles. Similarly, nutrient species were not actually predicted by the model, but rather were assumed to reflect currently observed speciation in streams.

3. There was no allowance in the modeling for uptake or immobilization of nitrogen and phosphorus in transit. The modelers argue that the transit time and the velocity of flow indicate the insignificance of these processes. More secure would have been some empirical demonstration that this is a correct assumption, but it does seem reasonable.

4. Scaling factors (adjustment factors designed to correct erroneous predictions) are

surprisingly large, as shown in Table 4-25. It would be reassuring have some explanation of these corrections based on monitoring.

5. The comparisons of modeled and observed concentrations show wild divergences on individual dates (often 1 order of magnitude). If hydrology is known, concentrations generally can be predicted fairly well for a given land use mixture. Perhaps the hydrologic modeling is introducing some unsuspected high degree of variation. Although the model is adjusted to produce means that reflect reality, predictions for individual dates show that the model does not understand the processes that control concentrations.

c. Answers to question 3: Lake Tahoe watershed model.

The choice of watershed model by Tetra Tech seems quite defensible. In addition, a great deal of monitoring information is available in support of modeling. Even so, the requirement for large adjustment factors and the large absolute value of deviations for concentrations between observations and predictions on specific dates shows that the model does not have a high degree of skill. The model is essentially forced by the adjustment factor process to produce means that correspond reasonably well with means for monitoring data. A lingering question is whether reliable predictions for changes in land use or control measures can be drawn from modeling, or whether they would be better drawn from direct use of data from monitored watersheds. I suspect the latter, although standard practice would be the former.

IV) Estimates of groundwater nutrient loading.

a. TMDL report.

1. The description of groundwater loading estimates in the TMDL report is insufficient

in detail to support a review. This review is focused on the technical support document.

b. Technical support document.

1. General agreement between two separate studies (Thodal's 1997 study and the USACE's 2003 study) increases confidence to the estimates for groundwater loading of nitrogen and phosphorus to Lake Tahoe.
2. On page 4-8, at the top of the page, the technical support document distinguishes between aquifer types. Shallow aquifers, which make contributions to streams, are assumed to be reflected in estimates of tributary loading to the lake, which seems quite reasonable and is standard. Groundwater, according to this paragraph, is treated as originating from deeper aquifers that enter the lake at rock faces well below the water surface. Unless something is missing in this description, it seems that a third component is not considered. While tributaries pick up shallow alluvial flow, some of the shallow alluvial flow is intercepted by the lake itself without reaching a tributary. Obviously, the importance of this source varies with topography, but it seems wrong not to mention it at all.
3. Table 4-4 and other parts of the text for the groundwater portion of the report are confusing in use of the term "ambient." Ambient means characteristic of a specific place and time. The word "background" means natural or without superimposed influences. In this case, the authors are using the word ambient to mean background.
4. The background concentrations for phosphorus in groundwater are surprisingly high. They align well with stream concentrations for undisturbed or minimally disturbed areas summarized by the Tetra Tech study, however.

5. The modeling approach used by USACE is standard. A specialized model was used only for the south Tahoe Basin. The general modeling was done by application of Darcy's Law, with numerous adaptations to the characteristics of individual sub-watersheds, as determined by sampling. The underlying problem, which plagues all groundwater flow estimates, is the applicability of Darcy's Law. Preferred flow paths, such as bedrock layers or cracks, may facilitate much faster flow than would be estimated from sampling based on bore holes. There is no easy fix for this problem, but it introduces tremendous uncertainty in estimates that cannot be calibrated or validated with actual observations at the discharge point.

c. Conclusions about question 4: Groundwater nutrient loading rates.

Estimation of groundwater nutrient loading reaching the lake follows standard practice and is backed up by substantial sampling. The groundwater contribution is small as a proportion of the total load, which means that even substantial errors in this estimate, which might occur through some unavoidable problems in estimating groundwater flows, would not likely change the overall conclusion. Given the literature on nutrient partitioning, a relatively small contribution of groundwater sources directly to the lake would be expected.

V) Atmospheric deposition as a source of particles and nutrients for Lake Tahoe.

a. TMDL report.

1. The availability of two separate studies, which appear to provide mutually consistent results, is advantageous.

b. Technical support document.

1. Figure 4-51 and associated text do not match up very well. TSP does not seem to

appear on Figure 4-51, nor are the axes explained. Too bad not to present more clearly what appears to be some very good work.

2. The procedure for allocating particles of a given size range to functional categories is not clear (page 4-121). For this reason, it is not easy to understand the basis for the third paragraph on page 4-121, which gives detailed information on the partitioning of particles within size classes. The apparent absence of any information on black carbon is unfortunate.
3. The good agreement mentioned on page 4-137 for CARB and TERC give confidence to the overall estimates, but only if CARB was fitted with deposition velocities that were developed completely in isolation of any information on the expected outcome based empirical data collection.
4. Estimates of loading from wet deposition for nutrients is accomplished in a rigorous manner with the benefit of a long term data record at one station. Although data for multiple stations are scarcer, they are sufficient to indicate relatively uniform deposition rates. This is somewhat surprising, given the potential for stagnation of polluted air in mountainous terrain, particularly during winter. However, comparison with NADP measurements in other states at locations of similar climatology is supportive. Absence of data collection on the lake's surface over extended periods of time is a disadvantage, especially in that precipitation over the lake might be cleaner than precipitation over terrestrial portions of the watershed, both the pollution sources and the natural terrestrial sources are associated with land. Altogether, however, the final estimate is responsibly made and is unlikely to be grossly erroneous.
5. The predominance of local sources of nutrients and fine particulate matter, as

discussed in section 4.5.5, is somewhat surprising. One would think that air movement across the Lake Tahoe basin from adjacent watersheds would have some influence on air quality. Certainly the results were arrived at in a careful way, but they are difficult to critique because the computations that are involved in producing the estimates cannot be followed. The validity of the is conclusion is rather important, as controls on loading that derived from the TMDL will be more or less effective according to the proportion of local sources in governing loading to the lake.

c. Answers to question 5: Atmospheric deposition of nutrients and particles.

The atmospheric component of the TMDL study was done at the state of the art for data collection and modeling and is backed up by a diversity of empirical studies. Inevitably, the dry deposition contribution to loading is more difficult to estimate than wet deposition, but the agreement between empirical and modeling studies is reasonably good, which offers some assurance that the overall conclusion is not severely flawed.

VI) Pollutant load reduction opportunities.

a. TMDL report.

1. Section 9.2.1 is confusing with respect to ground water. In the technical document, the term groundwater is used with reference to water that is pumped from wells bellow the surface alluvium. There is no indication in the results from the groundwater analysis, as presented in the technical document, that groundwater is universally polluted, as suggested in the text shown within section 9.2.1. There is some kind of terminology error or misunderstanding here.
2. Because the origin of fine particles in runoff is focused on urban uplands, it is unclear why it is cost effective to spend restoration dollars on forested upland or stream

channels.

b. Appendix: Pollution control opportunities.

The pollution control opportunities appendix gives details of the rationale and estimation procedure for various pollution control opportunities. This is a methodical and thoughtful component of the TMDL. There are enormous uncertainties, through no fault of the estimators, but a number of the more important opportunities are among the most confidently predicted.

c. Question 6: Pollution control opportunities.

The methodological text on pollution control opportunities is difficult to evaluate item by item. Overall, the approach seems comprehensive and defensible, and makes good use of the available information. As noted in the text, however, the predictions are uncertain in some cases. Given that the cost of the pollution control program can only be described as shocking, it is important that that an adaptive management procedure (as mentioned in the text and diagrammed) be a consistent feature of this program. Adaptive management is used in many long term environmental activities managed by government, but it is seldom implemented successfully. It is critical that evidence of ineffectiveness of a specific pollution control protocol lead to a redesign of the protocol. Acting against this enlightened way of proceeding is a natural but harmful entrenchment of attitudes and practices along lines that are preconceived at the beginning of the process.

VII) Appropriateness of the lake clarity model.

a,b. Comments on the TMDL report and the TMDL support document relevant to this question are as given above in Section I.

c. Answer to Question 7, lake clarity model.

There is no question as to the appropriateness of using a model based on the absorbance of particulate and dissolved constituents of water for explaining observed light absorbance in the water column of Lake Tahoe. The conceptual basis for the Lake Tahoe water clarity model is sound, and there is a considerable amount of underlying empirical information. The usefulness of a model in anticipating future conditions, however, is measured by the degree to which the model captures year to year variation over a period of validation. As mentioned in Section I above, the Lake Tahoe water clarity model in its present form fails to capture a significant amount of year to year variation in transparency of Lake Tahoe. Some explanation is needed for this failure to capture variability. Adjustments to the model that allow it to capture variability better could be a second step in model development. If not, the limitations of the model in predicting future conditions must be acknowledged. The model is certainly on the right track conceptually, but there are signs of an unresolved problem.

VIII) Allocation of allowable fine sediment particle and nutrient loads.

a,b. Comments on the allocation system are as given above under VI.

c. Answer to Question 8: Suitability of approach 2, load source weighted allocation.

Approach 2 is rational and is a significant step toward optimizing results per unit of expenditure. It may fall short of maximum cost effectiveness, however, in allocating some resources to the capture of nutrients or fine particulate matter from sources that are diffuse, such as non-urban upland. Resources allocated to controlling these sources may not return significant results, in which case it would be better to allocate these resources to the more potent sources (e.g. urban areas). In context of the full budget, this is not a major issue because the proportionate allocation of dollars is certainly weighted toward the strongest sources, but the millions to be spent on weak sources may be wasted.

IX) Overall, the TMDL and its supporting documentation is a very impressive body of work. It is rare that such a strong fundamental scientific basis is combined with a detailed analysis of source control, prediction of outcomes, and allocation of resources. There are a few significant weaknesses, as mentioned above, but these can be investigated and perhaps mitigated. Modeling of clarity and loads is more problematic than other aspects of the TMDL.

My overall concern about the implementation phase of source control is its enormous cost. Given the financial realities of the current economy, it might be good to have a companion document, of small size, outlining the results that could be obtained for expenditures of 50 percent or 25 percent of the proposed expenditure. Thus, in the event of a financial hardship, source control could proceed, and still could be meaningful.

My final point is to reiterate what is explained in VI c concerning adaptive management. It is critical that the true success of the projected methods of source control be assessed in a realistic way as time goes by. It is further necessary that any evidence of failure in a specific control strategy lead to the cessation and reformulation of the control strategy, rather than inertial continuation of expenditures on an ineffective strategy. Projects such as this often founder on the inflexibility of the action plan once implementation begins.

Congratulations to the contributors to this work, who did overall a very impressive job in addressing a complicated problem.

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