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September 9, 2010

Fred Kizito, Ph.D.
TMDL Pesticide Unit
Water Quality Control Board, Central Valley Region
11020 Sun Center Drive
Rancho Cordova, CA 95670-6114

RE: Organochlorine TMDL Source Analysis Draft Document (August 17, 2010)

Dear Dr. Kizito

I am providing comments on the OC TMDL Source Analysis dated 17 August 2010 on behalf of the San Joaquin County and Delta Water Quality Coalition and the East San Joaquin Water Quality Coalition. If any of the following comments are unclear, I will be happy to provide additional information. The comments are identified by section and/or page number followed by the commentary.

Sincerely,

Michael Johnson, PhD
Technical Advisor
San Joaquin County and Delta Water Quality Coalition
East San Joaquin Water Quality Coalition

Throughout: The term "data" is plural and sentences should be structured as "data are" rather than "data is."

Section 2.1.3.1, P. 8. There is a statement that efforts were taken to note reporting limits and quantitation limits for each study. There is no understanding of how this information will be evaluated or the consequences for the use of data in the analyses. Were data rejected or qualified in some way if the reporting limits were considered to be too high? What was considered to be "high" or "too high"?

Section 2.1.3.1, P. 8. The results of the significance tests are provided but the purpose of the test and the type of test are unclear. Also, the assumptions of the test are not provided and it is not clear that the test is the appropriate method to the data.

Section 2.1.3.2. The last line in the second paragraph should be moved to the surface water section.

P. 11. Tables 4, 5. There will always be a perfect correlation between the results of the power analysis and the significance of the test. When $p<0.05$, there will always be sufficient power to detect a significant difference. When $p>0.05$, there will always (practically speaking) be insufficient power to detect the difference. Therefore, the power analysis only is informative when the test is not significant.

Section 2.1.3.3. It is not clear that tissue concentrations from the Asiatic clam can be treated in the same way as tissue from fin fish. Justification needs to be provided.

P. 22. The curves in Figure 4 indicate that there will be no DDT and chlordane and very little dieldrin in fish tissue by 2020. However, staff provide the explanation that due to re-excavation, re-suspension, or atmospheric deposition, the curves do not provide an accurate depiction of the concentration of these chemicals in fish tissue in 2020 and that there will remain significant concentrations of these chemicals in fish tissue at that time. This explanation fails because the processes mentioned above have been ongoing since the time of the data included in the figures, and despite these inputs, concentrations are decreasing rapidly. Discussion provided later in the document (page 29) confirms that these processes have been occurring and consequently, the trends in Figure 4 should not be changed between the current time and 2020.

Section 3. There is a logic chain presented that says; DDT/DDD/DDE are present currently in fish tissue, sediment, and the water column; DDT was applied to soils at some time in the past, therefore soils are currently contributing to detectable levels of these chemicals in those media. This logic is faulty as no physical processes are presented that link applications prior to 1972 to current levels of DDT in soils especially as there are other mechanisms that can contribute OCs to surface water (atmospheric deposition). The data presented in Figure 8 are not sufficient to make the link (see comment below).

P. 29. The term “previously dominant” is used but it is not clear what that term means. However, the remainder of the sentence about the re-emergence of OCs due to conversion of agricultural land to urban land use is a refutation of the conclusion made earlier that processes such as this would lead to measureable levels of OCs in fish tissue beyond 2020. Again, unless these processes would occur at a frequency or magnitude far exceeding those of the last 10-20 years, the shape of the curve would be unaltered.

Section 3.5.1. Data from the Mischke et al. (1985) report are used (presumably, see comment below) to provide a concentration of DDT and Lindane in soil. These concentrations are then used to develop loadings from various land use categories. These data should not be used in any calculations because:

1. The data were collected in 1985, 25 years ago and only 13 years after the banning of the use of DDT in agriculture. The assumption that the concentration of DDT in soils is currently at the same levels measured in 1985 is flawed and the flaw is illustrated by Figure 4. If the concentration in soil is the same as the amounts measured in 1985, and if the rate of mobilization of that soil to surface waters is the same, the amount of DDT in fish tissue should not be declining over the last several years. The decline in the tissue concentration illustrated in Figure 4 requires that 1) the concentrations in the soil have dramatically dropped, 2) the rate of mobilization has decreased, or 3) both have occurred. Therefore, the concentrations from 1985 should not be used for calculations of loadings.
2. The sites selected for analysis of DDT concentration in soils by Mischke et al. were not selected randomly and are not representative of concentrations in agricultural soils across the Central Valley. The Mischke et al. (1985) report clearly states that the County Agricultural Commissioners were consulted to find locations that based on crop type, were likely to have received repeated (over many years to decades) applications of OCs. The concentrations found in the soils are likely to represent the maximum concentrations expected to be found, not the average concentration. To properly determine the magnitude of the problem, a sampling design with randomly located sample sites should have been developed. As a result, there is no way to determine how the concentrations used in the calculations of loadings (in the current source analysis) are reflective of true concentrations of OCs in soils of the Valley. Until it is clear that the concentrations of DDT and Lindane used in the calculations (from Mishke et al.) are within one, two, three, or four standard deviations of the true mean concentration, it is simply irresponsible to use these data. The idea that the Regional Board should use the Mischke et al. data because they are all that are available is built on the fallacious argument that any data are better than no data. The description of the sampling design in Mishke et al. (1985) clearly indicates that the data are not representative of conditions throughout the Valley (see above). Using data that are not representative and for which the accuracy is completely unknown is simply poor science and therefore should not be used as the backbone of a regulatory program. The consequences of using bad science could be significant expense spent on monitoring and managing a problem that does not exist.

P. 39. It is stated that DDT was banned in 1969 when it was banned in 1972.

Table 17. The concentrations in the table are not referenced. They do not correspond to the concentrations in the water column provided in Tables 6 and 7 so it is not clear how they were developed.

P. 53. The total number of samples should be 27, not 26.

P. 53. The method(s) used to calculate the loads reported in Table 21 are not provided. The equations provided (8, 9, 10) are not sufficient to make the calculations. Converting land use acreages to load is not explained. At the least, there should be a runoff coefficient for each land use that explains the amount of contaminated soil mobilized to surface waters, a rainfall and irrigation tail water runoff coefficient, and an OC mobilization coefficient for the soils (this could be included in the amount of contaminated soil mobilized). The assumption that all land contributes contaminated soil to the river is not valid as many parcels do not have surface water drainage. If WARMF simulation results were used for water running off of the watersheds from rainfall and irrigation, or the amount of sediment delivered to surface waters, the boundary conditions need to be stated. For example, which water year was used in the calculations, what was the rainfall applied to the landscape, and how was rainfall/runoff converted to concentration? WARMF simulates separately the sediment erosion of three particle types; sand, silt, and clay. Given that the soil concentrations did not specify the particle type used in the analysis, what concentrations were used for each particle type in the calculations of load? The rainfall detachment term in WARMF uses several constants including soil erosivity, rainfall intensity, rainfall detachment factor, and cropping factor of land use. Assigning a cropping factor parameter value requires an understanding of the management practice effectiveness in preventing erosion which requires substantial knowledge of site specific factors including which management measures are applied to which fields. Rainfall intensity is measured in mm/min which requires assumptions about storm intensity. If WARMF simulations were used in the calculations of load, these terms and parameters used should be explained, perhaps in an appendix to the source analysis document. If WARMF was not used, the methodology that was used needs to be explained thoroughly.

P. 53. There is a reference to data on land use runoff and discharge that is found in section 2 but those data are not found.

P. 53. The first sentence in the last paragraph references Section 3.1 as providing flow rates for each land use category. Section 3.1 covers background concentrations of OCs and no flow rates are provided in the doc