

Bayer CropScience



VIA ELECTRONIC MAIL:
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Mr. Daniel McClure, P.E.
Water Resource Control Engineer/Project
Manager TMDL Unit
Central Valley Regional Water Quality Control

Subject: Phase-III Water Quality Criteria (WQC) Derivation Method
Developed for Cyfluthrin

Dear Mr. McClure,

Bayer CropScience LP appreciates the opportunity to comment on the draft report "Cyfluthrin Criteria Derivation" by Fojut, Chang, and Tjeerdema from the University of California – Davis. As the primary USA registrant for cyfluthrin, and the closely related beta-cyfluthrin, it is in Bayer CropScience's interest to ensure that the document accurately reflects the available information and the potential risks to aquatic organisms. Detailed comments are attached.

It should be noted that cyfluthrin, and beta-cyfluthrin, have been reviewed by regulatory authorities across the globe and potential risks posed by both compounds' labeled uses have been found to be acceptable. In the USA the mitigation measures outlined on product labels, as accepted by US EPA, address the risks to aquatic organisms associated with these active substances.

If you have any questions, please contact me at 919-549-2628 or karen.cain@bayercropscience.com. For technical comments, please contact Dr. Michael Dobbs at Michael.dobbs@bayercropscience.com.

Yours sincerely,

A handwritten signature in cursive script that reads "Karen Cain".

Karen S. Cain
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Bayer CropScience, LP

March 5, 2010

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**Comments on the Draft “Cyfluthrin Criteria Derivation”(undated) report
by Fojut, Chang, and Tjeerdema, University California - Davis
Issued by the California Central Valley Regional Water Quality Control Board**

Data Collection and Selection: In any data analysis project the collection, review, and selection of relevant information is critical to the process. Many times errors or flaws or bias in an analysis can be traced to how the input data was selected and used. Often, data selection plays a more critical role than the analysis scheme chosen. It is clear that the authors have done a thorough job in collecting the available aquatic toxicity information for cyfluthrin. Based on the extensive review scheme used, it is also clear that data quality is recognized as an important factor. However, we are concerned that while the data collection process was extensive, and review highly structured, the process has not necessarily led to the use of highest quality and most relevant studies and information.

BCS believes that the authors have identified most of the parameters necessary to judge the quality of studies for criteria derivation, but application of these parameters via a strict scoring scheme is misguided. The data evaluation process must be conducted in the context of the needs of the overall analysis. For example, a study with poor control performance can be rated as RR, if the other parameters are acceptable and properly documented. However, most acute toxicity test guidelines consider a study invalid if a minimum control performance is not met. Some parameters, such as control performance, are “make or break”; either the study is within accepted norms and acceptable, or is not and therefore cannot be used. Also, the importance of some parameters or review criteria is dependent on the chemical being evaluated, such as metals and hardness. One would not want to use a study with a metal, without knowing the hardness. But, for organic chemicals, hardness is generally not considered a factor that has a strong influence on toxicity.

The availability, or lack thereof, of other studies can also influence whether a study should be included in the derivation process and how that study is used. One good example is that the authors’ have prioritized (*and BCS agrees*) flow-through studies over static, where both exist for a species. Looking at a different case, but on the same theme, does it make sense to exclude a study with a “new” species, just because the study was performed with a formulation? BCS agrees with the authors that studies conducted with technical grade active agreement should be prioritized over formulation studies, but do not think they should always be excluded. In cases where a study using a “non-preferred” design is available, an evaluation needs to be carried out whether more is gained or lost by including the study in the analysis. So, if inclusion of a formulation or other non-preferred study adds significant new information, like an additional test species it should be included in the analysis.

The authors combine results of studies with cyfluthrin which is a racemic mix of four isomers (I-25%; II-18%; III-35%; IV-22%) and beta-cyfluthrin which is a refined mixture of isomers containing a higher portion of the two most active isomers (II-35%; IV-62%). This is

inappropriate, since the two isomer mixture are not equally active: beta-cyfluthrin is approximately twice as active as cyfluthrin. This impacts the derivation of endpoints for a number of species, for example with the Bluegill sunfish, where the study of Gagliano (1994) is with cyfluthrin, while the study of Bowers (1994) is with beta-cyfluthrin. Taking the geometric mean of these two studies is inappropriate since the results reflect the different isomer composition and closely match the expected difference in toxicity. Typically environmental monitoring programs measure the sum of all four cyfluthrin isomers, therefore, it is most appropriate to base a criteria on the cyfluthrin toxicity data. In reviewing toxicity studies with cyfluthrin it is important that the methods make it clear what isomer mixture is used in the study since it will have a significant impact on the study results. For all the registrant sponsored studies it is clear what isomer mix was used, but some of the literature studies cited it is not and should be checked.

Finally, it must be noted that many of the studies referenced are neither cited in text, listed in Tables 3 to 9, or in the toxicity data summary sheets in the appendix, for example: Brander, *et al.* (2009), Froelich *et al.* (1984) or Maul *et al.* (2008a). We request references not relevant to the derivation of criteria for cyfluthrin be removed, and that any studies considered in the evaluation always have a corresponding toxicity data summary sheet. Some of the non-cited studies do appear to be relevant to criteria derivations, so recalculation may be necessary. Inclusion of these “extra studies” makes a fair evaluation of the document difficult. As a matter of transparency, it would be useful if the actual values assigned in the scoring for each parameter were included.

Bioavailability: The authors make an accurate summary in section 9.0 of the available information on the factors that impact pyrethroid bioavailability in aquatic systems. A number of the studies cited are very relevant to the question, although they have missed some (e.g. Maul *et al.* 2008a; Ortego and Benson, 1992). Clearly the authors recognize the importance of organic matter in impacting pyrethroid bioavailability. Therefore it is surprising that, despite the available information, the authors reject modifying the cyfluthrin criteria by the organic matter or carbon content. They cite some of the uncertainties associated with the available studies and implementation of a water quality correction into the criteria as reason for not making any adjustments. However, ignoring a known and accepted factor that strongly influences pyrethroid bioavailability and toxicity results in criteria that are less applicable and relevant to the real world. Binding of pyrethroids to particulate matter or dissolved organic matter greatly reduces their bioavailability to aquatic organisms. It is the freely dissolved pyrethroids that are bioavailable and toxic; the bound fraction does not significantly contribute to toxicity. In laboratory toxicity tests using water with minimal particulate or dissolved organic matter, nearly all the pyrethroid is bioavailable. In ambient water, only a small fraction of the total pyrethroid may be bioavailable. Comparing a criterion derived on concentrations of freely dissolved cyfluthrin, to a total concentration is not appropriate. For an accurate assessment the bioavailability of cyfluthrin must be taken into account both in generating a criterion and in

applying to environmental samples. Freely dissolved cyfluthrin can be measured directly using solid phase microextraction (or other techniques), or estimated using an equilibrium partitioning model. There is no technically valid reason not to include an adjustment factor.

Mesocosms, Microcosms, and Field Studies: In section 13 of the report the ecosystem level studies available to the authors are summarized. These complex higher tier studies are not used in the criteria derivation process other than indicate that the derived chronic criterion is well below any of levels examined in the studies. What the ecosystem studies actually indicated is that at concentrations greater than approximately two orders of magnitude above the proposed chronic criterion, no ecological significant effects, or at most slight and transient effects can be expected. The microcosm/microcosm findings suggest that adequate protection could be achieved with a drastically higher criterion than proposed in this report.

Methodology used for Cyfluthrin Criteria Derivation

The review of the data available to the authors led them to the conclusion that there was insufficient data from enough different taxa for them to use species sensitivity distribution (SSD) approach, so they applied an assessment factor to the lowest available acute toxicity value. As discussed further in following sections, if the study of Rodriguez *et al.* (2007) had been including in the evaluation, then a sufficient number of species would be available to use the SSD method. I Justification for the assessment factor should be given in the criteria document due to its importance in deriving the criteria. It is our understanding that the assessment factor was taken from Tenbrook *et al.* (2009) and relies heavily on data where most of the compounds are organochlorine insecticides. The role of the assessment factor is to compensate for uncertainty in a small data set where it is unclear about relative sensitivity of untested species. But in the case of cyfluthrin, and the other pyrethroids, it is well documented that amphipods, isopods, and similar taxa are the most sensitive species. Evidence comes both from single species testing, but also the ecosystem studies mentioned above.

Applying a large safety factor to lowest LC₅₀ in the cyfluthrin data set, which is *Hyalella* and therefore one of the most sensitive species just results in criteria that are overly conservative and unrealistic. While one can argue that the criterion is protective, being overly conservative or protective can result in unintended consequences. If one compares the draft acute criteria recently released by the same authors for two other pyrethroids, one would get the impression that cyfluthrin is 5 to 20 times more toxic to aquatic organisms than the other pyrethroids. An unbiased review of the available information does not support the assertion that cyfluthrin is up to 20x more toxic to aquatic organism than other pyrethroids.

Considering the available information, the limited acceptance of the methods used, along with the unresolved errors in the document, BCS request that this document be withdrawn until more information is available or a more robust method are available. USEPA currently has a project underway that is examining the methods to derive benchmarks for pesticides. We assert that it would be better to wait for the output of this effort, rather than to apply methodology that may

not be considered in the near future, the most appropriate for the derivation of water quality criteria for pesticides.

Specific comments by page number

Page 3 Was the BCF of 4231 listed in the report actually calculated in Yang *et al.* (2007) as cited, or calculated by this report's authors? It does not appear to have been reported in Yang *et al.* (2007). It should be noted that it is misleading to report a BCF value unless steady state has been clearly demonstrated. Yang *et al.* (2007) conducted bioaccumulation experiments at 200 ppt for 24 hrs, which is above the LC50 of 160 ppt used in this report for *D. magna* after 48 hrs. While the water used does influence the bioavailability, the bioaccumulation work of Yang et al was likely done at lethal levels, putting this value into question.

It should be noted that the BCF report by Laskowski is a recalculation of Carlisle and Rooney dataset. Also, a mean values was not given in the original report. A more robust evaluation of the study has been conducted since it was originally conducted generating a BCF estimate of 459.

Page 4 Lambda-cyhalothrin is referenced. The authors should confirm that all the data in the report is for cyfluthrin.

Page 4-5 The authors note that "Approximately 53 original studies..." , which this reviewer has not been able to confirm. Approximately 42 data summary sheets are in the appendix. Proper documentation of the studies reviewed and used in this study is critical in making a fair evaluation of the work. We request that the evaluation of the other studies be provided, and time be allowed for review, prior to finalizing this report.

Page 5 Text indicates six SMAV were used, yet Figure 2 shows seven. Please clarify.

Page 6 A more detailed rationale of why a specific assessment factor was chosen would be helpful. Is knowledge about the relative sensitivity of the available species used in assigning an assessment factor? How is it justified to say that final acute value is the 5th percentile when all that has been done is divide the lowest toxicity value by an AF? There appears to be insufficient information available to support assigning a percentile to the final criterion.

Page 7 The statement that pyrethroids have been found to cause toxicity in surface water should be fully referenced, or the statement deleted. Amweg *et al.* (2005) does not appear to be the appropriate reference. Equilibrium partitioning theory in general supports the statement at the end of the first paragraph under bioavailability.

Please clarify statement “They also measured the organic carbon (OC) content of the DOM and did not find a direct correlation, indicating that not only the OC content,...” attributed to Yang *et al.* (2007). It seems at best an oversimplification of the work described by Yang *et al.* (2007).

Page 8 *Hyalella* is not a true “benthic” organism and is not expected to be found in close proximity to pore water. It is epibenthic and a detritivore and tends to be associated with leaf packs or other decaying plant material at the surface of the bottom sediment. Maul *et al.* (2008a) demonstrated that toxicity of pyrethroid was reduced when *Hyalella* was exposed in the presence of its natural substrate, leaf material.

Page 9 A site specific partition coefficient are not necessarily to apply the model propose y needed. While there clearly is variability in Koc estimates, more uncertainty is introduced into the process by ignoring bioavailability, rather than trying to address it. The authors have failed to fully quantify the uncertainty in the process.

Page 10 Most aquatic toxicologists would consider LC50 values of 0.62 ppb and 0.46 ppb within normal experimental variation. Based on the information cited it does not appear the PBO has a significant impact on the toxicity of cyfluthrin to *Daphnia*.

Page 11 Would it be more meaningful to compare the proposed criteria to the results of mesocosm, microcosm and field studies, which are true ecosystems studies, instead of the laboratory database used to derive the value? It is a circular argument to confirm the validity of the water quality criteria with the same data used to derive them.

The results from a single species in laboratory studies are given more credence then ecosystem studies dealing with tens, if not hundreds of species in deriving a WQC. Using the mesocosm data to only confirm the criterion is under utilizing the available information.

Page 13 See comment on bioaccumulation on page 3

Page 14 As one of the limitations the authors should note that the acute criterion, which in turn the chronic is based on, relies on a sole publication (Weston & Jackson, 2009), whose focus was not on derivation of a pyrethroid LC₅₀ *Hyalella* value, but instead TIE methods. It is a comparative study, and in context of the hypotheses they were examining, it is a good study. However, it was not designed to generate a standard or benchmark LC₅₀ value for *Hyalella*. Test concentrations were not maintained or measured throughout the study, and are in fact are only an estimate, based on measurements at single test levels. In this study, the measurements were highly variable, with initial concentration ranging from 64 -189% of nominal, and the 48 hr

concentration ranging from <12 -72% of nominal.

The methods used to measure the toxicity of pyrethroids to aquatic organisms do matter as can be seen in the current database. For example, the LC₅₀ Bluegill under flow-through conditions with measured values is 0.998 ppb (Gagliano, 1994), while under static conditions with nominal concentrations the value is 1.5 ppb (Bowers, 1994). This pattern can be seen with other species.

Page 15 Typo? - heath instead of health?

Page 27,
table 3 Not all the values listed from Yang *et al.* (2007) are correct. The 0.0093 value is incorrect by a factor of 10. All values should be checked and geometric mean and the criteria recalculated. QC procedures for this report are not documented.

Standard methods call for *Daphnia* to be tested for 48hrs, so it is unclear why the Yang work has been given preference over Wheelock *et al.* (2004).

Page 28,
table 3 With respect to Weston & Jackson (2009) work: With the limited measurements made, the authors did as well as possible to estimate, but these methods are well below standard. It is surprising the study scored so well considering that no standard method exists for water column tests with *Hyalella* and the limitation of the reported analytical measurements. The desire to include *Hyalella* in the criteria derivation data set is understood, however we question whether this study was conducted close enough to current standards and that it should be relied on as the value that drives the derivation of the criterion.

It should be noted that Brander *et al.* (2009), one of the studies not used, also reports a *Hyalella* LC₅₀ for cyfluthrin, although the reliability of this study is unclear.

Page 28 Suprenant (1991) is a study with beta-cyfluthrin, and therefore should not be combine with studies with cyfluthrin. There is a clear difference in toxicity attributable to isomer composition. All available fish studies do not seem to have been utilized. Therefore the geometric mean for trout needs to be recalculated.

Page 29,
Table 4 Rejecting studies because they are not the most sensitive endpoint for the species is wrong, and adds an unnecessary bias to the process. When multiple valid studies exist for a species, the geometric mean should be taken, not just the lowest value. The exclusion Wheelock *et al.* (2004) with *Ceriodaphnia* is a good example of this bias.

Page 31,
table 5 It is difficult to understand why Rodriguez *et al.* (2007) was excluded from the analysis, compared to the studies that were included. It would provide the missing

insect species, allow for an SSD based estimate to be derived.

Page 38, It is unclear how the trout LC50 value of 0.1192 ppb? was derived. While ICE was
table 11 not run by this reviewer, the predicted LC50 values are surprising considering the
input values.

All Units should be included in all tables.

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