Monsanto

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MONSANTO COMMENTS ON THE EPA HEARINGS ON PCBs
EPA EXHIBIT B - AFFIDAVIT OF GILMAN D. VEITH, PH.D.

The first 9 pages consist of a rudimentary discussion of hydrocarbon chemistry concluding with a description of PCBs. The text has many typographical errors and is not clearly written, but is basically correct.

Page 10 begins an equally vague but essentially correct description of gas chromatography as applied to determination of PCBs. The definition of gas chromatography in the last sentence of the first paragraph on page 10 is very poor. For example, his definition could also apply to distillation. Exhibit 10 has been oversimplified to the point that it is incorrect. The "non-volatile liquid" coated onto "crushed firebrick" is actually packed uniformly throughout the column and the "nitrogen carrier gas" flows through the coated packing.

Exhibit 15 is a modification of a Monsanto tabulation of percentage compositions of Aroclor 1221, Aroclor 1016, Aroclor 1242, and Aroclor 1254. However, Veith's testimony in the first paragraph on page 15 incorrectly refers to Exhibit 15 as a list of relative retention times of major components in a chromatogram. There are no retention times in Exhibit 15, no major components, no chromatograms, and no reference to Aroclor 1428 (sic. presumably Aroclor 1248) or Aroclor 1260.

In discussing the EPA prescribed method for analyzing water for PCBs, Veith states on page 17 that "Although the basic method suggests a detection limit of approximately 1 µg/ℓ using a sample of 100 ml to 1,000 ml, the method can readily be adapted to larger sample volumes to provide reasonable reliability at much lower water concentrations".

This argument is theoretically correct if no interferences are present in the water and if the reagents and glassware contain no contaminants which show up as "background". In actual practice with

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real effluents, interferences are present. The EPA method makes no provision for removing most industrial chemical interferences such as polychlorinated naphthalenes and polychlorinated paraffins. Also, even pesticide grade reagents contain trace impurities which contribute a background when a large volume of solvent is concentrated by evaporation.

For these reasons, it is not always possible to attain a detection limit of 1 µg/ℓ for PCBs. In such cases, starting with larger samples will not lower the detection limit. Many laboratories located at manufacturing sites where PCBs are used experience a "laboratory background" corresponding to about 1 µg/ℓ, which precludes analyses at lower levels.

On page 20, Veith claims that "At 1 µg/ℓ, a standard deviation of 10 to 20 percent of the mean concentration can be expected". No data are presented to substantiate this claim. Is this for inter- or intra-laboratory comparisons? See related remarks in the section commenting specifically on the reproducibility of the EPA method.

On page 21, Veith introduces a mathematical model to approximate bioconcentration and makes predictions based on the model. What place do predictions based on models have in EPA hearings?! Experimental evidence is more credible than models and should take precedence.

The text on page 21 does not indicate whether the study presented in Exhibit 17 was conducted as a static or a flow-through test. There is considerable controversy among aquatic biologists over whether static tests are as reliable as flow-through tests. This might be a point to pursue. The whole concept of extrapolating laboratory bioconcentration studies to the environment assumes that PCBs in pure laboratory water behave the same as PCBs in natural water. This is not a good assumption if sediment or suspended solids are in the natural water. PCBs absorb onto the sediment and are less available to fish membranes. Hence, laboratory bioconcentration values may be higher than environmental values.

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