

The American Legion  
National Headquarters

15/10/90

Nancy, *2*

BASF Review.

OCT 16 1990

This is the second reviewer (the other was Dr. Jeanne Stellman.

Ron Trewyn is not an epidemiologist but a cancer expert so he asked one of his associates to review the BASF papers.

*[Handwritten signature]*

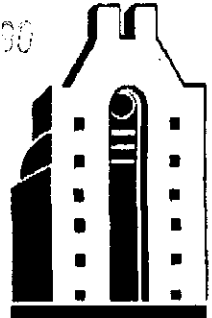
*As per*



\*\*\*\*\*  
F A X T R A N S M I T T A L M E M O  
TO: Dick Christean  
DEPT: Amer. Legion FAX #: (202) 261-2728  
FROM: Ron Trewyn PHONE: (614) 292-7015  
CO: OSU FAX #: (614) 292-4767  
Post-It™ brand fax transmittal memo 7671

NO. OF PAGES  
2

OCT 15 1990



A National Cancer Institute Designated Comprehensive Cancer Center

The ARTHUR G. JAMES CANCER HOSPITAL AND RESEARCH INSTITUTE

October 5, 1990

Dear Ron,

I've looked over the dioxin materials you received from the American Legion's Washington office. This stuff included the Rohleder 'paper', the Zober *et al* ( aka BASF-flunkies) paper in Occ. Environ. Health, Rachel's 'news', the New Scientist article, and the court case translation. Unfortunately, I find none of the new material credible or convincing. The data analyzed, the methodology used, the manner by which it is presented, and finally, the difficulties associated with expressing oneself in a non-native language, lead to the conclusion that the author(s) are unfamiliar with the scientific method. Everyone seems to have their own dogma to defend; the BASF guys (who I wouldn't trust all that much) at least present theirs in a reputable, peer-reviewed journal.

There are several problems with the data and the means by which they are analyzed.

The lumping of all malignant diseases, with varying etiology, is fraught with all sorts of problems; combining all fruit does not allow one to draw conclusions about the cost of bananas. In the peer-reviewed data I'm familiar with, there seems to be an association between dioxin exposure and soft tissue sarcomas, but not with other types of cancer. Still, I'm always careful to remember that association does not imply causation. Even with a plausible mechanism of carcinogenesis for dioxins, implying/assuming all malignancies to have a common mechanism is hard to swallow. However, I'm not aware that anyone has presented a convincing and general hypothesis for the mechanism of dioxin carcinogenesis or tumor promotion in these diseases.

The sample size is Very Small. The author states that he realizes the lack of statistical power (the large chance of coming to a false negative conclusion), and uses this lack of power to bolster his finding of statistical significance (a significant standardized mortality ratio - SMR). While the SMR may be significant, you're looking at a grand total of 17 individuals with cancer; 8 respiratory ca, 7 digestive ca, and 2 unspecified ca. Real Mole Hill = Mountain inferences. I re-ran analyses to see what would happen if you had a couple fewer cancers and, interestingly, if you had 3 fewer malignancies, 14 rather than 17, the p-value for the SMR drops to > 0.05; it's not significant.

There is no data presented on attempts to ascertain the frequency of other risk factors related to cancer in the cohorts studied. Basic factors such as smoking, drinking, family history, other occupational exposures, possibly diet are not discussed. These are especially important in the diseases mentioned here, respiratory and digestive cancers.

The determination of cause-effect relationships demands that such factors be included and evaluated.

The "article" by Rohleder makes serious allegations concerning the selection of the dioxin-exposed cohort by the BASF "researchers." There's a considerable amount of juggling of workers to come up with the number exposed and exhibiting signs of dioxin toxicity (chloracne). Regardless of whose figures you use, the numbers aren't convincing since real epidemiologic evaluation was NOT performed or initiated at the time of the accident in 1953. None of the data in either Rohleder's "article" or the BASF stuff details other factors I've mentioned above. If they wanted to do this right, they should have started the study in 1953 with a thorough evaluation of all the BASF workers and unexposed community controls with risk factor questionnaires, etc. Too little and too late. I've included a couple of Seveso articles to give you an idea of what's been done there. You'll notice that the chemical company, Icmesa, did not publish there "side" of the story - at least I've not come across it.

Here's a brief summary of the other items you sent me:


*New Scientist* article: They call Rohleder's results "Careful detective work"! It's just a newsy rehash of his presentation with no additional analysis or rebuttals. Another axe-grinding exercise. BS

*Rachel's Hazardous Waste News* #171/172: You've got to be kidding. They use the word "fraudulent" so often it becomes trite. I agree that dioxins are potentially very toxic, that conclusive data does not exist, and that the USEPA has inadequate information from which to establish exposure standards. However, the two studies cited, the BASF and Monsanto "fraudulent" studies, are on such shaky ground already (Rohleder's lack of peer-review) that I'd in no way consider them conclusive. I'll dig up Suskind's 1984 paper in *JAMA* and take a look at it.

**The Hamburg court case translation: Dueling Experts** (I can hear the twang in the background!). Yes, this guy did have obvious toxic effects of 2,4,5 TCP exposure, but concluding that it caused his esophageal carcinoma is stretching it. Such a sample size of one might be convincing to a lay jury, but not Science.

None of this stuff does anything to establish any dioxin-cancer link, in fact it does the opposite. If this is the evidence proponents are using to bolster the connection between dioxin and cancer, they're really desperate. Wish I could be more encouraging, but since I was a Chemical Officer, what did you expect?!!

Good Luck,



Donn C. Young, Ph.D.  
Director, Biostatistics Unit