

FAX COVER SHEET**To:**

**Paul Merrell & Carol Van Strum
Bradley & Merrell**

From:

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4 pages (including this cover sheet)**Sent on 14-Nov-93 at 18:41****Sent via FaxWorks OS/2**

November 14, 1993

Hi, Paul--

More thoughts on information it would be nice to get from Monsanto.

The attached 1978 letter from Dow discusses the relationship between dioxin in a human now [in 1978] vs. dioxin in that same human at the time of original exposure. Such a discussion presumes some "half-life" for dioxin in humans -- some period of time that it takes for half of the original amount of dioxin to leave the body.

We know today that the half-life for dioxin in humans is roughly 7 years. The Dow letter does not say what Dow believed the half-life in humans to be (as of 1978), but they clearly assumed some half-life; otherwise the letter wouldn't make any sense.

It would be useful to know what Monsanto thought the half-life was in 1978. When they broke the Nitro cohort into "acute exposure" and "chronic exposure" that seemed to imply that an acute exposure would leave the body quickly and would not remain in the body to create an on-going chronic exposure. We now know, of course, that that was an incorrect assumption, and that an acute exposure in 1949 would remain in those exposed and would create a chronic exposure situation over the years--so breaking the cohort into "acute" and "chronic" didn't really make any sense. If we could show that Monsanto knew something about the half-life of dioxin in humans in 1978, it would help us show that they knew (or should have known) there was no valid reason for assuming acutely-exposed people didn't thereby take on a chronic body burden that would create chronic exposure conditions for the rest of a person's life.



*private OS
public position*

Schultz

DOW CHEMICAL U.S.A.

6 January 1978

MIDLAND, MICHIGAN 48640

*(admits cause +
effect relationship
Ex 14/15/C)*

Dr. med. G. Reggiani
Member of the Research Board
F. Hoffman-La Roche and Co., Ltd.
Grenzacherstrasse 124
CH-4002 Basle.
SWITZERLAND

Dear Dr. Reggiani,

UB

Thank you for the information on the levels of TCDD found in the tissues of the woman who died 6.5 months after exposure. I concur with you that the levels appear high. The level in body fat suggests a daily exposure to approximately 0.01 µg/kg/day. For a 75 kg person, this means 0.8 µg/day. If ingested via diet, the diet would have to contain approximately 2 ppb. Presumably, this is not likely.

Skin absorption should not be discounted because of the absence of chloracne. First, the age of the woman may have decreased the sensitivity of the acnegenic activity. Secondly, chloracne is likely to be a systemic rather than a local effect in most cases.

Therefore, the real question is whether it is possible for the woman to have received 0.01 µg/kg/day via ingestion plus skin absorption.

A feature of the case that bothers me is why no TCDD was found in the liver in September and December of 1976. The level should have been higher than that found in the autopsy sample, 5 months later. Is it possible that the only tissue analyzed was the tumor? There is evidence that liver tissue in the fetal or neonate form is unable to concentrate TCDD, at least in rats. The only other solution seems to be that the woman incurred much higher exposure after September than between July and September.

With respect to your questions on our 3 generation reproductive study, I am somewhat confused. Perhaps I do not understand the issue. In the study, an effect was observed in rats receiving 0.01 µg/kg/day but not in those receiving 0.001 µg/kg. In the first generation, dilated renal



Dr. Med. Reggiani
6 January 1978
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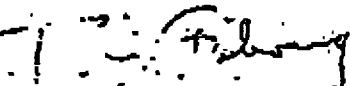
pelvis and a decrease in survivability were discerned in pups of females receiving 0.001 ug/kg/day upon the first mating. However, these were not discerned in pups derived from remating the same females or in subsequent generations. Therefore, the observed effects were judged to be random, spontaneous occurrences rather than treatment induced.

Your reference to reserpine was unknown to me. The decrease in fertility with continued treatment as noted for reserpine does not occur with TCDD. Indeed, as indicated above females of the first generation were remated. This remating resulted in an increased fertility compared to controls.

With respect to the 3-generation study, a final report has been drafted and is in the process of being final typed. As soon as it is available, I will send you a copy.

I hope my rambling has helped resolve some of your problems. However, I am not sure whether I addressed the critical issues.

Respectfully yours,


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