

W. R. Gaffey, Ph.D.
11269 Pineside Dr.
St. Louis, MO. 63146

May 11, 1990

Peter Montague, Ph.D., Editor
Environmental Research Foundation
P.O. Box 3541
Princeton, NJ 08543-3541

Dear Dr. Montague:

This is in response to the points raised in your letter of April 23. There were two studies of mortality and dioxin exposure at the Monsanto Nitro plant. The first was by Zack and Suskind. I enclose a copy marked Attachment I. The second was by Zack and Gaffey. I enclose it also, and have marked it Attachment II. Mr. Carr has no problem with the first study if I understand correctly the portion of his October 1989 brief that allegedly accompanied Dr. Jenkins' letter. His problem (and yours, I deduce) is with the second study. I will ignore the text of Jenkins' letter because compared with her garbled version of events, Carr's prose, turgid as it is, appears positively lucid. (One example. On the third page of her memo she confuses the mortality study by Zack and Suskind, published in 1980, with a morbidity study by Suskind and Hertzberg published four years later in a different journal, and attribute the alleged sins of the latter paper to the former pair of authors.)

The Zack and Suskind study was a follow-up mortality study, which means that the risk of death by cause could be calculated. The definition of exposure used was the following, which I will call Definition A: Having been exposed during the clean-up of the 1949 accident and getting chloracne and turning up in the plant safety records or the plant medical records or workmen's compensation records or some combination of the last three.

The Zack-Gaffey study was actually two proportional mortality studies, one of deceased people who had been exposed and one of deceased people who had not been exposed. A proportional mortality study is one in which data on denominators are not available, so what is done is to see if the distribution of deaths by cause is about what it ought to be. If there is no denominator then no rate can be calculated, that is, no risk can be calculated. If the PMR (proportional mortality ratio) for some cause is high, we cannot tell whether the risk from that cause is high or the risk from some other major cause is low. However, if the PMRs in two different groups look pretty much the same one can conclude that

EXHIBIT

K

the pattern of mortality is the same even if one cannot calculate what those risks might be. The definition of exposure in this study will be called Definition B: Worked for at least one year on the hourly payroll and was actively employed on or after the beginning of 1955 and was assigned to a job or a work area involving exposure to dioxin.

As far as I can see there are two possible views to take of these two definitions. One is that they are measures of acute and chronic exposure, respectively, and should be considered independently. This is the view that we took. It is set forth on pages 19 and 20 of the Report of Proceedings of the trial for July 22, 1985 which I enclose as Attachment III. You will note that Mr. Carr was present at that time and took no exception either then or later to this distinction between the two definitions.

However, Mr. Carr apparently favored a different view, which appeared to be that one should not distinguish between these two definitions, and that anyone who was exposed by one of the definitions should be considered to be exposed by the other definition also. My evidence for this view on Mr. Carr's part is the material beginning on page 21, line 4 and running on to somewhere on page 23 of Attachment III. If one favors that view then it makes sense to combine the results of the two studies by putting together all the deaths that were exposed by either definition and doing a PMR analysis on them. There are 86 such deaths, as explained on page 44, lines 5-16 of Attachment III. Note again that Mr. Carr was present and did not object either then or later to this reasoning. The resulting PMR showed no excess deaths, but these results will not be found in the trial record because Mr. Carr did object to revealing the results of that correct analysis (pages 55-56, Attachment III). Mr. Carr is a man who has thought through his priorities carefully.

The way in which Mr. Carr produced his excess cancers was to lift only the cancer deaths from the first Nitro study, add them to the deaths from the second one, and then discover that there was too much cancer. On the strength of this he has accused me of fraud because I did not go through that same idiocy.

Now to the points of your letter.

Point (1). When the dust raised by Carr had settled there were 8 deaths from the Zack-Suskind study that were also deaths in the Zack-Gaffey study, out of 162 deaths in the latter study (page 43, lines 22-24, Attachment III, with no objection from Mr. Carr). I therefore say that we were not studying the health of the 32 people who died out of the 1949 accident group, but were studying the 162 who had other exposures, including 8 from the first study. Are you seriously telling me that when your newsletter said that we studied the exposed workers in the 1949 accident, you meant the 8 out of the 162 deaths, ignoring the

other 154? Mr. Carr is referring precisely to this paper in his allegations, but as I explained above he got it all wrong. When his own witness (Dr. George Roush) detailed his errors in court (again page 44, Attachment III), Mr. Carr made no objection as long as the final correct figures did not go into the record.

Point (2). You have misquoted me. I said "rates," not "ratios." Both rates and ratios have rather precise meanings in epidemiology. Rates measure risk; ratios measure God knows what, depending on their definition. EPA and NIOSH have put time and money in the last decade into trying to find out whether or not a PMR is ever actually an estimate of the death rate, so the issue is not a trivial one. What we did measure, and why, is explained above. The phrase at the end of the second paragraph in the newsletter section titled "the Monsanto case" is therefore false even if Carr's calculations had been correct, which they were not.

Point (3). The transcript for the relevant day of the trial is enclosed.

Point (4). Mr. Carr was indeed referring to my work. He just didn't know what he was talking about. I doubt that he made up the whole thing. I prefer to think that the wrong arithmetic left him with such a good feeling that he never looked any further. If you need more evidence on the nonsense that Carr produced show the whole mess, including this letter, to any reputable epidemiologist.

One of the Kafka-esque things about this whole issue is that all Carr has to do to be believed is to utter an irresponsible charge without even explaining how he got to it. In order for me to be believed, I have to convince you that Carr is wrong.

There are other strange things. Jenkins' letter to Loehr, which you say is the basis for the Monsanto part of the newsletter article, has a date stamp from Loehr's office of March 7, 1990, which is the same date as the newsletter. I cannot believe, therefore, that the newsletter was based on the material that you copied for me with that assurance.

However, I am of such an age that I must limit the number of windmills against which I tilt. I will be satisfied if my name is cleared.

Yours sincerely,

William R. Gaffey
William R. Gaffey, Ph.D.

WRG:mw
Encl.