

and Chemother. 2:1 (Jan.) 1952] states in the title that his patient's aplastic anemia is "due to chloramphenicol" but in his conclusion is less positive, stating only the obvious, that it "appears that the blood dyscrasia followed prolonged administration of chloramphenicol". We are aware of some additional cases, some of which may be the subject of a report. In an exchange of letters on these, one of the authors who plans to publish stated "Admittedly proof that Chloromycetin caused the aplasia is lacking. The evidence is only circumstantial".

We will not criticize these reports and others which may appear from any viewpoint except to point out that they are based on the premise that as one event precedes another in time, there is a cause and effect relationship. One must realize that this is only the first step in such reasoning. The fact that a drug was administered prior to development of aplasia is by no means proof that the drug is the offender. At this time, there are absolutely no cases known to use in which such proof is existent.

We have had similar experiences with both Mapharsen and Dilantin when they were first introduced commercially and the same problems have been encountered with streptomycin, the sulfonamides, thiouracil and others; therefore, the present situation is not entirely unexpected. We are increasingly convinced of the clinical superiority of our product as demonstrated by the many millions of doses given throughout the world during the past several years. Clinical investigation of the effects of Chloromycetin on body cells and functions is continuing and several additional studies were recently initiated, but, to repeat, up to this date we cannot find any facts that will indicate that Chloromycetin causes aplastic anemia or agranulocytosis.

It is recognized that the publications referred to frequently give rise to inquiries from your customers and that this problem is further complicated by the unethical tactics being employed by representatives of certain competitors. With respect to the former, it is recommended that you reply to such inquiries in harmony with the comments contained in this letter. With respect to the latter, you may be assured that we will not stoop to combat this type of competition but will continue to detail Chloromycetin on its demonstrated merits.

The primary concern of Parke, Davis & Company has always been and always will be to develop and sell only drugs which will protect or promote health and to advance the cause of medicine. We are continuing to adhere rigidly to those precepts.

Sincerely,

H. J. LOYND, President.

PRESIDENT'S LETTER No. 1—JUNE 6, 1952

The New York Times of May 23, 1952, carried a story under a Boston date line to the effect that Dr. Louis Weinstein, chief of the Infectious Disease Service, Massachusetts Memorial Hospitals, told more than 2,000 physicians attending the 171st annual meeting of the Massachusetts Medical Society that he knew of forty cases in the country in which chloromycetin had produced "very severe depression in the bone marrow".

That same day, at my suggestion, Dr. Gray wrote Dr. Weinstein asking him to produce whatever evidence he might have in support of the statement attributed to him.

Dr. Weinstein's reply is reproduced verbatim herewith:

MASSACHUSETTS MEMORIAL HOSPITALS,
JOHN C. HAYNES MEMORIAL,
DEPARTMENT OF INFECTIOUS DISEASES,
Brookline, May 26, 1952.

J. P. GRAY, M.D.,
Parke, Davis & Co.,
Detroit, Mich.

DEAR DR. GRAY: I have your letter of May 23rd inquiring with respect to the statement in the New York Times concerning my remarks on bone marrow depression produced by chloromycetin. In the first place, I have no manuscript of my talk since it was given mainly from notes. I did not make the statement that I *knew* of 40 cases. The statement I made was, "It has been said that there are somewhere around 40 cases of severe depression of bone marrow which have