Letters to the Journal

Demethylchlortetracycline

To the Editor:—The article on demethylchlortetracycline (Declomycin) by Shapiro and Phillips (*JAMA* **176**:596-602 [May 20] 1961) should have appeared in the advertising section instead of the section on clinical science. How such a mass of uncritically collected and pseudoscientifically analyzed observations could have escaped editorial censorship is hard to understand. The fact that these observations were tabulated electronically fails to lend credence to the conclusions, despite the elaborate protestations of the authors to the contrary.

By their own admission, no bacteriologic controls were attempted, yet bacteriologic conclusions are drawn. Table 4 lists a dozen or more organisms, many of which are normal inhabitants of the respiratory or gastrointestinal tract, and the response of these organisms to demethylchlortetracycline is judged not by culture but by "clinical impression." Table 5, relating to respiratory tract infections, in which the drug was used with about 90 per cent favorable response, includes 400 cases categorized as "URI, cold, etc.," all self-limited diseases. There is no doubt but that demethylchlortetracycline had some effect on some of the infections reported, but how much is anyone's guess.

This paper is an example of specious "scientific writing," devised by hucksters who are to be congratulated on hitting upon a scheme for getting maximal distribution of advertising at minimal cost.

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This letter was referred to the authors of the article in question, whose reply follows.

To the Editor:—"Demethylchlortetracycline in Clinical Practice" was motivated by our desire to show that streamlined data-processing techniques enable physicians to receive relevant facts about new compounds much more rapidly. An explanatory abstract originally submitted with the paper was not printed. It is reproduced below in order to clarify further the original intention of the authors:

Special data-processing methods were devised to facilitate the handling of almost 5,000 case reports on demethylchlortetracycline. The reports were analyzed for therapeutic and bacteriologic effectiveness, incidence of side effects, and comparison with other anti-infective agents. Such very broad clinical investigations in many thousands of patients nicely supplement smaller carefully controlled studies and often yield information not always obtainable from investigations in a few hundred patients. Because subjective diagnoses and estimates of improvement are not enough to establish fact, the ultimate usefulness of a drug cannot be evaluated until many physicians have used it for many patients. And, because it is of vital importance for both the physician and the manufacturer to recognize as quickly as possible whether a drug will answer the needs of the patient, it is essential that large quantities of clinical data be accumulated, analyzed, and interpreted rapidly. Modern data-processing equipment makes this possible.

Correlating pretreatment bacteriological studies with clinical responses is, of course, controversial. Therefore, we attempted to indicate that the tabulated responses were clinical, not bacteriological, in nature. They were included merely to act as a rough guide as to how a certain infection might respond. Naturally, there is no substitute for in vitro sensitivity studies for the individual patient, but these cannot always be done.

Inclusion in Table 5 of 406 miscellaneous cases is also questioned. This criticism is difficult to understand, inasmuch as analysis of the table shows that the percentage of favorable responses in this category is equal to 1 and lower than 5 of the other 6 categories included.

Although presentation of all data as received from investigators inevitably leads to inclusion of controversial matters, it would seem to us far preferable to the alternative of arbitrary discrimination on our part.

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Barium Sulfate and RAI Test

To the Editor:—A probably erroneous statement regarding the effect of barium sulfate by ingestion or enema on the radioactive iodine uptake of the thyroid or the protein-bound iodine serum level appeared in the Question and Answer section of The Journal (**175**:1037 [March 18] 1961).

The consultant, in response to questions regarding the ingestion of barium or the use of barium sulfate enema on the I^{131} uptake, stated that a significant concentration of iodine from the barium

sulfate "obtains in the blood stream sufficient to affect the determination of the protein-bound iodine level or uptake of I^{131}."

In a complete review of this subject (Factors Which Influence Radioactive Iodine Thyroidal Uptake Test, *Amer J Med* 28:397-415 [March] 1960), I pointed out that the usual American intake of iodine varies from a calculated amount equivalent to 126 to 760 μgm of iodine per day. It was also noted (*ibid.*, p. 402) that "Barium sulfate (U.S.P., for radiologic diagnosis) contains less than five parts of total iodide and iodine per million by weight. About 200 gm. of barium sulfate powder used in the routine radiologic examination of the upper gastrointestinal tract would therefore provide less than 1 mg. of iodine per examination. Even if all the iodine were absorbed, this amount of barium sulfate in a single dose probably would not significantly effect the I^{131} thyroidal uptake." Barium sulfate by itself as used in patients who are *not* undergoing gallbladder studies or intravenous pyelograms probably does not have any effect on radioactive iodine uptake or the level of the protein-bound iodine. This important point has not yet been emphasized to the medical profession.

The recommendation of the consultant that any examination of the gastrointestinal tract be deferred until iodine metabolic studies are completed is a dictum which has been passed on through the years from one authority to another without documentation or experimental foundation. If further studies of a scientific nature on this particular question have been performed recently, it would be well to utilize such documentation in either supporting or refuting the above recommendation.

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Cow Dung and Insanitary Flooring

To the Editor:—In regard to the query on insanitary flooring by Dr. E. Walter Masters, of Columbia, S. C., and the reply thereto by Dr. Harry Most (Questions and Answers, *JAMA* 175:931 [March 11] 1961), let me be permitted to mention a very striking instance of the value of unbaked cow dung in preventing the spread of plague in infected areas, based on a controlled experiment made in Hyderabad, Deccan, years ago by a medical officer on plague duty.

Muslims and Hindus form the normal population of that state, and it was observed during a severe epidemic of plague that the incidence in Hindu homes was much less, almost negligible during some months, than in Muslim houses. The Hindus from time immemorial are in the habit of making an emulsion of cow dung in water and applying it with a piece of cloth over the entire flooring of their houses, and sprinkling it well in their front courtyards; however, cow dung is taboo to the Muslim. Incidentally the cow is held as a sacred animal by the Hindus and its dung is used as a disinfectant regularly even today. Controlled experiments revealed the fact that the rat flea *Xenopsylla cheopis* obviously hated the smell and taste of cow dung and did not thrive on floors smeared with it. The observation proved at that time to be a welcome prophylactic measure, and would appear to have been utilized in the control of the epidemic.

Dr. Most's letter in reply to Dr. Masters is to my mind unconvincing, for even though cow dung does contain human fecal organisms derived from fecally contaminated soil on which cows laid their dung, the baking referred to must be able to kill all pathogens—which normally die even at 80° to 85° C. (176° to 185° F.) if baked for 10 minutes. Even today the regular use of clean fresh cow dung is most popular and a daily routine feature in all Hindu households in South India; the women folk particularly, rich and poor alike, cannot be weaned from their practice of using the raw fresh cow dung for smearing floors and courtyards of houses, and the dried cow dung cakes for lighting the fires in ovens for cooking. The burnt ashes of these cakes are routinely used for wiping brass and other vessels clean.

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Permission for Autopsies

To the Editor:—Those of us who have considered the reasons why families deny permission for autopsies have long felt that love and consideration have little or nothing to do with refusals. We have pointed out that the highest autopsy rates in our general hospitals have frequently been in the pediatric departments. No matter how low one's estimate of humankind, one is forced to admit that most men love their children; it seems unlikely, therefore, that love is the motive for refusing necropsy permission. Recently, you have published data in THE JOURNAL which demonstrate this argument very clearly.

In Table 13 (*JAMA* 177:629 [Sept. 2] 1961), in the article on graduate medical education, you list the 20 nonfederal hospitals with the highest autopsy rates. Of these, 13 (65%) are children's hospitals, located in many parts of the country. Since pediatric hospitals make up only a small fraction of the total hospitals, the preponderance of high autopsy rates in children is even more striking than these figures indicate. Apparently, where there is the most love there is also the greatest understanding that doctors need to know all about illness and death.

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