

National Institutes of Health, we gave the conclusion that despite a substantial amount of material and a lengthy period of observation, our data provided no conclusions at all. Word was also received that Dr. Wied had reached a similar conclusion concerning his own voluminous data and therefore had not come to present it.

Following the presentations at this meeting, several participants suggested that Dr. Malemed refrain from submitting his report for publication until he had more data himself, or at least until one other group anywhere had confirmed his conclusions on the basis of their own studies. Dr. Malemed objected to these recommendations and felt that the study should be published at least as a preliminary warning.

Following the meeting in Washington, it is my understanding that Dr. Malemed submitted his report to the Journal of the American Medical Association. As is customary, the J.A.M.A. distributes copies of any submitted articles to several consultants to determine whether it is suitable for publication. *My understanding is* that the Journal editors were advised not to accept the report for publication, primarily because of major questions concerning the interpretation of the data. Subsequently, the article was submitted to other major medical journals and similarly was not accepted in the United States. Finally, the authors submitted the article to one of the major medical journals in England and it was published in the British Medical Journal although there were significant modifications in the article that was finally published, as compared to the one originally submitted in the United States.

In short, there was no conspiracy to keep this report from publication, but rather an honest, scientific doubt as to whether the publication of a very preliminary and controversial report, in a major medical journal such as the J.A.M.A., would lend too much weight to the conclusions drawn. With our present rapid communications and the quick transfer of information published in scientific journals to the press and broadcasting media, conservative scientists felt that such a report might cause great hysteria, among women using the pills, as well as their husbands. This, of course, could be acceptable if the conclusions were really definitive but in the absence of combined informed opinion there was little justification in causing such a tremendous scare.

While I am on the subject of publication of articles and again referring to the so-called conspiracy between the J.A.M.A. and the drug industry and some physicians; I would like to relate briefly my own experiences with the J.A.M.A. For many years, I have served with no compensation as a consultant (as have other doctors) to the Journal in reviewing articles that are submitted for publication which happen to be in my own particular area of interest. Over the years, I have had a substantial number of reports referred to me for review and recommendation. In looking over my files, I find that beginning about 1960 when oral contraceptive reports began to be prepared in increasing numbers, and I was consulted because of our early studies, I have suggested that the J.A.M.A. publish substantially more articles relating to side-effects than those that related to pro-pill data. Appended to this report will be photocopies of excerpts of correspondence between the editors and me relating to several reports which were favorably reviewed by me and then accepted by the J.A.M.A., and, when published, related to pill side-effects.

Specifically, in reviewing some of these reports I find, for example, that in October 1962, an article was submitted for publication by Doctor David O. Weiner and associates, of Dallas, Texas, which was entitled "Phlebitis Developing While Under Treatment with Norethynodrel-Mestranol (Enovid). This referred to only a single case report, and my note reads: "I feel that this case report is an interesting one and merits publication particularly in view of the recent (lay) publicity concerning oral contraception and its possible relationship to thrombophlebitis." My review sheet states "Accept" and, I believe, this was one of the earliest medical reports to link the pills with thrombophlebitis. The report was accepted for publication by the Journal at that time.

In November 1963, there was a report submitted by Doctor Ervin Schatz, from Henry Ford Hospital in Detroit, also relating thromboembolism to oral contraceptives and my recommendation was this article be published with perhaps some slight editorial revisions that appeared necessary. It is my understanding that this article was also published with subsequent revisions suggested by the Journal.

Also in November 1963, there was a report submitted by Doctor Charles