I have no better guess than Dr. Hellman, as to why this should be so, but I suggest that since this is far more frequent and far greater a problem than anything that has been attributed by anyone to the pill, it merits at least as much attention as the pill does.

Now, how do we study this very difficult kind of a problem? One way is to study whether pill takers themselves have a higher frequency than other groups.

This was the test approach by Drs. Drill and Calhoun when they compared the rate of thrombophlebitis in pill takers in various clinics with the rates in certain other populations. I have reproduced here the data from their controversial paper. The frequency on their combination type contraceptive trials was 0.55 per thousand women per year, and it is compared for perspective to incidences in other types of populations. Now, this study has been very properly attacked. It was not intended to be a definitive study.

We recognize the fact that the population from which this figure was derived was from clinics where women were monitored all too seldom, and where the dropout rate in certain instances was very high. Therefore we decided to look at one of our collaborative clinical trials, where one single preparation was used and where every woman was checked every month by questioning as to whether she had any symptoms. This study amounted to some 360,000 cycles in approximately 10,000 women. Our value of 0.56 is incredibly close to the value of 0.55 in the Drill and Calhoun study, in spite of the fact that a different drug was used, a different method of questioning was used, a different computer analysis was used, and in spite of the fact that our women were looked at every month and theirs were not.

I can draw only one conclusion from this: that even increased monitoring of the population failed to show a dramatic change in the incidence of thrombophlebitis. At best, it says that a substantial risk in numbers does not exist. It does not constitute proof or disproof of the original contention.

Now, how do we answer the original contention?

There are two ways: (1) We can do a prospective study where 20,000 women who are going to go on contraception, some on pills, some not, are followed for 10 years, and we look for changes in the frequency pulmonary embolism and thromboembolic disease. This takes a long time; it is very difficult to find the populations and terribly expensive. As a substitute, and statisticians recognize that this is a substitute, the (2) retrospective approach has been used in England and in the Sartwell study in the United States.

The crux of the matter is the reliability of the retrospective study. You have only to look at Mainland's book, "Elementary Medical Statistics," and Berkson's numerous comments in the literature, or those of many other statisticians who emphasize the inherent risks of the retrospective study carrying within itself an unknown bias which changes the results.

Now, where would the bias come from? The bias comes from the fact that a retrospective study is only as good as the matching of the patients with the controls. In order to match a patient with a control, you should know every factor which could possibly influence that comparison, so that the only thing that is left unmatched is the pill-