these conditions, no evidence on the point is provided. In fact, one could and, I think, should have undertaken to interview a sample of ostensibly normal women to form some estimate of the extent to which this very high level of selection might eliminate some proportion of oral contraceptive users in the population. A priori it seems likely that an appreciable fraction of ostensibly normal women might fail to pass such a test. The result of such heavy selection could be serious. An inadvertent bias which might have only a minor effect were the selection less rigorous could have a profound effect when such a high rate of rejection is employed.

Further unavoidable selection—reducing the case-control pairs from 261 to 175—resulted from failure in some instances to obtain interviews from the case or either control. Again, it is entirely possible that availability for interview may be correlated with characteristics which are related to use of oral contraceptives, or to liability to thrombo-embolic phenomena. (The potential effect of such relationships could be studied from the data on the abstracts for all the abstracted cases and controls. I understand that this data can be and will be made available. I should welcome the opportunity to review it.)

A final note on selection, and the possible bias which may be entailed: The report notes that among prospective controls those matching the demographic characteristics of the case in the record room index, it was sometimes necessary to review over 100 cases to obtain a suitable match. (see p. 50). Any inadvertent bias, say having a tendency to exclude oral contraceptive controls because of incidentally related characteristics, could be overwhelmingly magnified in the presence of such extreme selection as was exercised here.

Unfortunately, the commendable effort to exclude all cases from non-relevant strata of the population tends to open the door to other biases of comparable magnitude. There is no ideal middle ground, and this problem is one of the inherent difficulties faced by retrospective studies.

Evidence of possible bias

From the careful reporting in the study itself it is possible to see evidence of relevant population strata which are not equally represented in the two groups.

Table 5 (p. 70) shows the distribution of cases and controls on a number of factors which were not used as matching variables.

There is some evidence that Catholics are less represented among the cases than among the controls, substantial evidence that the less well educated are likewise less represented among the cases, and marked evidence that patients connected with medical settings are better represented among the cases. If, in fact, non-Catholics, better educated, and medically connected people tend more often to be oral contraceptive users, the disproportions in these variables between cases and controls could represent a serious bias.

A dramatic example of the difficulties which beset studies of this type is provided by the situation of the student nurses (p. 60). It is concluded that such individuals are more subject to hospitalization for thrombo-embolic phenomena than are others, and it is suggested that there may even be an elevated true rate of thrombo-embolic phenomena among hospital employees who must stay on their feet for considerable periods. One need only add the possibility that hospital employees may have readier access to oral conceptives than do most control patients, and the possibility for spurious association between oral contraceptives and thrombo-embolic phenomena is evident. (In fact, the number of admitted oral contraceptive users in this group was only two, and they do not contribute substantially to the evidence. The point is that there may be other more significant relationships of a similar kind. In part these may be elucidated by study of the individual case-control data).

Comments on specific conclusions in the report

Although most favorably impressed with the evident care and thoroughness in the conduct and reporting of this study, I disagree with a number of the assertions and conclusions drawn.

p. 45—The report says that the retrospective approach was appropriate and criticises the prospective approach as "difficult, costly, and slow." In view of the impossibility of obtaining convincing results from more retrospective studies, this affirmation seems to me misguided.

Both in the fact that this method is suggested for study of possible effects on cancer incidence *and* in the fact that the committee makes no recommenda-