To the Editor:

Your editorial on "The Environmental Time Bomb . . . That Never Went Off," which appeared in the January/February 1977 issue of Ca-A Cancer Journal for Clinicians, referred to the article by Dr. Leonard Chiazze et al. in the November 15, 1976 issue of JAMA (236:2310-2312). You wrote: "Their analysis shows that the abnormally high figure reported in 1975 was probably related to an unusually high mortality from influenza in early 1975 coupled with low rates during the same months in 1974."

I read the Chiazze article very carefully and found that they did not say that "their" analysis of the data showed what you reported, because they presented no data on this question. Chiazze et al. did say: "The staff of the NCHS believes that the abnormally high figure reported by August 1975 may have been (italics mine) related to an unusually high mortality from influenza in January and February of 1975 and unusually low rates for these months in 1974."

It is possible that someone on the staff of the NCHS may have said this sometime in 1976, but data that I have collected with the help of the NCHS on a possible relationship between increased actual annual mortality rates (in contradistinction to projections made on incidence during a limited period) for influenza during non-epidemic and interpan-
demic-epidemic years and for certain other diseases, including cancer, showed no such relationship. The annual mortality rate (not age-adjusted) for all malignant neoplasms, especially but not exclusively for cancers of the respiratory system, has been rising steadily in the United States from 1971-1975, without reference to epidemic outbreaks of influenza A virus, which resulted in small increases in mortality from influenza (primary diagnosis on death certificate) but not from pneumonia, etc.

I also found that annual mortality rates calculated from incidence during January-April are quite different from the actual 12-month mortality rates for the same year, and should not be used as indicators of what is happening in a given disease entity during the whole year.

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Editor's Reply:

The unusually high cancer mortality rate in the first seven months of 1975, based on the 10 percent sample, has been attributed by staff scientists at both the National Cancer Institute and the
American Cancer Society to earlier deaths associated with the influenza epidemic in the winter of 1975. Although there is no hard, substantive data, an analysis of annual cancer mortality rates from data collected by the Metropolitan Life Insurance Company over a long period of time indicates considerable association and supports this theory. Characteristically, a short-term increase in cancer mortality early in the year is presumably followed by a relative decrease in mortality in the balance of the year.

The point Chiazze and his associate made in their paper is that the 10 percent sample of deaths is not appropriate for evaluating long-term trends in cancer mortality. Rather than a 5.2 percent increase in cancer rates in the first seven months of 1975 compared to 1974 as originally reported, or a 2.3 percent increase based on crude death rates for the 12-month period, the authors show that most of this increase is based on the aging of the population and that the age-adjusted cancer death rate was only 0.7 percent higher in 1975 than in 1974.

The final irony that underscores the editorial in Ca-A Cancer Journal for Clinicians is that when the actual cancer mortality figures for 1975 became available recently, the age-standardized rate for 1975 was 130.9, a 0.7 percent decrease from 1974.

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To the Editor:

In the January/February 1977 issue of Ca-A Cancer Journal for Clinicians, the problem of treating a patient with postmenopausal bleeding and a diagnosis of adenocarcinoma of the endometrium was discussed in the Questions and Answers section.

As to whether or not Premarin is indicated in patients who have been diagnosed as having endometrial carcinoma, I think one can be relatively certain that although we have not proven it to be a carcinogenic agent, the incidence of adenocarcinoma in patients who have received exogenous hormonal therapy seems to be increased. This may indicate that although it is not a carcinogen in itself, it may play a role in enhancing tumor growth.

Until this issue has been settled, to suggest the use of Premarin in patients who are treated for endometrial carcinoma, especially those with advanced disease, should not be advised at this time.

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Author's Reply:

My answer related to the potential effect of Premarin on the breast after removal of the uterus. So far, there is little conclusive evidence to show that Premarin has a harmful effect on the breast, which cannot be reversed by stopping the Premarin. It is my practice to advise using Premarin only when symptoms cannot otherwise be controlled, although I do suggest minimal dosage under the direction of a gynecologist.

In addition, the first of my statements should have been deleted or changed to state: the prognosis of early uterine cancer adequately treated is excellent.

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