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The above letter was referred to the authors of the article in question, who offer the following reply.

To the Editor: Drs. Sheps and Lenfant raise several important issues in their discussion of our article. In responding, we hope to further clarify our position that economic evaluations are an important and underappreciated component of the process of creating national guidelines such as JNC VI.

As they mention, JNC VI was designed to guide primary care clinicians in the diagnosis and management of hypertension, using high-quality evidence when available and expert consensus when necessary. The force of JNC VI, however, goes well beyond the primary care physician. Adapting, implementing, and monitoring compliance with guidelines is a system-wide effort undertaken by health delivery systems and managed care groups that often have a direct economic stake in following (or not following) the guidelines. It is true that cost should be a secondary consideration for physicians when they are caring for their patients. Nevertheless, institutional decision makers cannot afford to ignore the economic considerations of their policies regarding treatment options that are laid out in guidelines.

Drs. Sheps and Lenfant state that the economic attractiveness of generic diuretics and β -blockers are self-apparent, yet a recent study shows that prescription patterns are following a trend sharply in favor of newer agents that are far more expensive than those recommended by JNC VI.^{1,2} Why is this so? We believe it is in large part due to the pharmaceutical industry, which suggests in its advertising that the newer agents offer substantive clinical advantages (eg, shorter time to control, fewer side effects) compared with older agents. Because physicians do apparently ignore cost in their care decisions, these new expensive agents are adopted with little regard to the cost consequences for insurers or society.

Our study was designed to show physicians and decision makers in health care delivery systems that even accounting for the nuances of hypertension care (compliance, monitoring costs), the price of the agent drives the cost of care, even in the short run. Like clinical trials, economic models have limitations in their methods and generalizability. Our model followed the recommendations of JNC VI and used data cited from this report wherever possible because we believe this report is the most internally and externally valid summary of hypertension care that is available. Of course, local costs and practice patterns will vary, but our sensitivity analyses suggest that these issues will not alter the bottom line.

Numerous studies have shown that it is expensive to alter practice patterns. These investments will necessarily be made by organizations that need to ask hard questions about the tradeoffs between costs and consequences of using their resources to promote change in the clinical community. It behooves the National Institutes of Health to make these economic tradeoffs explicit when they create guidelines for clinical practice. This process does not necessitate making recommendations based on economic outcomes. The economic section of JNC VI does not provide explicit quantitative data and thus is of little use for decision makers. In the case of managing hypertension, we show that following the JNC VI recommendations is economically and clinically a win-win situation. We expect other cases to be less clear.

In an era when economic factors can and often do influence medical decisions, we believe it is important to have high-quality, objective economic data available alongside clinical data for common conditions such as hypertension. The National High Blood Pressure Education Program is an ideal and yet unrealized forum for such information.

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In-Flight Radiation

To the Editor: Your readers should be aware that the article by Robert F. Barish on in-flight radiation¹ is based on a now-discredited linear no-threshold hypothesis of radiation health risk. *Nuclear Issues*² reports that the American Nuclear Society has issued a position statement to the effect that "there is insufficient scientific evidence to support use of the linear no threshold hypothesis (LNTH) in the projection of the health effects of low-level radiation on which regulation of low levels of radiation adopted by international and national radiation protection authorities is based."

Also, the US National Council on Radiation Protection (NCRP)³ has stated that "few experimental studies, and essentially no human data, can be said to prove or even to provide direct support for the concept of collective dose with its implicit uncertainties of no-threshold

linearity and dose-rate independence with respect to risk."

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References

1. Barish RJ. In-flight radiation: counseling patients about risk. *J Am Board Fam Pract* 1999;12:195-9.
2. Nuclear Issues 1999;21(5). Available from Ruvigny Mansions, Embankment, Putney, London SW15 1LE.
3. Principles and applications of collective dose in radiation protection. NCRP report: no. 121. Bethesda, Md: National Council on Radiation Protection and Measurements, 1995.

The above letter was referred to the author of the article in question, who offers the following reply.

To the Editor: I thank Dr. Foster for his thoughtful comments on my article, but he is incorrect in claiming that the linear no-threshold hypothesis (LNTH) of radiation dose versus effect is now discredited. At present there is a debate in the radiation protection community about the applicability of the LNTH. Compelling arguments exist on both sides, and the issue has not been settled.

Some professional organizations, including the American Nuclear Society (ANS), have taken positions on the subject, but none has the authority to discredit the hypothesis. The ANS position statement, with all of its supporting documentation, is readily accessible.¹ They recommend that independent experts conduct a review of available data and perform new studies with the goal of obtaining a better model. Many in the radiation protection community feel that any position statement rejecting the LNTH should not be published unless it is accompanied by a specific alternative hypothesis, advocated as a replacement. The ANS has not done so.

Similarly, Dr. Foster's quote from a National Council on Radiation Protection and Measurements (NCRP) document is taken out of context.² That line, in fact, appears in a section that reaffirmed use of the LNTH model as an underlying principle of radiation protection by the Council. Recently, the NCRP convened a panel of experts to examine the issue of the LNTH. The draft report of NCRP Scientific Committee 1-6 "Evaluation of the Linear Nonthreshold Dose Response Model" is available for viewing and comment.³ It is NCRP policy that draft documents cannot be quoted, so at the time of writing of this letter I can only suggest that interested readers look at the draft themselves and decide whether the LNTH is now discredited.

The issue of dose modeling and the LNTH has also prompted a request by the Environmental Protection Agency (EPA) to the National Research Council/National Academy of Sciences to form a new committee with the purpose of updating the BEIR V report on the biological effects of low-dose radiation.⁴ The new committee, which will produce BEIR VII (BEIR VI dealt with radon), has been the subject of intense political activism on the part of interest groups who believe that

proposed members might bring a preexisting bias based on previous associations with pronuclear or antinuclear causes. These issues of membership are presently delaying progress in the committee's activities.

In summary, the use of the LNTH is still endorsed by all of the regulators of radiation exposure in the United States including the EPA, NRC, OSHA, and the FAA. At present there are several professional societies that have taken sides in the ongoing debate about the validity of this model, but none has advocated a specific alternative hypothesis as a replacement. A forthcoming NCRP report will reflect the council's position. The BEIR VII committee will also evaluate the validity of the LNTH versus other dose-effect models. In my article, I was careful to state on page 196, "... the risk of very low dose radiation remains unproved and might in fact be nonexistent. . . ." I believe that I was quite clear in stating that I used the LNTH because it is the model currently advocated by national and international organizations that influence regulatory policy.

At the end of the paper I discussed the possibility of an early warning system for major solar particle events. A pregnant patient could briefly postpone her trip until conditions returned to normal, usually within a day or less. Such a system has now been put into place using a toll-free number that can be contacted just before boarding.⁵

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5. In-Flight Radiation Protection Services, Inc. 1-877-SUN-FLARE, or 1-877-786-3527.

Inpatient Care of Children

To the Editor: I read with some interest the article in the March-April 1999 issue of the *JABFP* by Drs. Bertolino and Gessner¹ dealing with pediatric admissions by family physicians and pediatricians in a semirural environment.

My comments and questions relate to the implication and comments made in the article as well as to the proposed conclusions. Although the article did a nice job of reviewing hospitalized patients, it did not do a com-