WHO Guidelines for Mild Hypertension

TO THE EDITOR:

The timely publication of the "1986 Guidelines for the Treatment of Mild Hypertension: Memorandum from the WHO/ISH" in your journal provides physicians with a rational protocol for managing mild hypertension and is a welcome step toward evolving a uniform therapeutic approach. As many physicians are likely to follow the information given in the flowchart (see Figure 1 in Reference 1) for its simplicity, we wish to call attention to certain disparities between the text and the flowchart.

The text states that the physician should consider drug treatment when the diastolic blood pressure (DBP) exceeds 95 mm Hg, after a second 3-month observation (during which nondrug measures have been tried). However, the flowchart indicates, at the corresponding decision point, that drug treatment should be considered when the DBP is 95 mm Hg or above after a 3-month observation. Thus, the period before initiating drug therapy is abbreviated to 3 months instead of the 6 months indicated in the text. A 6-month observation period is certainly needed, along with nondrug measures, if the DBP is below 100 mm Hg, as about 40% of all placebo-treated patients in the Medical Research Council Trial had DBPs of less than 90 mm Hg on annual revisits, and a similar observation was made in the Australian Trial.

The second disparity between the text and the flowchart regards the follow-up interval for patients whose DBPs remain below 95 mm Hg. A 3-month follow-up has been advised in the text to detect a rise of blood pressure and to reinforce nondrug measures effectively. The flowchart, however, indicates "BP measurement every 6 months" for this group.

As the flowchart is likely to be referred to far more often than the text by physicians using these guidelines in their daily practice, appropriate corrections need to be made in the flowchart so that the guidelines are accurately followed.

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References

AUTHOR’S RESPONSE:

Drs. Kumar and Reddy are drawing the reader’s attention to some inconsistencies in the "1986 Guidelines for the Treatment of Mild Hypertension." Their point is well taken. The text was the subject of a lengthy and lively discussion during which it was repeatedly amended in order to achieve compromises in a number of details. These amendments, however, are not reflected in the flowchart. In conclusion, two 3-month periods of nondrug treatment and observation have been suggested for patients with diastolic pressures between 95 and 99 mm Hg. As stated in the text, patients whose diastolic pressures remain between 90 and 95 mm Hg should be reassessed at 3-month intervals. Drug treatment is also to be considered for such patients, especially those at high risk.

Texts produced by committees are rarely perfect. The WHO/ISH Mild Hypertension Liaison Committee is committed (no pun intended) to the continuing improvement of the guidelines, and any constructive suggestion for the forthcoming 1988 revision of the text is most welcome.

T. STRASSER, M.D.
Coordinator, Mild Hypertension Liaison Committee

Hypertension 10: 350, 1987

Dietary Calcium Intake in Hypertension

TO THE EDITOR:

In their recently published analysis of National Health and Nutrition Examination Survey (NHANES) I and II data, Sempos et al. concluded that low dietary calcium intake and hypertension were not related. This conclusion is contrary to six other analyses of the relationship between nutrient intake and blood pressure in NHANES I data, including a previous report from Sempos et al., and an analysis of NHANES II. In these reports, a relationship existed between dietary calcium and blood pressure in the overall population or in a subpopulation. Each analysis used different criteria for subject inclusion and different statistical analyses, and therefore, each had different sample sizes and compositions (Table 1). Harlan et al. reported that dietary calcium, phosphorus, and alcohol were the only nutritional variables consistently related to blood pressure. This result was similar to our analysis of NHANES I by different methods, which demonstrated an association between hypertension and reduced calcium and potassium intakes. Although the first report of Gruchow et al. demonstrated this effect of calcium on blood pressure only in nonwhite men, a subsequent report indicated that the dietary sodium/potassium ra-
tio was significantly and positively related to blood pressure, but only in subjects with low calcium intake; the effect was absent in subjects with a high calcium intake. Furthermore, in their earlier report, Sempos et al. observed that diastolic blood pressure was significantly higher in the 20th percentile compared with the 80th percentile of calcium intake in NHANES I. The reason for the apparent contradiction between this report and the more recent publication of Sempos et al. is unclear.

Excluding analyses of NHANES I and II data, there have been 12 reports of an inverse relationship between dietary calcium and blood pressure in cross-sectional populations. Notably, these include reports in ethnically diverse populations from Europe, and the United States, analyses of data collected four decades ago, and analyses of large, well-known epidemiological studies, such as the Multiple Risk Factor Intervention Trial, the Western Electric Study, and the Honolulu Heart Study. The difficulties of accurately measuring nutrient intakes and blood pressure in cross-sectional studies bias toward the likelihood of finding no difference in blood pressure between divergent calcium intakes. Therefore, within these constraints we believe that the data are remarkably consistent in demonstrating a weak, though plausible relationship between dietary calcium and blood pressure. Moreover, we believe stronger evidence for this association exists in clinical studies that have assessed the effect of calcium supplementation on blood pressure. In randomized trials comparing the results of calcium supplementation with placebo treatment, a small though significant reduction in blood pressure has occurred with calcium supplementation in both normotensive and hypertensive subjects. We and others have interpreted these findings from intervention trials as direct evidence of a potentially important effect of calcium on blood pressure regulation. In our opinion, future progress in understanding any effects that calcium has on blood pressure will more likely come from additional intervention studies designed to test the effect of dietary calcium supplementation or restriction on blood pressure, and from basic research into the cellular mechanisms, rather than from cross-sectional epidemiological studies.

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References

Table 1. NHANES I Nutritional Analyses of Blood Pressure

<table>
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<tr>
<th>Reference</th>
<th>No. of subjects analyzed</th>
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<th>Analysis</th>
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<td>25–74</td>
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<tr>
<td>Harlan et al.</td>
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<td>Yes</td>
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</table>

(Hypertension 10: 350–352, 1987)

AUTHORS’ RESPONSE

The comments of Drs. Morris and McCarron concerning our investigation of the association of systolic (SBP) and diastolic blood pressure (DBP) with dietary calcium intake, based on data from the first and second National Health and Nutrition Examination Surveys (NHANES I and II),

2 seem to fall into four categories. First, in an early abstract we noted a statistically significant negative association between dietary calcium and blood pressure (BP), contrary to the results we presented in Hypertension. Second, six other published analyses based on NHANES data reported finding a statistically significant negative association between dietary calcium and BP. Third, other observational studies have also found an association. Fourth, clinical trials and basic research offer the best mechanism for resolving the issue. (Parenthetically, we suggest the following corrections in Table 1 of the letter of Drs. Morris and McCarron: The sample size given for McCarron et al. should be 10,321, and, as we stated in the title of the abstract, the age range for Reference 7 was 40–74 years.)

It is correct that in our preliminary analysis of NHANES I data mean DBP for persons with a calcium intake of 250 mg or less was significantly greater than the mean DBP for persons with a calcium intake exceeding 1000 mg, after adjusting for age, sex, race, and body mass index (BMI). It is also correct that the association in NHANES I was confined to black men.

In NHANES II, however, there were no statistically significant associations between SBP or DBP and dietary calcium intake after adjusting for age, sex, race, and BMI. Furthermore, when the calcium groups were formed by dividing calcium intake in NHANES I and II into quintiles instead of by arbitrary cutoffs, there were no significant differences in either survey after adjusting for age, sex, race, and BMI.

In a previous abstract we used the calcium intake cutoffs of ≤250, 251–500, 501–750, 751–1000, and >1000 rather than quintiles.) Thus, in using several different approaches for classifying calcium intake and in using both NHANES I and NHANES II data, we found a statistically significant relationship only for black men and only in NHANES I. As a result, we view this as a chance finding.

We are aware that at first sight our results appear to contradict the results by other investigators who used the NHANES I and II data. Drs. Morris and McCarron, as shown in Table 1 of their letter, have interpreted the results from these studies as supporting a negative association between dietary calcium and BP. As we have stated previously, however, a careful comparison of the reported findings resolves the apparent contradiction.

First, in attempting to replicate the findings of McCarron et al., it was found that the age-adjusted correlation was not statistically significant, in contrast to the unadjusted correlation between SBP and dietary calcium. Next, in each of two articles by Harlan et al., eight different stepwise regression models were used to predict SBP and DBP in different age, race, and sex groups in NHANES I, and in each report the coefficient for dietary calcium was negative and statistically significant in only one of eight models. Similar to our results for NHANES I, Gruchow et al. found an association only among black men. Because sodium intake data from the NHANES did not include discretionary salt use, any analyses that try to relate the NHANES sodium data with BP must be viewed with great caution; until the final data are published it is hard to judge the conclusions stated in the abstract. Using NHANES II data and stepwise regression models, Harlan et al. reported that the coefficient for the intake of foods containing calcium (not dietary calcium itself) was negatively associated with both SBP and DBP in women but not in men. In another NHANES II analysis, dietary calcium was not found to be associated with SBP or DBP in white men aged 40 to 59 years.

Finally, Drs. Morris and McCarron cite reports of a negative association between dietary calcium and BP in other cross-sectional studies, and they state that the results from properly designed clinical trials and basic research will be the key methods for resolving the issue. We do not dispute the fact that an association has been found in other observational studies. We also discussed at considerable length the epidemiological and statistical issues associated with cross-sectional analyses, in general, and with the NHANES data, in particular.

That being said, the central issue remains: Has a consistent, statistically significant negative association between dietary calcium and BP been found in the NHANES I and II data? In our view, the answer is no. The results from NHANES I and II do not support the
WHO guidelines for mild hypertension.
M V Kumar and K S Reddy

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