

### What's New in Hypertension?

The most important question about hypertension is: "Does the treatment of mild-to-moderate hypertension reduce cardiac mortality?" I was surprised to read in the ARCHIVES the statement by Pearce, "The risk for MI over a 5-year period is reduced by an average of 10% with pharmacologic control of stage I hypertension. . . ."<sup>1</sup> In my own recent review of this subject,<sup>2</sup> I had arrived at a different conclusion. Wondering what I had missed, I tracked down the author's sources.

Pearce cited a 1990 article in *Lancet*, which reported: "In prospective observational studies, a long-term difference of 5-6 mm Hg in usual DBP [diastolic blood pressure] is associated with about 30-40% less stroke and 20-25% less coronary heart disease (CHD)."<sup>3</sup>

The article itself, however, did not support this assertion but referred to a preceding article by the same group.<sup>4</sup> In that review of nine major, prospective, observational studies involving over 420 000 individuals in 6 to 25 years of follow-up, the authors concluded:

Previous analyses have described the uncorrected associations of DBP measured just at "baseline" with subsequent disease rates [which were not statistically significant for heart disease]. But, because of the diluting effects of random fluctuations in DBP, these substantially underestimate the true associations of the usual DBP (ie an individual's long-term average DBP) with disease. After correction for this "regression dilution" bias, prolonged differences in usual DBP of 5, 7.5, and 10 mm Hg were respectively associated with at least 34%, 46%, and 56% less stroke and at least 21%, 29%, and 37% less CHD. These associations are about 60% greater than in previous uncorrected analyses.<sup>4</sup>

These authors calculated the magnitude of this "dilution bias" to be 60%. Their conclusion is based on this projected 60% enhancement of the reported results from the actual studies.

Should we buy it? I would urge caution. This calculation represents an aspiration, not a finding. While the authors' observations and corrections are reasonable, they are not perfect. What they are calling "usual" DBP may very well, in fact, not be the usual DBP as we would find if we used continuous ambulatory BP monitoring, and this number may be dramatically inaccurate if "white-coat hypertension" was at all prevalent in the Framingham population.

Since this answer is critically important, I am ex-

tremely reluctant to accept it merely on faith. Until we have hard data from a study that is done properly, we may feel that these authors' conclusions provide a persuasive basis for action, but they should not be regarded as the definitive answer.

Colin P. Kerr, MD, JD, MPH  
The Hamburg Center  
Hamburg, Pa

1. Pearce KA. Challenges in the management of stage I hypertension. *Arch Fam Med*. 1993;2:717-720.
2. Kerr CP. Hypertension in the 1990s: a new disease. *J Am Board Fam Pract*. 1993;6:243-254.
3. Collins R, Peto R, MacMahon S, et al. Blood pressure, stroke, and coronary heart disease. II: short-term reductions in blood pressure: overview of randomized drug trials in their epidemiological context. *Lancet*. 1990;335:827-838.
4. MacMahon S, Peto R, Cutler J, et al. Blood pressure, stroke, and coronary heart disease. I: prolonged differences in blood pressure: prospective observational studies corrected for the regression dilution bias. *Lancet*. 1990;335:765-774.

In reply

Dr Kerr's reluctance to accept that treatment of mild hypertension with drugs reduces cardiac mortality is fully defensible, although his reluctance to accept that it reduces the risk for myocardial infarction (MI) is less so. Furthermore, his argument rests on his skepticism toward statistics associated with observational studies rather than statistics associated with the clinical trials that I cited.

I agree that we lack direct evidence that the treatment of mild-to-moderate hypertension reduces deaths from coronary heart disease, and I have not said otherwise. One reason for the lack may be the relatively short duration of the major clinical trials. However, the evidence is quite good that such treatment reduces the risk for MI, even in the short run. This point is best demonstrated by the five unconfounded major clinical trials of the treatment of mild hypertension,<sup>1-5</sup> included in the meta-analysis by Collins et al,<sup>6</sup> to which Dr Kerr and I have referred. Although Dr Kerr apparently became distracted by a reference by Collins et al to a companion meta-analysis of observational studies by MacMahon et al,<sup>7</sup> the meta-analysis of clinical trials clearly demonstrates an average 10% reduction in the incidence of coronary events (ie, fatal MI, nonfatal MI, and sudden cardiac death). This reduction was illustrated without the use of sophisticated statistics (always a plus for my own reading comprehension) in Figure 1 in the article by Collins et al.<sup>6</sup> The data therein were as follows: there were 468 coronary events among 15 238 patients in