Letters to the Editor

The Journal welcomes Letters to the Editor; if found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with journal style.

Management of Hypertension

To the Editor:

I would like to commend Dr. Forsyth on his survey of hypertension in office practice *(Hypertension in a primary care practice. J Fam Pract 10:803, 1980)*. It is encouraging that studies of office based populations are now being done. Of particular interest to me was his suggestion that the later onset of cardiovascular disease in women as compared to men may be related to the later onset of hypertension in women shown in his population.

However, I have some serious reservations about two of Dr. Forsyth's four conclusions, namely, that "the level of diastolic blood pressure receive primary consideration when a determination is made as to whether or not an individual should be treated . . . (and) . . . the diastolic blood pressure be used to monitor the response to medication." I contend that these conclusions on the one hand are not warranted by his data and, on the other, are misleading and not supported by the preponderance of current epidemiologic information. As regards the first point, it is interesting that Dr. Forsyth finds a marked difference in the patterns of systolic and diastolic blood pressures in different age groups; he also notes that the age distribution of diastolic hypertension in his study population correlates better with the age distribution for coronary risk found in the Framingham Study. But this correlation is hardly justification for his conclusions, particularly when one considers that Dr. Forsyth himself admits that his data bear no conclusive relationship to the prevalence of hypertension in the community at large. Secondly, data from The Framingham Study[1] showed clearly that elevated systolic pressure is a risk factor for cardiovascular disease. Other studies have shown that diastolic pressure is no better a predictor of coronary risk than is systolic pressure.[2,3] Lastly, he does not consider the risk of stroke which is clearly related to levels of systolic pressure.[4,5] Any physician following Dr. Forsyth's guidelines literally (eg, ignoring the systolic component of hypertension when judging response to medication) would probably be doing his patients a disservice.

I reiterate that I find Dr. Forsyth's data interesting and stimulat-

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ing as far as they go. The kind of inquiry he conducted needs to be done. But I object to the unwarranted conclusions of his paper and fear they may encourage mismanagement of hypertension.

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2. Salel AF, Fong A, Zelis R, et al: Accuracy of numerical coronary profile correlation of risk factors with arteriographically documented severity of atherosclerosis. N Engl J Med 296:1447, 1977

3. Reid DD, Hamilton PJS, McCartney P, et al: Smoking and other risk factors for coronary heart-disease in British civil servants. Lancet 2:979, 1976

4. Rabkin SW, Mathewson AL, Tate RB: Predicting risk of ischemic heart disease and cerebrovascular disease from systolic and diastolic blood pressures. Ann Intern Med 88:342, 1978

5. Kannel WB, Dawber TR, Sorlie P, et al: Components of blood pressure and risk of atherothrombotic brain infarction: The Framingham study. Stroke 7:327, 1976

The preceding letter was referred to Dr. Forsyth who responds as follows:

In criticizing my May 1980 article on hypertension, Dr. Hahn inaccurately states that the article advocates "ignoring the systolic component of hypertension" thereby risking strokes. He also states that "correlation is hardly justification for . . . conclusions." The relevant quotes from my article are that the "findings *suggest* that for the prevention of *coronary* heart disease, *emphasis* should be placed on the . . . treatment of diastolic hypertension" and "the diastolic blood pressure (should) be used to monitor the response to medication."

To emphasize the control of the diastolic blood pressure in the prevention of coronary heart disease is in no way equivalent to ignoring the role of systolic blood pressure in strokes. Dr. Hahn was not justified in ignoring prior qualifying statements.

His comments on correlations and conclusions have some merit. It is a fact that a correlation does not confirm the presence of a cause and effect relationship. However, this does not mean that it is improper to state one's findings and then note the implications regarding treatment. That is precisely what was done in the studies which Dr. Hahn cites. The Framingham study[1] found that there was a strong correlation between systolic blood pressure and strokes. The therapeutic implication of that finding is that systolic blood pressure should be lowered regardless of the level of the diastolic blood pressure (eg, treat a patient with a blood pressure of 170/80 mmHg). That recommendation has been made and is being followed despite the fact that the controlled studies which have demonstrated a reduction in strokes have been based on diastolic hypertension. I found that diastolic blood pressure correlated with coronary heart disease pattern more than did systolic. The therapeutic implication is that diastolic blood pressure

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be treated despite the systolic blood pressure (eg, 140/100 mmHg). To point this out does not constitute "an unwarranted conclusion."

What needs to be discussed are the contradictory findings and not the legitimate therapeutic implications of those findings. Dr. Hahn seems to imply that my findings are invalid because previous studies have found that systolic and diastolic blood pressures are equivalent risk factors for coronary heart disease. Were he to have critically analyzed the Framingham study, he would have found that it did not differentiate between data from hypertensives rendered normotensive by treatment and data from normotensives. It did not differentiate between data from reactive hypertensives and sustained hypertensives. It employed controversial statistical techniques[2] such as including data from the same individual up to five times in a ten-year cohort, and the application of multivariate analysis. It was not, as was my study, designed specifically to analyze hypertension. Hence, I cannot downgrade my findings and their implications simply because they are at variance with dogma.

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1. The Framingham Study: An epidemiological investigation of cardiovascular disease. Bethesda, Md, National Heart Institute, 1968

2. Werko L: Risk factors and coronary heart disease: Fact or fancy? Am Heart J 91: 87, 1976

Compliance-Oriented Prescribing

To the Editor:

It is a pity that Dr. Fischer's article "Compliance-Oriented Prescribing: Simplifying Drug Regimens" *(Fischer RG: J Fam Pract*

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10:427, 1980) is incomplete, in that he leaves out thyroxine and digoxin as two other drugs which, in this country at least, are still often given twice a day, and which could easily be given once a day.

It would also be a shame if many people got the impression that dose regimens were a very important factor in compliance. Many of us feel that this is much more dependent upon the physician-patient relationship.

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The preceding letter was referred to Dr. Fischer who responds as follows:

In response to Dr. Gau's letter, I am sorry he perceives my article to be incomplete. The main objective of this article *(Fischer RG: Compliance-Oriented Prescribing: Simplifying Drug Regimens. J Fam Pract 10:427, 1980)* was to make physicians aware that many drugs originally approved by the FDA for multiple daily use are now approved for single-daily administration. Other drugs, without FDA approval for single daily administration but with reports in the literature, are also described. Both thyroxine and digoxin have always been approved for once a day administration and it is my experience most physicians in the United States are aware of this fact. Medical teaching and practice may be different in London and, if so, I apologize for omitting these drugs. In regard to thyroxine, this is also routinely prescribed for once a day administration in this country. Since the plasma half-life approaches seven days, this drug may be administered less often. Several studies have reported success with once a week administration.[1-3]

I agree with Dr. Gau that it would be a shame if readers gained the impression that simplification of dosage regimens was the single most important answer to improving patient non-compliance. Obviously, there are many factors involved in patient non-compliance, some more important than others in specific patients. This article focuses on one area, simplifying drug regimens, which may be the prime reason for non-compliance in many patients, especially the elderly, the illiterate, and those on multiple medications.

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References

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2. Bernstein RS, Robbins J: Intermittent therapy with L-thyroxine. N Engl J Med 281:1444, 1969

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