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

To the Editor: Dr. Crouch's critique of my recent editorial is welcomed. In my brief essay, I stated my reluctance to administer cholesterol-lowering medication to an active asymptomatic, thin, 75-year-old woman who exercises regularly, is on a low-fat diet, and whose total blood cholesterol measures 8.20 mmol/L (318 mg/dL). Among reasons for my reluctance was my statement, "the relation between *total* cholesterol blood levels and either mortality from coronary heart disease or total mortality in persons aged 60 years or more is *uncertain*."[p 61] Dr. Crouch writes, "Froom's first argument, however, is not valid." Apparently my *uncertainty* is invalid because analyses of Framingham data reported by Anderson, Castelli, and Levy in 1987[1] differ from those reported by Castelli, Wilson, Levy, and Anderson in 1989.[2] Dr. Crouch is incorrect when he states that a larger number of elderly were involved in the 1989 study. It is likely, but uncertain, that they were identical. The number of patients followed for 30 years in the 1987 report is given as 4374 (excludes deaths and those with cardiovascular disease and cancer at entry). The 1989 report notes that 5209 persons from Framingham were enrolled but omits mention of the number of patients subjected to the analyses. That the 1987 report concerns mortality and the 1989 report incidence may be a source of Dr. Crouch's confusion.

It is curious that although the authors of both studies were virtually identical, the 1989 report does not cite or even discuss findings reported in 1987. Why is an increased level of incidence not accompanied by an increased mortality from the same disease in the same population? There are other concerns as well. The 1987 report notes,

> Falling cholesterol levels—the negative slope variable—were associated with elevated overall mortality and CVD mortality in both men and women and fourteen percent of men and 20 percent of women had cholesterol slopes that were negative.... Persons, whose cholesterol levels dropped 14 mg/dL during 14 years would be expected to have 11 percent higher death rate than persons whose cholesterol levels remain constant or rose during the same period.[p 2179]

Although cancer has been associated with dropping cholesterol levels, a full explanation of this finding in the Framingham population is *uncertain*. Dr. Crouch may derive a sense of certainty from these studies about the relation of total cholesterol blood levels and mortality in the elderly, but I confess that I remain *uncertain*.

Dr. Crouch acknowledges that data demonstrating benefit in the elderly for medical treatment of hypercholesterolemia are lacking and that such treatment may have risks. Yet, he suggests, "If, however, the existing functional status could be preserved for an additional several years, the benefit of such treatment would justify some degree of expense and an acceptable level of risk for adverse effects." Is Dr. Crouch implying that our current knowledge justifies such an approach? Do we have adequate evidence in asymptomatic elderly persons that cholesterol-lowering treatment will preserve existing functional status?

Dr. Crouch is correct when he notes that cholesterol-lowering treatment will not extend longevity. Dr. Taylor and colleagues[3] estimate that for persons aged 20 to 60 years, a lifelong program of cholesterol reduction might gain from 3 days to 3 months in life expectancy; for those at high risk, the estimated gain ranges from 18 days to 12 months.[3] Gains for persons more than 60 years will, of course, be less.

The article by Moore[4] was written for the general public and is an excellent antidote to the current hysteria engendered by the National Cholesterol Education Program (NCEP).[5] Dr. Crouch's criticism of this article would be more enlightening if specific inaccuracies were cited rather than the generalizations used in his condemnation.

Concerning my article on the consequences of the NCEP,[6] Dr. Crouch states, "Froom's earlier article, on the other hand, raised problematic practical issues that should be addressed." What does Dr. Crouch suggest? Short of repudiation of the NCEP by the National Heart, Lung, and Blood Institute, as we suggested, how will we deal with the estimated $13 billion in costs to test and classify the blood cholesterol status of all American adults older than 19 years? How will we deal with the generation of 15 additional daily office visits per 1000 adult patients? How will we manage the increased risks of malpractice and the adverse consequences of labeling asymptomatic patients as ill?

Dr. Crouch and I agree on a need for additional research, but we appear to differ on our approach to the management of a clinical problem in a specific age group. Lacking adequate data on benefits and risks of therapy, I prefer to follow a conservative path. We also agree to an individualized approach to the care of our patients. Unfortunately, the rigid guidelines for the testing and management of hypercholesterolemia proposed by the NCEP and the broad acceptance by the medical community impede an individualized approach. It was a reaction against this rigidity that prompted my essay.

Jack Froom, M.D.
Stony Brook, NY

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2. Castelli WP, Wilson PW, Levy D, Anderson KM. Cardiovascular risk factors in the elderly. Am J Cardiol 1989; 63:12H-19H.
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5. Report of the National Cholesterol Education Program Expert Panel on Detection, Evaluation, and Treatment of High Blood Cholesterol in Adults. Arch Intern Med 1988; 148:36-69.

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Postvasectomy Semen Analysis

To the Editor: As a family physician performing vasectomies, I read the article "Postvasectomy Semen Analysis: Why Patients Don't Follow-Up" by Dr. Smucker and his colleagues[1] with great interest. I was very surprised to read that one of the methods of semen sample collection was withdrawal. I don't consider withdrawal to be an effective form of contraception and would not want to recommend it to a patient while trying to establish aspermia. This would be especially unsafe for the occasional patient with an anomalous vas.

Daniel Leeny Stulberg, M.D.
Camp Verde, AZ

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

To the Editor: I appreciate Dr. Stulberg's thoughtful comment, which refers to an important detail when counseling patients regarding postvasectomy semen analysis.

While physicians in our group often mentioned withdrawal as a method for collecting a semen specimen, it was never recommended as an effective form of contraception during the initial weeks following vasectomy. Indeed, the most important task for the physician in giving postoperative instruction is to make certain that the couple will be using an effective method of temporary contraception until aspermia can be established. Most patients will choose to use oral contraceptives, a diaphragm, or condoms with or without spermicidal agents.

When a diaphragm or oral contraceptives are used for contraception, withdrawal during intercourse can safely be used as a method of semen specimen collection. If the couple is already using condoms for contraception, it is easiest to instruct them to use simply a plain condom to collect the specimen.

We encourage patients to use either masturbation or a plain condom as the best methods to collect a postvasectomy semen specimen. For the occasional patient who feels uncomfortable with these two methods, early withdrawal during intercourse is a reasonable alternative *if* another effective form of contraception is being used by the couple.

Douglas R. Smucker, M.D.
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Smoking Cessation and Stress

To the Editor: I am writing in response to Dr. Morris's letter regarding "Epidemiological Abuse" that appeared in the January-February 1991 issue of the *Journal*.[1] While I agree with his basic premise, I would like to take issue with the specific example he used regarding counseling patients not to discontinue smoking during periods of increased stress. For too long Dr. Morris's assumption has been perpetrated by the medical community with no medical or scientific data to back up this assumption. In fact, I would submit that the opposite is true. In support of this, I refer to articles on a smoke-free psychiatric unit, published in *Hospital and Community Psychiatry*,[2,3] and *Journal of Psychosocial Nursing*.[4] These articles concern a closed psychiatric unit that developed a no-smoking policy. The study found no increase in use of sedatives after the smoking ban compared with before the smoking ban. Many of the problems the medical community has assumed to be associated with smoking cessation have little factual basis. Until the medical community recognizes nicotine use for the addiction that it is and treats it as such, we will make little progress in reducing the morbidity and mortality from this disease. Following Dr. Morris's reasoning, we should also counsel our alcoholics, cocaine addicts, and narcotics addicts not to discontinue using their particular substance of choice during times of personal crises.

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