

Jan 19, 1978

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Arnold S. Relman, M.D., Editor
NEW ENGLAND JOURNAL OF MEDICINE
10 Shattuck Street
Boston, Massachusetts 02115

Dear Dr. Relman:

I have not had confirmation of your receipt of my letter dated December 15, 1977, commenting on the Medical Intelligence article of George Mann. It is my feeling that an expanded letter section may be as appropriate to that article as you have found it appropriate to the VA Bypass Surgery Trial article and editorial. I would hope that the Mann-directed letters would concentrate on the factual and conceptual errors in his report rather than assuming his polemical style.

At any rate, I would be grateful to hear from you on whether you plan to devote an issue to replies to his article. Certainly, the article has done much harm worldwide in turning by distortions of fact a vast majority of the profession (which accepts your publication as a peculiar form of gospel) against very important public health concepts of the cause and prevention of arteriosclerosis.

Sincerely,

Henry Blackburn, M.D.
Professor and Director
Professor of Medicine

HB/rs

P.S. Congratulations on your decision about the replies to the VA article and on your remarks about randomized trials for surgical procedures which could save the public much money and anguish.

encl. pg my Dec. '75 letter

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UNIVERSITY OF MINNESOTA
TWIN CITIES

December 15, 1977

Laboratory of Physiological Hygiene
School of Public Health
Stadium Gate 27
Minneapolis, Minnesota 55455

Arnold S. Relman, M.D., Editor
NEW ENGLAND JOURNAL OF MEDICINE
10 Shattuck Street
Boston, Massachusetts 02115

Dear Doctor Relman:

I shall leave to others a detailed and thoroughgoing reply to the numerous inaccuracies and distortions of Medical Intelligence by George Mann in your September 22, 1977, issue. I will reply only to the specific mentions of the work of this Laboratory which has been misrepresented and improperly referenced in that article.

The most remarkable and misleading of the citations is the following:

"The diet-heart hypothesis was inflated by Keys in 1953. He used a selection of data from the World Health tabulations to conclude that in six countries, experience with coronary heart disease was correlated with available food fat. The naïveté of such an interpretation of associated attributes is now a classroom demonstration."

Dr. Mann failed to cite the measured systematic studies of this Laboratory over the subsequent 24 years which grew out of that earlier first look at associations. These followed in a logical sequence with careful population comparisons and laboratory diet experiments. The latter established with mathematical precision the overriding importance of habitual diet in determining *population* distributions of serum lipids (as opposed to the predominant non-dietary factors in individual blood lipid differences). Epidemiological research goes in logical steps just as do clinical and bench research. It may be this sequence that escapes Dr. Mann as well as the important difference between causes of individual and population levels of blood lipids.

Dr. Mann completely misinterpreted and misrepresented and misreferenced the work of this Laboratory in regard to exercise "protecting" against coronary heart disease in the Seven Countries Study. In the first place, his Table 8 was from an article by myself in the reference given below. It deals with *prevalence*, not *incidence*, of coronary heart disease. Dr. Mann failed to interpret these epidemiological data correctly and this led him to potentially erroneous conclusions. The bias existing in such cross-sectional comparisons suggests a much more cautious conclusion about those findings. They certainly did not show *"a genuine sparing effect of occupational exertion on coronary heart disease, and the tables of Keys's 'seven-country study' indicate quite clearly that exercise protects in those populations."*

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On the contrary, the most elementary understanding of cross-sectional population data would indicate why people in such general populations having significant ECG findings might be involved in less active occupations and vice-versa.

Finally, Dr. Mann refers to the Finnish Hospital Study in which our Finnish collaborators participated. We would simply point out that he made a greivous error in citing the pooled adjusted data on all-causes death in men on diet. He gave rates as 38.84 per 1000 person-years compared to 39.50 in control periods. The actual data are 34.84 on diet versus 39.50 in control periods!

Sincerely,

Henry Blackburn, M.D.
Professor and Director

HB:msh

Reference: Blackburn, H., Parlin, R.W., and Keys, A.: The inter-relations of electrocardiographic findings and physical characteristics of middle-aged men. In: Epidemiological Studies Related to Coronary Heart Disease (A. Keys, ed.), Acta Med. Scand. (Suppl.) 460, 1967.