October 30, 1974

file

George V. Mann, M.D.
School of Medicine
Division of Nutrition
Vanderbilt University
Nashville, Tennessee 37203

Dear George:

For quite awhile I've carried around two issues of the NEJM and finally, between San Diego and New Orleans recently I enjoyed your review on obesity. You obviously enjoyed doing it. I would like very much to have a reprint if you're not already out of them.

Of your many useful syntheses, the criticism of Hirch's thesis I found specially important. Their idea is based of course, on quite weak and insufficient data.

It was good to point out the problems in relating overweight, hypertension and coronary disease. Despite the fact that our former mentor here has insisted on the lack of an independent contribution of overweight or obesity to coronary risk, I agree with you that a clinical trial of weight reduction is needed. However, I predict that such a trial, along with physical conditioning, will never be done, in our time, with the goal of primary prevention -- because of difficulty and cost. Meanwhile, I think that the association, probably causal, between calorie excess and elevated blood pressure, is sufficient to be positive rather than negative about the preventive potential of weight reduction and better yet obesity prevention.

You surely have noted in the Seven Countries Study, but did not specifically consider in your review, the rôle of habitual physical activity in the question of population obesity, as contrasted to the individual problem in which the higher the calorie intake of a population, the thinner the population. At least this holds for rural groups.

Regards,

Henry Blackburn, M.D.

HB:jp
enclosures

NEJM Editorial
Controversy
Progress
Amsterdam
Figures XVII.15 and 16 (pgs 182-3) of Seven Countries Study

11-13-74

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November 11, 1974

Dr. Henry Blackburn, M.D.
University of Minnesota
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School of Public Health
Stadium Gate 27
Minneapolis, Minnesota 55455

Dear Henry:

Thanks for your letter. I enclose a reprint from a dwindling supply.

Your pessimism about trials of the affect of treatment of obesity on CHD is justifiable. It probably couldn't be done--nor do I think it could prove worthwhile. But a trial of exercise would be relatively easy--likely to be productive. Indeed, the public is going it alone for lack of scientific leadership. You, in fact, are likely to be embarrassed because you obstruct. The neat thing about a fitness trial would be the ease of measuring adherence.

Oh well, we are still waiting for the sun to go down on the diet fat--giant molecule serpents--see, e.g., the AHA program. When that is finished maybe we can get on with useful work.

Regards,

George

George V. Mann, M.D.

pc + return of it.

George:
Passing opinion + evaluation is not obstruction. Being proved wrong is a revelation — not an embarrassment, if from a rational base. You keep saying for us to get on with "useful work." We still wait for the sun to go down on the promising hypothesis. Henry

vdk 11-14

UNIVERSITY OF MINNESOTA
TWIN CITIES

Laboratory of Physiological Hygiene
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November 15, 1974

Dr. George V. Mann
Vanderbilt University
School of Medicine, Div. of Nutr.
Nashville, Tennessee 37203

Dear George:

Your "trumpeter toward the rear" slap cues my annoyance with your superior attitude as the custodian of truth and your self-assurance that physical activity is "it". I still can't decide whether it is your self-deception or inadequacy in evaluating the data on this question or whether you simply evaluate the situation differently than I do, and others. I am asking for your basic evaluation of what I consider to be:

1) the apparent weakness of contribution of activity class to prediction of coronary risk in populations or individuals,

2) the consequent infeasibility (relative) of studying this contribution in a randomized trial in the free-living population over and above the contribution of the "stronger" risk elements.

Have you considered the sample size estimates for such a preventive effort? Have you considered that conditioning exercise would have to be examined in a subgroup (huge!) of a multiple risk factor trial because of the infeasibility of a "pure" single factor trial?

I do wish, maybe at Tampa next February, you would give me some evidence that you consider these design questions as well as you consider the mechanism questions.

Then we can better judge whether your comments are reasoned or otherwise. In other words, could you please answer for me these simple questions about your activity hypothesis?

1) What is the nature of the population group or groups you believe should be submitted to the definitive exercise trial which you say I am "obstructing" (age, sex, risk class, primary, secondary, etc.)?

2) What is the degree of risk reduction (for what endpoints of disease or death) you wish the trial to be designed to demonstrate and that you think is a reasonable estimate?

3) What do you estimate as a reasonable lag time for reaching optimal treatment effect (months or in years, etc.)?

5-15%/yr. x 3-5 yr

4) What is a reasonable estimate of drop-outs or poor adherance in a trial of how many years duration?

5) What statistical confidence do you want to have in any difference found (alpha)?

see about

6) What statistical power do you want to have to be sure to detect the difference you postulate in (2) above (beta)?

And I do wish you would stop imputing political and vested interests or "obstructionism" to those who simply evaluate evidence in ways differently from yourself.

I think my questions are fair and I challenge you to respond to them in a straightforward manner.

Regards,

Henry Blackburn, M.D.

HB/kn

December 9, 1974

Dr. George V. Mann
Vanderbilt University
School of Medicine, Div. of Nutrition
Nashville, Tennessee 37203

Dear George:

I'll sign off. Our correspondence is unhealthy; my intentions were honorable in opening it with admiration of your Progress article.

No claims are made for remarkable ideas or researches but I do enclose the list of research projects on-going here, for your interest. The testing of important hypotheses in generally well-designed clinical trials may not be your cup of tea but it is a logical outgrowth of prior observations and has nothing to do with an entrenched protective posture which you think I have.

If you really consider Morris' work as definitive, rather than simply consistent with a protective effect of exercise, and think it adequately accounts for confounding variables, then you may have real problems evaluating epidemiological evidence. Also, if you had had the courtesy to peruse rather than return peremptorily the opinions and observations I sent you, you would have seen that I shared your criticism of the Finnish Hospitals' Study.

I still hope you will listen one day to one person, who tells you openly that you may have some problems of evaluating certain types of evidence. Of course, this observationscomes from one who is much less sure of himself, and of passing scientific and moral judgments on others, than are you.

Cordially,

Henry Blackburn, M.D.

Enclose Framingham memo