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May 16, 1979

Professor Ancel Keys
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Dear Ancel:

Just a few belated comments on your fine opus. I quote from page 7 of the 10 year monograph on relative weight. "There is evidence that relative body weight can be estimated from the body mass index as 0.24 X BMI with no more than a trivial error." Is it not the other way around, that BMI can be estimated from relative body weight by .24 X RBW?

Page 9 of the same chapter at the end of paragrph 2 a typo "would occur by chance." In your chapter on multivariate analysis I wonder if it might not be of greater interest to add systolic and diastolic pressure separately to the other variables rather than simultaneously to determine their relative contribution. I don't expect much difference from the present situation but am always concerned when systolic and diastolic pressure are in the same equation.

Can you indicate for me the table which would justify your statements on Page 16 of the chapter on "Some Conclusions" that all-causes death rate tends to rise only if the cholesterol level is above 275 and CHD rates only with levels above 230? Your speculation on Page 20 of the chapter on "Conclusions" about the affect of nicotine on myocardial irritability. You might want to consider omitting this because it immediately asks the question whether you have documented the relationship to sudden death which we have systematically failed to do in our work. I suggest that this be the subject of a separate paper, to appear early, based on my detailed classifications of sudden death carried out the last couple of years. Your statement about irritability and blood supply being compromised may not make sense physiologically and it seems to me would require verification in the question of sudden death. It might be much better not to speculate here.

With regard to relative body weight, we're all subject to our prejudices but your lambasting of the viewpoint of the insurance companies is so frequent and repetitious throughout this document that it's almost embarrassing. A simple statement once would surely suffice, maybe in the Conclusions?

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I am wondering why you did not use absolute values of measures of overweight and fatness rather than deciles when there are dramatic differences within and between populations in this regard and when you do make occasional attempts to look at this matter for blood pressure and cholesterol. In all circumstances in which excess risk was found in the lowest deciles would it not be necessary to thoroughly address the question whether this excess were concentrated in the period shortly after entry into the study? I don't expect that to be a major issue but it is certainly a major question.

Finally, it seems to me that the importance of dependent relationships might have been emphasized a little more. Is it possible that your whole career, beginning with the study of physical activity and body composition on through the population studies has lost an exciting anthropological and human biological thrust, in discussion of such devices as: "multivariate analysis is usually required before arriving at a positive conclusion about any independent effect of ...."? You have shown so beautifully how active populations are lean populations. You have shown so beautifully how blood pressure and cholesterol make up a large fraction of the variance in cohort incidence rates and how they in turn are significantly correlated with overweight. Here you almost make fun of physical activity in western nations by characterizing it as "curious" with respect to the cultural values in agricultural populations. I would hope that your conclusions could resolve themselves in a public health view which would be useful for those who are attempting to implement reasonable and safe public health recommendations on a community level. Of course, it is good you point out so effectively areas that need more explanation in the epidemiological data. Some of the findings do give interesting pause. But instead of bringing the material together to suggest what you have so long observed to be salubrious lifestyles and risk profiles, it is possible to infer from your conclusions that even in a high risk culture such as the United States one might be better off heavy than light, obese than lean, that it really makes no difference as long as one's cholesterol is under 240, that it really makes no difference as long as blood pressure is in the lower 2/3rds of the distribution, whatever that might be, and that light to moderate smoking of good old sun-cured tobacco may not be very bad after all! As for physical activity the huffing and puffing of the western male is simply curious nonsense. This totally misrepresents the seriousness of these mass phenomena.

Personally I find the 7 countries findings fascinating. I find your conclusions and your insistence on repeatedly beating the insurance industry over its head (while it laughs all the way to the bank) a rather unfortunate salvo from a person who has contributed as much as you have to human biology, knowledge of disease and the potential for prevention, as best manifest in lifestyles of populationsyou have explored.

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I clearly will enjoy writing a book review on your monograph, rather curious for a supposed "contributor" and collaborator!

Your preoccupation with all-causes death rate in this monograph may be confusing in comparison to the 5 year monograph and to the general or specialized reader. It actually spends more energy on all-cause of death than it does on coronary disease or cardiovascular death. This is not necessarily bad but it certainly is going to be confusing.

I guess I am put off by sweeping conclusions as "what seems to be the most reasonable conclusion is:that the habitual physical activity or its lack cannot be an independent, universal positive factor in promoting or protecting from early death or the development of coronary disease."

To see in your summary statement the sort of negative terminology applied to what you are essentially responsible for initiating, i.e. public health and preventive practice, that is "there's much pressure to apply ..." "what seems to be established" "even" to general public health action, "argument is loud" about the introduction of preventive efforts aimed at changing risk factors by changing lifestyle and the "clamor" is increased by commercial interests. Such terms as pressure, seems to be established, loud, clamor and commercial interests are words with negative emotional impact with which I am frankly not happy to be associated. If you really feel that there is no indication for your colleagues Stamler, Blackburn and others to be involved in research and public health policy to implement these rather obvious, harmless and probably salubrious aspects into a sedentary, luxurious lifestyle, then one is terribly puzzled by the meaning of your life and work. The fact that you would insist on such negative language in your "Final Grand Salvo" in which we are "contributors" is an interesting legacy which can only be considered "curious".

I would like to recommend your section on "The Future" be changed. I assure you nothing will be lost and considerable will be gained. It would be a shame if this important contribution to epidemiology, preventive medicine and public health should contain statements that can be so grossly misconstrued, and involve your contributors, in my case much again my will, in negative language quite unnecessary for the thrust of your arguments. May I propose that the section read something as follows to eliminate what I find distinctly unnecessary, if not sniping at your colleagues on the intervention effort which your old laboratory is carrying on in the application of knowledge to the public health. Your arguments against such applications, it would seem to me, would be much more appropriate in a vigorous scientific discourse with your colleagues rather than in print here. The opportunity to explore the epidemiological (non-intervention) ideas you have with younger colleagues and fellows of the Laboratory of Physiological Hygiene is always open. There is nothing in our conduct

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of preventive and public health measures and their evaluation, or the activities of your good friend and colleague, Jerry Stamler, which is inimical to continuing epidemiological explorations of the challenging sort that you are proposing.

Why end up with such a grumpy negative view? Why not reword your "Future" section somewhat as follows:

"The epidemiological approach to the problem of coronary heart disease, initiated barely thirty years ago, has now amply demonstrated great power in finding relationships, unearthing clues to etiology, and suggesting preventive measures. But many of the relationships are proving to be more complex than first proposed. There is much interest to apply what is known and what seems to be prudent, in preventive trials, preventive practice and guidelines to the general public. Irrespective of such public health advice it is clear that many people are already changing their habits of diet, smoking and exercise on their own accord.

The period of epidemiological exploration of these matters is not over and we insist there is still much to learn from study of "experiments of nature". The need for more and better epidemiological studies was never so obvious as now. Until now, all prospective studies, including the 7 Countries Study, have dealt with too few persons, the standard errors of the means and rates are too large. The instability of lifestyle in many populations greatly complicates the collection of representative data and their analysis but perhaps the instability itself should be included in the analysis as a risk factor. Moreover, the almost universal assumption that risk is a linear function of the characteristics of interest must give way to less restrictive ideas and mathematical models. Finally, besides our great concern about the incidence of coronary heart disease, new epidemiological programs should be more broadly concerned with all disease and death."

Doesn't this give a more positive message without emotional language such as "simplistic thinking," "we must counter the argument that now is the time for intervention", "argument is loud", "there is much pressure to apply what is known", and so forth, all evidence of your apparent irritation at our and others, "non-scientific" activity. Is this not inappropriate closure to such a fine dissertation? Is this language in a real sense not fair to your colleagues and collaborators who are doing the best they know to carry on efforts you started, without the advantage of your counsel?

Sincerely,

Henry Blackburn, M.D.

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cc: Henry Taylor