

April 24, 1957

William B. Bean, M.D.
Department of Internal Medicine
State University of Iowa
Iowa City, Iowa

Dear Dr. Bean:

Many thanks for your long and friendly letter re the dehydration experiment. In spite of your good work, we lost to bureaucratic manipulation. The purpose of which I don't completely understand.

The result of the conference we had with the Quartermaster Corps and the Office of the Surgeon General was that the experiment was postponed for at least a year. This will allow us time to write up the work that we have done in previous years and this is all to the good, as we have a tendency to do more experimental work than we can properly report. None of the work is classified and we will send you reports as they appear.

The Surgeon General's officials stated that they did not have any medical officers or consultants who knew enough about this subject to provide reasonable medical supervision. It occurred to me that you might possibly be interested in acting as a consultant in the event a new contract for work of this type is drawn up for 1958-59. (Our current contract terminates in May of 1958) In any experiment of this type the actual time that would require your presence would be short (3 or 4 days) and I am sure that agreement could be reached on experimental plans and procedures in the course of one preliminary conference. We have had a good deal of experience in working with collaborators from other institutions and have experienced very little difficulty in agreements on experimental plans, co-authorship, etc.

I realize that this is rather tenuous and that the work may never be done, but we have found that it is useful to explore possibilities of this kind early rather than at the last minute.

We have been told that Dr. John Youmans will assume a major full-time responsibility as scientific consultant to the Surgeon General of the Armed Forces next fall and we expect that we will have more active support and intelligent cooperation from this office than we have had in the past.

Sincerely yours,

HLT:ha

Henry L. Taylor, Professor

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UNIVERSITY HOSPITALS
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March 28, 1957

Dr. Henry L. Taylor
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Dear Dr. Taylor:

I have your letter of the 27th and the information you gave me in our conversation by telephone on the 27th. As I mentioned in our discussion, the experiments we did at Fort Knox on some 400 subjects studied under controlled experimental environments in various kinds of heat indicated that at the extremes of any large group of subjects were two kinds of people. They had apparently a different thermostatic arrangement in their thermo-regulatory mechanisms. One group was found to have a relatively low rectal temperature and a relatively high pulse rate for a given degree of work in the heat, and this pattern persisted even after acclimatization when the trend was that both pulse and temperature returned towards the levels found in the control work in the cool. The other extreme was the person with a high rectal temperature and a relatively slow pulse. Thus it would seem that adaptation can occur and if the temperature level is or has to kept low the circulatory strain is greater. If the temperature is high, it is less, though we do not know which is cart and horse; at least these two reciprocal relationships prevail, though the majority of any group will be in the middle with respect to the similar effects on temperature and pulse.

The second point was that for each new heat load, and I conceive this might be the same if dehydration is the equivalent of added heat as it might be in work, there had to be a reacclimatization. To be sure, this was simpler for people who had worked at 120° in the dry heat when the temperature of the new exposure was 150 than it was for those brought in from a cool environment to work for the first time in the heat at 150. But for each new heat load there had to be a new acclimatization period. Thus I can conceive, if my judgment is correct, and dehydration has certain equivalents to increased heat (environmental) during work, one might get higher temperatures earlier in an experiment than later. This point, however, though logical may not be correct, and I think the only way to learn is to do the actual test.

In all of our experiments in the hot room we encouraged drinking adequate supplies of water which contained salt since we were trying to eliminate those variables in studying acclimatization to heat.

Page -2-

Dr. Henry L. Taylor

March 28, 1957

Now the crucial point comes about the question of whether and where an arbitrary upper limit for temperature should be set. I see from your protocol that any sign of illness, even a clinically undefined one, will be ample reason for stopping a subject in any given experimental period of working. Certainly for a first-run trial, a rectal temperature of 105° is a very liberal one to set. It may turn out to be dangerous to let men work for periods of time with temperature between 104 - 105. Certainly some preliminary limit has got to be set and if everything works well in this experiment, another one might be established with a higher temperature. I do not believe, however, that under a program of severe dehydration as set forth in your protocol that subjects will be able to go on even though their temperature may not get as high, if they are severely dehydrated.

I had a very interesting experience in the desert in California when I was a Captain and persuaded a Colonel, I believe from West Point, but the details slip me, to take a walk with me in the desert having a jeep to lead us and letting him follow his program of absolute water restriction and let me drink as much as I wanted. We had a scale to weigh ourselves at the end of each hour. By the time the third hour of the walk was going on, I had kept my weight almost up to the starting weight drinking much more water than I wanted, and he had completely bogged down and was unable to go further, having lost approximately five pounds. We did not have ice and the thermometers were of no value since the ambient air was about 110 by that time so we couldn't tell anything about our temperatures but I am sure he had a fairly high temperature. Drinking water restored him and he could hardly believe that the demoralization of his fitness and morale were based on so simple a thing as water intake.

It was my impression then and from subsequent experience that what is called heat exhaustion is literally dehydration exhaustion. It may completely incapacitate a person without causing a very high rectal temperature and I would be of the opinion that would be the kind of trouble you would get into with your subjects rather than the problem of heat hyperpyrexia or heat stroke from an inadequate supply of water for sweating. This is all intensely interesting to me and as you can see I am indulging in much speculation.

If I may summarize, I would stress the following points: 1) in a first run trial under the circumstances of the experiment, I would agree that a rectal temperature of 105 should be an automatic cutoff point; 2) I suspect that dehydration exhaustion will incapacitate subjects considerably before the temperature gets to this level; 3) the possibility that heat hyperpyrexia, as well as heat dehydration exhaustion, may occur in these subjects should make this pilot test one of potential danger and thus requiring the most scrupulous and careful protection

Page -3-

Dr. Henry L. Taylor

March 28, 1957

for the subjects. It is all very well for us to speculate on what must happen, but in the last analysis the physician in charge of the procedure should have complete jurisdiction and the ability to withdraw any subject from the experiment when he deems it advisable.

This strikes me as a most interesting and important set of observations and if the material is not classified, I would be greatly indebted to you if you would send me a report of the highlights of your findings. Good wishes.

Sincerely,



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Professor of Medicine

(One-time Commanding Officer,
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