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F. D. [unclear]

November 29, 1973

Dear Geoff:

In reply to the query in your letter of November 20th I am unaware of any reason why a copy of Hurst's paper which he gave in October 1972 to the American College of Chest Physicians should not be passed on to TRC for circulation. I am enclosing another copy of Pg. 7 of the paper which hopefully may be better than the one you already have, but the footnote indicates even Hickson's original page was, in places, unreadable.

As I mentioned in another letter to you this morning, I was at a CRC Scientific Committee Meeting in New York yesterday. The decision was taken to discontinue the present experiment at Industrial Bio-Test Laboratories but for them to retain their colony of rats which is of the same strain as used by Passey. Apparently this is the only remaining colony of that particular strain and should they be lost it would introduce another variable into the repetition of the Passey-Eison experiment. (I think some of us secretly hope that the colony might be accidentally exterminated because we have had much discussion as to whether or not it is a suitable strain of rat to use). The decision does not mean that we will not go back to Industrial Bio-Test at a later date but we saw no point in having the present experiment continued.

During the discussion yesterday on what to do about the continuation of the Passey-Eison experiment, several of the basic questions concerning the objectives and the directions the experiment should take were again brought forth. I proposed, that as none of us were particularly experienced in animal exposure work, we ought to appoint a consultant and discuss with him the objectives the CRC wished to achieve in this area, Passey's and Eison's work, and the experiment at Industrial Bio-Test and to give serious consideration to his advice before launching another experiment. I proposed that Dr. Paul Hitesheia at Oakridge would, in my opinion, be an excellent person with whom to consult and also that it would be a good laboratory in

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which to have any future work carried out. The first suggestion was readily accepted but there was some concern that if the experiment itself was to be carried out at Cambridge, it would be very much in the control of our staff and we would not be able to control the dissemination of the results. They put forth the proposal that maybe we should have a consultant of a rather neutral stature and proposed Donald MacFarland of Bio Research, Pointe Claire. I had with me the letter concerning his proposed visit to Sweden which I think Mr. Gibb sent to you and the idea was immediately accepted. I did not at that time broach your suggestion of having the work done in Pointe Claire but preferred to wait and see what MacFarland's approach might be. John Hickson knew of Cliff Chappel, who is the President and owner of Bio Research, and had a high regard for his capability of handling laboratory animals as a result of some work done some time ago on saccharin.

The meeting's final decision was to arrange to meet with both MacFarland and Nettech-in separately on December 18th and to also write to Elson for any suggestions he might have if he were going to repeat the experiments. Since that decision we have learned that MacFarland will not be back until December 14th and I am not sure if our proposed date of Dec. 18th will hold.

Incidentally when the contract was let to Industrial Bio-Test the Committee felt that it was actually buying expertise at Industrial Bio-Test which would contribute to the setting up of a good experimental design. IBI's main laboratory is located at Northwood, Illinois and not too long ago they expanded to Decatur, Ill. We did not know at the time of letting the contract that our work would be carried out in the new laboratory at Decatur and although John Goode who is the head of the Decatur Laboratory is supposedly well experienced in the handling of animals and their exposures at Northwood, he has been too occupied with still further expansion plans to pay much attention to our experiment. This has no doubt contributed in large measure to difficulties which our work has experienced. (I know Reynolds were satisfied with IBI) It is quite readily accepted, however, that the Committee or at least a portion of the Committee will, in future, have to more carefully supervise projects of this nature.

You are certainly very right on the cost of the Lorillard Machine. The people at Cambridge did not know exactly what it would cost but they said it would be between \$50-75,000.

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To change the subject I am enclosing a copy of a letter that I received from Dr. Arthur Finch who is a visiting Professor at the University of Waterloo and is using letterhead stationary from Dr. Forbes' department. As he explains in his letter he has been directing research at the University of London (England) for a number of years. I should have sent you this letter earlier but I was away the entire week of the 19th and just didn't get around to it before. I have talked to Dr. Finch on the telephone and arranged for him to come and visit us on Dec. 4 and I hope that Jim Hogg will be able to sit in on the discussions. I have talked with Jim about the possibility of his doing this and while I think he was only moderately interested he indicated that if he were free he would be happy to come down.

I would consider Finch's finding an unexpected one and I am curious as to whether you may be aware of any of his work in England. I will try to give you a further report after his visit here.

With kind regards,

Yours sincerely,

*R.S. Wade*  
R.S. Wade

RSW/cp  
Encl.

cc: Dr. S.J. Green  
Mr. R.M. Gibb

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