

MEDICAL RESEARCH COUNCIL

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PRIVY COUNCIL

UNIT FOR BACTERIAL CHEMISTRY
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Dear Dr Spiegelman,

Thank you very much for your letter of March 12 which I was very glad to get. I enclose the reprints you ask for. I am very pleased you enjoyed the Nickeren paper & would like to discuss the points you raise in detail but I don't really think this can be done satisfactorily in a letter. I am much looking forward to meeting you some time during your visit here.

The mannitol v. formate question is to a large extent a mystery because although I quite agree that *Coli* will grow on mannitol & not formate as sole C source, I don't think anybody has any clear idea why. What, for instance, do you mean by "efficient energy source"? Formate will reduce O_2 or nitrate in the presence of organisms more rapidly than mannitol, therefore in one sense formate is a more efficient energy source than mannitol (or glucose). One is rather forced to the conclusion that formate cannot be synthesised into some essential higher C compound, while mannitol can. But I admit it might be, & you suggest, a question of energy relationships if you make the hypothesis that the energy from formate oxidation cannot in fact be utilised by the cells at all, or at least not for the purpose of protein synthesis.

The point you raise about the double adaptation is interesting. I cannot say definitely that the nitrate is not acting as a source of assimilable N, though I think it unlikely; if but

I did not go into this point in detail
I can think of further experiments better
planned to test your point than the ones I did
at that time. I could hardly accept the
difference of lactate activity in Table 7 of 50
to 17 as evidence of increased utilization enzyme
adaptation since it would be the other way
round with lactate. (see column 6)

You will, I think, be interested to hear
that quite recently, in the course of systematic
search for substances promoting lactate adaptation
in the presence of an essential N source (but
before the beginning of cell growth) I have
found that certain purines or pyrimidines have
a very pronounced stimulating effect. The
work is at present in its early stages & I
do not like to draw hasty conclusions, but
the relationship to nucleic acid nucleoprotein
synthesis is obvious. If this line proves
profitable I hope to bring it up at the
Copenhagen Congress in July.

Please let me know approximately
when you will be in this country & I
am much looking forward to meeting you.

Yours sincerely
M.R. Pollock

P.S. Is Sevag taken seriously in the U.S.? I have just been
reading the "adaptive enzyme" section of his review in the latest
"Advances in Enzymology". I think this article has stirred a number of
people here & I must confess I felt rather annoyed myself.
His objections, though superficially ingenious, do not bear scrutiny;
he is inconsistent & often illogical, he ignores work that
does not fit in with his own ideas, & ends with a perfectly
shattering misquotation from Marjorie Stephenson's paper on
the adaptation of formic hydrogenase. The way he dismisses
melibiose adaptation with a wave of his hand is quite
incredible & he does not even mention our work on lactate.
I would not bother about him myself, if his article did not appear
in an otherwise valued & respectable journal, if I should be interested to
know how he is held & what influence he has in the States. M.R.P.