

① undervalued? Surely not by his contemporaries. He was always cited with the highest esteem in all the circles I knew. But try Howard Schneiderman, now at Monsanto, who recruited him to Western Reserve.

[For Ass. NAs 1961... Many biographic notices...]

(Berman)

Perhaps he is not so well remembered now. But who is?

② You've made this point before. Did BE really think that diffusible "hormones" were not made by enzymes in 1947, that they were closer to primary gene products?

Re you (28). 1:1 is a slogan (a goddam!) not a theory. They never occurred up to how it could be falsified. Taken strictly it is of course now known to be false - but it was not very constructive to think so prior to the development of direct chemical approaches to protein biosynthesis.

from page 11: see Forsberg + Greenstein in Ann. Rev. Cell Biol. 5: 953- 1989 in a recent review of nuclear/mitochondria interplay.

re confirmation 1:2: exactly who held the view of "substrate ~~dependence~~" that you quote? Do you have a citation? I am not sure I understand the assertion, but my hindsight may be a blinder.

weleda as a plasmagone?

③ p. 13. which misidentifications?

p. 14 Case Inst.? I thought it was Western Res. (They've subsequently merged → Case-Western Reserve University)

NOTES TO BERMAN

p. 15 ⑦ Boris loved to quote my quip that "in the long run we have to study embryogenesis in embryos [not model systems]."

p. 19 ⑧ you might mention that this walk was the precursor of monoclonal antibody production — fusing lymphocytes with myeloma.

p. 21 ⑩ Won't you ever mention his marriage to Harriett Taylor? [I wonder how self-consciously he parents named her Harriett(t) — did they know of J. S. Mills' wife? {She tried hard to "de-mendelize" the pneumococcus transformation.}]. She was one of the most brilliant women I ever knew, and an ardent feminist; but in the end she also sought out an extremely [to her] paternal figure. So perhaps BE had much more impact on her than vice versa, perhaps to the point of putting her in umbra. It seemed to me she never fulfilled her earlier promise — but as you know she died tragically young.

p. pneumococcus

⇒ I guess she is mentioned in footnote (77). Her name, by the way, was Harriett.

~~p. 25~~ p. 26 "differentiation determinants" in prokaryotes. What system would he have used? Phase variation (in *Salmonella*) might have been a remote model — of a reversible "epigenotypic" change [I thought so] — but is quite remote. And I don't think he would have enjoyed the marker-intervenor formal genetics he would have needed to apply in those days.

All in all, I like it very much.

Missing, perhaps wisely, is a portrait of more personal events of character. I was on the verge of intimacy with him, especially when I was younger — but he still displayed a public character of very great self-confidence & a touch of arrogance & audacity. I was startled to read of his discouragement & in "Sapp" — correspondence with Sauerbom] about being outdone by Beadle. He would have needed all that drive to break through the rigidities of the French system —

John

P.S. Did BE really put much weight on morphology of mitochondria (p. 4 (12)?)? But he was just 24 then.

P.S. re nuclear changes in somatic cells  
see Galata — *PNAS* 86: 9233 12/15/89.  
That may not be the usual mechanism, outside of Ig,  
but I am sure many other examples will be found.

And (? outside of protozoa) we still haven't found much of a role for "cytoplasm".