

To: Richard Burian
Subject: My comments on his article on Boris Ephrussi XJ/BBGBDY
date: 2 April 1990

transcribed from handwritten
XJ/BBGBDY

4/2/1990

My comments

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...]

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 1942, that they were closer to primary gene products?

Re your (24). 1:1 is a slogan (a good one!) not a theory. They never owned 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.

Your page 11: see Forsburg and Guarente in Ann. Rev. Cell Biol. 5:153 - 1989 for a recent review of nucleus mitochondria interplay.

12: exactly who held the view of "substrate reformation" that you quote? Do you have a citation? I am not sure I understand the assertion, but my hindsight may be a blinder.

nucleoli as a plasmagene?

3. p.13 which misidentification?

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

p.15 51 Boris loved to requote my quip that "in the long run we have to study embryo genesis in embryos [not model systems]."

p.19 62 You might mention that this work was the precursor of monoclonal antibody production - fusing lymphocytes with myeloma.

p.21 70 Won't you even mention his marriage to Harriett Taylor? [I wonder how self consciously her parents named her Harriet(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. I guess she is mentioned in footnote 77. Her name, by the way, was Harriett.

p.26 determination!! in procaryotes. 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-intensive 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] and charm! I was startled to read of his discouragement [in "Sapp" - correspondence with Sonneborn] about being overtaken by Beadle. He would have needed all that drive to break through the rigidities of the French system -

Josh

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

P.S. re nucleic changes in somatic cells, see Yahota - 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".