

Avery d

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Dear Dr. Olby,

I was very pleased to see your letter "Avery in Retrospect" in the September 29th number of Nature. You have, of course, hit the mark very much more precisely and insightfully than Wyatt.

The last sentence of your letter was phrased very gently. Wyatt did not merely confine himself to explicit citations; it seemed to me that he went out of his way to misinterpret some of them and to ignore others.

Had I seen your letter sooner, I might have spared myself the effort of composing the enclosed which you may well see in print even before you receive this letter. However, it may be just as well that the occasion has impelled me to collect some volatile documentation about that time, and I think I will expand on it to put some more detail on the record as a contribution to the mythopoiesis that is rapidly enveloping that era. I realize that there are hazards connected with my personal involvement; on the other hand, I do see my own contribution to molecular biology as being sufficiently away from the mainstream of its actual development that I hope I can still retain some detachment. I wonder if you realize what a selfconscious decision was involved in the way I wrote up my Nobel lecture in 1958. It was a calculated effort to legitimize the purely chemical interpretation of heredity that the work of the previous decade had substantiated and which, of course, had advanced very much further than my own initial contribution to it.

That paper may inadvertently have included a demonstration of the way in which common knowledge does not get to be explicitly cited. Notice that a bibliography of 93 items seems to overlook an explicit reference to Avery, but... he is named without citation in the text.

I think you are quite right in reflecting the attitude that geneticists were less concerned about the chemical identity of the transforming principle than about its biological significance. After all, what could the geneticists say about a chemical controversy! That was a matter that had to be settled by the experts in that field, and I have to say that Mirsky's criticisms were more legitimate at the time than now appears in retrospect, the issue having been resolved.

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With regard to the biological interpretation it is not clear to me that "the 1944 interpretation was an advance in this position". What was sorely lacking was any demonstration that the pneumococcus transformation had some generality with respect to the genetic content of the bacterium, and this, of course, was confounded by the murkiness of the whole arena of bacterial heredity. It did take Hotchkiss' work with further markers and the crystallization of the overall context of bacterial genetics to clear this up.

I also resonated with your emphasis on Boivin and I think you may be interested in a few fragments of correspondence that I have been able to extract from my files. Tatum was in very close sympathy and correspondence with Boivin, probably from the time of publication of the *Experientia* article. People liked Boivin in 1947, and McCarthy in 1946 did not appear at the Cold Spring Harbor by magic or by self-invitation - they had to have been recognized as significant contributors by the organizing committee which was very much dominated by contemporary geneticists. Tatum and I were very much excited about the prospect of being able to conduct experiments on DNA-mediated transformation in an organism that we felt would be tractable in our hands in with our techniques like *E. coli*. We were very much disappointed in 1947 and 1948 to be unable to reproduce these experiments and according to my recollection Boivin did write to Tatum that he was no longer able to verify them either with the strains still in his hands. Boivin died shortly thereafter and there seemed little point in driving home the issue of non-verification. I remained acutely disappointed about the lack of this type of technique for some years thereafter, and would periodically make a spasmodic and unsuccessful effort to promote a transformation in *E. coli* or some similar organism. This was overtaken as you know by other developments; and in fact in 1959, when I moved to Stanford, I substantially dropped my work on the *E. coli* system altogether and have devoted most of the effort of our lab to the *Bacillus subtilis* DNA transforming system.

Wyatt's note does open up the issue of the resistance of an established discipline to innovation from other sources, but I think I am agreeing with you in the perception that he has done little service in clarifying what actually happened. I hope that others will read the original sources, for example Mirsky's critical discussions, rather than rely on Wyatt's attributions about them. I have no doubt whatever that Mirsky was very well aware of the work that Hotchkiss was doing relative to the purity of the DNA preparations. His characterization of that paper as being confined to such a critique seems to be contradicted by its very text. If

If you wish to amplify the context from which Boivin was able to derive his prescient views of the function of DNA, I think you should also look at Caspersson's writings of that general era.

I would be grateful to you for copies of your own writings in this general sphere and will promise to reciprocate.

Sincerely yours,

JOSHUA LEDERBERG