

Seymour Cohen

DEC 29 1986

PSB draft... Stanley etc

Dear Seymour

I have no comments now, except
thanks; if more come to mind I'll
write again.

* In my limited encounter with
Stanley, I never thought he had much
of an intuition in the biological or
genetic. Perhaps that's why he was
undoubtedly by the idea of "crystallin-
izing life."

In like vein, as he later admitted,
he was very critical of Avery and
doubtless played a critical role in
Avery not being recognized by the
Nobel Prize committee.

(*no C.A. Knight!)

P.S. I just went to my bookshelf to
look up WMS's Nobel lecture in
David Baltimore's Compendium on the
Prize in Mol. Biol. of course it's
not included.

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Seymour Cohen on W Stanley

these are now known to be derived from longer rods broken in the preparation of
the sample for microscopy. How homogeneous was a population of virus
particles? Lauffer, analyzing the spreading of a sedimenting boundary of
tomato bush stunt virus, concluded that the diameters of the particles could
deviate from the mean by no more than 1%. In an exuberant moment, Lauffer
referred to "living molecules."

Nevertheless, it is of considerable interest that neither group tested the
infectivity of viral RNA before 1956. Despite the availability of appropriate
viral RNA after 1936 and inactivating crystalline ribonuclease in 1940, despite
the demonstration of DNA as Pneumococcal transforming agent in 1944 and the
apparent infectivity of phage DNA, accepted by the community of phage workers
in 1952 after Hedley and Chase {what did the model have to do with it?}
in 1953 following the discovery of the Watson-Crick model, the thought that the
viral RNA might be the genetic element of this virus was not tested before
1956.

In 1940 E. Pfankuch et al. had studied X-ray induced mutations of the
virus and had attributed differences in the phosphorus contents of the parent
and mutant strains to irradiation-induced alterations in the nucleic acid part
of the virus. These data were not considered convincing in 1941 by C.A. Knight
and Stanley who had found differences in the amino acid compositions of various
strains. They had concluded that "the chemical differences between strains
probably lies not in the nucleic acid but rather in the protein part of the
virus molecule." They apparently did not consider the possibility that the
nucleic acid might determine the composition of the protein. Following this
line of thought, Miller and Stanley modified amino acid residues with a variety
of reagents but found that, although many groups could be modified without loss
of biological activity, the virus propagated was normal virus. At this point
in the work, early in the entrance of the United States into World War II, the

* see com note.

revised
Avery