

October 12, 1972

Memo of telephone conversation with Kim Atwood about the early 1940's.

He reports that the Avery story was very widely discussed, for example in Dobzhansky's course in 1942, and that there may have been one or two seminars before 1944 from people at the Rockefeller group. Sol thinks that Dawson may have given such seminar.

He is also confident that Francis Ryan was well aware of the work before the paper was published. There were even rumors prior to that publication, that the capsular polysaccharide had been disqualified presumably with the help of some of the enzymes that Avery had worked with.

Harriett Taylor was quite close to Bernie Davis in the period before 1944, and this could well have been another line of communication.

Kim thinks that Harriett had a rather low opinion of her own work on yeast, but does not now remember how she happened to get into it. He suggested looking at the thesis as it was deposited at Columbia. He promised to inquire about that at the department or with the university library.

He refers to Burnett as someone else who was highly prescient and little known -- he had run into Burnett's work at the library at NYU Medical School. I commented that I had had a somewhat similar experience quite early in the game, having been very much impressed by Burnett's review on bacteria phage.

He suggested also talking to Pittendrigh.

In general, Kim reinforces the concept of the very wide currency of knowledge of Avery's work and that how could one have expected a more rapid development than in fact happened. I reminded him that he and I were among the few people who had heard of genetics before going into molecular biology, and that many of our colleagues were in the opposite position.

continued memo (Kim Atwood - 10/12/72)

Kim also discussed some follow-up on Hirschhorn's work on the labelling of hemoglobin loci on the 10th chromosome. This experiment should not have worked but Kim has been able to corroborate it with more sensitive DNA labeled reagents. This suggests that there is some kind of amplification factor in the way in which Hirschhorn has been doing his experiments, and this, of course, could be extremely useful even if it was discovered by a blunder.

Memo from
JOSHUA LEDERBERG

To: Hammett - Severid

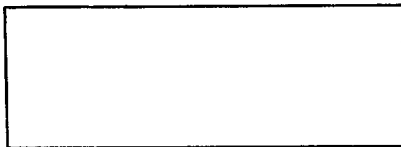
1941-1944. How.
discussed for its genetic significance

Before known from Avery's lab.
in DNA, already "known"

Only - in his course in 1942.

Seminar? Hitchcock

FSR knew before the paper was
published. Seminars at
Berkeley.



PROFESSOR JOSHUA LEDERBERG
Department of Genetics
School of Medicine
Stanford University
Stanford, California 94305

Memo from
JOSHUA LEDERBERG

To:

KCA.

Burnt
present and
little known

very paper.

Bernie / Harriet
1944.

transmission

Sol. Dawson survey?



PROFESSOR JOSHUA LEDERBERG
Department of Genetics
School of Medicine
Stanford University
Stanford, California 94305

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JOSHUA LEDERBERG

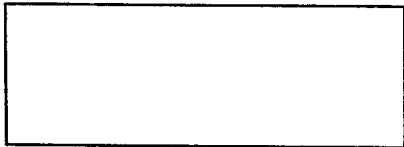
To:

low opinion of his own work.

general knowledge at
Columbia
1940's.

see if before.

Ravin
Pitt



PROFESSOR JOSHUA LEDERBERG
Department of Genetics
School of Medicine
Stanford University
Stanford, California 94305

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To:

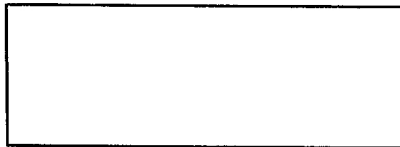
Hirschhorn

17b locus.

~~_____~~

10^5 out of range.

DNA



PROFESSOR JOSHUA LEDERBERG
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Stanford University
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