

Seattle T-12; October 27, 1990
(The Transition from Biochemical to Molecular Genetics, HSS mtgs)

On February 1, 1944.... SLIDE 1 Avery
2 Wyatt

Avery's own understanding

O.T. Avery letter to brother Roy

?cited in McCarty

Important new idea; foundation of modern molecular biology
and biotechnology

a) gene is DNA

b) use bacteria for new progress in genetics (prior v.v. e.g. 1943)

1) an exptl. assay of "DNA" function OR NUCLEO-PROTEIN

2) specific hypothesis (facit in "directed mutation") gene=DNA

3) use bacteria

How it was CON/RE CEIVED

conceived - see McCarty

received + built upon

telling for efficiency of scientific progress

pros/cons of participant commentary

SLIDES

Avery 1944

JL 1/20/1945

CSH 1947.. Harriet there. McCarty 1946

Muller 1947

JL 1951..

Hershey 1953 post 1952

SCI 1945-54...

letter to brother

bacterial genetics

CSH

Muller's imprimatur

lively discussion

W+C presented there

+OBI! frequent

SLIDES

McCarty 1985 complexity of issues

JL 1956

JL 1958 NP

NP's

Return to issues

1 DNA transformation system

pn. too hard

B. subtilis in late 50's and CaP in 60's

transfunction + plasmids--> full blown

now shotgun

2 gene=DNA

research and unreasonable doubts

don't judge by hindsight

when should controversy have been closed? when counterproductive?

a) diluted interest in chemistry of DNA. Hardly race --> 1953?

b) frustration to claimants; inequity of credit; ?try harder

c) waste a lot of effort in argument. If had gone on much longer.

hazards of premature closure...

stop looking for analysis

Need one "BELIEVE".

asif -- <<ignores proof

sources of resistance

Levine model 1931 -- a stepping stone!
Northrop & Summer 1936!
Stanley fiasco
EB Wilson volte fall

2 gene=DNA
popularize by plasticity of mechanism. W&C 53.
theoretical image compelling
By 1958 important to express my own conviction lingering vitalism

shortly thereafter, linkage groups sediment w/DNA; broken by shear
proof

Kornberg 1965
enzymatic replication of virus
Khorana - synthesis of a gene
CHECK

3 use bacteria for genetics
E coli recombination
Salmonella viral transduction
Monod & Jacob many more
biotechnology industry

3 FIRST practical application of DNA knowledge 1980.
YW Kan diagnosis 76?
exploded in that decade

broader consideration
what happened
what does it say about efficiency of science

was there resistance?
unreasonable?
external vs internal factors?

My own biases are tragic inevitability of stumbling blocks to
conception after prior stepping stones.
If only... step out of existing dogmatic framework, jump of the
giant's shoulder
many "postmature" discoveries internalized resistance.
use RKM corr. SLIDE

Need active engagement and focus of attention a prerequisite SLIDE
Inattention of geneticist to bacteria
Why not more remark on the nearly total neglect of Griffith 1928.
(I was only 3 years old at the time).
Even Avery follow up was biochemistry SLIDE
No genetics tale
Neglect far greater less than active debate
attention, ferment, dialectic
>>important than static uncritical belief
postscripts of participant observer

Nobel Prize for Avery (Mac McCarty)
Official history
W Stanley role. How he was burned.
Back to where to fix the system. Reduce disincentives.
encourage more critical theory
HoS indisposible for that

Future slides:
tetranucleotide theory and its discordants
WC - Nature 1953; pix them
St, Avery, McCarty today - oblit'd.
Avery letter to brother
Khorana give support

DNA sequencing

precursor refs (Sapp)

- extensive notes re Avery
- folder on Dubos... ?my review
- computing models of assimilation
- new info repl... transcr...

Schlenk - 1988

tetranucleotide as stepping stone

deoxyribose 1930? DNA as a 4x nucleotide; cf 1931 monograph

see Chargoff 1955

1945 mtg ashMCC - was Stanley there?

T/12 Seattle DNA history Oct 90. -- Avery reception
>>> inbox:721

From: "Richard M. Burian; Department Head"
Date: 06 Dec 89 11:37:11 EST (Wed)
Subject: History of Science Society Symposium

Memo to: Bernardino Fantini, Lily Kay, and Joshua
Lederberg.
From: Richard Burian
Re: History of Science Society Meeting, Oct. 25-
28, 1989
Date: December 6, 1989

I have been asked to submit a session abstract for our symposium session for the HSS meeting in Seattle. A draft of an abstract follows. I consider the formulation preliminary and will happily revise it to reflect a consensus about the best way to formulate the abstract to reflect the perspectives that each of us will bring to bear on the topic. I am told that the session is not likely to be scheduled for more than 2 1/2 hours. This being so (and I will of course correct the information if I have been misinformed, I propose that we restrict ourselves rigorously to one-half hour for our formal presentations so as to allow about 1/2 hour for discussion. I will ask whether there is any way to add one-half hour to the schedule, for I think that the result will be a far better session if we can be allowed the extra time.

The session organizers wish to publish the abstract with titles for the presentation. At this stage I recommend generic titles so as to allow us maximum freedom in developing our views (unless you are quite confident of what you wish to do in the session and can put a nice title on it.) The title I have listed for myself is VERY tentative; I may switch gears entirely. Incidentally, Bernardino, if this symposium goes well, is HPLS a suitable venue for publishing the papers? (I don't want to commit to anyone at this early date, for we should talk through which of the many options is best; the question is merely informational.)

I would like to request two immediate responses from the three of you: please suggest improvements in the abstract and please supply a title for your presentation WITHIN TWO WEEKS. The organizers of the meeting are trying to put the first version of the program to bed early enough to circulate it along with a call for works-in-progress papers. I will be out of town (in Boston for the Am. Soc. Zool.) Dec. 26-31. I hope very much to have sent off this info. before I leave. In the worst case I MUST send it off immediately on my return.

Thank you for your help. I look forward to a genuinely exciting session, with fruitful controversy on a topic that is of fairly deep importance to the understanding of both a large swath of recent history and of the current scene.

P.S. Lily Kay is involved in another session at this meeting. The organizers will ensure that there is no conflict; I cannot predict the precise date they will set for our session.

Symposium:
The Transition from Biochemical to Molecular Genetics

For the History of Science Society Meetings,
Seattle, WA, Oct. 25-28, 1990

Session Organizer: Richard M. Burian

The 1940s saw the flourishing of biochemical and physiological genetics. During the following ten or fifteen years, there was a transition to a new style of genetics, now known as molecular genetics. Yet there is immense disagreement about the character of this transition. To some, the labels convey no substantive difference: to practice molecular genetics is "to practice biochemistry without a license." To others, the interaction and unification of "structural" and "informational" approaches forged a new discipline, markedly different from traditional biochemistry and biochemical genetics. Yet other positions have been strongly defended. In this symposium we shall attempt to characterize the differences in question, the nature of the transition. We shall also cast some glances at correlations between changes in the style and content of scientific practice with institutional changes and with networks of scientific communication.

Chair: [Open]

Scheduled presentations:

Bernardino Fantini: " "

Joshua Lederberg: " "

Lily Kay: " "

Richard Burian: "Precursors of the Central Dogma: The Transition from Cellular Physiology and Cytochemistry to the Molecular Biology of the Genetic Material."

>>> inbox:748

From: "Richard M. Burian; Department Head" <RMBURIAN%VTVM2.BITNET@VTVM1.CC***T.EDU>
Date: 12 Dec 89 16:14:52 EST (Tue)
To: Joshua Lederberg
<JSL@caspl.rockefeller.edu>
Subject: Double check

Dic you receive my e-mail of about 6 Dec. re the Seattle meeting? If not, the dates are Oct. 25 - 29 and I will resend a tentative session description with an included request for a title of your (1/2 hr.) presentation. I have received your Morange paper. It reads well and opens up a lot of interesting leads. In short, I enjoyed it greatly. BEST!

Dick

P. S. If by some odd chance you are at the Am. Soc. Zool. meetings in Boston Dec. 26-30, let's get together.

>>> inbox:752

To: rmburian@vtvm2.bitnet (Richard M Burian)
Subject: Re: Oct 1989

I've just had a chance to review my calendar.

I probably can NOT leave NYC sooner than in time to arrive Seattle Fri eve. Oct. 26. And I'll have to leave by about 1 pm. Mon Oct. 29

If that will suit, I'd like to join in.

Perhaps I should talk about "How DNA was received, 1944 - 1950"

a) to dispel some Stentian myths about its being premature discovery
(and some analysis of what THAT means)

b) to analyze what were the claims, and how they were settled
b') was this a paradigm shift?

c) include the vicissitudes of DNA (basophilic chromatin) as the genetic material, e.g. in E B Wilson's mind.

I'll be writing a "Perspective" for Genetics on H J Muller's Pilgrim Trust lecture (delivered 1946, publ. PRS(B) 1948) at about the same time; so they'll fit very well. And I have the SCI for 1945-54 to document the contemporary citation record. Cf. also

203. Lederberg, J., 1972.

Letter to the Editor of Nature, in reply to H.V. Wyatt (Nature 1/14/72);
Nature 239:234, 9/22/72.

But I welcome your advice.

Do you have Lily Kay's email address?

Abstract: 2/12/90

"How DNA was Received, 1944-1953."

Joshua Lederberg

The Rockefeller University, NY 10021.

The publication by Avery, O. T., MacLeod, C. M. and McCarty, M. on February 1, 1944 "Studies on the chemical nature of the substance inducing transformation of pneumococcal types", in the Journal of Experimental Medicine is the unquestioned initiator of modern molecular biology. The substance was, of course, DNA -- an identification that was the culmination of a search that began shortly after Fred Griffith's first report on the pneumococcal transformation in 1928. Although only nine more years were to elapse before the description of DNA as a double helix, the interval has often been characterized as one of resistance and incomprehension on the part of the scientific community (Stent, Wyatt), with the implication that these reflect systemic flaws in the conduct of science.

Assisted both by personal recollection and the recently available Science Citation Index for 1945-54, I will review how this work was criticized, received and ultimately assimilated by the community in a time shorter than Avery himself needed to reach the conclusion that DNA was a (the?) hereditary substance. I will also discuss the expectation that revelations, however well vindicated in the long run, should be promptly believed, or believed in, and a contrast between lively engagement and criticism versus neglectful oblivion. Since the methodology, to some extent the very language, of DNA biochemistry was outside the experience of most geneticists, and few teachers were available even within biochemistry, the development of molecular genetics entailed the rearing of a new scientific generation with the competence to do the experiments and the interest to pursue them as an extension of the classical traditions in genetics.

In addition, the central claim, that "gene = DNA", was an extrapolation and generalization that in 1944 went far beyond a) what was known of

genes in bacteria generally, or of the control of the pneumococcal polysaccharide in particular, and b) formal chemical proof of the sufficiency of DNA, devoid of possible protein contamination. The 1953 double-helix model was not logically connected with either a) or b), but lent further theoretical plausibility to the notion that DNA could, in principle, have genetic functions.

How hard it was for D Luck to find DNA in the centriole. W Stanley fiasco.

_891212 Revue of Levene on DNA (? TIBS?) calc or found?
do sci on Levene -- new SCI on Avery; Kitcher/ 1953 & all that
See avery.list

_921226
Cited by Fruton. Cf Olby; T/46
Pirie, N.W. The Criteria of Purity Used in the Study of Large
Molecules of Biological Origin. BIOL REVS 15: 377-404, 1940.

_930620 Bring in story of neglect of Nobel Prize. Include
Wendell Stanley remarks. Any more in RAC on that! (Who's looked
at Stanley papers for that purpose?)

_930811 Bring in image of obliviousness of genetics to forthcoming revolution
?? First citations of Avery in Genetics. Anticipations by Dobzhansky, Wright
_930913 Cite John Moore