The Nobel Prize.

12/31/85

In that restless mood, I began to take more seriously some bids to move from Wisconsin. In 1955 Ted Puck and Roger Mitchell of the Colorado Medical Center in Denver tried to organize a genetics dept. or unit that might attract me a bit westward. A Rocky Mountain setting had some definite appeal; but Denver as a city and the academic limitations of the Medical Center (the University was at Boulder) did not quite override static factors. Helen Koprowski got wind of this; on his way to the directorship of the Wistar Institute, he asked me to join him; but I was not eager to move to Philadelphia which seemed to have all the limitations and few of the advantages of a metropolis like N.Y. or Chicago (both of which I had eschewed).

Nevertheless I greatly valued my friendship with Helen & Drew.

Implicit in these moves was the prospect of a shift in my own work to the genetics of somatic cells. Koprowski organized a conference of the NYAS on this theme and invited me to give the keynote: this was a manifesto arguing that somatic cells were now as promising for genetic analysis in 1955 than bacteria had been in 1945. The empirical advances by Puck and Eagle in cell culture and by Henry Harris on the induction of somatic cell fusion by Sendai virus were needed for the eventual realization of these speculations.

Hints of cell fusion as spontaneous accidents or in abnormal environments were eagerly adduced, as were expectations

DIR. WEBB INST.

see EZL notes.

sup 27 Feb

(continued) that chromosome segregation could be made to occur in somatic cells — if needbe with the help of chromosome breaking radiation {Earlier experiments with Edelweiss as precedents.}

At the N.Y. meeting I met George Klein and then and in later encounters {Words 64 — date} we had the mutual exhilaration of comparing notes. My own perspectives on the genetics of bacterial populations were a counterpoint to the research George + Eva Klein had done on the evolution of ascites tumor cells in the mouse.

At our point I learned that George was on the Nobel Committee of the Karolinska Institute. The Nobel Foundation gave some support to his laboratory research. By custom he had the responsibility of writing many nomination dossiers on the candidates proposed for the Prize in Physiology and Medicine. I chided him about whether the prizes had really done science a service, citing well-worn arguments about the arbitrariness of the awards, the public attention given to singular personalities, the potential displacement of the intrinsic values of research ... His main response was to ask what I thought any specific awards in the sciences had been in appropriately, and to adduce which one of them had really done less good than harm. My main rebuttal was that his point was probably better apt on his own work; his reply was that the award process actually enhanced his own chances

At no point did he give any hint that my own name was already in nomination, as doubtless it was. He must have had an amusing chuckle at the dilemma I might have to face some day in dealing with the award.

Skeptron 1957 – Melbourne – Innerby.

The International Microbiology Congress in 1958 was held in Stockholm; and I, obtaining a grant, entirely unconnected from the organizers to ensure my participation; for 1956 I had prepared the same request in Rome when I was unable to raise travel funds to enable Esther to come as well. This time Esther had a grant too. I would realize later that this was a fine feat – my presence in Stockholm (this was my first occasion) would allow other members of the Nobel committee same opportunity to meet and know me at first hand.

It was a marvelous visit, hospitality especially by the warmth of hospitality offered by Georges & Eva Klein, whom I here had the opportunity to meet. { { Besides the Congress & their labs at the Karolinska, and the fascinating museum we could learn, we were treated to many other sights. Visiting shops in Scientific conversations with Georges very prominent; Eva & Esther friendship; new friendships relationships. But still no publishing.

S'58
O 58
May 59.

That seemed also necessary. I would [illegible] Stanford] dated
spent [illegible] trying to explain to my colleagues why I was
"deserting" them.

↑ [illegible] Real meeting for more — with the [illegible] —

Our main preoccupation that fall was arranging our move from Wisconsin to Stanford. There were myriad details of course, mostly concerning the establishment of the new labs to minimize delays in the resumption of research: budgets; personnel recruitment; who would move with us (no more than one or two graduate students) equipment & supplies for the labs; not to mention our own meager possessions, books and records. We had the psychic work of separation, trying to do this in a way from our family of a dozen years at UW that would sustain the respect and affection built during our early years of marriage.

*Our schedule was to move at the end of January, a time
beginning of February
budget that seemed barely adequate

UW Starkey it was to be welcomed by a phone call (the early morning of October 30th) from a voice that claimed to be a reporter from a Stockholm newspaper. How did I feel about being a Nobel laureate. This bit out of the blue

project – get on paper what I already consent
fill in with expository material

– personal history
– inside of discovery
– photos, outtakes
– annotated programs

Compose chronologically

seemed most likely to be a mischievous joke or a false rumor. The latter would be even more calamitous: I would be congratulated by many friends, and then estranged by the unbearableness of owning up to it's having been a mistake. I could not face anyone's congratulations until the validity of the award was certified. After another couple of ~~hour~~ press queries, I thought it best to go, as I can immediately, and repeated to the head of a visiting research fellow to wait out the indeterminacy. In those days, the press in Stockholm made quite a game of pursuing the secret conclusions of the Nobel committee during an interval before these were ratified by the Karolinska faculty. The procedures have never been changed.

The next day, a more reliable telegram of notification arrived from the Nobel committee in Stockholm, and after that another requesting formal acceptance on my part.

In fact, I was in a dilemma - aggravated by the awkward ~~circumstances~~ timing of the award. The Nobel prize is as much gratification to the laureate's institutions as to himself. But I was in limbo: not yet at Stanford, but contracted to move. At a particular remote time, and when my frame of reference was about to change abruptly, I had to make a hasty decision, namely whether to accept the prize. Were my protestations to George sincere? → I thought they were. But there were many temptations — the dignified esteem the awards represented (then perhaps as much

later.

than now; the ~~guilds~~ Bureau for American science, for the special
(Internal)
^ for Jewry,
enjoyment of publicity that was the American dream, the vindication
of WIH and WSF, and a $20,000 award tax free was worth
almost two years' salary, and I had little expectation of more than
the most modest of material comfort. On the dark side:
while critique of a system is logically compatible with accommo-
dating to its benefits (most socialists accumulate private
riches), I could not gracefully criticize the prizes in
future. ~~I pondered~~ Were I to reject the prize, it would
attract far more notoriety than my tranquil acceptance. Scraps
of Saiva — in character — could hardly be more notorious.
Some would believe that to be my purpose; others would
simply see me as idiosyncratic or peculiar. Finally,
a rejection would be a slap at Tage Klein, and even more
a repudiation of my colleagues Brodie and Talmon who
surely entertained ~~the~~ thought other than the consummation
of expected reward. I would doubtless have reached the same
decision were I a totally free agent; but I resented being
trapped, of being caught in a system that allowed me no
alternative but to accept some notoriety, and so depriving
me sense of my individuality. I acutely feared that
my persona would be submerged under the epithet of
"Nobel prize winner", that this would interfere with the
authenticity of any new intrapsychic discoveries: in
some measure, that terrain of celebrity elegy are to
this day.

When was Eastland assumed?

ELT

at 33 I'd've laughed & said, many more years ahead.

I hoped for some consequences, that the award celebrity could give me some voice about the applications of scientific thinking to human affairs, and about contributing support for my research. But I had won my new niche at Stanford without the Nobel imprimatur; and I had no reason to think that could be authenticated to the public by that label.

Compared to my expectations, the instant notoriety (at least of the home town papers, i.e. Modesto + Palo Alto) stayed within dignified bounds — a newspaper strike that interrupted the daily N.Y. Times may have contributed to diminished visibility. Within my own discipline, the prize is hardly material: for every prize, we all know of a dozen equally worthy. It may have made me an attractive target for hypercritical peer review of some grant applications. I hoped it would not make me complacent, and even more that I could use any emerging public influence wisely. Surely it has opened doors that I ~~could~~ would not have — as chairing President Kennedy's transition committee on health policy, chaired by Wilbur Cohen, and access to the media of the press: the column I started writing for the Washington Post in 1966; very likely an augmented public interest in these memoirs. To the public, the Prize can be a certificate of scientific credibility; the multiplication of American prize winners since 1958, and the short idiosyncrasy of a handful of them, have rendered that certification less automatic as soon as the laureate's wide fame exceeds the bounds of his specialized expertise.

I stayed a week, learned a lot, but spoke to very few Scientists in Stockholm that centered on Alfred Nobel's birthday, December 10. ~~At this~~ All November I studied all I could find about Alfred Nobel & the prizes. I was particularly pleased to find a loophole in the statutes: a formal address was required but could be postponed up to six months. It was a relief not to have to devote November to that task.... so ~~but~~ I settled down before noon. I did prepare a brief ~~address~~ oration to discharge my sense of paradox about the prize and gamed how to express it at the award ceremony.

= 1907
= prior accounts of Nobel events. Esther's reactions
= our diary of week.
= reception in Italy

Mrs Hoover will was Attn.

In dividing the prize that year, half to Bodde + Tatum, half to myself, the Nobel committee favored the notion of allocation of credit for bacterial genetic recombination as between Tatum and myself. I was grateful for the insight that had given my award to things so delicately. The decision left no room for Stiefel others. Monod & Jacob were to receive due merit in ~~.~~ Many others probably never would; one of the untold costs is the inevitably quiet swallows of crushing among those so close to the lightning bolt.

Wolfman Jenkins Nomich

(U.J.) What comment could I make that would not sound pat and of Konigsberg, souvenir of Stockholm, Nobel events printed thanks for good wishes (by hundreds) OK helped

Save synopsis cases: S. Cohen M. McCarty

deepen the wound, or attract the incidents devotees more personally to myself.

Leaving Wisconsin — Luria — Stanley Cohen — Bernstein Blacks Chicago — Hot Spice Corn.
Backwash — Ehrman
Set up at Stanford ← the Great Experiment
Brandt home
design + build house.
temporary labs

The remaining obligation of the Nobel Award was the formal lecture at the Karolinska Inst. My voluntary postponement, until they gave more time for reflection + composition, this meant a more carefully prepared paper was reasonably expected.

What to talk about? The 1946 discovery was old hat. I was not yet historical enough to relish delving into its antecedents (as I have done in Chapter). To chronicle the consequents of 1946 would have been to parade the work of many others and heighten the ambiguity in allocation of credit. What statement was most needed? I felt that an affirmation of all of genetics as "molecular genetics" would be the most constructive. ~~It would be, however, self-serving~~ In any case, it would be pointless to recount the genetics of E. coli K-12 without restating that is

from first draft — not used — not my style — not my audience — his part in role — not known by — Ehrman

sought to be feature = ~~main~~ top

did not get [illegible] ~~about~~ [illegible]

the contemporary paradigm of DNA. That aim was after all my main motive in moving from to Stanford. Many "classical" biologists were still resentful about the primacy of DNA research as central to their discipline; Jacob was resented as Bernt and as well notorious as the main one still actively resisting it. I hoped my statement, self-serving only by my lodging a claim to be some part of the new tradition, would accelerate the canonical acceptance of Crick + Watson, and of Kornberg. (Whether accelerated I cannot say; their Nobel Prizes came in and respectively. Avery ... nun).

My talk was entitled: A view of genetics.

[What was it? Report].

Another trip to STO. Etc again.
... Italy again ...

Exobiology.

Ancient interest — H G Wells.
Lowell; Sunday supplement. / Mars occultation.. signals.
Cant Stern seminar / 1938. Orson Welles
Exobiology - Ojemann ...
ICBM's. mid 50's
[Sputnik / Calcutta, Haldane. prior writings? letter to Gov.] — cryogenic / computation / robot / oxygen - water - lab.
dictate.
don't not have more than one - four hours to "compute" with Kenton.
Western.

Mariner	need files best dictate outline. extract candidates.

1950 51 52 53 54 55 56 57 58 59 1960 1970

Med Centers Syst. (uu) (su)
Mental Retard. MH (Shorter...)
Sun cells.
Immunology.
exobiology.
Stars Centers
AI; diseases; graph theory, search
STARS.
WHO
Cardiology

Feasibleness of psycho-cardiology? Fewer things truncated.

penicillin
Env. mutagen.
"EVENTS" 51. NP months stopped.
Human Bio
Posts. Offs