The Nobel Prize: 12/31/85 14

DIR. WEBB INST.

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 friction. Hilary 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 irritations 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 Hilary & Irene.

see EZL notes.

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 no less promising for genetic analyses in 1955 than bacteria had been in 1945. The empirical advances by Puck and Eagle on cell culture and by Henry Harris on the induction of somatic cell fusion by myxoviruses were needed for the exptl. realization of these speculations.

see p 27 file

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

[illegible]) that chromosome segregation could be made to occur in somatic cells — if need be with the help of chromosome breaking radiation. {earlier experiment with colchicine as precursor.}

At the N.Y. meeting I met George Klein and then and in later encounters {Woods Hole ... date} we had the mutual exhibition 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 one point I learned that George served on the Nobel Committee of the Karolinska Institute. The Nobel Foundation gave some support for his laboratory research. In return he had the responsibility of reviewing many ~~proposed~~ nomination dossiers on the candidates proposed for the Prize 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 internal values of research ... His main response was to ask whether I thought any specific awards in the sciences had been inappropriate, and to ask which one of them had really done less good than harm. My main rebuttal was that his time was probably better spent on his own work; his reply was that the review process actually enhanced his own education

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 inner chuckle at the dilemma I might have to face some day in dealing with the award.

Skipour 1957 – Melbourne – Immunology.

The International Microbiology Congress in 1958 was held in Stockholm; and I received a particularly warm invitation from the organizers to ensure my participation; In 1956 I had foregone the same congress in Rome where 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 final heat – my presence in Stockholm (this was my first occasion) would allow other members of the Nobel committee some opportunity to meet and hear me at first hand.

It was a marvellous visit, heightened especially by the warmth of hospitality offered by George + Eva Klein, whom I here had the opportunity to meet. { }. Besides the Congress + their labs at the Karolinska, and the fascinating music we could learn, we were treated to every Alan sight Skansen Viking ship ... Scientific conversations with George were prominent; Eva + Esther joined in; near fraternal relationships. But still no inkling.

S'58
O 58
May 59.
(387)

That summer also was eventful. I would have dinner
[illegible] 7 PM [illegible] Stanford } dates
spent { martyrs } explaining to my colleagues why I was "deserting" them.

{ Real motive to move — intellectual climate —

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, from our friends of a dozen years at UW, trying to do this in a way that would sustain the respect and affection ~~[illegible]~~ built during our early years of married life.

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

How startling it was to be walsmed 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 bolt out of the blue

project — get on paper what only I can write
fill in with expository material
" " " more technical

— personal history
— inside of discovery
— phelps. outlook
— connected perspectives

Compose chronologically
reasons by things

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 awkwardness of ominous eyes to its having been a mistake. I could not face anyone's congratulations ~~not~~ until the reality of the award was certified. After another couple of ~~new~~ press queries, I thought it best to go incommunicado, and repaired to the home of a visiting research fellow to wait out the uncertainty. In those days, the press in Stockholm made quite a game of prying out the secret conclusions of the Nobel committee during an interval before these were ratified by the Karolinska faculty. The procedures have now 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 institution as to himself. But I was in limbo: not yet at Stanford, but contracted to move.

later.

At a particular harried 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 shade more

than now; the ~~pride~~ kudos for American science, (literature) for Jewry, for the social engine of mobility that was the American dream, the vindication of NIH and NSF; and a $20000 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 accommodating to its benefits (most socialists accumulate private income), I could not gracefully criticize the prizes in future. ~~So I pondered~~ Were I to reject the prize, it would attract far more notoriety than my tranquil acceptance. {example of Sartre – 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 George Klein, and even more a repudiation of my colleagues Bradle and Tatum who surely entertained no 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 alternatives but to accept some notoriety, and so dispiriting me some of my individuality. I correctly feared that my persona would be submerged under the epithet of "Nobel prize winner", that this would interfere with the authenticity of any new interpersonal relationships: in some measure, that barrier of celebrity clogs me to this day.

when was Pasternak announced?

ELT

at 33 to be labelled a sage; many more years ahead.

I hoped for some compensations, that the same celebrity could give me some voice about the application of scientific thinking to human affairs, and about nothing exciting for my research. But I had won my new niche at Stanford without the Nobel imprimatur; and I had no cause that would be authenticated to the public by that label.

Compared to my expectations, the resident notoriety (outside of the home town papers in 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 guess it would not make me complacent, and even more that I could use any emerging public influence wisely. Surely it has opened some doors that I welcome — including as President Kennedy's transition committee on health policy, chaired by Wilbur Cohen, and access to the medium of the press: the columns I started writing for the Washington Post in 1966; very likely an augmented public interest is these memoirs. To the public, the Prize can be a certificate of scientific credibility; the multiplication of American prize winners since 1958, and the dubious idiosyncrasy of a handful of them, have rendered that certification less automatic as soon as the laureate's voice transcends the bounds of his specialized expertise.

Trapped or not, harried or not, I expected to enjoy the celebration in Stockholm that centered on Alfred Nobel's birthday, December 10. ~~It is the~~ 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 ~~[illegible]~~ little time before more. I did prepare a brief ~~address~~ peroration to discharge my sense of paradox about the prize and gained have to express it at the award ceremony.

= prior accounts of Nobel work. Esther's reactions
= own diary of week.
= recuperate in Italy

In dividing the prize that year, half to Beadle + Tatum, half to myself, the Nobel committee finessed the problem of allocation of credit for bacterial genetic recombination as between Tatum and myself. I was grateful for the insight that had joined my award to theirs so delicately. The divisions left no room for still 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 rankling away those so close to the lightning bolt.

(U.Z.) What concurrent could I make that would not

used postcard of Koncerthuset, souvenir of Stockholm, Nobel with printed thanks for good wishes (by hundreds) OK helped

Wallman Pandee Nossik

Jos Hoover old was AHIT.

12/31/85 Bonaire → Boston. also a 707.)

Save igneous cases: S. Cohen M. Nictarty

deepen the wound, or attract the invidious version more personally to myself.

Learning Wisconsin ... Study Sutton ...
cornstorm blocks Chicago. Hot Exec Comm.
Backwash ... Khorana

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 in a detail either left to their own hands and heighten the ambiguity in allocation of credit. What statement was most needed? I felt that ~~a statement~~ affirmation of all of genetics as "molecular genetics" would be the most constructive. ~~It would be less self-serving~~ In any case, it would be pointless to recount the genetics of Ecoli K-12 without casting that in

Ryan still alive. Mann might I did not sign the apply to authenticity his functional role. Did not know how somewhat eulogy

sought to be future = omitted

did not get opportunity [illegible] [illegible] [illegible] [illegible]
[illegible]

the contemporary paradigms of DNA. That aim was after all my mens motive in the movie to Stanford. Many "classical" biologists geneticists were still resumed about the primary of DNA research as central to their discipline; figures as renowned as Bennet and as re-notorious as Commoner were still actively resisting it. I hoped my statement, self-seeing only by my lodging a claim to be some part of the new traditions, would accelerate the canonical acceptance of Erich + Watson, and of Kornberg. (Whether accelerated I cannot say; their Nobel Prizes came in and respectively. & Avery ... mon).

My talk was entitled: A view of genetics.

[What was in it? Reprint]. exob.

Another trip to STO. G+E again.
... Italy again ...

Exobiology.

Ancient interest — HG Wells.
Lowell, Sunday supplement; Mars occultation.. signals.
Curt Stern seminar — 1938. Orson Welles

Exobiology - Oparin ...

ICBM's. mid 50's

[Sputnik — prior writeup?
Calcutta, Haldane. letter to Cor.]

urgent / computation c mod biol
opply - visit. lab.
dictate.
did not have more than num. facilities to "compute" with Kenby.

Westex.

Marion | mid files but dictate outline.
extract conclusion dates.

1950 — 55 56 57 58 59 1960 — 1970

Mid Genetics Dept.
Mental Retard; MH
Sm cells.
Immunology.
exobiology.
SIMB Cetus
AI; dendral; graph theory, SUMEX
SAMS.
WHO
Cardiology

uw — su — (Shorter...)

Timelines of pyrite corrhop? Few things truncated.

penicillin
Env. mutagen.
"EVENTS" 57 — NP — moved to stanford.
Human Bio
POSTS. [illegible]