

1969

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intended as preface to
a book of my
newspaper columns
(never published)

FRAGMENTS FROM A SCIENTIFIC AUTOBIOGRAPHY

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[initials]

The 1950's were a decade of philosophical upheaval in biology. I can illustrate this with some personal history, part of which is already represented elsewhere in this collection of writings.

My twenty-first birthday in 1946 fell in the midst of the first conclusive experiments that Edward L. Tatum and I were doing on genetic interchange between bacterial cells.

By fall, 1947, I had earned a Ph.D. at Yale and taken a position at the University of Wisconsin. This afforded a better chance to continue my research in cellular genetics than my previous plans of returning to Columbia Medical School to complete my M.D.

The next ten years were a time of extension and consolidation. My principal collaborators were Norton L. Zinder (now Professor at Rockefeller University), Melvin L. Morse (now at Colorado Medical School), Esther Zimmer Lederberg (to whom I was married between 1946 and 1967), and Luigi L. Cavalli, of Pavia University, with whom I cooperated by a combination of mail correspondence and brief research visits. We were pursuing an idea that seemed revolutionary when we started, but is now commonplace: that the fundamental ideas of genetics that had been developed earlier with studies on higher plants and animals were applicable to bacteria and viruses. This helped to unify our scientific understanding of these medically crucial forms of life. Perhaps, even more important, it helped to open a new arena of experimentation for the application of the most powerful techniques of biochemistry.

TT: ZAS, Jerry
L...
R... [unclear]

For several years this work was received with mild skepticism. For some reason, many fellow scientists were far readier to invoke totally novel processes for the exchange of genetic information than the possibility that bacteria might undergo processes of sexual fusion analogous to those of higher forms. Much of this debate proved to be meaningless when the biochemical basis of genetic transfer was more fully understood. Bacterial cells do indeed undergo a form of cellular mating which enables the genetic information from one cell to pass into another without being exposed to the external environment. However, the brilliant experiments of Francois Jacob and Elie Wollman at the Pasteur Institute in Paris showed that this process was not the simple cellular coalescence that I had assumed, but the progressive transfer of a long linear molecule of DNA across the fusion bridge between the two cells. For many years I remained so sensitized to defending the obvious implications of my simpler experiments on genetic recombination that I was unduly obstinate in accepting the full implications of the very beautiful experiments published by the group in Paris. I was also misled by some obscure details that are still only dimly understood, but which do not stand as important obstacles to the Jacob-Wollman theory of genetic transfer. Meanwhile, some of the more fanciful ideas that had confused the issue from other directions were also dispelled and ~~since 1956 there has been little~~ since 1956 there has been little disagreement about the mechanism of fertilization in these bacteria.

This satisfying conclusion was only possible in the light of very striking advances in our knowledge of the biochemistry of DNA: particularly the studies on its enzymatic synthesis by Dr. Arthur Kornberg, and on its molecular structure by Francis Crick and James D. Watson.

In 1958 I shared the Nobel Prize for Physiology and Medicine with Ed Tatum and G. B. Beadle.

My first intimation of this life-distorting event was a phone call that reached me at the laboratory on a Sunday morning, October 26, 1958. The New York correspondent of a Stockholm newspaper had an "absolutely reliable" tip and wanted

to know my "innermost reaction".

Most of these were none of his business, but I took some minutes to dispose of one - namely, that it was a hoax. It then took a few days to be sure that the reporter was not simply factually misinformed.

Meanwhile, the rumor had unfortunately hit the press wires, and I did indeed have a deep inner reaction, namely, annoyance and embarrassment. It seemed to me most probable that the rumor was false and there would then be a painful period of adjustment in the aftermath of misplaced congratulations about it. So I went into hiding for a few days until the facts had straightened themselves out. Eventually a more authoritative-looking story appeared in the newspapers, followed by a telegram signed by the Nobel Committee.

So it was true. In many ways this was even more embarrassing than a false rumor might have been. It was a very awkward time. During that year, I had learned that Arthur Kornberg was moving ^{from Washington University in St. Louis} to Stanford to establish a Department of Biochemistry at the School of Medicine being newly built on the Palo Alto campus. I had been delighted when he suggested that I consider moving there as well, and when I investigated this further, it was too exciting a prospect to turn down. I had been at Wisconsin for eleven years, ^{still} in my first job after completing my graduate education, and was beginning to get a bit restless. The main source of this restlessness was my perception that we would have to use much more sophisticated biochemical tools to make further advances in cellular genetics. While Wisconsin was famous for its pioneering work in biochemistry, and I greatly enjoyed the company of many estimable colleagues in that field, none of them was specifically interested in the biochemistry of DNA, of protein synthesis, or of gene action, ^{the} which fields would be the most relevant to my own interests. By the summer of 1958 I had then firmly accepted an invitation to move to Stanford, but had decided to wait until February 1959 to make the actual move. Consequently

the news of the Nobel award came just while I was preparing to move my laboratory, my home, and all the rest, from Wisconsin to Stanford.

It is never easy to make a graceful departure from an academic position, and especially one where I had so many personal friends and where I had had such generous nurturing during the early part of my academic career. The intrusion of the award made that situation very much worse, especially as Wisconsin had never captured a Nobel bird in its aviary.

Ambiv. → Pr. 2c.

There was also the question of whether to accept the award. The Nobel awards had always seemed to me to distort one of the most important features of the scientific enterprise - that every advance is based on the cooperative effort and criticism of a very large number of people whose part in crucial discoveries would be impossible to allocate fairly. Furthermore, I could easily point to dozens of scientific advances of equal or greater significance, whether judged by their humanistic value or their intellectual elegance. I had already shared these ideas on several occasions with many of my friends, including one of my colleagues who was serving on the Nobel Committee. He must have been laughing up his sleeve, knowing, as I did not, that I was on the active list of candidates, but his only remark was that Nobel's will was deeply embedded in Swedish law as well as world tradition, and there was no practical way to alter the system.

Gen. Klinc Tiselius

Pr. x Nobel '65? St Tiselius speech quoted from letter - it was from Josh. → Nobel Symposiums

This still left me in November 1958 with the dilemma whether to accept the award, but a number of considerations really did leave very little choice.

I could take only pride in sharing the plaudits with G.W. Beadle and Ed Tatum. My having worked with Tatum would attract more unwarranted attention to personalities than to principle if I did not accept. To question the propriety of the awards on the grounds of their incongruence with the reality of the scientific enterprise would merely create more notoriety. Finally, a rejection would offend many men, the selection committees for the sciences, whose past efforts at an impossible task of choice were more creditable than one could hope for. These reservations were rather rabbinical and a quiet acceptance was the simple answer.

Pr. prag- within limits of human ability - Pr. prejudx. Fns of Prize for public

My decision to accept was perhaps vindicated by the repudiation of another award that year. Boris Pasternak, author of "Dr. Zivago", was compelled by political fiat, not his individual conscience, to turn his back on Stockholm. His chair was conspicuously vacant at the ceremonies, with no little discomfiture to the Russian physicists (Tamm, Cerenkov, Frank), who, in distinction to their literary compatriot, were permitted to take part. The toast to the laureates was given by Professor M. E. Rudberg of the Swedish Academy of Sciences: "Knowledge should build for men, for mankind, for humanity. Such is Nobel's message. Scientists are needed for this--and poets, and men of good heart. To genius we look for leadership, albeit humbly: with a prayer that genius will be spared--yes, even spared the fate of Galileo."

The allusion to Galileo was the only formal comment on Pasternak's absence. (The climate of freedom of speech in the United States in 1968 makes it awkward to pass judgment too smugly on the suppression of dissent elsewhere.)

My formal response to the toast had, of course, been written out beforehand. However, it expresses a similar mood, that the only possible justification for the Prize was the homage it paid to new knowledge as a transcendent value in human affairs.

REPLY TO PERORATION BY M.E. RUDBERG:

Le premier à répondre à ce discours fut M. LEDERBERG qui s'exprima en ces termes:

Your Majesties, Your Royal Highnesses, Your Excellencies, Ladies and Gentlemen.

Pride is humbled as humility is exalted in the dignity and splendor of this occasion.

Who would deny his pride in the appreciation of his fellows and to join therein with GEORGE W. BEADLE and EDWARD L. TATUM whose exploration and teaching have inspired a generation of discovery. Here pride must merge with humility in the same contemplation of the webs of interdependence of each investigator in the global community of scientific research, of each elusive fact in the continuum of human knowledge.

But formal éclat and public attention are so unaccustomed a distinction that a scholar may ask by what lasting motive he is elevated from the simpler satisfactions of academic life. We must concede that some aspects of Nobel's dedication have been deflected by the force of history. His contrition for chemical inhumanity is shaded in the gloom of cosmic insecurity. Many sciences, and genetics in particular, have germinated and flowered only since his time. The growing complexity of science and the reticulation of its advances must make the task of singular choice ever more difficult and arbitrary.

But if Nobel's honors are celebrated in Stockholm his passion is enacted in Oslo. His zeal for peace and international understanding is further expressed in his testamentary "wish that in awarding the prizes no consideration whatever shall be given to the nationality of the candidates". The merit the prizes have won is in the faith and courage of this trust.

The illumination of human aspirations in intellect and in charity which transcend nationality is then the enduring warrant of Nobel's legacy. Our presence honors his hopes for the fraternity of mankind.

My greatest pride and humblest gratitude is to join in this dedication.

* the Peace Prize

There remained the question of the scientific lecture expected of each laureate. I ^{had} rummaged through the library, and every other source of information I could find, about the Nobel festival and its traditions. Some anecdotal papers by Philip Hench were especially useful. But I also found the statutes of the Nobel awards, and discovered that these gave me a reprieve. Although the formal celebrations were held on the anniversary of Nobel's death, on December 10, the formal lecture did not have to be presented for another six months.

It was then possible to participate at some level of consciousness in the pomp and celebration, to rest a few days with the Cavallis at their villa near Rapallo, and to return to the Wisconsin winter in time, somehow, to pack, leave, and re-establish at Palo Alto.

Our departure from Madison was hilarious and poignant. Our friends and well-wishers managed to get us to the local airport, only to discover that Chicago was closed to air traffic by a snow storm that was rapidly heading north. If we had had to go by snowshoe, we nevertheless had to depart! Fortunately, there was a plane bound for Minneapolis, and my wife and I did eventually reach San Francisco in short, feeder-line hops.

During the next few months while setting up a new laboratory, I had time to think about the content of the formal lecture. Traditionally, this was devoted to a retrospective review of the author's own work, enough time having passed to assess its place in the ongoing stream of scientific progress. But I found it difficult, eventually impossible, to pursue this approach. My own contributions had long since been assimilated into the customary practice of experimental work -- even high school students were repeating the key experiments in some syllabi. I was more inclined to ponder upon my errors in judgment during the past ten years, mainly ^{reflecting} an unconstructive skepticism that I had sustained ^{then} about what ^{then} seemed to me over-simplified chemical interpretations of fundamental biological questions. I can well recall the nit-picking in which I indulged at any number of scientific meetings on such questions as the purity of DNA used in experiments on genetic transfer, or the rigor of the evidence for the "one gene-one enzyme" theory that had been promulgated by Beadle and Tatum, and on which such a magnificent superstructure has come to be built.

These criticisms might have had a certain validity at the time they were presented. The new chemical interpretation of heredity was formulated before it could be bolstered by irrefutable evidence and there were many misleading side-tracks on questions like the way enzyme-substrates and antigens elicited specific macromolecules from cells (note my article at page). However, by 1958 no informed scientist could deny the validity of "molecular biology", and I now felt impelled to look ahead, to throw my energy behind this revolutionarily simple outlook on the nature of life.

Part of the impetus for this was the continued lack of appreciation by many biologists of the older schools of the extent of this revolution of knowledge-- that it was no longer tenable to sustain even a trace of vitalistic mystique in biological thought.

When I gave my Nobel lecture in Stockholm on May 29, 1959, it was then dedicated to a prospectus, to a forward look, about the implications of the biochemistry of DNA for biology. This was quite personal, "A View of Genetics". However, my own research was already too remote to play a central part in that discussion. This was a severe but justifiable judgment: at age 33, I was characterizing the main style of my own investigative work as having become nearly obsolete and deserving to be superseded by more powerful techniques.

The world scientific community and the Nobel committee have, however, shared these premises, as signified, for example, in the awards to workers on nucleic acid biochemistry like Kornberg (1959), Ochoa (1959), and Watson, Crick, and Wilkins (1962).

Since that time my own scientific efforts have made the fullest use I knew how to contrive of the new biochemical methodology. This field is, however, now populated by any number of the most skillful experimentalists with whom it would be preposterous^e to compete on their own terms. There has, however, been very little innovation in the process of innovative thinking, and in recent years I have devoted an increasing part of my time and thought to the possibility of augmenting human creativity by (1) seeking more formal representations and languages for expressing complex ideas, and (2) exploiting the information-processing power of electronic computers to support such functions.

There is still a long way to go!

C. J. Davisson, in accepting his award in 1937, remarked that a Nobel laureate is transformed "overnight from an exceedingly private citizen to something in the nature of a semi-public institution". This hazard is hardly noticeable in professional life among one's peers, except possibly that a

laureate may be a more tempting target for criticism when he makes a fool of himself. The Nobel club has, however, achieved a distinctive notoriety, if not prestige, which is sustained mainly by the public press. It can be rather irritating to have one's individuality submerged under the inevitable attribution of the Prize at any introduction or reference in the press. And the club is constantly called upon to give its collective support to public issues that may have only the remotest connection with the scientific prestige of its members. I hope that thoughtful readers will not pass judgment ^{too harshly} on the idiosyncracies of public behavior of many Nobel laureates if they first give some thought to the artificial pressures that have been focussed on them.

Above all, I hope it may be more generally realized that the label "Nobel laureate" attaches to a very wide variety of human beings, perhaps the most individualistic of any group, with a wide variety of tastes, styles of life and motive, and creative talent.

The years just following the Nobel award ^{were} a time of great personal upheaval, culminating, among other matters, in the eventual breakup of my first marriage. During this time I learned problems of human communication their due weight. Up to this point I had regarded scientific work as the most creative avenue of personal insight and one that was happily beyond the foibles of every day human experience. Indeed its objectivity might be a useful ideal for frail mortals to apply in that experience. This was not inconsistent with the determination to bring my own experience and skills as a scientist to bear on broader human problems but this was out of a rather general sense of compassion and social responsibility.

These motives found outlets like President-elect Kennedy's task force on health and his later Panel on Mental Retardation. These were fields in which I had no special expertise; nevertheless, my work in them could profit from my own vantage point as an experimental biologist.

In November 1962 I attended a unique conference, sponsored by the CIBA Foundation in London, on "The Biological Future of Man". I expected to hear a grand debate between Peter Medawar and H. J. Muller about the utility of eugenic improvement of man. However that debate might already be obsolete if it overlooked the new findings of chemical genetics and their prospects for a much deeper understanding and management of genetic material. As it happens Muller was too ill to attend and I took a rather more active part in the discussion, elaborating on some detailed ways in which molecular biology might come to human use or abuse, than I would have predicted. I was surprised at the conservatism of many of my colleagues in perceiving the long range impact of these ideas, their unwillingness to see how rapidly we would reach substantial changes in the technology of biological alteration. My discussions there with Medawar and with Crick were nevertheless very

provocative for my own thoughtful consideration of these new directions.

That meeting had come at a time when I was particularly depressed, but was the occasion of an uplifting insight about the human character of science. The conflict between the rational and affective ideals had been a troublesome one since I was a teenager, wondering what rationale I could have for wanting to dedicate my energy to rational thinking. That question remains unanswered, But I have discovered that research is grounded far more deeply in human, social activity and by a more rigorous argument than I had previously understood.

Scientific advance is, by definition, a penetration from the frontier of existing knowledge. But that frontier bounds the insights available to the whole human species, not those of any single individual. The fundamental requirement for objective novelty of content (contra style) sets scientific research apart from many other forms of creative personal expression.

Of much more practical consequence, the CIBA conference demonstrated the gap between scientific and political foresight about technological change. We scientists might argue about timing, ten years versus a hundred, but we knew change was coming fast. The information and education of the public and its political representatives was plainly far behind the needs for wise decision. I happen not to believe that scientific training confers any magical wisdom about human affairs and would be as loath to relegate the management of a nation to its scientists ^{any more than} as to any other restricted group. Nevertheless the new era of biological science would necessarily pose many new opportunities and challenges, the facts of which simply had to be more widely understood.

For some time my personal response to this challenge was to pay more

attention to my responsibilities as a teacher in general education at Stanford, to put together a few writings, and to start on the path of seeking some order and satisfaction in my personal affairs.

In January 1960, returning from a meeting on space research in Nice, via London, I found that my seat mate was Nigel Calder, whom I had already met very briefly as founder and editor of the English science news magazine "The New Scientist". The plane ride from Nice to London was a good occasion for us to discuss the problem of public information about science, for which I felt his magazine was doing a unique service. In October, 1964, having exchanged a few casual thoughts in the meantime, he wrote what was to me a rather novel proposal, that I become a regular essayist for his magazine. This was particularly startling since my previous experience at popular writing had not been a very fruitful one, at least by ^{my editors'} the standards, ~~of the editors I had tried to deal with.~~ I had to say "no" to Calder's invitation but it did set me to thinking about the gap in communication between scientist and citizen and about the most appropriate format in which it would be possible for a scientist like myself to add a new kind of commentary about scientific advance. In other words, what kind of proposal would be so attractive that I would not refuse it and then why not take the initiative myself!

After some thought I concluded that a regular, short column in a newspaper of wide, literate circulation could be the most effective channel that could be devised, at least for my own contribution to that gap.

During the next 18 months I gradually put together some material for a prospectus for such a column and a few sample pieces. Fortunately my friend and associate, Professor David Hamburg of Stanford's Psychiatry Dept. thought well of the project and also knew some of the people involved in the

management of the Washington Post and helped to convey my material to them. In the course of time and with the particular interest of Mr. Howard Simons, who had just been elevated to Associate Managing Editor from having been a well known science writer himself, the proposal for a weekly column was tentatively accepted and I have been enjoying this function ever since.

Q The articles that have appeared in the Washington Post form the major part of the present book. They are reprinted here in a form which restores some sentences and paragraphs that have had to be deleted for the column to fit into its allotted space, but which also takes advantage of some of the editorial criticism suggested by the Post's staff. At the present time the articles are appearing regularly in a small number of American and foreign newspapers by special arrangement with the Post, independent of its regular syndication. *Q* I am particularly grateful to Mr. Simons and his associates on the newspaper for their patience and interest in dealing with the most naive neophyte in the newspaper game ever allowed within range of their offices.