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PRESIDENT

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Dr. John A. Fuerst
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Dear Dr. Fuerst:

I was very pleased to see you address the history of "reductionism in molecular biology" in Social Studies of Science. Max Delbruck's idiosyncratic views about "complementarity" were always quite puzzling to me. They would be even more so if I did not see occasional manifestations of a similar strain of thought amongst other physicists -- including, for example, my predecessor Dr. Frederick Seitz (quotation enclosed). I have received similar communications from Eugene Wigner.

The physicists were of course very deeply shaken by indeterminacy 50 years ago; biology during the 50s and 60s did, I suppose, have to leave some room for the potential inadequacy of physical and organic chemistry as a sufficient level of explanation.

The only point that I might question about your account -- ...... a tender one -- is how little such deprecation of biochemistry prevented them from welcoming the biochemists like Seymour Cohen...

Cohen might speak rather more eloquently about the frustrations that he and other chemists encountered in their efforts to interest Delbruck in their line of approach. Luria, and especially Hershey, were of course far more facile with, and receptive to, molecular biochemical techniques. To that extent it is probably something of an over-simplification to talk about A "phage group". Especially after 1952 there was substantial dissidence in the experimental approaches actually used by the different investigators.

I was also interested in your attributions to Jacques Loeb at The Rockefeller Institute as a mainspring of mechanistic thinking, at a time when this was more an article of faith than concrete accom-

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plishment in biological investigation. Whether Avery needed particular support in this respect is an interesting question; and I wish our archives could say more about Loeb's influences on Avery's thought.

In his History of The Rockefeller Institute, Corner reminds us that its faculty also embraced Alexis Carrel! (See p.31 enclosed)

I learned something about "reductionism" as an operating program in my conversations with Yehuda Elkana some years ago. An avowed reductionist in principle, for many years, I despaired that we would be able to penetrate the actual complexity of living systems at a molecular level within my own lifetime. I was excited and inspired by Arthur Kornberg's courage in his determination to see how far pure enzymology could go in penetrating to the very core problem of genetics: the molecular mechanisms of DNA replication! Until then, my working strategies in experimental investigation might not have been readily distinguishable from those of an avowed "anti-reductionist"! Confidence in what is pragmatically achieveable, at a given stage in the development of a science, should probably be given as much prominence in the analysis of intellectual influence as the eschatological principles.

I believe that my own convictions on these matters were not far different from the main stream of physiologically oriented biologists from the mid-40s on. It was for that very reason that I put so much emphasis on the achievements on the structure of DNA, rather than my own investigations, in my Nobel Lecture "A View of Genetics" given in 1959. I thought the time had arrived to put a closure to any pessimistic restraint about the potential scope of physicochemical investigation. You will see other manifestations of that pragmatism in a few other writings that I also enclose.

To recapitulate, I would say that more than most historians you have understood the complexity of thinking of what went on within the "phage group"; but even so, that story remains to be properly told from the perspective of some of the "outsiders" like Seymour Cohen. If you look carefully at Al Hershey's comments -- and he is always careful to be polite -- you will see still further evidence of that complexity. The platform of our perspectives may well also account for the controversy between Gunther Stent and myself as to just how far and how well Avery's findings in 1944 were understood by his contemporaries. It is all the more remarkable (as I learned just lately) that Roy Avery promptly discussed the famous letter he had received from his brother, with Max Delbruck when they were both

on the Vanderbilt campus: and that this was received with considerable and sympathetic interest on Max Delbruck's part. My remarks about the complexity of thought within the "group" might very well apply to the not always consistent strains of thinking that we all entertain as individuals as well.

Please treat these remarks as a private correspondence for the time being. In due course I will have constructed them more carefully for the record.

Yours sincerely,

Joshua Lederberg

12-28-68

Joshua Lederberg

Man Can Be Called 'Machine' —But a Most Complex One

DOES MODERN science dehumanize man?

It is easy to find deeply ambivalent attitudes about science among intellectuals (even including some scientists), in Congress, among alienated youths and among bewildered citizens. We live in a scientific age whose glories and terrors are both credited to science. At this level, we can hardly deny that our ever-growing scientific mastery over the forces of nature imposes an almost unbearable responsibility on political authority and on a democratic electorate to learn about, think about, plan for and use these forces for real human benefit.

In this climate, many people have become highly sensitized to more ethereal questions that are raised by the scientific study of man. One such question is the doctrine of mechanism. Dr. D. E. Wooldridge, a well-known physicist and systems engineer and a successful industrialist—formerly president of TRW (Thompson-Ramo-Wooldridge) Inc.—has written several excellent syntheses of present day thought in biology. His latest work, "Mechanical Man —the Physical Basis of Intelligent Life," concludes "that a single body of natural laws operating on a single set of material particles completely accounts for the origin and properties of living organisms. Accordingly, man is essentially no more than a complex machine."

A FEW ECCENTRICS aside, the whole community of contemporary science shares the view that the same laws of nature apply to nonliving and living matter alike. All of us who investigate the chemistry and physics of living organisms pursue our work as if organisms were complex machines, and we find man to exhibit no tissues or functions that would except him from this way of analyzing human nature.

Nevertheless, we are or should be careful to state just what we mean before we assert that "man is a machine," and much more so before using the phrase "merely a machine." The statement that man is "a mere machine," or a mere anything, is a needless irritant to precise communication between scientists and laymen. (We might better proclaim that "man is merely the most complex product of organic evolution on earth, the only organism whose intelligence has evolved to the point that his culture far transcends his biological endowment.")

The "mere machine" phrase is usually a retort to the claim that there are mysteries of human nature that are, in principle, beyond the reach of scientific investigation. Scientists would do better to save their breath quarreling about what they can analyze in principle; in their own work, they are mercilessly pragmatic about confining their conclusions to what they can examine in practice.

THERE ARE, in fact, theoretical limits to scientific analysis that may justify men in repudiating Dr. Wooldridge's assertion that "the concept of the machine-like nature of man is incompatible with a long-cherished belief in human uniqueness." There is nothing "mere" about a machine as complex as a man; the word "machine" is just a manner of speaking about the scientist's faith in a universe ordered by natural law. That faith was expressed most eloquently by the French philosopher the Marquis de Laplace, who averred that, given complete knowledge of the universe at one instant, the scientist could in principle compute all of its future states in infinite detail.

In practice, we must now remind ourselves, the scientist and his computers are machines that occupy space and consume energy. Dr. Rolf Landauer of IBM has pointed out that the process of calculation itself soon reaches fundamental limits. If the whole visible universe were one gigantic computer, made of components at the theoretical lower limit of size and energy consumption, it would still be insufficient for some problems that are soluble "in principle."

Far short of the complexity represented by a human being, some mere machines called computers nevertheless have already reached the point where their actual behavior is predictable only to a rough approximation, and we must be careful to program internal checks to detect when these highly individualized robots deviate from their intended instructions.

("Mere machines" embrace organic indeterminacy)