Prematurity and Uniqueness in Scientific Discovery

A molecular geneticist reflects on two general historical questions: (1) What does it mean to say a discovery is "ahead of its time"? (2) Are scientific creations any less unique than artistic creations?

by Gunther S. Stent

The fantastically rapid progress of molecular genetics in the past 25 years now obliges merely middle-aged participants in its early development to look back on their early work from a depth of historical perspective that for scientific specialties flowering in earlier times came only after all the witnesses of the first blossoming were long dead. It is as if the late-18th-century colleagues of Joseph Priestley and Antoine Lavoisier had still been active in chemical research and teaching in the 1930's, after atomic structure and the nature of the chemical bond had been revealed. This somewhat depressing personal vantage provides a singular opportunity to assay the evolution of a scientific field. In reflecting on the history of molecular genetics from the viewpoint of my own experience I have found that two of its most famous incidents—Oswald Avery's identification of DNA as the active principle in bacterial transformation and hence as genetic material, and James Watson and Francis Crick's discovery of the DNA double helix—illuminate two general problems of cultural history. The case of Avery throws light on the question of whether it is meaningful or merely tautologous to say that a discovery is "ahead of its time," or premature. And the case of Watson and Crick can be used, and in fact has been used, to discuss the question of whether there is anything unique in a scientific discovery, in view of the likelihood that if Dr. *A* had not discovered Fact *X* today, Dr. *B* would have discovered it tomorrow.

Five years ago I published a brief retrospective essay on molecular genetics, with particular emphasis on its origins. In that historical account I mentioned neither Avery's name nor DNA-mediated bacterial transformation. My essay elicited a letter to the editor by a microbiologist, who complained: "It is a sad and surprising omission that... Stent makes no mention of the definitive proof of DNA as the basic hereditary substance by O. T. Avery, C. M. MacLeod and Maclyn McCarty. The growth of [molecular genetics] rests upon this experimental proof.... I am old enough to remember the excitement and enthusiasm induced by the publication of the paper by Avery, MacLeod and McCarty. Avery, an effective bacteriologist, was a quiet, self-effacing, non-disputatious gentleman. These characteristics of personality should not [cause] the general scientific public... to let his name go unrecognized."

I was taken aback by this letter and replied that I should indeed have mentioned Avery's 1944 proof that DNA is the hereditary substance. I went on to say, however, that in my opinion it is not true that the growth of molecular genetics rests on Avery's proof. For many years that proof actually had little impact on geneticists. The reason for the delay was not that Avery's work was unknown to or mistrusted by geneticists but that it was "premature."

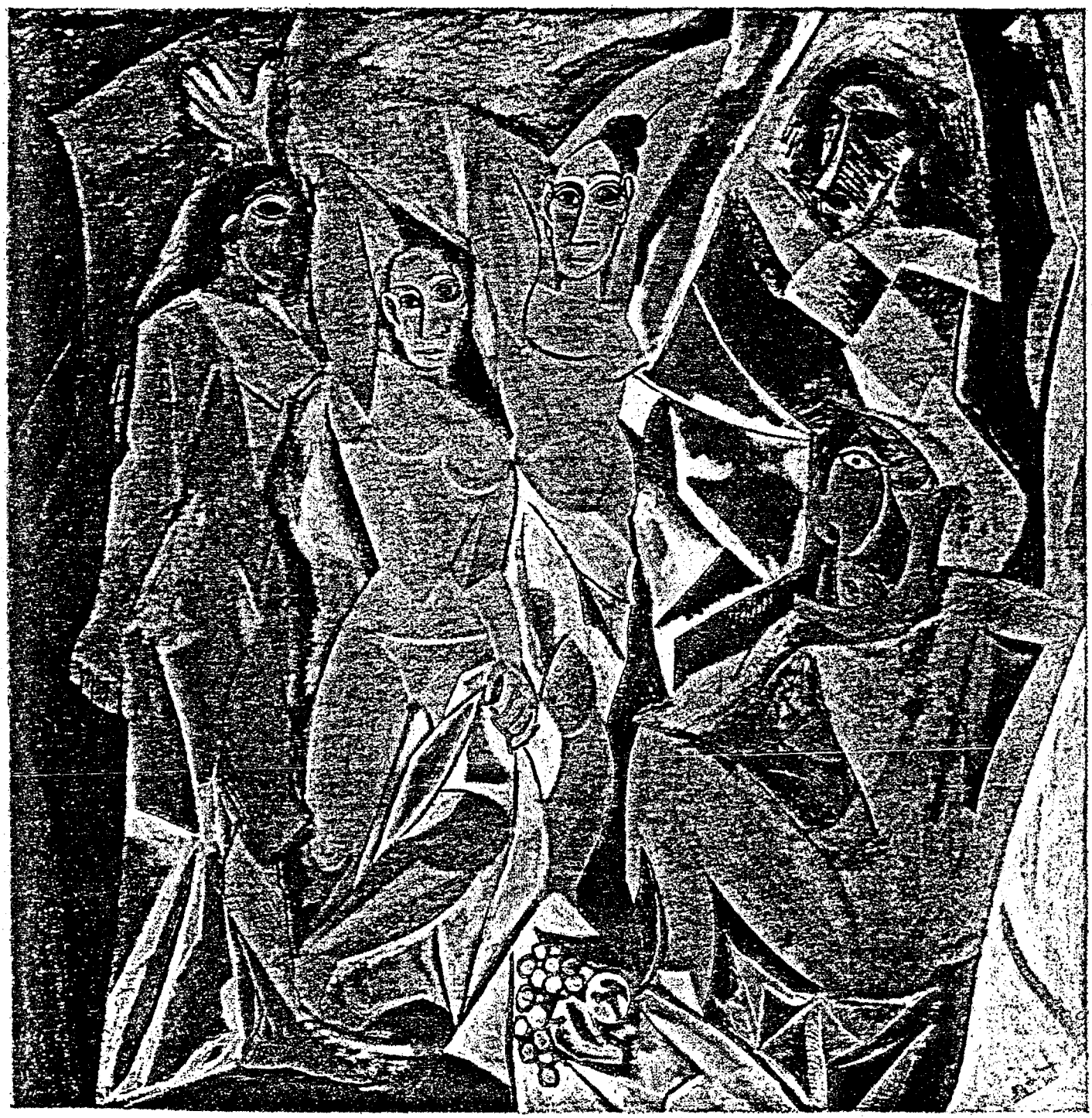
My *prima facie* reason for saying Avery's discovery was premature is that it was not appreciated in its day. By lack of appreciation I do not mean that Avery's discovery went unnoticed, or even that it was not considered important. What I do mean is that geneticists did not seem to be able to do much with it or build on it. That is, in its day Avery's discovery had virtually no effect on the general discourse of genetics.

This statement can be readily supported by an examination of the scientific literature. For example, a convincing demonstration of the lack of appreciation of Avery's discovery is provided by the 1950 golden jubilee of genetics symposium "Genetics in the 20th Century." In the proceedings of that symposium some of the most eminent geneticists published essays that surveyed the progress of the first 50 years of genetics and assessed its status at that time. Only one of the 26 essayists saw fit to make more than a passing reference to Avery's discovery, then six years old. He was a colleague of Avery's at the Rockefeller Institute, and he expressed some doubt that the active transforming principle was really pure DNA. The then leading philosopher of the gene, H. J. Muller of Indiana University, contributed an essay on the nature of the gene that mentions neither Avery nor DNA.

So why was Avery's discovery not appreciated in its day? Because it was "premature." But is this really an explanation or is it merely an empty tautology? In other words, is there a way of providing a criterion of the prematurity of a discovery other than its failure to make an impact? Yes, there is such a criterion: A discovery is premature if its implications cannot be connected by a series of simple logical steps to canonical, or generally accepted, knowledge.

Why could Avery's discovery not be connected with canonical knowledge? Ever since DNA had been discovered in the cell nucleus by Friedrich Miescher in 1869 it had been suspected of exerting some function in chromosomal processes. This suspicion became stronger

in the 1920's, when it was found that DNA is a major component of the chromosomes. The enormous molecular nature of DNA, however, made it well-nigh inconceivable that DNA could be the carrier of hereditary information. First, until well into the 1930's DNA was generally thought to be merely a tetranucleotide composed of one unit each of adenylic, guanylic, thymidylic and cytidylic acids. Second, even when it was finally realized by the early 1940's that the molecular weight of DNA is actually much higher than the tetranucleotide hypothesis required, it was still widely believed the tetranucleotide was the basic repeating unit of the large DNA polymer in which the four units mentioned recur in regular sequence. DNA was therefore viewed as a uniform macromolecule that, like other monotonous polymers such as starch or cellulose, is always the same, no matter what its biological source. The ubiquitous presence of DNA in the chromosomes was therefore generally explained in purely physiological or structural terms. It was usually to the chromosomal protein that the informational role of the genes had been assigned, since the great differences in the specificity of structure that exist between various proteins in the same or-



PICASSO'S "LES DESMOISELLES D'AVIGNON," painted in Paris in 1907, is often cited by art historians as the first major Cubist painting and a milestone in the development of modern art. It is reproduced here as an archetype of the proposition that works of artistic creation are unique (in the sense that if Picasso had not existed, it would never have been painted), whereas works of scientific creation are inevitable (in the sense that if Dr. *A* had not discovered Fact *X* today, Dr. *B* would discover it tomorrow). The validity of the proposition is disputed by the author. The painting is in the collection of the Museum of Modern Art in New York.

ganism, or between similar proteins in different organisms, had been appreciated since the beginning of the century. The conceptual difficulty of assigning the genetic role to DNA had not escaped Avery. In the conclusion of his paper he stated: "If the results of the present study of the transforming principle are confirmed, then nucleic acids must be regarded as possessing biological specificity the chemical basis of which is as yet undetermined."

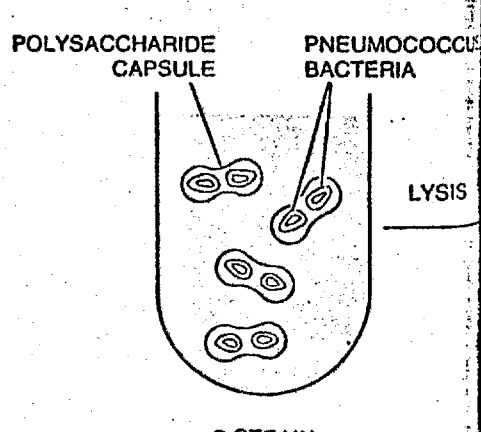
By 1950, however, the tetranucleotide hypothesis had been overthrown, thanks largely to the work of Erwin Chargaff of the Columbia University College of Physicians and Surgeons. He showed that, contrary to the demands of that hypothesis, the four nucleotides are not necessarily present in DNA in equal proportions. He found, furthermore, that the exact nucleotide composition of DNA differs according to its biological source, suggesting that DNA might not be a monotonous polymer after all. And so when two years later, in 1952, Alfred Hershey and Martha Chase of the Carnegie Institution's laboratory in Cold Spring Harbor, N.Y., showed that on infection of the host bacterium by a bacterial virus at least 80 percent of the viral DNA enters the cell and at least 80 percent of the viral protein remains outside, it was possible to connect their conclusion that DNA is the genetic material with canonical knowledge. Avery's "as yet undetermined chemical basis of the biological specificity of nucleic acids" could now be seen as the precise sequence of the four nucleotides along the polynucleotide chain. The general impact of the Hershey-Chase experiment was immediate and dramatic. DNA was suddenly in and protein was out, as far as thinking about the nature of the gene was concerned. Within a few months there arose the first speculations about the genetic code, and Watson and Crick were inspired to set out to discover the structure of DNA.

Of course, Avery's discovery is only one of many premature discoveries in the history of science. I have presented it here for consideration mainly because of my own failure to appreciate it when I joined Max Delbrück's bacterial virus group at the California Institute of Technology in 1948. Since then I have often wondered what my later career would have been like if I had only been astute enough to appreciate Avery's discovery and infer from it four years before Hershey and Chase that DNA must also be the genetic material of the experimental organism.

probably the most famous case of prematurity in the history of biology is associated with the name of Gregor Mendel, whose discovery of the gene in 1865 had to wait 35 years before it was "rediscovered" at the turn of the century. Mendel's discovery made no immediate impact, it can be argued, because the concept of discrete hereditary units could not be connected with canonical knowledge of anatomy and physiology in the middle of the 19th century. Furthermore, the statistical methodology by means of which Mendel interpreted the results of his pea-breeding experiments was entirely foreign to the way of thinking of contemporary biologists. By the end of the 19th century, however, chromosomes and the chromosome-dividing processes of mitosis and meiosis had been discovered and Mendel's results could now be accounted for in terms of structures visible in the microscope. Moreover, by then the application of statistics to biology had become commonplace. Nonetheless, in some respects Avery's discovery is a more dramatic example of prematurity than Mendel's. Whereas Mendel's discovery seems hardly to have been mentioned by anyone until its rediscovery, Avery's discovery was widely discussed and yet it could not be appreciated for eight years.

Cases of delayed appreciation of a discovery exist also in the physical sciences. One example (as well as an explanation of its circumstances in terms of the concept to which I refer here as prematurity) has been provided by Michael Polanyi on the basis of his own experience. In the years 1914–1916 Polanyi published a theory of the adsorption of gases on solids which assumed that the force attracting a gas molecule to a solid surface depends only on the position of the molecule, and not on the presence of other molecules, in the force field. In spite of the fact that Polanyi was able to provide strong experimental evidence in favor of his theory, it was generally rejected. Not only was the theory rejected, it was also considered so ridiculous by the leading authorities of the time that Polanyi believes continued defense of his theory would have ended his professional career if he had not managed to publish work on more palatable ideas. The reason for the general rejection of Polanyi's adsorption theory was that at the very time he put it forward the role of electrical forces in the architecture of matter had just been discovered. Hence there [illegible] tion of gases must also involve an electrical [illegible] ction between the gas molecules and the solid surface. That point of view, however, was irreconcilable with Polanyi's basic assumption of the mutual independence of individual gas molecules in the adsorption process. It was only in the 1930's, after a new theory of cohesive molecular forces based on quantum-mechanical resonance rather than on electrostatic attraction had been developed, that it became conceivable gas molecules could behave in the way Polanyi's experiments indicated they were actually behaving. Meanwhile Polanyi's theory had been consigned so authoritatively to the ashcan of crackpot ideas that it was rediscovered only in the 1950's.

Still, can the notion of prematurity be said to be a useful historical concept? First of all, is prematurity the only possible explanation for the lack of contemporary appreciation of a discovery? Evidently not. For example, my microbiologist critic suggested that it was the "quiet, self-effacing, non-disputatious" personality of Avery that was the cause of the failure of his contribution to be recognized. Furthermore, in an essay on the history of DNA research Chargaff supports the idea that personal modesty and aversion to self-advertisement account for the lack of contemporary scientific appreciation. He attributes the 75-year lag between Miescher's discovery of DNA and the general appreciation of its importance to Miescher's being "one of the quiet in the land," who lived when "the giant publicity machines, which today accompany even the smallest move on the chess-board of nature with enormous fanfares, were not yet in place." Indeed, the 35-year hiatus



EXPERIMENT OF 1944 with which Oswald Avery correctly identified the chemical nature of the genetic material is regarded by the author as a classic example of a premature discovery. The virulent normal, or S-type, pneumococcus, a bacteri-

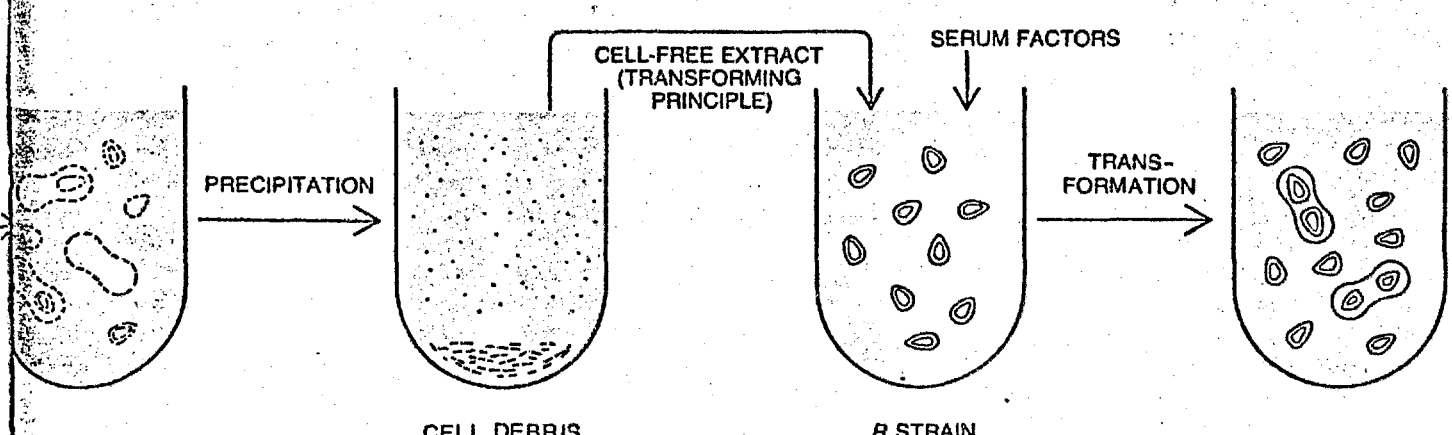
in the appreciation of Mendel's discovery is often attributed to Mendel's having been a modest monk living in an out-of-the-way Moravian monastery. Hence the notion of prematurity provides an alternative to the invocation—in my opinion an inappropriate one for the instances mentioned here—of the lack of publicity as an explanation for delayed appreciation.

More important, does the prematurity concept pertain only to retrospective judgments made with the wisdom of hindsight? No, I think it can be used also to judge the present. Some recent discoveries are still premature at this very time. One example of here-and-now prematurity is the alleged finding that experiential information received by an animal can be stored in nucleic acids or other macromolecules.

Some 10 years ago there began to appear reports by experimental psychologists purporting to have shown that the engram, or memory trace, of a task learned by a trained animal can be transferred to a naïve animal by injecting or feeding the recipient with an extract made from the tissues of the donor. At that time the central lesson of molecular genetics—that nucleic acids and proteins are informational macromolecules—had just gained wide currency, and the facile equation of nervous information with genetic information soon led to the proposal that macromolecules—DNA, RNA or protein—store memory. As it happens, the experiments on which the macromolecular theory of memory is based have been difficult to repeat, and the results claimed for them may indeed not be true at all. It is nonetheless significant that few neurophysiologists have even bothered to check these experiments, even though it is common knowledge that the possibility of chemical memory transfer would constitute a fact of capital importance. The lack of interest of neurophysiologists in the macromolecular theory of memory can be accounted for by recognizing that the theory, whether true or false, is clearly premature. There is no chain of reasonable inferences by means of which our present, albeit highly imperfect, view of the functional organization of the brain can be reconciled with the possibility of its acquiring, storing and retrieving nervous information by encoding such information in molecules of nucleic acid or protein. Accordingly for the community of neurophysiologists there is no point in devoting time to checking on experiments whose results, even if they were true as alleged, could not be connected with canonical knowledge.

The concept of here-and-now prematurity can be applied also to the troublesome subject of ESP, or extrasensory perception. In the summer of 1948 I happened to hear a heated argument at Cold Spring Harbor between two future mandarins of molecular biology, Salvador Luria of Indiana University and R. E. Roberts of the Carnegie Institution's laboratory in Washington. Roberts was then interested in ESP, and he felt it had not been given fair consideration by the scientific community. As I recall, he thought that one might be able to set up experiments with molecular beams that could provide more definitive data on the possibility of mind-induced departures from random distributions than J. B. Rhine's then much discussed card-guessing procedures. Luria declared that not only was he not interested in Roberts' proposed experiments but in his opinion it was unworthy of anyone claiming to be a scientist even to discuss such rubbish. How could an intelligent fellow such as Roberts entertain the possibility of phenomena totally irreconcilable with the most elementary physical laws? Moreover, a phenomenon that is manifest only to specially endowed subjects, as claimed by "parapsychologists" to be the case for ESP, is outside the proper realm of science, which must deal with phenomena accessible to every observer. Roberts replied that far from him being unscientific, it was Luria whose bigoted attitude toward the unknown was unworthy of a true scientist. The fact that not everyone has ESP only means that it is an elusive phenomenon, similar to musical genius. And just because a phenomenon cannot be reconciled with what we now know, we need not shut our eyes to it. On the contrary, it is the duty of the scientist to try to devise experiments designed to probe its truth or falsity.

It seemed to me then that both Luria and Roberts were right, and in the intervening years I often thought about this puzzling disagreement, unable to resolve it in my own mind. Finally six years ago I read a review of a book on ESP by my Berkeley colleague C. West Churchman, and I began to see my way toward a resolution. Churchman stated that there are three different possible scientific approaches to ESP. The first of these is that the truth or falsity of ESP, like the truth or falsity of the existence of God or of the immortality of the soul, is totally independent of either the methods or the findings of empirical science. Thus the problem of ESP is de-



um that causes pneumonia in mammals, is enclosed in a smooth (hence *S*) polysaccharide capsule that protects the bacterium from the ordinary defense mechanisms of the infected animal. The avirulent mutant, or *R*-type (*R* for rough), strain has lost the genetic capacity to form this protective capsule and hence is comparatively harmless. When [illegible] *S* donor bacteria was added to mutant *R* recipient bacteria, some of the mutants were found to regain the genetic capacity to form the capsule and thus were transformed back into the normal, virulent *S* type. Avery purified the transforming principle and succeeded in showing that it is DNA. The significance of Avery's dis- [illegible] until 1952.

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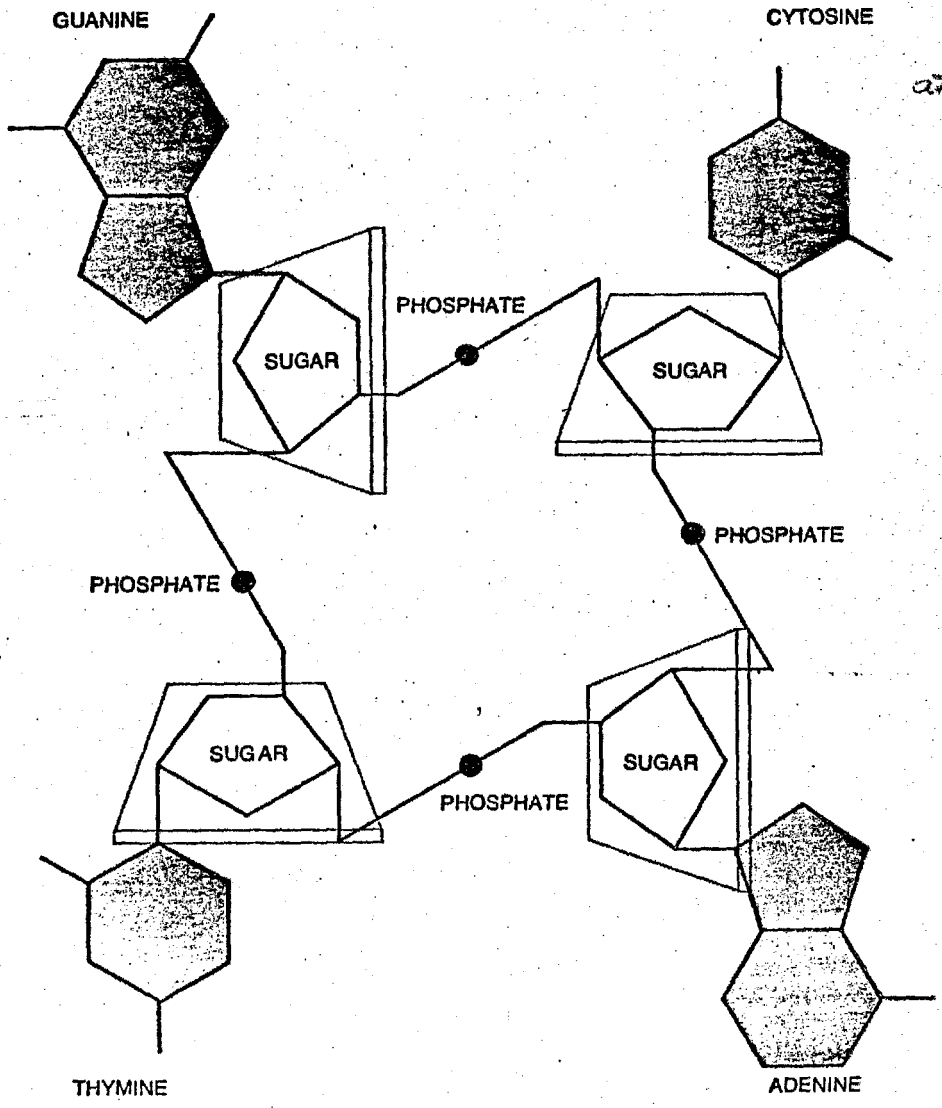
was more or less Luria's position.

Churchman's second approach is to reformulate the ESP phenomenon in terms of currently acceptable scientific notions, such as unconscious perception or conscious fraud. Hence, rather than defining ESP out of existence, it is trivialized. The second approach probably would have been acceptable to Luria too, but not to Roberts.

The third approach is to take the proposition of ESP literally and to attempt to examine in all seriousness the evidence for its validity. That was more or less Roberts' position. As Churchman points out, however, this approach is not likely to lead to satisfactory results. Parapsychologists can maintain with some justice that the existence of ESP has already been proved to the hilt, since no other set of hypotheses in psychology has received the degree of critical scrutiny that has been given to ESP experiments. Moreover, many other phenomena have been accepted on much less statistical evidence than what is offered for ESP. The reason Churchman advances for the futility of a strictly evidential approach to ESP is that in the absence of a hypothesis of how ESP could work it is not possible to decide whether any set of relevant observations can be accounted for only by ESP to the exclusion of alternative explanations.

After reading Churchman's review I realized that Roberts would have been ill-advised to proceed with his ESP ex-



OLD VIEW of the chemical structure of DNA, widely held until well into the 1930's, saw the molecule as being merely a tetranucleotide composed of one unit each of adenylic, guanylic, thymidylic and cytidylic acids. This hypothesis demanded that the molecular weight of DNA be little more than 1,000 and that the four nucleotide bases (adenine, guanine, thymine and cytosine) occur in exactly equal proportions. Even when it was finally realized in the 1940's that the molecular weight of DNA is much higher (in the millions or billions), it was still widely believed that the tetranucleotide was the basic repeating unit of the large DNA polymer. The mistaken belief in a uniform macromolecular structure proved to be an obstacle to the eventual acceptance of the idea that DNA is the genetic material.

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Is the lack of appreciation of prem
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his theory half a century after first ac
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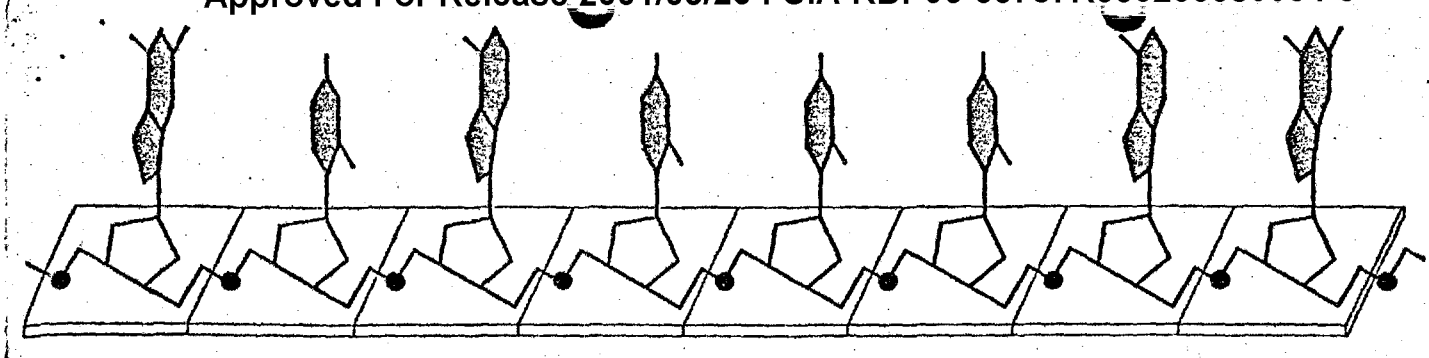
that any evidence which contradict
this view is invalid must prevail. Such
evidence has to be disregarded, even i
it cannot be accounted for, in the hope
that it will eventually turn out to b
false or irrelevant."

That is a view of the operation of sci
ence rather different from the one com
monly held, under which acceptance o
authority is seen as something to b
avoided at all costs. The good scientist
is seen as an unprejudiced man with an
open mind who is ready to embrace any
new idea supported by the facts. The
history of science shows, however, that
its practitioners do not appear to act
according to that popular view.

Five years ago Chargaff wrote one of the many reviews of *The Double Helix*, Watson's autobiographical account of his and Crick's discovery of the structure of DNA. In his review Chargaff observes that scientific autobiography is "a most awkward literary genre." Most such works, he says, "give the impression of having been written for the remainder tables of bookstores, reaching them almost before they are published." The reasons for this, according to Chargaff, are not far to seek: scientists "lead monotonous and uneventful lives and... besides often do not know how to write." Moreover, "there may also be profounder reasons for the general triteness of scientific autobiographies. *Timon of Athens* could not have been written, 'Les Desmoiselles d'Avignon' not have



PRESENT VIEW of the chemical structure of DNA sees the molecule as a long chain in which the four nucleotide bases can be arranged in any arbitrary order. Although the proportion of adenine is always equal to that of thymine and the proportion of guanine is always equal to that of cytosine, the ratio of adenine-thymine to guanine-cytosine can vary over a large range, depending on the biological source of the DNA. With the elaboration of this single-strand structure it became possible to envision that genetic information is encoded in the DNA molecule as a specific sequence of the four nucleotide bases (*see illustration on next page*).

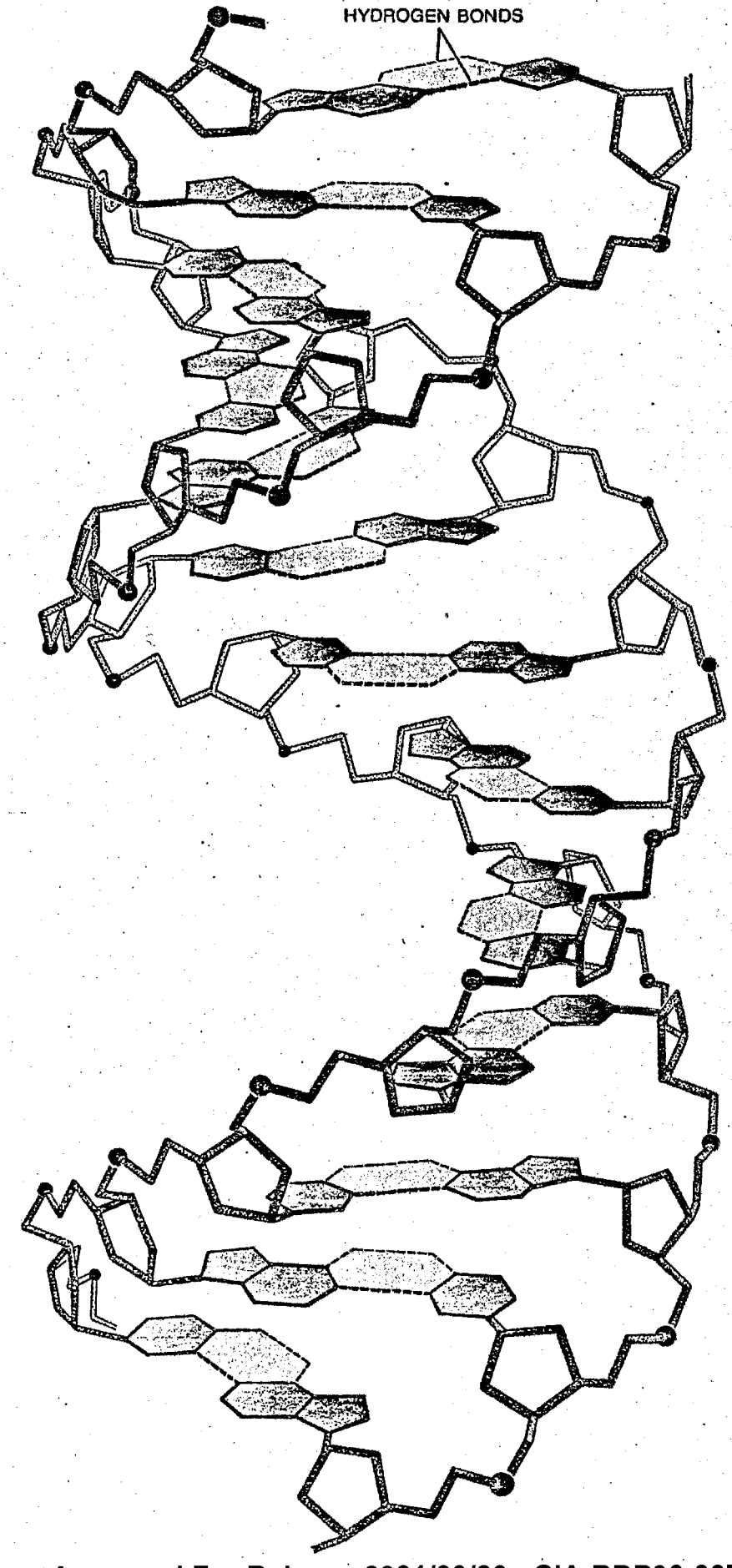
been painted, had Shakespeare and Picasso not existed. But of how many scientific achievements can this be claimed? One could almost say that, with very few exceptions, it is not the men that make science, it is science that makes the men. What *A* does today, *B* or *C* or *D* could surely do tomorrow."

On reading this passage, I found myself in full agreement on the general lack of literary skills among men of science. I was surprised, however, to find an eminent scientist embracing historicism (the theory championed by Hegel and Marx holding that history is determined by immutable forces rather than by human agency) as an explanation for the evolution of science while at the same time professing belief in the libertarian "great man" view of history for the evolution of art. Since it had not occurred to me that anyone could hold such contradictory, and to me obviously false, views concerning these two most important domains of human creation, I began to ask scientific friends and colleagues whether they too, by any chance, thought there was an important qualitative difference between the achievements of art and of science, namely that the former are unique and the latter inevitable. To my even greater surprise, I found that most of them seemed to agree with Chargaff. Yes, they said, it is quite true that we would not have had *Timon of Athens* or "Les Desmoiselles d'Avignon" if Shakespeare and Picasso had not existed, but if Watson and Crick had not existed, we would have had the DNA double helix anyway. Therefore, contrary to my first impression, it does not seem to be all that obvious that this proposition has little philosophical or historical merit. Hence I shall now attempt to show that there is no such profound difference between the arts and sciences in regard to the uniqueness of their creations.

Before discussing the proposition of differential uniqueness of creation it is necessary to make an explicit statement of the meaning of "art" and of "science." My understanding of these terms is based on the view that both the arts and the sciences are activities that endeavor to discover and communicate truths about the world. The domain to which the artist addresses himself is the inner, subjective world of the emotions. Artistic statements therefore pertain mainly to relations between private events of affective significance. The domain of the scientist, in contrast, is the outer, objective world of physical phenomena. Scientific statements therefore pertain mainly to relations between or among public events. Thus the transmission of information and the perception of meaning in that information constitute the central content of both the arts and the sciences. A creative act on the part of either an artist or a scientist would mean his formulation of a new meaningful statement about the world, an addition to the accumulated capital of what is sometimes called "our cultural heritage." Let us therefore examine the proposition that only Shakespeare could have formulated the semantic structures represented by *Timon*, whereas people other than Watson and Crick might have made the communication represented by their paper, "A Structure for Deoxyribonucleic Acid," published in *Nature* in the spring of 1953.

First, it is evident that the exact word sequence that Watson and Crick published in *Nature* would not have been written if the authors had not existed, any more than the exact word sequence of *Timon* would have been written without Shakespeare, at least up until the fabulous monkey typists complete their random work at the British Museum. And so both creations are from that point of view unique. We are not really concerned, however, with the exact word sequence. We are concerned with the content. Thus we admit that people other than Watson and Crick would eventually have described a satisfactory molecular structure for DNA. But then the character of *Timon* and the story of his trials and tribulations not only might have been written without Shakespeare but also were written without him. Shakespeare merely reworked the story of *Timon* he had read in William Painter's collection of classic tales, *The Palace of Pleasure*, published 40 years earlier, and Painter in turn had used as his sources Plutarch and Lucian. But then we do not really care about Timon's story; what counts are the deep insights into human emotions that Shakespeare provides in his play. He shows us here how a man may make his response to the injuries of life, how he may turn from lighthearted benevolence to passionate hatred toward his fellow men. Can one be sure, however, that *Timon* is unique from this bare-bones standpoint of the work's artistic essence? No, because who is to say that if Shakespeare had not existed no other dramatist would have provided for us the same insights? Another dramatist would surely have used an entirely different story (as Shakespeare himself did in his much more successful *King Lear*) to treat the same theme and he might have succeeded in pulling it off. The reason no one seems to have done it since is that Shakespeare had already done it in 1607, just as no one discovered the structure of DNA after Watson and Crick had already discovered it in 1953.

Hence we are finally reduced to as-



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WATSON-CRICK MODEL of the structure of DNA, the discovery of which was announced

serting that *Timon* is uniquely Shakespeare's, because no other dramatist, although he might have brought us more or less the same insights, would have done it in quite the same exquisite way as Shakespeare. But here we must not shortchange Watson and Crick and take for granted that those other people who eventually would have found the structure of DNA would have found it in just the same way and produced the same revolutionary effect on contemporary biology. On the basis of my acquaintance with the personalities then engaged in trying to uncover the structure of DNA, I believe that if Watson and Crick had not existed, the insights they provided in one single package would have come out much more gradually over a period of many months or years. Dr. *B* might have seen that DNA is a double-strand helix, and Dr. *C* might later have recognized the hydrogen bonding between the strands. Dr. *D* later yet might have proposed a complementary purine-pyrimidine bonding, with Dr. *E* in a subsequent paper proposing the specific adenine-thymine and guanine-cytosine nucleotide pairs. Finally, we might have had to wait for Dr. *G* to propose the replication mechanism of DNA based on the complementary nature of the two strands. All the while Drs. *H, I, J, K* and *L* would have been confusing the issue by publishing incorrect structures and proposals. Thus I fully agree with the judgment offered by Sir Peter Medawar in his review of *The Double Helix:* "The great thing about [Watson and Crick's] discovery was its completeness, its air of finality. If Watson and Crick had been seen groping toward an answer, if they had published a partly right solution and had been obliged to follow it up with corrections and glosses, some of them made by other people; if the solution had come out piecemeal instead of in a blaze of understanding; then it would still have been a great episode in biological history; but something more in the common run of things; something splendidly well done, but not in the grand romantic manner."

Why is it that so many scientists apparently fail to see that it can be said of both art and science that whereas "what *A* does today, *B* or *C* or *D* could surely do tomorrow," *B* or *C* or *D* might nevertheless not do it as well as *A*, in the same "grand romantic manner." I think a variety of reasons can be [illegible] for this strange myopia. The first of them is simply that most scientists are not familiar with the

to picture the artist's act of creation in the terms of Hollywood: Cornel Wilde in the role of [illegible] Chopin gazing fondly at Merle Oberon as his muse and mistress George Sand and then sitting down at the Pleyel pianoforte to compose his "Preludes." As scientists know full well, science is done quite differently: Dozens of stereotyped and ambitious researchers are slaving away in as many identical laboratories, all trying to make similar discoveries, all using more or less the same knowledge and techniques, some of them succeeding and some not. Artists, on the other hand, tend to conceive of the scientific act of creation in equally unrealistic terms: Paul Muni in the role of the one and only Louis Pasteur, who while burning the midnight oil in his laboratory has the inspiration to take some bottles from the shelf, mix their contents and thus discover the vaccine for rabies. Artists, in turn, know that art is done quite differently: Dozens of stereotyped and ambitious writers, painters and composers are slaving away in as many identical garrets, all trying to produce similar works, all using more or less the same knowledge and techniques, some succeeding and some not.

A second reason is that the belief in the inevitability of scientific discoveries appears to derive support from the often-told tales of famous cases in the history of science where the same discovery was made independently two or more times by different people. For instance, the independent invention of the calculus by Leibniz and Newton or the independent recognition of the role of natural selection in evolution by Wallace and Darwin. As the study of such "multiple discoveries" by Robert Merton of Columbia University has shown, however, on detailed examination they are rarely, if ever, identical. The reason they are said to be multiple is simply that in spite of their differences one can recognize a semantic overlap between them that is transformable into a congruent set of ideas.

The third, and somewhat more profound, reason is that whereas the cumulative character of scientific creation is at once apparent to every scientist, the similarly cumulative character of artistic creation is not. For instance, it is obvious that no present-day working geneticist has any need to read the original papers of Mendel, because they have been completely superseded by the work of the past century. Mendel's papers contain no useful information that cannot be better obtained from any modern textbook or the current genetic [illegible]

cal literature. In contrast, the modern writer, composer or painter still needs works of Shakespeare, Bach or Leonardo, which, so it is thought, have not been superseded at all. In spite of the seeming truth of this proposition, it must be said that art is no less cumulative than science, in that artists no more work in a traditionless vacuum than scientists do. Artists also build on the work of their predecessors; they start with and later improve on the styles and insights that have been handed down to them from their teachers, just as scientists do. To stay with our main example, Shakespeare's *Timon* has its roots in the works of Aeschylus, Sophocles and Euripides. It was those authors of Greek antiquity who discovered tragedy as a vehicle for communicating deep insights into affects, and Shakespeare, drawing on many earlier sources, finally developed that Greek discovery to its ultimate height. To some limited extent, therefore, the plays of the Greek dramatists have been superseded by Shakespeare's. Why, then, have Shakespeare's plays not been superseded by the work of later, lesser dramatists?

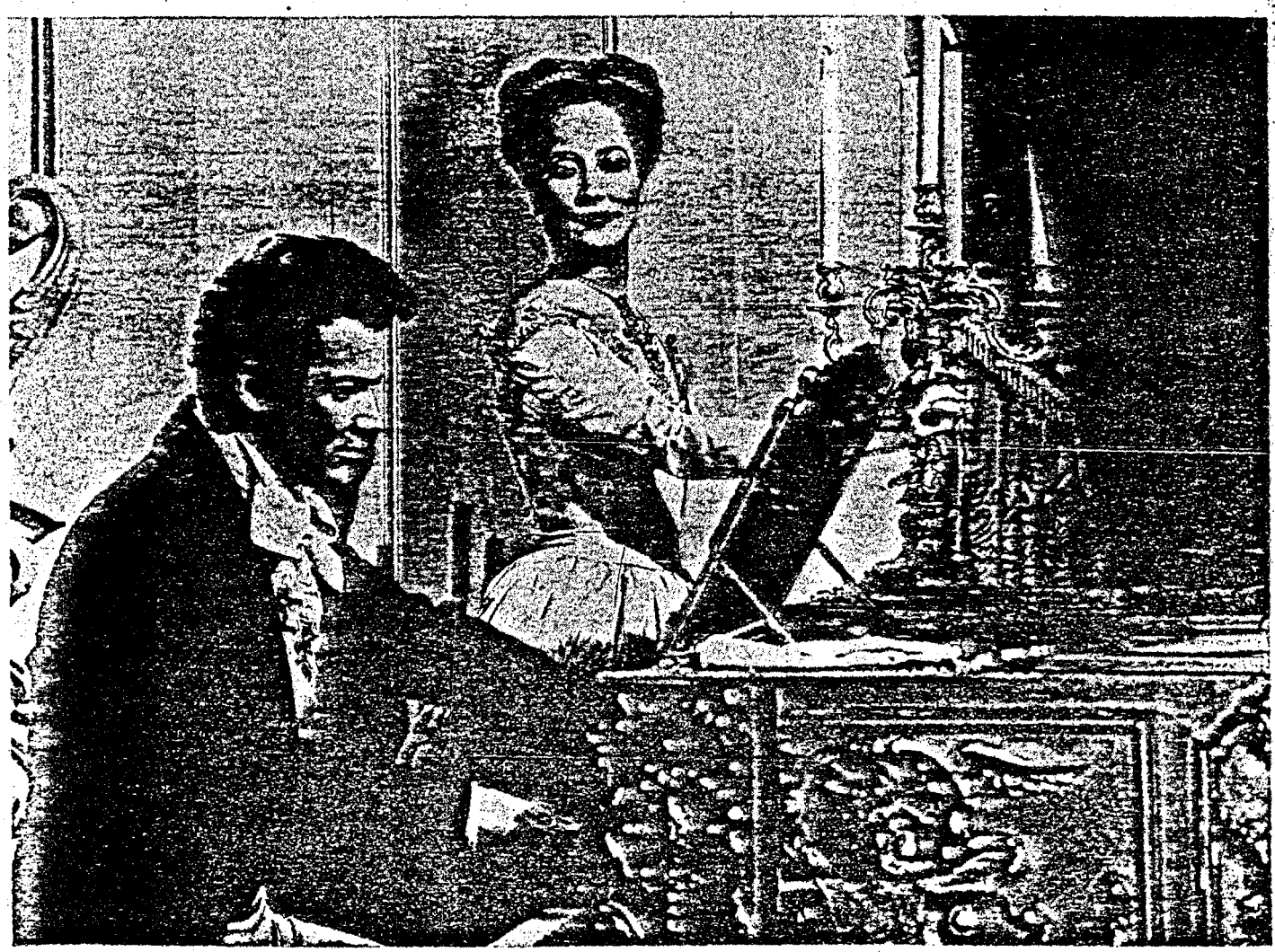
Here we finally do encounter an important difference between the creations of art and of science, namely the feasibility of paraphrase. The semantic content of an artistic work—a play, a cantata or a painting—is critically dependent on the exact manner of its realization; that is, the greater an artistic work is, the more likely it is that any omissions or changes from the original detract from its content. In other words, to paraphrase a great work of art—for instance to rewrite *Timon*—without loss of artistic quality requires a genius equal to the genius of the original creator. Such a successful paraphrase would, in fact, constitute a great work of art in its own right. The semantic content of a great scientific paper, on the other hand, although its impact at the time of publication may also be critically dependent on the exact manner in which it is presented, can later be paraphrased without serious loss of semantic content by lesser scientists. Thus the simple statement "DNA is a double-strand, self-complementary helix" now suffices to communicate the essence of Watson and Crick's great discovery, whereas "A man responds to the injuries of life by turning from lighthearted benevolence to passionate hatred toward his fellow men" is merely a platitude and not a paraphrase of *Timon*. It took the writing of *King Lear* to paraphrase (and improve on) *Timon*, and indeed the for-

mer has superseded the latter in the Shakespearean dramatic repertoire.

The fourth, and probably deepest, reason for the apparent prevalence among scientists of the proposition that artistic creations are unique and scientific creations are not can be attributed to a contradictory epistemological attitude toward the events in the outer and the inner world. The outer world, which science tries to fathom, is often viewed from the standpoint of materialism, according to which events and the relations between them have an existence independent of the human mind. Hence the outer world and its scientific laws are simply there, and it is the job of the scientist to find them. Thus going after scientific discoveries is like picking wild strawberries in a public park: the berries *A* does not find today *B* or *C* or *D* will surely find tomorrow. At the same time, many scientists view the inner world, which art tries to fathom, from the standpoint of idealism, according to which events and relations between [illegible] their reflection in human thought. Hence there is nothing to be found in the inner world, and artistic creations are cut simply from whole cloth. Here *B* or *C* or *D* could not possibly find tomorrow what *A* found today, because what *A* found had never been there. It is not altogether surprising, of course, to find this split epistemological attitude toward the two worlds, since of these two antithetical traditions in Western philosophical thought, materialism is obviously an unsatisfactory approach to art and idealism an unsatisfactory approach to science.

It is only in the past 20 years or so, more or less contemporaneously with the growth of molecular biology, that a resolution of the age-old epistemological conflict of materialism v. idealism was found in the form of what has come to be known as structuralism. Structuralism emerged simultaneously, independently and in different guises in several diverse fields of study, for example in psychol[illegible] linguistics, anthropology and biology.

Both materialism and idealism take it for granted that all the information gathered by our senses actually reaches our mind; materialism envisions that thanks to this information reality is mirrored in the mind, whereas idealism envisions that thanks to this information reality is constructed by the mind. Structuralism, on the other hand, has provided the insight that knowledge about the world enters the mind not as raw data but in already highly abstracted form, namely as structures. In the preconscious process of converting the primary data of our experience step by step into structures, information is necessarily lost, because the creation of structures, or the recognition of patterns, is nothing else than the selective destruction of information. Thus since the mind does not gain access to the full set of data about the world, it can neither mirror nor construct reality. Instead for the mind reality is a set of structural transforms of



SCIENTISTS' IDEALIZATION of the working methods of artists is idealized in this scene from the 1945 Columbia Pictures production *A Song to Remember*. Frédéric Chopin (played by Cornel Wilde), after gazing fondly at his muse George Sand (Merle Oberon), returns to the piano and composes his "Preludes." Science, as any scientist knows, is done quite differently.

primary data taken from the world. This transformation process is hierarchical in that "stronger" structures are formed from "weaker" structures through selective destruction of information. Any set of primary data becomes meaningful only after a series of such operations has so transformed it that it has become congruent with a stronger structure preexisting in the mind. Neurophysiological studies carried out in recent years on the process of visual perception in higher mammals have not only shown directly that the brain actually operates according to the tenets of structuralism but also offer an easily understood illustration of those tenets.

Finally, we may consider the relevance of structuralist philosophy for the two problems in the history of science under discussion here. As far as prematurity of discovery is concerned, structuralism provides us with an understanding of why a discovery cannot be appreciated until it can be connected logically to contemporary canonical knowledge. In the parlance of structuralism, canonical knowledge is simply the set of preexisting "strong" structures with which primary scientific data are made congruent in the mental-abstraction process. Hence data that cannot be transformed into a structure congruent with canonical knowledge are a dead end; in the last analysis they remain meaningless. That is, they remain meaningless until a way has been shown to transform them into a structure that is congruent with the canon.

As far as uniqueness of discovery is concerned, structuralism leads to the recognition that every creative act in the arts and sciences is both commonplace and unique. On the one hand, it is commonplace in the sense that there is an innate, or genetically determined, correspondence in the transformational operations that different individuals perform on the same primary data. With reference to science, cognitive psychology has taught that different individuals recognize the same "chairness" of a chair because they all make a given set of sense impressions from the outer world congruent with the same *Gestalt*, or mental structure. With reference to art, analytic psychology has taught that there is a sameness in the subconscious life of different individuals because an innate human archetype causes them to make the same structural transformations of the events of the inner world. And with reference to both art and science structural linguistics has taught that communication between different individuals is possible only because an innate human grammar causes them to transform a given set of semantic symbols into the same syntactic structure. On the other hand, every creative act is unique in the sense that no two individuals are quite the same and hence never perform exactly the same transformational operations on a given set of primary data. Although all creative acts in both art and science are therefore both commonplace and unique, some may nonetheless be more unique than others.

ARTISTS' MISCONCEPTION of the scientific act of creation is equally unrealistic. In this scene from the 1935 Warner Brothers film *The Story of Louis Pasteur* the great scientist (played by Paul Muni) has the sudden inspiration to discover the vaccine for rabies. Art, as any artist knows, is done quite differently. Both photo-