GENES, INTELLIGENCE AND EDUCATION

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Ladies and Gentlemen: I am both pleased and honored to have a small part in this second Stadler Memorial Symposium for I share the admiration and respect all of us had who were privileged to know and be influenced for the better by LEWIS STADLER. In recalling his work on the R locus in maize I am reminded of two episodes in genetics that considerably antidated his career in genetics.

Back in 1909 THOMAS HUNT MORGAN and R. A. EMERSON presented successive papers before an annual meeting of the American Breeders' Association. As many today do not know MORGAN was skeptical of Mendelian interpretations, and accused those who went along with them of practicing a kind of hocus pocus--of inventing complex systems of factor interaction to account for observed ratios such as 9:7, 9:3:4, 13:3, etc., then arguing that the agreement of observation and hypothesis proved the correctness of the latter.

He was followed by EMERSON who presented data showing that segregation of mottled coat color in garden beans could not be accounted for in terms of one factor pair, but that if one postulated two such pairs with a certain interaction, all would be well. Some thought EMERSON had lost that round, but it later turned out otherwise.

Shortly thereafter MORGAN discovered a *Drosophila melanogaster* male with white eyes and through its behavior in inheritance was wholly won over to classical Mendelism.

As you will recall, MORGAN soon found a second sex-linked character which in the two-factor hybrid female showed recombination which was interpreted as genetic crossing over.

It was now EMERSON's turn to be the skeptic.

MORGAN's interpretation would violate the well-established principle of the purity gametes, he argued in a short paper in *Science*, for if parts of chromosomes recombined in this way, the break would sometimes have to be right through a gene pair with a consequent change in both allelic members. In the short run EMERSON appeared to be wrong, for MORGAN's crossing over hypothesis was soon abundantly confirmed. But as we are all aware, in the long run, he was correct in concluding that such a mechanism would lead to cross contamination within pairs of the alleles. Unfortunately he did not live to know that.

STADLER's R-locus studies in corn played a highly significant part in undermining the simplistic notion many of us then had that a gene was a kind of unit which in recombination was indivisible.

The late DAVID BONNER was stimulated and encouraged by STADLER in his studies of intragenic recombination in Neurospora. I confess to being one of the skeptics who persisted too long in a view of gene nature that in retrospect seems incredibly naïve.

Now that we know the gene to be a segment of DNA of scores of nucelotides, it all becomes clear--at least clearer.

STADLER was an insistent and persistent prober into the consequences of both experimental findings and ideas. I have vivid recollections of his constructive influence on EPHRUSSI and me and later on TATUM and me in our Drosophila and Neurospora work. Because of him we thought more clearly and more deeply. I am sure this was the case with many other workers with whom STADLER had contact.

As all of you here know, it is now seventeen years ago that WATSON and CRICK worked out the now famous double helix structure of DNA, a most significant turning point in the history of genetics, for it immediately suggested how genes might carry information, their manner of replication, the molecular nature of mutation and the way in which genes could determine the order of amino acids in proteins and hence the specificity of enzymes. The result was that no longer could any informed and fully rational person contend, as the distinguished biologist WILLIAM MORTON WHEELER had as late as the mid-twenties, that the gene was a purely hypothetical, rather mysterious creation of geneticists that had little or no significance to biology as a whole.

That such skepticism could have persisted so long seems incredible. Yet there have been in the history of science as a whole, and biology in particular, many examples of such reluctance to accept evidence that today in retrospect seems fully persuasive.

Let me remind you of several other examples.

You know the textbook story of the rediscovery of MENDEL's work a third of a century after it was published--the contention that it was not known about because of its publication in a rather obscure journal. In fact, there is persuasive evidence that it was known to a good many who should have recognized its significance. KARL von NÄGELI, of course, knew about it through rather extensive correspondence with MENDEL. Because NÄGELI worked on the hawkweed Hieracium in which seeds are produced without chromosome reduction, his disbelief is understandable. And since MENDEL also worked with a species of that unfortunate genus, one can well understand that his own faith might well have been shaken. In addition to NAGELI's knowing about MENDEL's paper, there is clear evidence that three others who could have appreciated it were also aware of it. One was the Austrian botanist FOCKE who cited it in his extensive monograph on plant hybridization. LIBERTY HYDE BAILEY, an American botanist, also knew about it, although it seems he may have done no more than take the reference from FOCKE.

In addition to the two dozen or so who heard MENDEL present his findings orally before the Historical Society of Brünn, there must have been others who were in a position to understand and recognize the significance of his work, for he is known to have received 40 reprints of his paper. To whom did he send them? Aside from the disposition of three we shall probably never know. Of these three one was never read, for its pages remained uncut for several decades. We know of von NÄGELI's reaction to the one he received. A third one went to the Dutch botanist BEIJERINCK who gave it to HUGO de VRIES, suggesting he might find it of interest. de VRIES did read it but evidently was not persuaded, for years later when he reported genetic results interpreted by him in MENDEL's terms he did not cite MENDEL. When confronted by CORRENS, who had by 1900 independently rediscovered both the principles and publication of MENDEL, he is said to have replied that he had indeed seen MENDEL's paper but had forgotten about it. If the account is correct, one could be led to question de VRIES' intellectual honesty. I hasten to add, however, that such doubts may well be quite unjustified and unfair. How many of us remember where all our ideas have had their origins--just what is original with us and just what we subconsciously recollect from material we have read or conversations we have had? I doubt if a single one of us can claim infallibility in this regard.

One fascinating question that probably will never be fully answered is whether DARWIN knew of MENDEL's work. It seems most doubtful, for MENDEL had the answer to the seemingly fatal flaw in DARWIN's theory of evolution based on heritable mutations. You will remember that the Scottish engineer FLEEMING JENKIN argued most persuasively that DARWIN could not be right for the very simple reason that mutations occurring in cross-breeding plants or animals would be quickly swamped out through blending inheritance, then widely believed. Halved in each successive generation, such a mutant type would be diluted in ten generations by a factor of 1/1024. DARWIN had no answer and was greatly distressed by this. MENDEL, of course, had provided the answer, for persistent purity of his hereditary units, genes in today's terminology, was fundamental to his interpretation. It seems incredible that DARWIN could have read MENDEL's paper and missed that point.

We do, however, have good evidence that MENDEL was aware of DARWIN's hypothesis, for there survived him a copy of the German edition of "the Origin" with marginal notes in his own hand. It is perhaps understandable that MENDEL would have felt restrained, either by religious conviction or through direction of the Church, from active participation in discussion involving organic evolution. But it is difficult to believe he would have been hesitant about sending a reprint to DARWIN or otherwise calling DARWIN's attention to his paper. All we can conclude is that, if he did, DARWIN missed the essential point. And that too seems quite incredible.

With the so-called rediscovery of MENDEL's paper and the independent confirmation of both his results and his interpretation, this almost simultaneously by CORRENS, de VRIES and von TSCHERMAK, general acceptance by biologists might have been expected. But as we well know, this was not the case. I have already mentioned MORGAN's reluctance to accept the interpretation. But there were others. KARL PEARSON in England and his associate WELDON persisted in believing in the generality of blending inheritance and are said to have developed much of the biometry of the time in the hope of discrediting MENDELISM. The biometry was both good and useful but their conclusions were not. Their controversy with WILLIAM BATESON, the eloquent and aggressive defender of MENDEL, was protracted and vitriolic. In view of their dedication to one phase of the Mendelism hypothesis, it seems strange indeed that for two decades BATESON strongly opposed the view that Mendelism determinants were carried in chromosomes. It is said that he was converted in the early 1920's as a result of hearing and seeing the genetic and cytological evidence of CALVIN BRIDGES, and that after the AAAS meeting in Canada in 1922 made a special trip to New York to visit BRIDGES and others of the MORGAN school for the express purpose of learning more about it.

Then strangely, so the story goes, after returning to England and bringing the cytologist NEWTON to the John Innes Institution, he reverted to his original position of disbelief.

In addition to his very significant role in defending Mendelism, BATESON had a significant hand in the development of biochemical genetics. At the time of the "rediscovery" of MENDEL's paper in 1900, the English physician-biochemist ARCHIBALD E. GARROD discovered diseases in man that seemed to be inborn. Among these was alcaptonuria which in affected individuals is characterized by the excretion of urine that turns very dark on exposure to air. This striking symptom is expressed almost immediately after birth. In studying the histories of families in which affected persons were known, GARROD conferred with BATESON and PUNNETT who suggested the disease might be inherited in a Mendelian fashion as a recessive character. Evidence in favor of this was soon found by GARROD.

GARROD's biochemical studies suggested that alcapton, found chemically to be 2-5 dihydroxyphenylacetic acid, might well be derived from the amino acid phenylalanine and that in alcaptonurics a specific enzyme--then called a "ferment"--that in normal individuals catalyzed the further metabolism of alcapton, was either absent or inactive. He tested the hypothesis by feeding alcaptonurics excess phenylalamine and found that indeed alcapton was correspondingly increased. He then fed suspected chemical intermediates between phenylalamine and alcapton on the assumption that if such compounds were really intermediates, alcupton excretion would increase, but, if not, there would be no observed rise in excretion. In this way he identified a sequence of successive reactions.

Thus GARROD had a remarkably clear concept of the relation of genes to enzymes and of enzymes to specific chemical reactions. In addition, he was very much aware of how genetics could be used as a powerful tool in working out reaction sequences in metabolic processes.

He was not only the first biochemical geneticist, he was a superbly competent one.

Unfortunately, like MENDEL, he was not properly understood and appreciated by his contemporaries. But this neglect was not for the same reasons as in the case of MENDEL. His findings were not published in a single obscure journal as were MENDEL's--quite the contrary: He reported his work in a number of papers published in readily available journals. In 1909 he summarized all his work on hereditary biochemical abnormalities in a book entitled Inborn Errors of Metabolism--the published versions of the Croonian Lectures given in London shortly before. A second updated edition appeared in 1923, but again there was little attention paid his work either by biochemists or by geneticists. Why? Although the full answer can never be known, it is my belief that he was ahead of his time by several decades. Biochemists of his time took little interest in the upstart field of genetics and geneticists were not prepared to think in chemical terms.

BATESON referred to GARROD's work in his 1909 book Mendelism but it thereafter dropped out of the genetic literature until it was revived in about 1942 separately by J. B. S. HALDANE and SEWALL WRIGHT.

I recall giving a lecture at the University of California, Berkeley, in the mid-forties in which I recounted this remarkable story of the neglect of GARROD's work. RICHARD GOLDSCHMIDT, then on the faculty of that university was in the audience. He told me after the lecture that he could not understand how he had omitted mention of GARROD's work in his well-known book Physiological Genetics and that he had indeed been well aware of it, but had forgotten about it when he wrote the book. That seems to me a pretty clear indication that he had not really appreciated its significance, much as de VRIES had not properly assessed the work of MENDEL when he first read about it.

It is easy to forget that in MENDEL's time, and also in GARROD's, there was great skepticism about simple interpretations in biology. Vitalism still had great influence and explanations such as MENDEL's and GARROD's were all too easily dismissed as inevitably naive. Even in the mid-thirties, following SCOTT-MONCRIEFF's and earlier studies on anthocyanin pigmentation in plants, KUHN's and CASPARI's work on Ephestia and EPHRUSSI's and mine on eye-pigments in Drosophila, there remained many who were unprepared to accept the one-gene-one-enzyme concept that grew out of such studies.

In fact, after TATUM and I had devised systematic ways of relating genes to known biochemical reaction sequences in Neurospora, there were many who resisted the simple interpretation that seemed so obvious to many of us. Even a decade later at the time of the Cold Springs Harbor Symposium on Quantitative Biology, I recall only three persons who were convinced that genes were as simply related to enzymes and chemical reactions as we had postulated. I could name them if pressed, but I'd rather not.

Prior to 1944 it was widely assumed that genetic specificity must reside in protein, for the other large component of chromosomes was known to be deoxyribonucleic acid (DNA) which was thought to consist of rather monotonous molecules built of a large number of identical tetranucleotides and which therefore could not carry large amounts of genetic information. This structure was proposed by the chemist P. A. LEVENE, one of the acknowledged authorities on the structure of nucleic acids. Confidence in this view was badly shaken when in 1944 AVERY, McLEOD and McCARTY reported that one pneumococcus serological type could be permanently converted to a second type through treatment with pure DNA prepared from the second. That is, the DNA was as pure as these workers could prepare it using proteolytic enzymes calculated to degrade contaminating protein molecules that might have remained.

Again there were skeptics, those unprepared to doubt the old tetranucleotide structure. I still retain a vivid memory of their arguments, one of which was: "Avogadro's number is very large, so how can we be sure there were no remaining protein molecules in the transforming DNA preparations?"

By that time the way to further progress was being prepared by the bacterial virus workers DELBRÜCK and ELLIS, HERSHEY and co-workers and others who came to be known as the "phage group." They were laying the foundations of phage genetics. By 1952 the well-known HERSHEY-CHASE experiment could be carried out in which phage DNA was labeled with radioactive phosphorous and the protein coats and tails with radioactive sulfur. This said that much phosphorous and very little sulfur was carried from one phage generation to the next and that therefore the phage genes must be DNA, not protein.

This set the stage for the WATSON-CRICK attack on the intimate molecular structure on the DNA molecule which, as all high school biology students by now know, is a double helix. What many do not know, however, is that the two sets of administrative persons responsible for the support of WATSON and CRICK were so annoyed with their leaving the work they had proposed to do, for what was thought to be a diversionary wild goose chase, that both were in real danger of losing their positions, financial support and facilities with which to work.

I repeat, the double helix structure of DNA represents a major turning point in 20th century biology.

There is no need to continue to detail the story from there on, for you know it well. I shall be content, simply to remind you that genetic DNA is now known to consist of successive triplets of the four nucleotide subunits, that there are just 64 such triplets, that the significance of all of these "three-letter words" in terms of protein synthesis appear to be known, that DNA replication <u>in vitro</u> has been accomplished in cell-free test-tube systems, that the steps by which DNA information is transcribed into its messenger RNA counterparts, that such messenger molecules move to the cytoplasm where in combination with ribosomes they serve as templates against which RNA-labeled amino acids are properly ordered to form specific proteins, that many of the characteristics of proteins are now understood, that all steps in protein synthesis can be experimentally carried out in the absence of intact cells, and that there are known mechanisms by which DNA gene activity is regulated in ways consistent with requirements of the organism of which they are a part.

Chemists have by now succeeded, by purely chemical methods, in synthesizing proteins, RNAs and DNAs of predetermined sequences. Active enzymes and hormones have been made in this way. It therefore seems probable that before long, specific genes will be synthesized by chemical methods.

Darwinian natural selection experiments are now being carried out with viral ribonucleic acid molecules, and it is no longer beyond hope that clever chemists will in the foreseeable future be able to synthesize simpler virus-like systems capable of replicating in living cells.

Recognizing that this is but a sample of what can be done now and what we can look forward to, is it not likely to be discouraging to young people contemplating careers in modern biology? What will remain for them to do?

The answer is simple. Despite the spectacular advances that have been made up to now, far, far more remains to be done. To indicate just one promising application in the control of genetic diseases in man: I point out that virus carriers of extraneous DNA and RNA can now be engineered and that there is this reasonable hope that by the use of non-disease producing carrier viruses, such as the Shope papiloma virus of rabbits or the SV40 virus of monkeys, correcting genetic information may in the foreseeable future be used therapeutically in man and in other animals and in plants. We are also aware that cytoplasm, as well as DNA and RNA, is specific for different organisms, that it carries information of vital significance, and that its changes in development underlie the cellular differentiation that we know to be so important but about which we now have so little knowledge. I am sure RUTH SAGER will persuade you to be interested in that most promising area of future advance.

We are only beginning to understand the nature of differentiation in both single-celled organisms and in multicellular forms. How is it that cells, initially presumed to carry identical genetic information, come to have such differing final fates in complex organisms, in man for example?

In all that I have talked about so far man is not fundamentally different in his biological inheritance from other living creatures--spinach plants, snails, guinea pigs and elephants.

But in another respect we know that man differs from them in a specific and tremendously important way, namely that we supplement our biological inheritance with a large cultural complement. Our knowledge of how we inherit our biological traits is itself part of cultural inheritance. No other animal knows that about itself, nor does any possess the capability of finding out. In fact the gap between present day man and his closest non-human relatives is so great that it is fair to say man is essentially the only cultural animal on earth.

The late Polish-born engineer-philosopher ALFRED KORZYBSKI, who spent many productive years of scholarship in this country, deserves special credit for emphasizing this point. He assigned all living creatures to three categories: First, energy binders--those organisms such as bacteria, algae and higher plants that transform sunlight into energy-rich materials. The food, coal, oil and gas on which we so largely depend are examples.

Second are the space binders, those organisms that use plant-captured energy to move about--that is to bind different areas of space into a single habitat. And finally there are those KORZYBSKI called time-binders, a category almost totally dominated by man. We have, so-to-speak, bound human culture into a time-continuous whole from that ancestor of a million or two years ago who first made a useful tool and then taught his fellow men and his children to do likewise.

Metaphorically, man's cultural horizons have been broadened through his standing on the shoulders of myriads of men long since dead.

This second type of inheritance--transmission of cultural information and characteristics from person to person within and between generations--depends on that uniquely developed part of our nervous system called the brain.

This is a truly amazing organ in its capacity to record information, rearrange it and make it available at will--or unconsciously, for information that controls such instinctive and reflexive responses as breathing and the beating of the heart.

Just as are other parts of our bodies, the brain is constructed according to the DNA instructions we inherit biologically from our parents. Exactly how this most intricate construction job is carried out we do not know, just as we do not know how our many other specialized cells, tissues and organs are built on the basis of DNA directions that seem to be identical for all cells of the body. This is a major challenge for the future generations of biologists. Many geneticists, physicists-turned-geneticists, and biochemists are now attacking this intriguing and important problem--CRICK, BENZER, NIRENBERG, BRENNER, to name a few.

What we do seem to know is that there are two kinds of information in the brain--that built in according to DNA instructions, and that received through the senses. We seem not to differ significantly from our closest non-human relatives in kind or amount of the former. But for the "put-in" information--that received through the senses--we differ enormously, probably in amount, but surely in ability to rearrange, recombine, synthesize and retrieve it, as well as meaningfully to respond to it in its various permutations. It is this "put-in" information that is largely responsible for our cumulative cultural heritage. Birds do not learn to build nests; they do it instinctively. In contrast, we have no such built-in information telling us how to build houses. Unlike the bird, we are able to learn from our fellow men through the spoken and written word, through pictorial representation or by observing others do it. In addition to learning to build houses and scores of other man-made things, we have learned to cultivate plants and to domesticate animals--these for food, for work, or for simple pleasure.

Thus, because of cultural inheritance our behaviour is more flexible, less reflexive and less instinctive than that of other animals. Our "put-in" information is enormously greater than theirs in both amount and kind. As GEORGE GAYLORD SIMPSON and others have pointed out, our curiosity, imitation, attention, memory and imagination are far greater than theirs and we use these abilities in more intricate ways. Our reason is more highly developed. We make and use highly sophisticated tools and machines of many kinds. We are self-conscious. We reflect on the past, the future, and on life and death. We think in abstract and symbolic ways, which makes possible language, literature and art. Some of us have a sense of beauty, many are religious and only a few lack a moral sense. We are sensitive to many ethical restraints and to learned codes of conduct. In short, we are cultural and social animals, which has led us to develop unique societies and cultures of ever increasing complexity, including the arts, amusements, religions, philosophies, literatures, agricultures, technologies, sciences, complex industries, governments, educational systems and so on.

So here we are--the product of two kinds of inheritance, with the knowledge to control both. What do we do about it?

The first, biological change, which we could control and direct in man just as we do in the animals we domesticate and the plants we cultivate, is slow and difficult of reversal. Furthermore, there is no consensus as to what we want in man. Hitler had a specific program, but few of us today--none, I hope--approved of either his objective or his method. But I remind you, he convinced the majority of persons in a major modern nation. That is indeed reason for pause.

It is abundantly clear that organic evolution depends ultimately on genetic diversity. Thus, other things being equal, genetic diversity per se is advantageous. It follows that in our present state of doubt as to just what we desire in future generations of man, our wisest course, within reasonable limits, is to perpetuate maximum genetic diversity.

The same reasoning is, I believe, applicable to future cultural evolution. The world of man would be far less interesting if we were all alike culturally--and almost certainly far less viable.

Compared with biological change, cultural change is potentially far faster. Let me illustrate by giving examples: Professor JOHN PLATT of the University of Michigan's Mental Health Research Institute points out that within a century, little more than the life span of one of us, we have increased our travel speed a hundred-fold, our controllable energy resources a thousand times, our speed of computation a million times, our speeds of communication by a factor of ten million. These are all cultural changes, and decidedly unlike the biological change we call organic evolution, they could in theory be reversed in a single generation.

Many of the major social problems we face today are cultural. You know them: over-population with attendant poverty and famine, the ever-widening gap between the haves and have-nots, crime, polluted environments, medical resources inadequate to meet obvious needs plus maldistribution of those we do have, racial and cultural intolerance that leads to discrimination, social economic, educational, in employment, and in many other ways, inadequate educational systems and so on. Transcending all these is the ever present threat of nuclear war. All wars are bad, nuclear wars are unthinkable. Yet here we are a species so intelligent and so skillful in our science that we can travel to the moon and return, control the energy of the atom, build a machine that will automatically determine the sequence of amino acids in an enzyme of several hundred such units, construct another device that will automatically resynthesize that same enzyme with precisely the same order of amino acids, synthesize a gene, transplant a human heart or substitute an artificial one--even synthesize a simple virus-like living system in a test tube. Yet we still seem incapable of preventing the production and use of nuclear weapons that could destroy us all, friend and foe alike. How can we be at once so intelligent and skillful, at the same time so incompetent in solving this overriding danger?

We already have much of the knowledge and most of the resources required to solve them, especially in this privileged nation of ours. All we need is the will to do so and the determination to redirect resources. There is real danger that with the ever increasing tempo of change, our time may run out--we may respond too slowly. On the other hand, there is a counter danger that we may be prodded into changing too rapidly. There are some among us who say we are so deeply immersed in our social illnesses that only a major revolution can bring about the necessary correction--that before we can construct a more desirable social order, we must first destroy the existing one. Too often, I fear, they fail to appreciate that our present social-political-economic-industrial systems have now become so complex and delicately balanced that any major disturbance in their principal components--transportation, to take just one example--could quickly lead to mass suffering, rioting and starvation beyond our present comprehension. We must not forget--to mention just one element--that our major urban centers have at one time no more than a few days' food supply.

It seems clear to me that unless we are prepared to accept a terrible cost in human suffering and death, our only sensible alternative lies in continuing the slow process of orderly evolutionary correction of the many faults in our present system--and hopefully speeding it up substantially.

Priorities must be re-examined. To mention just one example--perhaps not of greatest importance but one representing a glaring inconsistence in policy of Federal government--our Department of Agriculture continues to subsidize the production of tobacco at the same time that our Public Health Service points out daily, through radio and TV, the health hazards of smoking that same tobacco.

But we are not without hope. A recent issue of the _Saturday Review_ points out that the United States Congress will soon consider enacting into law the following resolutions (I paraphrase in the interests of brevity):

> That emergent national problems, physical and social, constitute a major threat to the nation.
>
> That these problems are largely the result of growth of populations, rapid consumption of natural resources, and the erosion of our environments.
>
> The modern technology is or can be a pivotal influence in both causing and solving these problems.
>
> That after obtaining adequate and timely information, the Congress act to protect against these threats and at the same time assure the nation of all possible benefits.

In this we see the hand of Congressman Daddario of Connecticut. Let us all hope such reassuring words will be followed by prompt and effective action. The recent national EARTH DAY demonstration was in many ways an encouraging sign of constructive effort to solve one category of problems--pollution of the environment.

But that is but one of a large number of problems about which we are doing far too little.

An overriding one to which no one now seems to see a socially acceptable solution is the control of human population growth. Our population doubling time is approaching 30 years, and it's becoming shorter with every passing year. By the year 2000--when each of us is just 30 years older--the projected world population is 7 billion, up from the present 3-1/2 billion. And it has been predicted that by 2100, by which time our grandchildren will take over, the habitable land of the earth will be as densely populated as is present day Manhattan Island.

Let me again emphasize the complex interaction of the many cultural problems. Unless we learn sensibly to control human population growth, depletion of natural resources will escalate, wars will be inevitable, pollution of environments will continue to grow exponentially. Nuclear energy will have to replace that derived from fossil fuels and that will replace one kind of environmental pollution with another perhaps equally threatening.

Our transportation facilities are now near the breaking point. Think of its state with one or two or three more population doublings --say to a world population of 28 billion. Remember, on the present curve that could come within the lifetimes of our own children.

Let me now turn specifically but briefly to education--one of the very important ways by which we transmit cultural knowledge and cultural values from generation to generation and by which social evolution takes place. In the broadest sense it includes all that we learn from birth--through parents, brothers and sisters, playmates, other associates, man-made things (books, machines and so on) and the formal educational system.

It is startling and almost impossible to appreciate that in the absence of such influences, even with all physical needs met, individuals would revert in a single generation to a cultural state of possibly a million years back in human evolution. That state might be little different from that of our present primate relatives--the chimpanzees, for example. Can you imagine the experimental test and the outcome? It is difficult. Yet, so great is our capacity to acquire cultural patterns and information that, given proper conditions, such as deculturization process--whatever its magnitude--could, at least in theory, be completely reversed in a following generation.

We know a great deal about the acquisition of cultural patterns, but by no means all. We know the process begins early--at birth or even before. But it is only recently that we have come to appreciate how very much depends on the earliest months and years. It is only when children are deprived of the early experience we are so prone to take for granted in our own cultural context, that we see the significance of what happens early. Thus we come to realize the difficulties of moving individuals from one cultural pattern to another, unless the process is begun very early--much before the usual beginning of formal schooling.

We know that there is great variability among individuals in capacity to acquire specific cultural traits. In the extreme, in some forms of feeblemindedness, for example, it is clear that such differences may be genetically determined through faulty DNA directions. Within the normal range, it is less easy to demonstrate that differences may have a genetic basis. Nevertheless, carefully designed and executed studies, such as those comparing differences within identical twin pairs with those of their fraternal counterparts, show clearly that there are such differences. It is equally obvious that developmental factors can influence intellectual capability. To take an extreme example, lead poisoning in childhood is known to cause mental retardation. Surely there are myriads of far more subtle and as yet unknown environmental factors that are influential in this regard. Malnutrition in early development, prenatal or postnatal, is probably one of them, for in the extreme it can actually reduce the final number of cells in the brain.

These complex and subtle interactions make it extremely difficult to identify and assess innate cultural potentials, and this is the basis of much of the present controversy about our educational practices, such as have been stimulated by the recent publication of the educational psychologist, ARTHUR JENSEN.

That the educational system should be designed better to recognize and take into account quantitative and qualitative differences in intellectual capacity, whether genetically or culturally determined, is widely recognized, even though not often enough translated into effective practice. That should not be controversial.

What does deeply stir the emotions of so many of us, is the question of whether or not there are significant racial or ethnic differences in innate intellectual capacities.

Taking a specific example, JENSEN contends there may well be significant differences between American Negroes and whites in our cultural context, and that this should be taken into account in our educational system--that there is evidence indicating Negroes, statistically speaking, are relatively stronger in associative learning and less so in conceptual reasoning; that in whites the reverse is the case.

Even if I possessed the competence in this area to evaluate the facts and the conclusions of such studies, which I have not, I would not have the time to do so on this occasion.

As a geneticist, I can say that I believe it likely that any two populations of man reproductively separated over hundreds of generations will come to differ statistically with regard to a great many measurable genetic traits. I agree with the view once expressed by the late J. B. S. HALDANE who said that as a geneticist he believed in racial differences, but that he did not know who surpassed whom in what.

Thus I would expect such differences in many of the components of intelligence. The psychologist LOUIS THURSTONE many years ago identified seven such components--which he called verbal comprehension, word fluency, numerical ability, space visualization, memory, perceptual ability and reasoning. These are by no means independent, nor does anyone contend that they adequately represent the totality of what we call intelligence. In fact, THURSTONE's successors have identified many more such components of intelligence.

One point I wish to make about all this is that one may score relatively high in some of these components and less so in others.

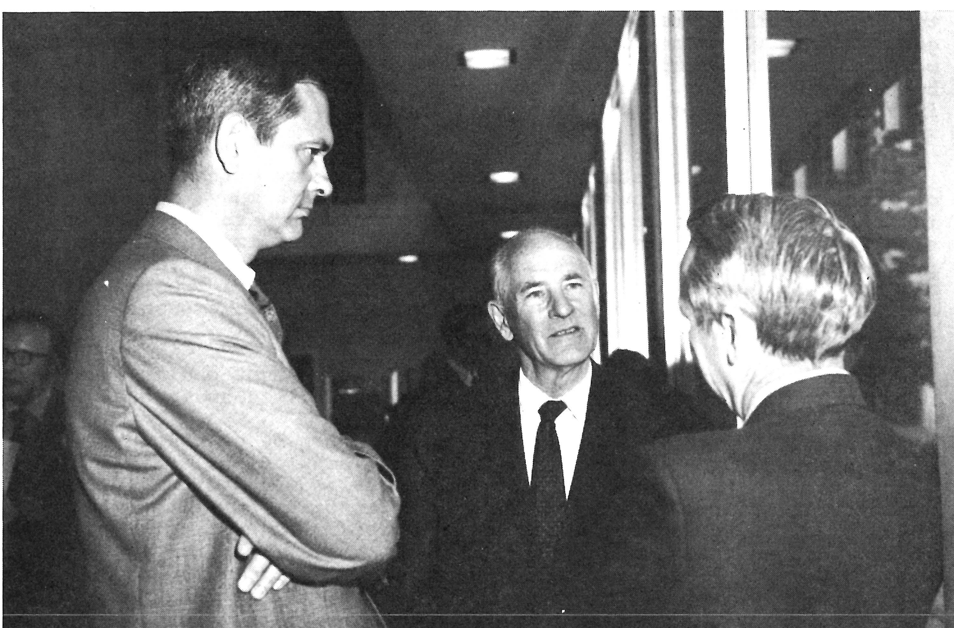
A second point is that there can therefore be no absolute scale of intelligence for all individuals and all populations. It is highly almost certain that an intelligence test devised in one cultural context will differ from those formulated in other contexts. Thus there is a strong possibility--really a presumption--of cultural bias in any such test. JENSEN purports to have taken this into account in various ways, for example, by comparing children of middle class Negroes with whites of the same socio-economic-cultural class. In this it seems to me he fails to take into account that in our cultural context skin color and physical features are themselves of profound cultural significance. This being so, one really cannot easily evaluate blacks and whites under strictly comparable circumstances. Therefore all such tests of innate intelligence are almost sure to be culture-bound, to use an expression of current popularity.

A third point is that by whatever criteria intellectual ability is measured, there will surely be overlaps, almost surely large overlaps, among racial or ethnic groups.

My final point, and the one I want most strongly to emphasize, is that with regard to all matters related to intellectual characteristics--educational patterns, job training programs, occupational opportunities and others of comparable nature--we should think and act in terms of individuals, not of racial or ethnic groups. Thus, to me it seems crystal clear that we should assign our highest priority to giving every individual of our species the best possible opportunity to develop and use his or her full potential--genetic or other--in ways that will maximize both the well being of that individual and of the society of which he is a part. That should be a primary goal of all patterns of culturalization and systems of education.

It is a big order and of course we shall never fully fill it. But we must do the best we can. That means we must do our best to favor our intellects and surpress our unjustified preconceptions and prejudices.

I have resolved to try harder. I hope all of you will join me. Thank you.



From left to right: Dr. Yanders, Dr. Beadle and Dr. Pittenger