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GENES, DRIVES, AND INTELLECT[1]

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CONTENTS

This review attempts to integrate the developments which have occurred recently—largely independently—in three areas of psychology: behavior genetics, motivation, and the theory of intelligence.

The concept of intelligence occupies a rather paradoxical position in psychology. On the practical side, Spearman and Jones (1950) have observed that the success of intelligence tests could

> with little exaggeration, be regarded as amazing, or even alarming. Over and above their attainment of extraordinarily wide acceptance, they threaten to exercise an immediate and revolutional influence upon daily life (p. 1).

This appears under the heading "A New Menace," and the note of anxiety reflects the chronically unsatisfactory state of intelligence theory. Hearnshaw (1951) observed that "We have no adequate current theory of intelligence, and test content is determined by . . . convenience . . . analogy with practical tasks . . . intuitions, and . . . vestiges of past theories" (p. 316). As Hebb (1958) put it, Binet "learned how to measure something without any very clear idea as to what it was he was measuring" (p. 246). Today, some 50 years later, we may measure it a bit more satisfactorily, and we can certainly measure it with a greater variety of techniques; but we are still vague about what "it" is.

There is more involved here than the trivial but vexing problem of agreeing as to which meaning should be assigned to the word *intelligence.* In its broadest sense, this word refers to a large assortment of biological and behavioral variables which are interrelated in ways we do not yet understand. Attempts to simplify matters by defining the word in terms of a small number of variables merely invite conflict with other definitions which refer to other variables. It may be more fruitful in the long run to postpone precise definition, and tentatively include all the shades of meaning in current use. (However, it may develop that some of these meanings refer to entities or phenomena that do not

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actually occur in the real world of behaving organisms. Any such meanings must eventually be rejected.)

Some psychologists are interested in the comparative intelligence of various species, but others consider the concept of intelligence applicable only to man. The present discussion will adopt the broader of these viewpoints, despite the disturbing lack of a single set of operations for measuring the intelligence of various species. Perhaps a set of operations can be formulated, eventually, which will permit us to make the same kind of comparison between rat and monkey on the one hand, and moron and genius on the other.

Some authorities think of intelligence as an innate characteristic, while others see it as a collection of acquired abilities. Both groups will be accomodated here, to some extent, in the belief that innate and experiential factors are both relevant. Those who insist that intelligence is *purely* innate are merely using the word in a special, narrow sense—they do not deny that experience affects the actual manifestation of innate capacity. On the other hand, those who are still skeptical of hereditary influence are concerned with the adequacy of the data; and it must be admitted that much of the evidence for inheritance of human intelligence is less than perfect. However, these skeptics would doubtless admit that the intellectual difference between man and monkey is genetically determined; and any characteristic which displays genetically controlled interspecies variance may be assumed to display intraspecies variance as well, though to a lesser extent. The real question, then, is whether the variance within our species can be considered negligible.

Skepticism about the genetic aspect of human intelligence is often due as much to humanitarian or democratic values as it is to consideration of the data. As Fuller and Thompson (1960) have observed, "It is not a coincidence that genetics has been the biological science most prostituted in both Fascist and Communist states" (p. 1). Value systems necessarily and properly influence our application of knowledge to practical, social problems, but need not influence our attempts to understand the facts. (A physicist who abhors wholesale destruction does not therefore deny that mass can be converted to energy.) This discussion will be much more concerned with understanding facts than with applying them; however, certain matters of social implication may be noted briefly. (a) The occurrence of hereditary individual differences does *not* imply any such differences among the average levels of various ethnic or racial groups; but if any such differences do exist, their directions are not now known, and their magnitudes are probably negligible compared to the diversity within each group. The essence of the democratic ideal lies not in insisting that all groups are identical, but rather in dealing with each individual on his own merits, without assuming that he matches a group average. Some environmentalists may be surprised to learn that such ardent champions of behavior genetics as Fuller and Thompson (1960, pp. 323-324) doubt the existence of important racial differences in behavior. (b) Although injustice might result from erroneously ascribing an individual's intellectual status to genetic causes, it might also result from the opposite error. (c) It is not true that acceptance of genetic factors necessarily leads to fatalistic acceptance of intellectual status as immutable (David & Snyder, 1954, p. 6; Dobzhansky, 1955, pp. 74-77; Fuller & Thompson, 1960, pp. 9, 318, 326; Masland, 1958, pp. 15, 47, 48).

Fuller and Thompson (1960, p. 10) have suggested that much misunderstanding may be due to reliance on oversimplified, obsolete, genetic models. Their book, *Behavior Genetics,* may introduce many psychologists to the more sophisticated concepts of modern genetics.

The much-discussed problem of the proportions of variance attributable to heredity and environment is meaningless, except with regard to a specified population. In the case of a highly inbred population with a broad range of living conditions, environment must contribute most of the variance; but in a genetically diverse population with a highly standardized environment, the observed variance must be due primarily to heredity. It may be anticipated that social progress will involve a gradual equalization of educational and related opportunities in any actual population, so that environment will contribute progressively less to intellectual variance. Thus, hereditary factors may, eventually, be the predominant source of differences in intelligence—whatever their present contribution may be (Fuller & Thompson, 1960, p. 323).

The majority of psychologists probably agree with Anastasi (1958) that the important question today is not *whether* intelligence is determined by genes or experience, but rather *how* each of these factors produces its effect. There is little information available on the details of how environment affects intellectual ability. It cannot be stated with confidence exactly which learning opportunities are necessary and sufficient for development of specific abilities. However, it is easy to see, in a general way, how opportunity to learn is reflected in subsequent performance, and this problem presents no serious obstacle to the development of intelligence theory in broad outline. The situation is much less clear in the case of genetic factors. The mechanisms intervening between genes and intelligent behavior are numerous and largely unknown, and various hypotheses dealing with this problem will be of central concern in this discussion.

A MOTIVATIONAL-EXPERIENTIAL THEORY OF INTELLIGENCE

The purpose of this paper is to re-examine an old theory in the light of more recent developments. This theory may conveniently be introduced in terms of a purely hypothetical set of events. Consider a pair of human, fraternal twins with unusually similar hereditary makeup. Suppose that the only significant differences in genetic composition happen to be of a type which cause these individuals to differ motivationally—not with respect to the so-called homeostatic drives, but rather with respect to their tendencies to engage in certain kinds of intrinsically-motivated activities. Specifically, twin *A* displays a strong preference for linguistic activity. At the age of 2 years he is unusually talkative, and at 8 years he does an unusual amount of reading. Twin *B*, on the other hand, shows an equally strong preference for athletic activity. The twins' brains do not differ appreciably in size, or biochemistry, or capacity to form associations or develop complex neural circuits. In the course of time, *A* acquires a larger vocabularly than *B*, as well as a larger store of linguistically transmitted information—simply because he devotes more time to relevant activities. At the age of 16 years, both twins are given the Stanford Binet, and *A* is found to have a considerably higher IQ than *B*.

Three alternative interpretations of this story may be noted. The first would be that it is absurd, since activity preferences or interests are acquired, not inherited. A second reaction might be that this case simply illustrates a limitation of present intelligence tests: Although the twins really had the same intelligence, their test scores were distorted by irrelevant personality factors. The third interpretation summarizes the theory to be considered: Intelligence is acquired by learning, and inherited motivational makeup influences the kind and amount of learning which occurs. The hereditary basis of intelligence consists of drives, rather than abilities as such.

History

This conception of intelligence, though it is not well known, is by no means new. It was suggested by Groos (1898; 1901) some 60 years ago. In discussing the evolution of intelligent species, Groos (1901) said:

> Play is the agency employed to develop crude powers and prepare them for life's uses, and from our own biological standpoint we can say: From the moment when the intellectual development of a species becomes more useful in the 'struggle for life' than the most perfect instinct, will natural selection favour those individuals in whom the less elaborated faculties have more chance of being worked out by practice under the protection of parents—that is to say, those individuals who play (pp. 374-375).

Many later authors have considered either the theory as a whole or some part of it. Some authors have mentioned the motivational factor as a minor source of intellectual variance, while attaching greater importance to other mechanisms (e.g., Spearman, 1927, p. 340; Thorndike, Bregman, Cobb, & Woodyard, 1926, p. 421). Harlow (1953) mentioned "inherent motivational forces" as important determiners of the characteristic intelligence levels of species, and observed that "It is certainly more than coincidence that the strength and range of curiosity-manipulative motivation and position within the primate order are closely related" (p. 29). Bayley (1955) suggested that "Differences in drive or motivation or curiosity, especially as they relate to intellectual areas, often play a role in developing mental ability" (p. 5). Piaget (1952) has emphasized the role of intrinsically motivated activity in intellectual development, but his treatment is not concerned with differences in intelligence. The unusual interests of gifted individuals have been emphasized by Terman (1954) and by Warren and Heist (1960).

Wechsler's (1943; 1950) "non-intellective" factors in intelligence are *motivational*, but Wechsler seems to be primarily concerned with the later influence of the individual's motivational characteristics, at the time of performance, rather than with earlier effects on intellectual development. He has noted the strong influence of the traditional sharp distinction between the affective and cognitive aspects of behavior: Psychologists

> still think of intelligence as consisting primarily of cognitive abilities. Any bit of behavior that seems concerned with or related to instinct, impulse, or temperament is ipso facto considered as having no direct relation to general intelligence (1950, p. 83).

Cattell (1957) also rejected the traditional "modalities" of capacity, temperament, and motivation, and preferred to include all such traits in a single factor analysis, since "however easily the modality difference may be *theoretically* stated, no one can agree in practice, for example, where perceptual ability factors end and personality factors begin" (p. 79).

The argument supporting the motivational-experiential theory involves four main points: (a) Differences in motivation may be genetically determined. (b) These motivational differences, along with differences in environment, cause differences in experience. (c) Differences in experience lead to differences in ability. (d) The differences commonly referred to as intellectual are nothing more than differences in acquired abilities. These points will be examined in detail in the next four sections.

INHERITANCE OF MOTIVES

In the recent history of psychology, it has been generally supposed that such human motivational characteristics as, e.g., a preference for linguistic over athletic activity, were determined by experience, rather than heredity. This has been largely a matter of theoretical bias, since little directly relevant evidence has been available, as noted by Fryer (1931, p. 212) and G. G. Thompson (1959, p. 13). It is difficult to attack this problem within the narrow context of human motivation, since experience and heredity are both uncontrolled in man, and variance is restricted. Terman (1931) observed that the difficulty has often been compounded by unreliable measures of interest. Nevertheless, a few studies of twin similarities cast serious doubt on the assumption of experiential determination.

Carter (1932) studied the Strong vocational-interest-group scores of 43 sets of identical twins and 43 sets of like-sexed fraternal twins. He found average correlation coefficients of .50 for identical pairs, and .26 for fraternal pairs.

Such deviant forms of behavior as criminality, delinquency, and homosexuality may be considered reflections of motivational characteristics; and Fuller and Thompson (1960, p. 299) have reviewed a number of twin studies which indicate that genetic factors are of considerable, though not overwhelming importance in this area. A particularly striking example (not directly relevant to intellectual development) is provided by Kallman's study (1953, p. 70) of homosexuality in twins. He located 95 cases of male homosexuality in the index members of pairs of twins, and found that the co-twin was also homosexual in all of the 44 monozygotic pairs, but in only 13 of the 51 dizygotic pairs.

Comparative psychologists, unlike students of human behavior, can control the heredity and environment of their *Ss*. Furthermore, they can observe the effects of much greater differences in both of these variables than can be found within a human population. Motivational differences between species are often quite obvious, but their significance tends to be obscured by concomitant differences in other characteristics. Intraspecies differences are therefore of more interest, and are also more relevant to both theoretical and practical problems.

The literature cited by Fuller and Thompson (1960) shows that many genetically controlled motivational differences do occur within species. Examples include: preference for different kinds of habitat in two varieties of deermouse (p. 135); difference in sex drive between two strains of guinea pigs (p. 175); difference in maternal behavior tendencies in two varieties of rabbits (p. 181); abnormal appetite in genetically obese mice (p. 183); different preferences for alcohol in different strains of rats (p. 185); different hoarding tendencies among three strains of rats (p. 186); development of strains of active and inactive rats by selective breeding (p. 244); differences in exploratory behavior among various strains of mice and rats (pp. 247-251); different activity levels in various breeds of dogs (p. 251); difference in aggressiveness between two strains of rats (p. 253); and numerous reports of differences in "emotionality" among varieties of mice, rats, and dogs.

Searle's (1949) study is especially relevant here. He found that Tryon's "bright" and "dull" strains of rats differed in neither general learning capacity nor intelligence, but rather in a number of motivational and emotional characteristics (which influenced performance in the particular type of maze used by Tryon). Other reports of motivational differences between "bright" and "dull" rats have been reviewed by Fuller and Thompson (1960, p. 219).

Similar results have been obtained by comparing various breeds of dogs. Scott and Charles (1953) reported that

> important innately caused differences in performance are produced by differences in emotional, motivational and peripheral processes, and . . . genetically caused differences in central [intellectual] processes may be either slight or non-existent (p. 225).
>
> The most significant conclusion which can be related to human behavior is the necessity of attempting to test emotional and physiological differences in the important 'dimensions of personality.' This is not easy to do and certainly cannot be done effectively by asking questions in a simple paper and pencil test. . . . Until such tests are made and analyzed

> for correlation with performance tests, any statement regarding alleged differences in . . . [intellectual capacity] are suspect as having been caused indirectly by differences in motivational and emotional reactions (p. 227).

Much current work is being directed toward finer analysis of both the genetic and psychological aspects of hereditary motivational characteristics. Thus, McClearn (1959) has reported on the exploratory activity of crosses, as well as pure strains, and has measured exploratory behavior in a variety of novel situations. Some indication of the subtle and varied nature of the genetic control of motivation is provided by McClearn's (1960) finding that the exploratory activity of C57BL mice is reduced, and that of A mice is increased when illumination is changed from bright to dim.

The data from other species prove nothing conclusive about human motivation; however, taken together with the meager human data available, they support the plausibility of human motivational inheritance.

Genetically controlled motives are in no way incompatible with experiential modification of motives. If a special training procedure causes ducks to avoid water, this does not force us to deny that ordinary, untrained ducks differ from ordinary, untrained chickens in their attitude toward water.

EXPERIENCE-PRODUCING DRIVES

The next point to consider is whether the presumed hereditary differences in motivation can cause important differences in experience. This subject has received a great deal of attention recently, and need not be argued extensively here. Berlyne (1960) and R. White (1959) have reviewed the literature on the class of motives variously known as exploratory, manipulatory, curiosity, play, etc. Much of this literature has emphasized two points: (a) These cannot reasonably be considered secondary or derived motives. (b) Despite their diversity of expression, they serve the common biological function of promoting learning in the well-nourished and otherwise satiated organism. A special term may be useful to distinguish these motives from derived or secondary motives, on the one hand, and from the traditional "biogenic" or "homeostatic" drives on the other. They have previously (Hayes & Hayes, 1954, p. 296) been referred to as *experience-producing drives* (EPDs), and this term will be retained here, since it seems more descriptive than R. White's "effectance" (1959, p. 321).

The Concept of Drive

There has been a great deal of dissatisfaction recently with the traditional conceptions of "drive" (Cofer, 1959; Harlow, 1953; Hebb, 1955; Ryan, 1958; Stellar, 1954). This term has commonly implied an innate, homeostatic mechanism that reacts to a deficit state by initiating behavior which tends to eliminate the deficit. The hunger drive is probably the archetype of the concept: after a period of food deprivation, an organism tends to eat. However, a number of studies (e.g., Ghent, 1951, 1957; Hebb, 1949, p. 193) have indicated that the mechanism responsible for this homeostatic behavior is learned rather than innate. Rats reared with food constantly available show little enhanced interest in food after their first period of deprivation; and they perform well at food-rewarded learning tasks only after a week or more of experience with a deprivation schedule. But if hunger is strictly an acquired motive, all rats should starve to death, since they do not eat unless hungry, and do not get hungry until they have had experience at eating after deprivation. Fortunately, this impasse is prevented by the tendency of rats to consume a bit of food from time to time, whether they need it or not—as illustrated by Ghent's control rats which ate substantial amounts during the test period even though they had not been deprived. It appears that food-oriented behavior might be better understood if two mechanisms were explicitly recognized—a food drive which is innate but not homeostatic, and an acquired hunger motive which is homeostatic but not innate.

The word *drive* will be used in this sense hereinafter, to indicate the neural mechanism responsible for an organism's tendency to engage in certain behavior, independently of deficit states. The driven behavior is not assumed to necessarily result in any change in drive level, and drive reduction is not assumed to be essential to learning. Although some authors have attempted to force the concepts of exploration and curiosity into an S-R drive-reduction framework, the resulting forced fits have left much to be desired—as emphasized in Walker's (1961) review of Berlyne's *Conflict, Arousal, and Curiosity.*

Use of the plural, "experience-producing drives" (EPD), raises the question of how many such mechanisms are involved. Ultimately, this question must be answered by the data of behavior genetics. If the data reveal one, or a dozen, or several hundred genetically independent motivating mechanisms, the data must be accepted, regardless of whether the resulting picture of behavior is simple and esthetically satisfying, or complex and unlovely. If two behavioral tendencies are controlled by two genetically-independent, neural mechanisms, then two drives are involved, no matter how similar the two behaviors may be. If it were shown that a tendency to listen to music by Bach was inherited independently of a preference for Brubeck (which seems unlikely), then separate Bach and Brubeck drives would be inferred. If, on the other hand, it were found that the tendencies to listen to music and to play football were always transmitted together (which also seems unlikely), these would be considered alternative expressions of a single drive.

EPD Balance

It cannot reasonably be assumed that intellectual development depends simply on the sum of the strengths of all EPDs; a much more complex relationship is likely. Some drives may be "experience-producing" only in a negative sense. Although animals have more or less tendency to explore novel stimuli, they also have more or less tendency to avoid them, and if the latter tendency is too strong, experience will be restricted. On the other hand, as everyone knows, a cat whose curiosity is untempered with timidity may not live long enough to accumulate much experience. The *idiot-savant* may be a victim of EPDs which are not so much generally deficient as abnormally balanced. The case described by Scheerer, Rothman, and Goldstein (1945) seems quite as remarkable for his abnormal motivational makeup as for his unusual assortment of abilities and disabilities.

The mere tendency to be active might be considered a sort of minimal-essential EPD; but an excessive level of overt activity is incompatible with certain kinds of experience. It must also be noted that an important kind of experience-producing activity is not overt at all; thus Diamond (1939, p. 84; 1957, p. 21) has referred to "a need for manipulation of the content of awareness."

In an ideal organism, EPD makeup would be a function of age, so that activities appropriate to infantile learning would be replaced by activities conducive to the later learning of more complex skills; and it may be supposed that real organisms would have evolved mechanisms for making such changes. It may also be supposed that EPD strength in general would tend to drop as the individual approaches maturity, and achieves mastery of the more useful skills. Such a decline in EPD strength would have survival value in relieving the sophisticated individual of risks which would no longer be justified.

Pleasure and Achievement

To avoid some confusion, it may be well at this point to emphasize explicitly that the concept of EPDs does not imply either enjoyment of the activity or anticipation of its results. The exploring rat may, for all we know, feel pleasure as he enters a novel part of his environment; but it is also conceivable that such behavior may be affectively compulsive—perhaps he hates every minute of it, but cannot help doing it. For the present purpose, our ignorance of the rat's feelings is no obstacle, since the experience-producing function of the activity is served regardless of its hedonic tone. Much human behavior is presumably influenced by desire to master a skill or achieve a goal; but this is not what is meant by EPD. Such goals are probably determined largely by social factors, and are only very indirectly influenced by innate mechanisms. It may be questioned whether mastery or achievement motives are very effective in producing relevant experience, however much effect they may have on the individual's life, in other ways.

The Environment

An organism's experience is considered to be a joint function of its experience-producing drives, and the environment in which these drives operate. Two rats with very different amounts of exploratory drive may accumulate very different amounts of experience if they are both free to exploit a large and very varied environment, but not if they are both confined to small, bare cages.

LEARNED ABILITIES

The third step in the argument contends that when drives and environment interact, the resulting experience produces ability. In a sense this point may be obvious, or simply a matter of the definitions of "experience" and "ability." However, there may be some question as to whether the abilities produced by this kind of experience have anything to do with intelligence. One example may suffice to clarify the point. Birch (1945) studied the ability of six young chimpanzees to use sticks as tools for raking in pieces of food. In the initial test they performed miserably, showing little or none of the insightful behavior that earlier studies might lead one to expect. At this point, *E* may have suspected that chimpanzees were, after all, quite stupid. Birch then left a number of sticks in his subjects' cage for the next three days. It happens that chimpanzees have a strong EPD of a type which leads them to manipulate sticks. (In plain English, chimpanzees like to play with sticks.) After three days of exposure to a stick-containing environment, the chimpanzees were retested on the original problem. They all solved it quickly and efficiently, with the kind of behavior responsible for the chimpanzee's reputation as an intelligent species.

THE NATURE OF INTELLECT

Granting that intrinsically-motivated activity may contribute to the development of rather impressive abilities, one may still reject the fourth step in the argument, which contends that the characteristic known as intelligence is *nothing more* than a collection of acquired abilities. This is, perhaps, the most unorthodox and questionable claim, and probably requires the most attention. It would be easier to defend a more moderate version, contending only that acquired abilities constitute a large and important part of intelligence. However, it seems worthwhile to attempt the larger task of showing that *all* of the phenomena subsumed under the term *intelligence* can be understood as results of intrinsically motivated learning. This will involve the examination of some alternative conceptions of the nature of intellect.

Higher Mental Functions

It has long been customary to speak of an hierarchy of mental functions, and the higher members of the hierarchy have traditionally been considered more relevant to intelligence than the lower. Thorndike, *et al.* (1926) noted that the "standard orthodox view" divided intellect

> into a lower half, mere connection-forming or the association of ideas, which acquires information and specialized habits of thinking, and a higher half characterized by abstraction, generalization, the perception and use of relations and the selection and control of habits in inference or reasoning, and ability to manage novel or original tasks (p. 414).

The orthodox view of the physiological basis of intellect held that the lower

> *functions depend on selective conduction at synapses* but that the higher processes depend on something quite different. There would be little agreement as to what this something was, indeed little effort to think or imagine what it could be, but there would be much confidence that it was *not* the mechanism of *habit formation* (p. 414).

They also observed that "'being able to do more things than someone else' is not so clearly and surely having more intelligence as 'being able to do harder things'" (p. 24). "Common sense recognizes the greater importance of altitude. It rates a Pasteur far above the most widely competent general practitioner" (p. 35). Furthermore,

> it seems, at least, much easier for a good home or school to increase the number of easy things which a child can do than to enable him to do harder things than he has ever done (p. 458).

(Thorndike found this orthodox view untenable, and offered a very different alternative, which will be discussed later.)

Higher mental functions have been invoked by some comparative psychologists to account for the large behavioral differences among species, and in this field there has been more effort to imagine what the physiological mechanisms might be. In 1943 Morgan expressed the then-current opinion that

> new psychological capacities are created by the differentiation and elaboration of the cerebral cortex. . . . The capacity to form concepts, to organize the elements of a sequence of adjustments, to solve problems requiring central symbolic or 'recall' processes—all these mark the later stages of mammalian development, and in some cases it is rather clear that the frontal lobe is basic to the capacity (pp. 120-121).

It must be emphasized that, although a word like, e.g., "abstraction" could be used in a strictly descriptive sense, to indicate certain characteristics of a specific bit of behavior, it implies much more in the context of "higher mental functions." Such a function has commonly been conceived of as a genetically determined brain power, which the individual has more or less of, and which he can use in all appropriate situations. If Smith deals with words more abstractly than Jones does, the same relationship should hold when they deal with geometric designs or numbers.

This conceptualization of intellect was not derived from mathematical analysis of objective measurements of behavior. Spearman (1927) has discussed its prescientific origins and observed that it is "nothing less than an intensification of that old 'faculty-psychology' from which we so often congratulate ourselves upon having nowadays become emancipated" (p. 28).

Despite its intuitive or introspective origins, the concept of higher mental functions may yet be incorporated into an objective science of behavior—if it meets certain requirements. An obvious first step would be, as Spearman and Jones (1950) put it, "to determine ranges of functional unity. By such a range is meant the sphere within which the ability at issue tends to vary concurrently from one person to another" (p. 5). This is, of course, precisely the kind of information factor analysis is meant to provide. There has been considerable disagreement as to how factors should be extracted from test data, and how the results should be interpreted (e.g. Loevinger, 1951), but one point seems clear: Factor analysis does not confirm the existence of the traditional faculties or higher mental functions. Wechsler (1958) has suggested that one of the more "revolutionary discoveries" in this field is that factors "do not coincide with the historic attributes of intelligence" (p. 4). Lashley (1958) observed that the tasks which characterize different primary abilities "seem to involve the same sort of logical manipulations" and "differ only with respect to the materials manipulated (visual, somesthetic, verbal)" (p. 10).

Factor analysis has dealt primarily with human intellect, and the higher mental functions should, if they have any reality, appear more clearly in the behavioral differences among species with widely differing brain structures. The literature of two or three decades ago appears to confirm this expectation, but today's laboratory animals are displaying heights of mental function that put their forebears to shame. The change is due primarily to recognition of the role of experience in developing intellectual skills. Perhaps the best-known example of this is Harlow's (1949) work on discrimination learning sets in monkeys. However, it must be emphasized that the discrimination learning set is just one instance of something much more general.

The significance for intelligence theory of experience-produced abilities lies in their transfer potential (Hebb, 1949, p. 109; McGeoch, 1942, Ch. 10). Thus, experience with visual discrimination problems has not only enabled laboratory primates to solve such problems rapidly, but has also permitted them to solve non-spatial delayed response problems (Harlow, 1944; Hayes & Thompson, 1953). It was formerly supposed that non-spatial delayed response required special "symbolic processes" which were not provided for in the brains of sub-human primates (e.g., Yerkes, 1943, p. 188).

It might be said that there is still a big difference between the intellectual behavior of rats and monkeys. But it should be noted that the rat's sensory and motor equipment and limited life span prevent its having the kind and amount of experience a monkey has. Although rats have not rivaled the impressive performances of primates, it has been shown that even their modest intellects are enhanced by experience (Forgays & Forgays, 1952; Forgus, 1954; Hebb, 1949; Hymovitch, 1952).

There seems to be a trend away from the concept of higher mental functions in behavior theory. In 1949, although Lashley felt that the "learning ability of higher animals involves a perception of relations which is beyond the capacity of the lower" (p. 40), he warned that what appear to be inabilities of an animal may actually reflect the ineptness of an experimenter (pp. 28-29). He also questioned the common belief that the relative amount of association cortex has increased in the course of evolution, and noted that various radical changes in the organization of the nervous system have not been accompanied by striking changes in behavioral capacity (pp. 32-33). In 1958 he suggested that

> the general principles of logical structure, the formation and manipulation of abstractions, are a general property of neural organization . . . . The number of kinds of logical relations with which the mind deals is not great; few symbols are employed in symbolic logic. Instances of animal behavior fitting every logical symbol can be found. The limitations of animal intelligence are set by the nature and complexity of the materials dealt with, not by fundamental logical relations (pp. 10-11).

Harlow (1958) saw no fundamental difference between discrimination learning and the oddity problem, except for additional error factors, and "would be very much surprised if there is any fundamental difference in the learning of the oddity problem and the learning of differential equations—other than that of complexity" (p. 288).

Aside from the lack of supporting evidence, the concept of higher mental functions has been chronically afflicted with a much more basic weakness: The presumed functions have not been clearly and consistently defined (which may be understandable if it is recalled that these concepts originated long before operationism). The "central symbolic process," for instance, has not been clearly differentiated from an ordinary engram, or memory trace, except with regard to when the word is used. If a monkey remembers something, he does it with an engram, but when he fails to remember, it is because he lacks central symbolic processes.

Dealing with materials abstractly has been considered a higher function than dealing with the same materials concretely; but there has been much confusion as to just how abstract a given sample of behavior really is. In discussing Goldstein's concept of abstract attitude (as well as Brickner's "power of synthesis" and Watts' "foresight"), Hebb (1945) objected that such concepts

> explain at once too much and too little. They are too broad and ill defined to be meaningful . . . . It is too easy to think of all the facts they account for and not to ask whether there are others which they do not explain or whether symptoms are absent which they require . . . . Behavior that seems to involve abstraction is called concrete and vice versa . . . . One must object on logical grounds to Goldstein's emphasis on cases which support his interpretations and his disregard of those which are opposed (pp. 12-13).

Similar objections have been expressed by O'Connor (1959a), Sarason (1959, pp. 41-42, 52-58, 116-133), and Shaffer (1952, p. 32). Goldstein's use of "abstraction" certainly involves some rather special shades of meaning. For instance: any "kind of seeming abstraction which does not involve *conscious will* is not abstraction at all" (Goldstein & Scheerer, 1941, p. 23).

Guilford (1960) asked:

> What are the phenomena that exist in the domain of human thinking? What are the different kinds of operations or processes? Much experimentation has proceeded on the basis that we know what the fundamental kinds of operations are, too often without questioning whether we could not have better conceptions (pp. 6-7).

He listed problem solving, reasoning, deduction, and induction among the terms that have never been adequately defined as psychological concepts (pp. 12, 14).

Cattell (1957) insisted that the operations or processes or variables used in the study of intellectual function should be unitary.

> If it be asked what most commonly justifies the use of the term *a unity*—in psychology or anywhere else—the operational answer appears to be that *a set of manifestations all suffer the same fate*. . . . they grow together, or react similarly to a reagent, or decline together, or share organic reactivity such that something done to one affects all of them. . . . [But] it is usually found that a mere lip service to the principle has prevailed. At worst, the experimenter has taken for granted that because he has a unitary concept in his mind there is a corresponding unity in nature. At best, the unitariness is found to rest on clinical impression, based on the unaided memory of the observer concerning cases and functions seen. . . . the experimenter repeats the old, seemingly ineradicable error of faculty psychology of assuming that a single word means a single function . . . or, worst of all, the unity of the concept stems more from introspection than from observation (pp. 12-13).

It is interesting that the concept of higher mental functions has persisted in psychological theory despite the lack of supporting evidence. As Spearman and Jones (1950) said of faculties,

> times without number they have been submitted to the most scathing criticism, and even ridicule. And yet they still appear to dominate the field, if not explicitly, at least implicitly (p. 4).

Perhaps this is simply an example of the general rule that inadequate theories are not abandoned until better theories are available to replace them. But replacements are becoming available now. For instance, Hebb's (1949) theory explains complex behavior as the end result of a large amount of learning, with simple skills serving as the raw material for more sophisticated achievements. His conception of brain function, emphasizing autonomous activity, provides a plausible mechanism for thought. Although Hebb discusses intelligence at length, his theory makes no use of "higher mental functions." Complex neural functions develop naturally from the basic concepts of cell assembly and phase sequence, without the postulation of special kinds of neural structure. This treatment removes both the "higher" and the "mental" from such functions, leaving insight and abstraction, for instance, as neural processes which may or may not occur in either a rat or a man, depending on the relationship between previous learning and the present situation. Hebb does postulate an hereditary capacity for intellectual development, but this is the same capacity that provides for simple learning—not an additional, special mechanism.

In summary, there appears to be very little evidence to support the intuitively attractive notion that the higher levels of intellectual activity depend on special kinds of innate capacity. The trend of opinion seems to be toward something more like the view expressed by Thorndike, *et al.* (1926):

> In their deeper nature the higher forms of intellectual operation are identical with mere association or connection forming, depending upon the same sort of physiological connections but requiring *many more of them* (p. 415).

It seems, in short, that higher mental functions are pure concepts, which have no counterparts in the real world of behaving organisms.

Learning Capacity

If intelligence consists entirely of learned skills, how can differences in intelligence be ascribed to heredity? A common but unacceptable answer has been that the individual inherits a brain with more or less learning capacity. Lashley (1949), discussing the evolution of intelligence, noted that

> intelligence is usually defined as the capacity to profit by experience, or the capacity to learn. . . . [but] under favorable conditions every animal, at least above the level of the worms, can form a simple association in a single trial. In this sense the capacity to learn was perfected early and has changed little in the course of evolution (p. 30).

Similar views have been expressed by Anastasi (1950, p. 75), Fuller and Scott (1954), Guilford (1961, p. 15), Harlow (1958, p. 288), Hebb (1949, p. 115), Morgan (1943, p. 108), and O'Connor (1959b, p. 175).

The idea that all animals have equal learning capacity may, at first glance, appear to contradict common sense. However, it must be emphasized that this discussion is not concerned with the net learning performance of whole organisms, but rather with the abilities of brains to form new engrams (or memory traces, or pathways, or whatever term may be preferred). In order to demonstrate a difference in sheer learning capacity between, e.g., monkey and rat, it would be necessary to show that any observed difference in performance was not caused by differences in sensory or motor capacity, motivational or emotional factors, or transfer from earlier learning.

This last requirement—ensuring that both learners begin at the same starting point—would be extremely difficult to satisfy, and it may, for all practical purposes, be considered impossible. It would, of course, be completely inadequate to consider previous experience equated simply because the two animals were reared in the same kind of cage and provided with the same amount of formal training, or lack of it. Differences in EPDs would certainly upset such a superficial balance.

If differences in learning capacity cannot be demonstrated, neither can it be shown conclusively that such differences are absent. The equality of learning capacity must be considered an assumption, rather than an established fact. As an assumption, however, it has the virtue of simplicity, at least, and it does not conflict with known facts. Furthermore, the basic point of the main argument would not be affected by modifying the assumption to state that differences in sheer learning capacity are not great enough to account for the observed range of intellectual abilities.

Memory Capacity

Even if brains do not differ in the ability to put a given item of information into storage, there may still be differences in the number of such items that can simultaneously be kept in storage. A very simple nervous system, after learning a few items quite readily, might learn additional items only at the expense of forgetting the earlier ones. When Thorndike, *et al.* (1926) suggested that the higher forms of intellectual function depend on the same mechanisms as simple association, but *many more of them,* they were, in effect, equating intellectual capacity with memory capacity—the number of engrams, or amount of information, that can be stored simultaneously, and recalled when needed. In a sense, they were certainly right: Memory capacity obviously sets an upper limit on the organism's repertoire of intellectual skills. Furthermore, comparative psychology and anatomy appear to support this notion: The species that are considered more intelligent tend, in general, to have larger brains. Within the human species, brain size does not correlate at all well with intelligence; however, brain size is, at best, a rough measure of memory capacity. Perhaps the number of neurons would give a higher correlation—especially if one knew which cells to include in the count and which to omit. (The common assumption that only cortical cells contribute to memory is surely an oversimplification; and some cortical cells may be reserved exclusively for sensory or motor functions.) However, cell counts would still be a very crude index of memory capacity, which may be, for instance, a nonlinear function of the number of potential connections among cells. Our ignorance of the memory mechanism precludes any serious attempt to clarify this point at present.

Although it is obvious that memory capacity sets an upper limit on the individual's store of learned facts and skills, it is not obvious that this limit is ordinarily reached by most individuals. Thorndike's hypothesis seems to imply that information goes into storage at the same rate in all brains. If this is true, differences in memory capacity must be reflected in eventual differences in information content or knowledge. However, it seems clear that there are differences in the rates at which various species and individuals put information into storage. Although it cannot be assumed that this is due to differences in sheer learning capacity, it can be ascribed to differences in the amount of time devoted to learning. Actually, Thorndike, *et al.* did not overlook this factor, but apparently considered it of minor importance. They mentioned, in a footnote, that "certain other inner conditions, such as the strength of curiosity, the satisfyingness of thought for thought's sake, and the appeal of non-intellectual activities . . . would have to be allowed for" (1926, p. 421).

EFFECTS OF BRAIN DAMAGE

If the brain is the organ of intellect, the effects of damage to that organ should reveal something of the nature of the intellectual functions. At one time it seemed quite obvious to many authorities that this type of data supported the concept of higher mental functions. Such functions appeared to depend on structures which were found primarily (or even exclusively) in the frontal cortex of the human brain. Extensive damage to these structures was thought to have drastic effects on foresight, planning, symbolic processes, abstraction, "and all that is good and great about man" (Hebb, 1958, p. 84), without corresponding effects on the "lower" functions of learning, memory, and drive. However, it now appears that this interpretation was built on a foundation consisting largely of inadequate anatomical and pathological data, insufficient or inappropriate normal control data, and debatable interpretation of behavior (Hebb, 1945). Later and more satisfactory investigations indicate that the "frontal association area . . . appears to play no essential part in man's normal behavior" (Hebb, 1958, p. 84). In summarizing the Columbia-Greystone studies, Landis (1952) reported that the traditional defects were observed in some cases, but that they were mild and temporary, and "they are altogether unimpressive, and . . . are not primarily connected with frontal lobe tissue" (p. 263). Teuber (1952) observed, more generally, that

> the one definite result of current work, on monkey or man, is essentially negative: we are agreed that functions are not lost according to preexisting logical categories, as if the organism would fall apart according to the subdivisions of our textbooks (pp. 259-260).

This is not to say that brain damage has no effects on intelligence, but rather that the effects are, from the traditional point of view, puzzling, and often surprisingly slight. Hebb (1949) has shown that these effects can be better understood if intelligence is seen as an accumulation of acquired abilities. Perhaps a still better understanding may be possible if the acquisition of these abilities is seen as dependent on EPDs. It is well known that small brain lesions may have drastic effects on hunger, thirst (e.g., Morrison & Mayer, 1957), fear (e.g., Kluever & Bucy, 1939; Weiskrantz, 1956), and sex drives (e.g., Kluever & Bucy, 1939; Phoenix, 1961; Wood, 1958). It would not be surprising if EPDs were also found to be quite sensitive to brain damage.

Effects of Brain Damage on EPDs

There has been little investigation of the effect of lesions on the more generally recognized and specific EPDs. However, a great deal of work has been done on general activity level which, although loosely defined, may be thought of as reflecting one or more drives which are experience-producing in a very basic sense. An animal must be awake and alert if it is to accumulate any experience, and some degree of overt activity is essential to many kinds of experience.

It has been known since 1890 that hypothalamic lesions may produce a condition of more or less constant sleep. The early literature on this effect has been reviewed by Nauta (1946). The neural structures involved are now commonly thought of as part of the reticular activating system, which is very generally concerned with the regulation of alertness (Magoun, 1950). Much of the work on this part of the brain has been concerned with damage so severe as to leave *S* permanently comatose, so the question of intellectual effects does not arise; however, milder damage of the same kind leaves the subject drowsy and relatively inactive (Ranson, 1939).

It has been known since 1876 that lesions of the prefrontal cortex may cause hypermotility, and lesions in the head of the caudate nucleus have been found to have the same effect. The earlier literature has been reviewed by Reed (1947, pp. 404-406). Although the activity which increases has often been considered "spontaneous," there is some evidence that the experimental animal is hyper-reactive, especially to relatively novel stimuli (e.g., French & Harlow, 1955). In this case, it might seem reasonable to speak of an increase in exploratory drive; however, the extra activity does not seem to be of a typical exploratory character. The prefrontal monkey, when placed in a strange cage, paces about in a highly stereotyped manner, paying no apparent attention to the details of the novel environment. Whether the extra activity be considered spontaneous or responsive, it appears to decrease rather than increase the probability of learning. Dean and Davis (1959, p. 533) observed that "the normal spontaneity and explorative interest in surroundings was greatly reduced by caudate surgery" which increased the locomotor activity of their monkeys.

In man, emotional-motivational changes are commonly observed after frontal lobe damage. In reviewing the literature, Rylander (1939, p. 16) cited such reports as early as 1887. In his own study, 14 out of 32 patients displayed "restlessness": they

> feel a constant desire of activity; they keep walking around . . . desire that everything be done quickly; they bustle about and rush with their work, even when there is no necessity (pp. 265-267).
>
> Several of the patients appear to have acquired an exceptional endurance. . . . Some of them also seem to require remarkably little sleep (p. 290).

This added energy was not necessarily advantageous, however, since some of these same patients displayed a loss of initiative and interest in their normal activities.

Kluever and Bucy (1939) found that temporal lobectomy caused a dramatic increase in the monkey's tendency to examine its environment. This might have the effect of facilitating spontaneous learning. However, the change was apparently not so much an increase in curiosity as a decrease in timidity. The monkeys had lost their normal caution about strange objects, and Kluever and Bucy doubted if such animals could have survived long in a normally hazardous environment. Butler and Harlow (1954) have presented data which suggest that extirpation of temporal neocortex may depress the monkey's visual exploratory drive; however, these authors noted that individual differences made firm conclusions impossible.

Davis, Settlage, and Harlow (1950) found that monkeys with various cortical lesions showed a reduced tendency to manipulate mechanical puzzles.

Krechevsky (1937) found that normal rats, allowed to choose either a constant or a variable path to food, took the variable path about half the time; but rats with lesions in various parts of the dorsal neocortex tended to avoid the variable path, although it was no longer, and involved no added choice points.

The Soviet concept of the orienting reflex corresponds closely to exploratory tendency, and Razran (1961, p. 115) has reported that recent Soviet work shows that the orienting reflex is disrupted by brain injury.

Intellectual Effects of Early Brain Damage

If brain damage altered the EPDs of a very young individual, the resulting lack of normal experience could interfere seriously with intellectual development and eventual adult intelligence. The same damage to an adult brain would be too late to prevent experiential development, and might thus have less effect on intelligence. However, the situation is complicated in practice by the effects of damage to structures which serve functions other than EPDs. The literature indicates that early damage to sensory and motor mechanisms is followed by much more rapid and complete recovery than is damage to the same structures in the adult brain (Kennard, 1938; Benjamin & Thompson, 1959).

Penfield and Roberts (1959, pp. 184, 198, 204, 244) have reported that injury to the speech areas

> in early childhood may produce a complete aphasia. This is followed by a period of silence, to be succeeded in time by complete relearning of language. The ability of an adult to relearn speech after injury is much inferior to that of a child (p. 244).

Removal of the dorsolateral prefrontal cortex in adult monkeys causes a profound loss on the delayed response test. However, Akert, Orth, Harlow, and Schlitz (1960) performed this operation on two-day-old monkeys, and found no deficit whatever when the monkeys were tested on delayed response six months later.

Tsang (1937) reported that cortical lesions in 22-day-old rats had relatively little effect on their ability to learn a maze three months later. (Unfortunately, this study was not well controlled. Both the normal and late-damage data were taken from another experiment done several years earlier.) Tsang's work, though interesting in its own right, does not provide a good analog for the development and measurement of intelligence in man. Laboratory rats are reared in cages which provide extremely little opportunity for learning; and a maze-learning task, unlike most intelligence tests, permits very little use of previously acquired skills and information. Lansdell (1953) reported an experiment which differed from Tsang's in that rats were reared in a more complex environment; and their intellectual development was evaluated with the Hebb-Williams test, which presumably permits substantial transfer from previous learning, although it does not permit such direct application of previously learned information as the Binet does. Lansdell found a smaller difference than Tsang had reported, but the late-damaged rats were, again, more retarded. (This raises the question of whether the difference might be reversed if rats were reared for a longer time, in a still more enriched environment, and tested in situations permitting direct use of acquired abilities.)

Hebb (1942; 1949, pp. 289-294) has suggested that, in contrast with the preceding reports, damage to the brain of the human infant may have much more serious effects on subsequent IQ than the same damage would have if it occurred later. However, as Hebb noted, the evidence leaves much to be desired. Perinatal brain damage does not ordinarily take the form of gunshot wounds, prefrontal lobotomies, or surgical removal of neoplasms. It is typically caused by infectious disease or metabolic disturbance, and may be scattered throughout the brain. Such damage is not observed directly, but is inferred from its later neurological and behavioral effects; and there are no satisfactory data on the nature, location, or extent of the damage. Early brain damage of *some* sort can, undoubtedly, cause severe mental deficiency; however, it is quite conceivable that the *same* damage to the adult brain would be so disabling that the question of intellectual effects, *per se,* would not arise. Sensory, motor, and language deficits (from which an infant would soon recover) might leave the adult victim permanently untestable, uneducable, and unemployable.

Belmont and Birch (1960) found less deficit on the marble board test in brain-damaged adults than Werner and Strauss had reported for brain-damaged children. However, there is no indication whatever that the adult damage was comparable to the early damage in either locus or extent; and it may be significant that the adult *S*s displayed none of the motivational disturbances (hyperactivity and distractability) that are commonly seen in brain-damaged children.

There seems to be no compelling evidence that a given kind and amount of brain damage is more intellectually disabling if it occurs early. On the other hand, it cannot be denied that early damage of *some* kind and amount can have crippling effects on intellectual development, and the question remains: How is this effect produced? Hebb's interpretation (1949, p. 293) emphasizes the loss of tissue in which conceptual development would normally occur. However, such development is assumed to consist in the establishment of orderly relationships among cells whose interconnections are originally random; and it may be objected that one mass of randomly connected cells should serve the purpose as well as another. The data on early brain damage in experimental animals show that this kind of equipotentiality does exist, at least for some functions.

The EPD interpretation, in contrast, emphasizes loss of tissue which would normally contribute to development by virtue of its intrinsic organization. If an innate neural mechanism which promotes exploratory behavior is destroyed, its function cannot, presumably, be taken over by a mass of randomly interconnected cells. Unfortunately, there is very little evidence directly relevant to this point. There have been only a few experiments involving early brain lesions in animals, and these have not been concerned with motivation, nor with lesions outside the neocortex. However, Tsang (1937, pp. 232-233) observed that his rats with early damage to the frontal cortex were still unusually "restless" three months later. The lasting motivational-emotional effects of early brain damage in man are, of course, well known (e.g., Strauss & Lehtinen, 1947) at the clinical level, although they have not been thoroughly measured, analyzed, and related to specific lesions.

Intellectual Effects of Late Brain Damage

Damage confined to the EPD mechanisms of the adult brain would have no effect on intellectual skills and information which had already been acquired. However, even such restricted damage could still affect the individual's intrinsically motivated behavior; and a reduced tendency to engage in certain activities would, in some situations, have the same net effect as reduced ability. There is some evidence that the intellectual effects of adult brain damage are, to a considerable extent at least, due to altered motivational mechanisms, rather than to reduction in capacity to learn, remember, perceive, abstract, etc.

Damage to the adult human brain is followed by a characteristic pattern of intelligence test performance. As Hebb (1949, pp. 289-294) has emphasized, there tends to be little or no loss on "Binet type" test items, which directly assess *S*'s vocabulary, or his store of other learned information—and this is precisely the kind of item which has generally proved to be reliable and valid, as well as convenient (Hebb, 1949, pp. 290-291; Wechsler, 1958, p. 65; Terman & Merrill, 1937, pp. 5, 7). Cattell (1957, pp. 618-620, 871-880) has suggested that such items represent a second-order factor, which he calls "crystallized general ability," in contrast with "fluid general ability," which is represented by test items that are more sensitive to brain damage.

Weisenburg and McBride (1935) found that patients with damage in the non-dominant hemisphere did poorly on the Porteus Maze, and on a test which required them to fill in the missing words in sentences. Prefrontal lobotomy and topectomy are followed by a temporary drop in performance on the Porteus Maze (King, 1949, pp. 195-200; Porteus, 1950, Ch. 7). The profile of Wechsler subtests has often been used in evaluating the effects of brain damage, and the Digit Symbol substitution test has commonly shown the greatest deficit (King, 1949, pp. 188-195; Klebanoff, Singer, & Wilensky, 1954; Morrow & Mark, 1955; Shaffer, 1952).

These damage-sensitive tasks are quite varied in form and content, but they have at least one common feature—novelty. They all require the testee to perceive an unfamiliar stimulus complex, and to produce a response which he has not produced before. As Hebb (1949, p. 292) said, both new perceptions and new responses require more effort (stronger motivation) than is needed for mere recitation of old information in response to familiar stimuli. This difference would be of no consequence if the testee's performance were extrinsically motivated, and if the social or other presumed motive were independent of brain damage. But if it is assumed that test-taking behavior, like much other behavior, is motivated largely by the attractiveness of the activity itself, it is apparent that those test items which depend heavily on curiosity, exploratory drive, or the like, will show greater effects of damage to the relevant motivational mechanisms.

Although Hebb mentioned the greater motivational demands of tasks which are sensitive to brain damage, he also characterized these tasks as "puzzle-like," and as having to be "worked out logically;" and he put greater emphasis on this feature. The Porteus Maze is undoubtedly puzzle-like; however, the Digit Symbol Test appears to require very little in the way of logic, or discovery of a solution; rather, it is a well-defined, routine task which would not be particularly difficult were it not for the short time limit specified. Although it could, conceivably, serve as a measure of learning, Luchins and Luchins (1953) have shown that it does not function this way in practice. Wechsler (1958) observed that some neurotic individuals do poorly on the Digit Symbol Test "because they have difficulty in concentrating and applying themselves for any length of time and because of their emotional reactivity to any task requiring persistent effort" (pp. 81-82).

Tomkins (1952, pp. 87-89) has emphasized the motivational factor, in a slightly different way, in explaining why performance on Raven's Progressive Matrices declines with ageing so much faster than performance on a vocabulary test.

Although there is increasing recognition of the role of intrinsic motivation in many kinds of behavior, psychology has traditionally emphasized the more obvious extrinsic motives, and this viewpoint still prevails in the interpretation of test results. In testing animals, the traditional view might seem especially appropriate: A rat has been on a deprivation schedule for two weeks, has lost weight, and has had no food for the past 24 hours; therefore his behavior must be dominated by the hunger drive. But anyone who has tested more than a few rats must have suspected, at times, that certain actions were really motivated by the animal's exploratory, or fearful attitude toward the novel test situation.

If this suspicion is correct, it necessarily follows that test performance may be altered by a brain lesion, even though the only effect of the lesion is a change in exploratory or avoidance tendencies. Thomas, Moore, Harvey, and Hunt (1959) have provided an example in the case of maze learning by rats with septal lesions:

> Excessive time on the first trial was caused by a peculiar hyperactivity on the part of the more irritable septal *Ss*; they showed long periods of intense exploratory sniffing about over the floor and walls of the maze, interrupted by periodic bursts of energetic running back and forth in the same alley. This type of behavior gradually decreased as training progressed (p. 531).

Similarly, Kluever and Bucy (1939, pp. 986-987) observed that temporal lobectomized monkeys were exceptionally prone to pick up and examine discrimination objects before taking the food reward—a tendency which could produce a spurious impression of incorrect choice. Peretz (1960) found that rats with lesions in the anterior cingulate cortex were retarded in learning to avoid noxious stimuli. This result, by itself, might suggest that learning capacity, or some other strictly intellective function, was impaired. However, these same rats, when working for food reward, learned a visual discrimination problem more quickly than normal controls; and other studies have indicated that cingulate lesions reduce fearfulness (e.g., Ward, 1948). Instances where frontal hypermotility disturbed normal test behavior have been described by French (1959) and by Maher and McIntire (1960).

Human *Ss* are commonly tested without benefit of any material incentive, and the remarkable fact that this is possible is commonly explained by reference to a presumed secondary social motive derived from early association of people with material rewards. (Where testing is clearly related to vocational, educational, or therapeutic procedures, the testee may hope to profit by his efforts; but most research appears to depend on *Ss* who take the test simply because they are asked to.) Nevertheless, some appreciation of intrinsic motivation is revealed by the fact that test constructors often assure prospective users that the materials and procedures are interesting and attractive to normal *Ss*. However, such tests are not guaranteed to appeal to abnormal *Ss*. As Shaffer (1952) has emphasized, "a patient's score is determined not only by what he knows and can do, but also by his interest, docility, persistence, and zest to succeed" (p. 30); and the data suggest that these motivational factors may be very sensitive to brain damage.

SYNTHETIC BRAINS

Although the study of real brains has provided much information on the molar aspects of intellectual function, the techniques of surgery, histology, etc., are as yet too crude to tell much about the detailed mechanisms. We still have no direct knowledge of the neurophysiological nature of motivation, learning, memory, or perception—to say nothing of abstraction or the eduction of relations. These gaps in our picture of neural function can be filled, tentatively, either by developing "conceptual nervous systems" or by the study of inorganic mechanisms with functions similar to those which are called intelligent when they occur in living organisms. The former approach is exemplified by Hebb's theory. The latter approach is becoming increasingly interesting because of current rapid progress in the design of electronic, information-handling machines. Since these devices are man-made, their components and the connections among them are known; and if there is any doubt about the details of the machine's operation, its parts, unlike neurons, are sufficiently few, large, rugged, and accessible to permit any desired measurements. Although it is conceivable that organic and electronic "brains" may achieve the same results by very different means, certain features of the "inorganic psychology" of computers may nevertheless provide helpful hints for the theory of organic intelligence.

An important part of computer function is the internal manipulation of information (analogous to thought or reasoning) which is performed by circuits called "logic units." From the orthodox point of view, it might be supposed that a computer's "intelligence" would be limited by the *kind* of logic units it contained; but the fact is that logic units are quite simple, and only a few kinds are needed in the most powerful computers (General Electric Co., 1960, pp. 125-135; von Neumann, 1956, pp. 46-61). The "innate intellectual capacity" of such a machine is largely a function of its information storage capacity (roughly analogous to memory); and its "manifest intelligence" is largely a function of the "program" (analogous to education) which has been fed into it.

It would be interesting to apply the concept of EPDs to computer function. These machines display the same characteristics that lead us to postulate motives in living organisms: they are more or less active, and their activity has a certain direction at a certain time. But although this "teleological" aspect of machine function has been discussed occasionally (Ashby, 1960; Miller, Galanter, & Pribram, 1960, p. 42; Taylor, 1960), the people who design and use computers make little use of motivational concepts. Engineers do not design motivation circuits, and operators do not adjust knobs labelled "drive strength." The reason appears to be that since computers are built by man for his own use they have a characteristic appropriate to the ideal slave: their motives are precisely matched to the job at hand and thus do not appear as separate variables. A computer may be thought of as motivated to store the program presented to it, just as a student is motivated to study his textbook; but computers, unlike students, are invariably motivated, to the optimum degree, to "learn" the whole program and nothing else.

A computer's "experience" is normally restricted to the "formal education" provided by its program; however, in certain experimental applications (e.g., Newell, Shaw, & Simon, 1958; Samuel, 1959) computers have accumulated additional, more informal experience "on the job," and the motivational aspect of such behavior is more closely analogous to organic EPDs. If a computer were arranged to operate entirely on the basis of informal experience, it would provide a much closer analog to organic intelligence (Turing, 1956); but computers are not ordinarily so arranged, for the simple reason that the people who pay for million-dollar machines are looking for something *other* than organic intelligence. Although computers are "almost human" in some ways, their behavior is, in other respects, quite unlike that of man or any other intelligent animal. As Selfridge and Neisser (1960) have remarked,

> the intelligence implied by . . . [computers'] activities has an elusive, unnatural quality. It is not based on an orderly development of cognitive skills. In particular, the machines are not well equipped to select from their environment the things, or the relations, they are going to think about (p. 60).

The machines' deficiencies in this regard include sensory, motor, and motivational features. The typical computer's sensory activity is limited essentially to reading magnetic tapes, and its effector activity to recording on other magnetic tapes. This restricts its range of exploratory behavior rather severely, of course; but it could still choose to read some tapes thoroughly and skim others, *if* such an experience-selecting, motivational characteristic were designed into it.

In short, the machine's utility, as well as its difference from a living organism, seems to depend largely on the fact that its peculiar EPDs restrict it to accepting the information given it.

LANGUAGE, CULTURE, AND HUMAN INTELLIGENCE

It is sometimes said that man's intelligence is not merely greater than that of other species, but qualitatively different—or even that man is the only animal that really has intelligence. Aside from such extreme views, it is generally agreed that man has a very good lead over his closest competitors. Most authors consider it more than coincidental that man is also unusual in his use of language; but there is some question as to what causal relationship accounts for the occurrence of these two distinctions in the same species.

It might be supposed that man's linguistic ability is a result of his superior intelligence (though this would be a vague hypothesis with the meaning of "intelligence" as unsettled as it is). However, language is certainly not man's most intellectually demanding achievement. On the contrary, people with MAs of two years commonly use language more effectively than do the members of any other species.

Language as a Thinking Tool

The Watsonian hypothesis.—A more promising hypothesis holds that man's intellectual status is due largely to his linguistic ability. A once-popular form of this hypothesis supposed that thought was an essentially linguistic phenomenon.

This conception was very useful in the framework of early behaviorism: "Thinking" could be de-mentalized by assuming that it consisted of nothing more than covert speech. An animal without language would, of course, be unable to "think" in precisely this sense. However, it has become increasingly obvious that even non-linguistic species display behavior which requires the postulation of something in the nature of "thinking"—though this term is still commonly avoided, and many authors prefer to discuss "mediational," "representational," or even "symbolic" processes. The history of this approach has been discussed by, e.g., Goss (1961) and Osgood (1954, pp. 392-412, 648-665).

Early conceptions of neural function could not readily accomodate mediational processes in the brain, so peripheral stimulus-producing responses were postulated; but newer conceptual nervous systems provide autonomous central processes. In introducing such a theory, Craik (1943) said:

> I have not committed myself to a definite picture of the mechanisms of synaptic resistance, facilitation, etc.; but I have tried . . . to indicate what I suspect to be the fundamental feature of neural machinery—its power to parallel or model external events—and have emphasized the fundamental role of this process of paralleling in calculating machines (p. 52). My hypothesis then is that thought models, or parallels, reality—that its essential feature is . . . symbolism, and that this symbolism is largely of the same kind as that which is familiar to us in mechanical devices (p. 57).

Hebb (1949) proposed a rather definite mechanism for the thought process; and although it cannot be assumed that Hebb's picture of central autonomous processes is correct in detail, it may well be correct in general outline, and it is certainly plausible.

The term *symbol* seems to have introduced some confusion into discussion of the relationship of speech to thought. In general, a symbol is something that represents something else, but it is hazardous to use the term in its general sense in the present context. A spoken word which represents an object or event is one kind of symbol. A neural process which represents an object or event is another kind of symbol. It will not do to assume that animals which cannot use the former kind are therefore unable to use the latter kind. Hallowell (1950) made this distinction explicit in discussing "the transition from capacities for *intrinsic* representative processes in animals below man, to the creation when we reach the human level of *extrinsic* symbolic systems" (p. 166).

We may now reasonably suppose that an external object (or event) is represented in the brain by a distinctive neural process. If the brain is human, and if the object has a name, this name may be represented by another distinctive neural process. Thinking—the internal manipulation of such neural representations, or analogs of reality—can proceed more directly in terms of processes which represent objects than in terms of processes which represent words which represent objects. It is introspectively obvious that human thinking is commonly *accompanied by* internal language activity; but it does not follow that such activity is *necessary for* human thinking.

The Whorfian hypothesis.—Although the popularity of the "Watsonian hypothesis" seems to be waning, there is growing interest in another possible dependency of thought on language. This conception of "linguistic relativity," developed largely by Whorf (1956), suggests that human thought is significantly influenced by the particular language an individual uses. Thus, a person who speaks only English could not perceive the world in the same way as a person who speaks only Hopi (which is very different from English and other Indo-European languages, in structure as well as vocabulary). This is essentially an extension of a point that semanticists have often warned us about. Language may be used as a model or analog of the physical world; but this is a risky procedure, since words are often very poor models of objects and events. Whorf adds that the general structure of a language may be a very poor model for reality. More specifically, a certain aspect of reality may be expressed more readily in Hopi than in English (for example), while some other feature of the physical word may be better dealt with in English. The Whorfian hypothesis is concerned only with the *relative* facilitation—*or inhibition*—of thought by various specific languages. It implies nothing whatever about the *absolute* effect of any specific language, or of languages in general, and it permits no prediction of the thinking effectiveness of an organism (human or otherwise) that employs *no* language. The research bearing on Whorf's hypothesis has been reviewed by Cofer (1960) and Kendler (1961, pp. 459-460). Results have been rather variable, and when allowance is made for the effects of non-linguistic aspects of cultural differences, there is only moderate support for a mild form of the hypothesis.

Special languages.—The hazards of using ordinary language as a model of reality can be avoided by careful use of such special languages as those of mathematics and symbolic logic; but it may be questioned whether even these function as "thinking tools." If "thinking" refers to processes which occur inside the organism, then mathematical models and symbolic logic are not used as part of the thinking process at all, but serve as additional processes, external to the organism, which supplement the limited powers of thought itself. The value of symbolic logic is only realized when the sheer complexity of a problem is such that the thinker is unable to keep track of all its aspects simultaneously. The numerous objects, events, and relationships are expressed as written symbols, which can be manipulated on a sheet of paper. This external manipulation can be frozen at any stage while the operator checks his work to be sure that none of the numerous elements has been overlooked. In any case, these special languages are rather beside the point. The hypothesis that language facilitates thinking usually refers to ordinary language, of the sort available in preliterate societies; and there appears to be no convincing evidence that such language does, in general, assist the thought process—at least not to an extent that would account for the difference between human and non-human intelligence.

Thought process versus thought content.—Consideration of how well an individual thinks commonly implies that this is a question of the nature of the thinking *process.* It seems to be generally assumed that a "good thinker" makes use of *functions* which are not available to a "poor thinker." However, there is no direct evidence for this assumption, and an alternative interpretation is available. If "thinking" is taken to mean internal manipulation of information, it is immediately apparent that the product of an individual's thinking depends not only on which manipulations he performs, but also on the information he manipulates—which, in most cases, will be largely stored or remembered information. It is not certain that individuals (or even species) differ greatly with regard to the kind of information processing functions they are equipped to use; but there can be no question whatever that they differ radically in the kind and amount of information available for processing. Traditionally, the problem of thought has been concerned with the thought process; but it might be well to explore the implications of a greater emphasis on thought content.

Culture

It seems quite possible that the contribution of language to human ability can be adequately accounted for by its use in the transmission of information. It may be argued that man's really basic distinction is neither language nor intelligence, *per se,* but culture, the growing body of knowledge shared by many individuals and transmitted from one generation to the next as a sort of social inheritance.

The definition of man.—Anthropologists have often encountered the problem of deciding whether an extinct primate was or was not human; and this has forced them to examine the meaning of "human." The present consensus seems to be that the definition of "man" should emphasize functional rather than morphological criteria. Hallowell (1956) observed that

> the use of behavioral or functional criteria such as speech or tools represents, of course, a piecemeal approximation to a categorical distinction which has been current in general anthropology for a long time. It is that man is unique among the Primates, and stands apart from all other animals as well, in possessing culture. In fact . . . the identification of a human level of existence with a cultural mode of adaptation seems to have become a commonplace (p. 89).

Earlier students of human evolution dealt with man's anatomical distinctions —posture, hands, brain size—as features which had to be developed before a human type of behavior could occur. However, evolution operates by trial and error, not by foresight, and these anatomical features would have little or no survival value in a non-cultural primate. Taking culture as the primary adaptive mechanism, "it is probably more correct to think of much of our structure as the result of culture than it is to think of men anatomically like ourselves slowly discovering culture" (Washburn, 1959, p. 21; see also Hallowell, 1956; Oakley, 1957).

A simple culture could probably be transmitted by non-linguistic means of communication (Hayes & Hayes, 1954, pp. 298-301; Kroeber, 1928, p. 340; 1948; p. 223). However, the efficiency and flexibility of language appear to be essential for anything approaching even the simplest of recent human cultures; and Hallowell (1956, p. 93) has suggested the term "protoculture" to distinguish the limited, pre-linguistic variety.

From this point of view, man's intellectual status is due to his culture which, in turn, is made possible by his linguistic ability.

Creativity.—It may be objected that this formulation makes human culture too exclusively a matter of communication. After all, cultural knowledge must be created before it can be transmitted, and it may appear that the creative capacity of the human individual is even more unique and essential than his communicative ability.

However, as Campbell (1960) has recently emphasized, "creative thought" may be understood as a process of "blind variation and selective retention," which requires no unique capacity. Furthermore, it is not at all certain that man is an exceptionally creative animal. To be sure, it was a man and not an ape that invented the vacuum tube (for instance); but it is essential to remember that (a) the overwhelming majority of men did *not* invent the vacuum tube, and (b) none of the men who lived before about 1900 could possibly have invented it, because the necessary preliminary steps had not yet been achieved. Invention of the stone axe did not depend on such an elaborate pre-existing culture, and it may be supposed that early man invented it immediately and easily, as soon as the need arose; but this is pure speculation, and contrary to the evidence of the slow rate of progress of early technology. It is just as reasonable to speculate that one man in a thousand *and* one chimpanzee in a thousand are capable of inventing a stone axe, in the particular circumstances in which this invention occurred. So far as the evidence is concerned, it is entirely possible that one in a thousand chimpanzees *does* invent something useful. We would not expect to learn about such creative activity, since, in a non-cultural setting, the tools or techniques would neither spread through the population nor be passed on to the inventor's offspring.

Kroeber (1917) referred to culture as a "superorganic" process, by which he meant, essentially, that the information contained in a culture is something more than the sum of the contributions that all the past and present members of that culture could have made *as individuals.* In the same vein, L. White (1950) argued against overemphasizing the role of the individual in the cultural process. It is, of course, the individual who thinks, builds, or invents; but insofar as these things are done in a distinctively human fashion, the pre-existing culture determines *what* the individual thinks, builds, or invents.

The cultural mode of adaptation, far from putting a premium on inventiveness or originality, serves exactly the opposite function: it relieves the majority of individuals of the burden of creativity, by giving them all access to the slowly accumulated products of creative acts scattered thinly through the history of the species. Communication makes this possible. As Hooton (1942) put it:

> There comes a time when the perfection and power of tools contrived by a few ingenious brains put within the hands of the most moronic human animals almost unlimited constructive and destructive potentialities. Many of us are almost as incapable of inventing and understanding the scientific contrivances which we use as are chimpanzees. It is probably easier to operate an airplane than to make a stone arrowhead (p. xxxix).

It may be suggested that the unique thing about man's creative thinking is not the way he thinks, but what he thinks about. Man's thought involves cultural as well as individual content.

Although the communicative function of language is perfectly obvious, its relevance to intelligence may be less so. In the classical framework, intelligence was a sort of innate mental power which was quite independent of acquired information, however valuable the latter might be in its own right. In this sense it would be absurd to suggest that linguistic communication could enhance intelligence. If, however, intelligence is seen as nothing more than a collection of learned skills and facts, it is quite reasonable to suppose that it would be enhanced by a technique for communicating such skills and facts.

Even if the experiential interpretation of intelligence is accepted, it may be difficult to accept the idea that man's intellectual advantage over the apes is due primarily to his communicative ability. Our egocentric natures make it all too easy for each of us to take some personal credit for the unique success of our species in dealing with its environment. We readily accept an exaggerated picture of the mental ability of the individual *as an individual;* and it is relatively difficult for us to realize the extent to which our abilities depend on information received from other people, rather than directly from our natural, physical environment. It would be interesting to know something about the behavior of a healthy, adult human who had never had any communication of any kind with other people. In the absence of any reliable data whatever on this point, it may be asserted with confidence that such a creature would impress both clinical and comparative psychologists as no more intelligent than an ape.

Language and EPDs

If it be granted that man's special intellectual status can be ascribed to culture facilitated by the ordinary communicative use of language, a crucial question remains: Why is man the only animal that learns to use language? The concept of EPDs provides a ready answer. Young humans display certain kinds of intrinsically motivated activity which do not occur in the young of other species, and which are highly relevant to the acquisition of linguistic skills.

Babbling.—The significance of one of these activities, babbling, has long been appreciated. Groos (1901), discussing "the babbling, chattering, and gurgling of infants," held that

> this instinctive tendency to motor discharge produces movements of the larynx, mouth, and tongue muscles, and the child that attains now to the voluntary production of tone is fairly launched in experimentation. Without this playful practice he could not become master of his voice, and the imperative impulse to imitation which is developed later would lack its most essential foundation (p. 32).

The intrinsic motivation of babbling is not, of course, universally accepted. Mowrer (1952) has suggested that

> much of the motivation for the babbling and cooing that infants normally engage in stems from the fact that the human voice . . . has taken on pleasurable (secondary reinforcing) properties. Although baby's voice does not sound exactly like mother's voice, the similarity will usually be sufficient to cause a carry-over of some of the pleasurable qualities of one to the other (p. 267).

This hypothesis is apparently based on the dubious assumption that infants often hear human voices while their needs are being satisfied, and seldom hear them while hungry or uncomfortable. Mowrer's suggestion that the infant babbles only to produce a sound resembling its mother's voice implies that babbling should only occur in the absence of other voices, which is hardly the case. Strangely, Mowrer himself mentioned a study in which it was found that mothers —especially "*good* mothers—kept their infants 'bathed in sound' most of their waking hours . . . these mothers vocalized almost continuously" (p. 267), which would appear to make babbling unnecessary. Ewing and Ewing (1938, pp. 87, 204, 209) observed that the early babbling of congenitally deaf infants was perfectly normal, which indicates that the activity itself is intrinsically motivated and does not depend on any rewarding property of the acoustic product.

Non-talking primates.—Many anthropologists and comparative psychologists have been concerned with the question of why apes do not talk, even when reared in human society, with ample exposure to human speech. The answer cannot be found in the minor differences in the anatomy of mouth and larynx, since the question is not concerned with excellence of articulation, but rather with the complete lack of speech. The apes' intellectual level is likewise irrelevant, since they do not even express stupid ideas with limited vocabularies and atrocious grammar. A number of investigators have attempted to teach apes to talk by methods similar to those used with deaf and otherwise handicapped human children (Furness, 1916; Garner, 1896; C. Hayes, 1951; Hayes & Hayes, 1950; Kellogg & Kellogg, 1933, p. 286; Witmer, 1909). These efforts have never produced more than a few words, which were usually enunciated poorly, reluctantly, and with considerable effort.

Kellogg and Kellogg (1933) reared their chimpanzee *S* in the company of a human infant, and they report that "in the case of the human subject a continuous vocalized play was apparent;" but such behavior was never observed in the chimpanzee, who "never vocalized without some definite provocation . . . and in most cases this stimulus was obviously of an emotional character" (p. 281). Langer (1942) was apparently the first to recognize the significance of the Kelloggs' observation. She noted that the chimpanzee

> makes no stumbling attempts at words, as he does at using tools, decorating his body, dancing and parading, and other primitive pursuits. He is conceptually not far from the supreme human achievement, yet never crosses the line. What has placed this absolute barrier between his race and ours?
>
> Chiefly, I think, one difference of natural proclivities. The ape has no instinctive desire to babble in babyhood. He does not play with his mouth and his breath as human infants do (p. 116).
>
> Man . . . must trace his descent from a vocalizing race—a genus of ape, perhaps, in which the rudiments of symbolic conception, that apparently are dawning in the chimpanzee, were coupled with an instinctive tendency to produce sounds, to play with the vocal apparatus (p. 117).

The Kelloggs' investigation only lasted for nine months, and their *S* was already seven months old when it started, so some babbling could have been missed. However, Dr. Glenn Finch (unpublished report) adopted a newborn chimpanzee in 1937 and kept it in his home for more than two years; and although this infant occasionally vocalized spontaneously around five months, it did nothing resembling the babbling of a human child. Subsequent work (C. Hayes, 1951; Hayes & Hayes, 1950, 1954) confirms that infant chimpanzees are, indeed, extremely deficient in vocal play; and this deficiency is reflected in the extreme difficulty with which they learn to vocalize for reward in a simple operant conditioning situation, as well as in their lack of speech.

It appears then that the primary source of man's "supreme achievement" is his wholly undignified tendency to make a variety of meaningless sounds. Although this is necessary for the development of the motor skills of speech, it is surely not sufficient for the development of an effective system of communication.

Later linguistic play.—Having learned to produce a variety of vowels and consonants, the child must next learn to join these elements in the particular combinations which are used as words by the adults of his society. Children accomplish this by means of their playful imitation of adult speech. However, there is no need to postulate a uniquely human, vocal-imitation drive, since this form of play can be understood as one particular expression of a more general tendency to imitate. Chimpanzee, as well as human children, are strongly motivated to imitate the behavior of their adult associates, even though such activity yields no material reward for the child, and even though the child does not see the significance which the behavior has for the adult (C. Hayes, 1951; Hayes & Hayes, 1952a, b).

A primate who babbled and imitated and thus learned words would, at this point, have all the communicative skill of a parrot, but no more. The child must next learn the meanings of words. It is easy to imagine how he might do this in a strictly practical manner in the course of trying to get what he needs from other people; but the child does not seem to operate this way. As Woodworth (1958) said:

> On occasion he communicates his need for food, but he acquires his speaking vocabulary in other ways for the most part. Of the hundreds of words that the child picks up before the age of three, very few are concerned with bodily needs. Most of them designate persons, animals and things that interest him or activities in which he participates. Vocalization and speech begin as autonomous activities rather than mechanisms driven by the necessities of life (p. 127).

A child who has learned the meanings of several hundred words would probably forget most of them shortly if he did not thoroughly overlearn them by frequent practice. Still more practice is needed if he is to develop an adult vocabulary, and acquire skill in recalling words promptly when he wants them, and in combining words to express complex ideas according to the arbitrary syntax of his culture. This extensive practice is provided for by the fact that children like to talk, whether they have anything of practical significance to say or not. More precisely, children display intrinsically motivated communicative behavior which usually takes the form of speech, but which may take other forms. Heider and Heider (1941) have described this kind of behavior in young deaf children who had not yet been taught either speech or conventional sign language. Such children communicate a considerable amount of information by means of "pictorial gestures" which suggest the intended meaning. "A deaf child likes to point at things and persons just as a hearing child likes to name them," and these authors were impressed with "the sometimes excessive use made of pointing which seems to go far beyond the needs of social communication" (p. 8). Much of the deaf child's gestural communication occurs when

> the child wants to tell something only for the sake of telling it when the telling has no means-end quality. He does not want help or explanation; he simply wants to state a fact and communicate it. To this group belong most of the more complicated [gestural] phrases (p. 31).

They also observed that the most sophisticated uses of gesture merely "serve to establish contact with the other person, not to induce cooperation" (p. 31). It is interesting to note that chimpanzees also use gestural communication; but they, unlike deaf, human children, use it primarily for its "means-end quality," and seldom, if ever, just for the sake of communicating. They thus do a smaller total amount of gesturing and, probably for this reason, develop less skill at it (Crawford, 1937; C. Hayes, 1951; Hayes & Hayes, 1954).

Jesperson's (1922) treatment of language was concerned largely with the child's very active role in the origin and evolution of languages. He emphasized that the child does not function as a mere passive recipient of his native tongue, but aggressively modifies, combines, and invents words. He discussed several cases where pairs of young children, in relative social isolation, actually developed their own primitive languages, *de novo;* and he endorsed the theory, originated by Hale in 1886, that new language families have originated in this way when children have been isolated from adult speech.

There is some reason to believe that the experience-producing activities of animals in general are concentrated largely in the earlier part of the individual's life; but man's intrinsically motivated speech activity shows little sign of dropping off after an adult level of language skill has been attained. Langer's (1942) discussion is largely concerned with the "non-functional" speech of adults, and provides many examples of such behavior. It seems strange, at first glance, that there has been no weeding out of individuals who waste energy in such pointless activity; however, closer examination indicates that it is by no means pointless.

Language learning, unlike the learning of some simple skills and mechanical relationships, requires a model. If a child's babbling and his imitative and communicative play occurred in the presence of adults who spoke only when necessary, the child would have little opportunity to mold his vocal output into useful form. Although speech communication provides man with an extremely valuable tool, its utility for the task of survival does not depend on its being used constantly Most adults could probably function perfectly well if they were silent 99% of the time. But the descendants of any such silent people would have trouble when they encountered those crucial, if infrequent, situations where fast and accurate communication is essential. (It is important to remember that such "professional talkers" as lecturers and salesmen are products of very recent cultural evolution, while the biological nature of modern man is still that of the primitive hunter, farmer, and handcraftsman.)

Language and brain structure.—The species-specific EPDs postulated to explain man's unique behavioral characteristics must be presumed to have a structural basis; but this must be a matter of complex patterning in the fine structure of the brain, frustratingly inaccessible to present histological techniques. It would be more comforting, at least, to relate man's linguistic ability to the gross anatomy of his brain. Thus, one is tempted to ask whether Broca's area is present in the chimpanzee's brain; but it quickly becomes apparent that such a question is operationally meaningless. (There is certainly no hole in that part of the chimpanzee's cortex where Broca's area should be.) The speech areas of the human cortex can only be identified by the loss of previously existing speech when these areas are damaged (or temporarily inactivated by electrical stimulation). This procedure is clearly inapplicable to chimpanzees. In any case, the evidence cited in a previous section on early brain damage suggests that no specific areas are essential for speech development in the child.

Gerard (1959) has expressed his suspicion "that you could not teach a chimpanzee to speak chimpanzee, let alone English, because he doesn't have large enough motor areas for his tongue and his larynx" (p. 17). But young children are not incapacitated by loss of motor cortex; and the point is not that chimpanzees speak slowly and clumsily, or omit certain phonemes. The point is that they do not speak at all. Washburn (1959) suggested that, aside from specific areas, "The reason that a chimpanzee cannot learn to talk is simply that the large amounts of brain necessary for speech are not there" (p. 28). From a strictly quantitative point of view, it is reasonable that a small brain might be unable to handle a large vocabulary; but the ape has *no* vocabulary, while the minute brain of the parrot accomodates dozens of words.

Delayed and defective speech.—Most genetically controlled characteristics display substantial intraspecies variance. If this is true of the EPDs that promote language learning, it may be expected that a few individuals will fail to engage in enough pre-speech and early-speech activity to develop normal language skill. There are, undoubtedly, children without speech, or with seriously delayed or defective speech, whose abnormality cannot be ascribed to known structural causes. Ewing (1930) reported two such children who had not babbled in infancy, and suggested "It seems likely that the symptom of not babbling is much more important than has hitherto been realized" (p. 5).

However, most speech therapists, if they consider motivational factors at all, tend to concentrate on *extrinsic* motivation. Thus, although Berry and Eisenson (1942) recognized that babbling provides practice in the production of speech sounds (p. 4), they held that if the child is not speaking normally by four years it is probably because he gets everything he wants from his over-indulgent parents without speaking, and therefore has no reason to bother with learning to talk (p. 9, 275).

MENTAL "GROWTH"

One of the more basic and obvious features of human intelligence is its gradual increase during the period of physical growth. (This is presumably a rather general phenomenon, but it is much better known in man than in other species.) The human brain has its full quota of neurons at birth, but the individual cells show considerable postnatal development in structural details such as perikaryon size, fiber myelination, ramification of cell processes, and appearance of Nissl granules. Maturation of this sort proceeds at different rates in various parts of the brain. The literature in this field has been summarized by Eayrs and Goodhead (1959). The weight of the human brain increases approximately fourfold from birth to about ten years, when it is essentially full-size.

The Traditional Interpretation

If intelligence is conceived of as a direct expression of brain power, the increase of intelligence with age is quite naturally assumed to reflect the physical growth of the brain. Few, if any, psychologists believe that the behavior involved in test performance, school work, or vocational success is determined directly by brain maturation. It seems quite obvious that all intelligent behavior involves learned abilities, and learning, like growing, takes time. However, it can be argued that the acquisition of these abilities depends on the maturation of structures which provide the brain power that is the proper referrent of the term "intelligence." But this viewpoint provides no means for distinguishing between the growth of brain structure and the concurrent accumulation of learned abilities. So far as the evidence is concerned, it is quite conceivable that the structures essential for "intelligence," in this sense, are already mature at birth, and the slow development of behavioral abilities is purely a matter of learning. (The very considerable brain growth that occurs after birth might still have functional significance. It could, for instance, provide increasing sensory, motor, and memory capacities, rather than "truly intellectual" capacities.) The obvious shortcoming of the traditional interpretation is that it ignores learning, which we *know* is relevant to intelligent behavior, and emphasizes brain growth, which we *suspect* may be relevant, but which has never been observed to produce intelligence without the help of learning. It may be fruitful to examine the implications of a reversal of emphasis.

The Motivational-Experiential Interpretation

If intelligence is seen as a collection of learned facts and skills, its increase as a function of time need not be causally related to structural growth. From this point of view, it might be hypothesized that a child's intellectual development would proceed normally even if his structural development were halted at birth by a powerful growth-inhibiting drug. This hypothesis seems rather absurd, but it is interesting to note that available evidence cannot disprove it. However, it involves the bizarre assumption that a 350-gram, infant brain is just as good as a 1400-gram, adult brain. This assumption can be avoided by a modified hypothesis which accepts postnatal brain growth as having functional significance, but still puts the major theoretical burden on learning. This more moderate experiential interpretation assumes that brain growth provides increased capacity for sensory acuity, fine motor control, and memory. However, it assumes that these growth-determined capacities never actually limit intelligence because (a) brain growth is rapid, (b) learning takes time, and (c) the learning which occurs first requires relatively little sensory, motor, and memory capacity. If learning could be greatly accelerated, or if brain growth were greatly retarded, intelligence would be limited by the maturational status of the brain; but, in practice, brain growth stays well ahead, and the child's intelligence at any age is limited by the amount of time he has had for cumulative learning.

Rate of intellectual development.—The experiential interpretation of intelligence equates the rate of intellectual development with the rate at which learned facts and skills are accumulated. It is easy to see how this rate must be influenced by environmental factors which provide more or less opportunity for learning; but it has not been so obvious how hereditary factors might operate (unless the dubious concept of learning capacity be invoked). Experience-producing drives provide a plausible explanation. Individual differences in rate of intellectual development may be ascribed to inherent differences in the tendency to engage in activities conducive to learning.

An individual's rate of intellectual development may change rather abruptly. Sontag, Baker, and Nelson (1958) have published 140 individual "mental growth" curves, many of which are characterized by a sudden change of slope (in either direction) at about six years. There is no known phenomenon of neural growth to which these changes may be attributed. A few cases might, presumably, be explained by changes in the individual's opportunity to learn; but such changes would not be concentrated at one particular age. Six years is, of course, the age when most children enter school; but how can this environmental change account for both increases and decreases in rate of intellectual development? These changes in rate can be understood in terms of EPDs if it is assumed that the drives conducive to learning in a school environment are not identical with those conducive to learning in a preschool environment. Thus, a child's innate motivational makeup might be such that he would learn the basic perceptual, motor, and speech skills at a greater than average rate, but would display less than average tendency toward activities relevant to the learning of reading, writing, and arithmetic (or vice versa).

Final level of intellectual development.—Intelligence test performance tends, *in general*, to level off (or increase less rapidly) at *about* the same time physical growth is completed, which would seem to support the brain-growth interpretation of intellectual development. However, closer examination reveals that (a) there are individual differences in the duration of intellectual development which are not correlated with differences in the duration of structural growth; (b) maximum performance is attained at different ages on different tests (Wechsler, 1958, p. 25); and (c) the age at which test performance, in general, tends to level off, also corresponds rather closely to the age at which most people terminate their formal educations. The roughly simultaneous leveling off of structural growth and intellectual development can be accounted for within the motivational-experiential framework in at least two ways.

First, it is not certain that the individual's total store of learned abilities actually increases more slowly after 15 or 20 years, despite the implications of intelligence-test data. The apparent leveling off may well be an artifact produced by the manner in which intelligence is estimated by tests. If intelligence is seen as a collection of learned facts and skills, the ideal way to measure it would be by a complete inventory of everything the individual has learned (and still remembers). This is obviously impossible, and actual tests *sample* the individual's abilities. However, the sample of items included in a test is always an arbitrary kind of non-random sample of the universe of possible test items. No test provides a *representative* sample of *all* the learned abilities of *all* men. Some tests may provide for a relatively representative sampling of the abilities dealt with in the grade-school curriculum; but sampling becomes progressively less representative as we consider the progressively more diverse and specialized abilities of high school, college, and graduate students—to say nothing of the tremendous variety of vocational specialists (Thorndike, *et al.,* 1926, p. 468).

Second, if intelligence (in this sense) is seen as an adaptive mechanism which serves to promote survival, it would be appropriate for maturing individuals to devote progressively less time and energy to acquiring new abilities, and progressively more time and energy to making use of those already acquired. This is precisely what would happen if EPD strength decreased in the maturing individual. Although the evidence on this point is informal, observational, and non-quantitative, it does suggest that something of this sort happens in the average, young, human adult.

IQ PREDICTION

One of the great frustrations of people who work with intelligence tests is their inability to predict the ultimate intelligence of young children. The young child's IQ is seldom of any great interest for its own sake. There is little utility in knowing a child's IQ at the age of 3 years unless it can be assumed that his IQ will be pretty much the same 15 years later. Longitudinal studies (e.g., Bayley, 1949) have shown that the correlation between adult IQ and that determined at 3 years or less is, for all practical purposes, negligible.

Attempts at prediction appear to have been based on the implicit assumption that adult intelligence is simply a fuller development of the innate brain power responsible for the child's modest accomplishments. The motivational-experiential theory suggests a different way of looking at the problem. If adult intelligence is the product of 15 or 20 years of learning, it can best be predicted at 3 years by a forecast of learning activity during the next 15 years, rather than by an assessment of what has been learned in the first 3 years. From this point of view, the important characteristic of a 3-year-old child with high intellectual endowment is not that his present behavior is outstanding—it is idiotic by adult standards—but rather that he prefers activities which favor the gradual accumulation of abilities which will eventually permit him to behave intelligently, by adult standards. The implication is that the intellectual potential of young children may be measured more effectively with a test of motivation than with a test of childish achievement.

A longitudinal study from the Fels Research Institute (Sontag, Baker, & Nelson, 1958) provides some support for this notion. One hundred and forty children were tested and observed periodically for their first 12 years. Many of them showed changes in Binet IQ which could not be attributed to test unreliability or to the difference in test content at various age levels. Certain motivational characteristics, evaluated by rating scales, at 6 years, were found to be significantly related to changes in IQ during the following 4 years. The investigators intended the rating scales to reflect social and socially-derived "achievement motives;" however, the data can also be interpreted in terms of innate activity preferences. Thus, the children whose IQs increased tended to get high ratings on "general competitiveness," but this rating included credit for "originality in the use of materials in solitary play" (p. 98). The rating scales are rather complex and it is not easy to interpret them simply and unambiguously. In general, however, it might be said that the children who gained in IQ were those motivated primarily by interest in activities, while those whose IQs declined were motivated primarily by the social pressures exerted by parents, teachers, and other children.

Kagan, Sontag, Baker, and Nelson (1958), using the same children, evaluated motivation by projective tests, and found that the children whose IQs increased tended to show more "achievement imagery" and "themes of curiosity about nature" on the Thematic Apperception Test. The cause and effect relationship is less clear in this study, since motivation was measured after the IQ change had occurred.

SUMMARY AND CONCLUSIONS

There has been widespread dissatisfaction with traditional conceptions of intelligence. Theoretical discussions have commonly postulated qualitatively distinctive, structurally determined, "higher mental functions;" but practical measurements have been based on assessment of the individual's learned abilities. This paper has reviewed the literature relevant to *a motivational-experiential theory* which proposes that: (a) manifest intelligence is nothing more than an accumulation of learned facts and skills, and (b) innate intellectual potential consists of tendencies to engage in activities conducive to learning, rather than inherited intellectual capacities, as such. These tendencies are referred to here as *experience-producing drives* (EPDs).

The following conclusions appear to be warranted. (1) Although it has been customary to assume that activity preferences are determined by experience, there is ample evidence to show that such preferences may be genetically controlled. (2) Genetically controlled tendencies to engage in specific kinds of activity lead to the acquisition of corresponding skills and information, independently of the operation of the traditional "homeostatic" or "biogenic" drives. (3) The traditional assumption of special, structurally determined capacities for "higher mental functions" is not adequately supported by the evidence. Such functions have not been shown to be unitary, and the behavior in question can be accounted for as the result of large amounts of ordinary learning. (4) Learning capacity and memory capacity have sometimes been proposed as structural bases of intellectual development; but the evidence does not demonstrate that either of these is responsible for the observed variance in intelligence. (5) Experience-producing drives may be altered by brain damage, and the resulting change in behavior may suggest a loss of "intellectual capacity." If EPDs are altered by early brain damage, all subsequent intellectual development may be affected. Brain damage which alters EPDs after intellectual development is complete may affect performance on tasks which are sensitive to motivation. (6) Some of the difference between human intelligence and that of electronic computers may be ascribed to the computer's very special and unnatural EPDs. (7) The unique intellectual status of man can be ascribed to EPDs which promote the acquisition of speech and communicative skills. There is little evidence to support the hypothesis that language facilitates the thought *process;* but efficient communication affects thought *content* by giving the individual access to the experiences of others. (8) "Mental growth" is not a direct result of the spontaneous, physical growth of the brain, but is rather a result of the gradual accumulation of knowledge, at a rate determined by an individual's EPDs. (9) Prediction of a young child's later intellectual development may be facilitated by assessment of his EPDs.

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