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ABSTRACT

Areas of agreement and disagreement concerning intellectual developmental and the problems of mental retardation espoused by the author and by Uzgiris are discussed in this paper. The importance of environmental factors, along with the genetic and/or constitutional nature of the organism on which these environmental events impinge, is seen as central to the interactionist's position. A naive or premature environmentalist approach of the determination of development is asserted to be as erroneous as a naive or premature hereditarian approach. Considerable attention is devoted to the presentation of materials that highlight the importance of genetic factors in the interactionists' equation. The paper is considered (1) to insure that the reader never loses sight of the biological components of intelligence, (2) to point out that certain psychological functions may be more amenable to environmental manipulation than others, and (3) to highlight the issues that must be resolved before the manner in which environment affects both the intellectual and the non intellectual components of behavior is comprehended. (Author/AM)

THE NATURE - NURTURE ISSUE RECONSIDERED: A DISCUSSION OF UZGIRIS' PAPER[1]

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It is indeed a pleasure to have the opportunity to discuss Dr. Uzgiris' fine paper. At the outset, let me state that I agree with a great number of points made by Dr. Uzgiris. Indeed, I have made many of these same points in my own formal treatments concerning intelligence, its nature and its development. It is appropriate that workers continue to argue for the need of an explicit theory of intelligence rather than be content with the implicit views and piece-meal efforts that characterize so much of the work in this area. With Uzgiris, I agree to the bankruptcy of any simple maturational or predeterministic view of the development of intelligence. I, too, have championed a cognitive developmental approach over a purely psychometric one (Zigler, 1967a), feeling that the former approach holds the greatest potential for unraveling the mysteries of the developing intellect. I have heartily endorsed the significance of the process-content distinction and concur with Uzgiris that it is not only important to determine the particular cognitive processes that mediate a behavioral achievement, but that we must also discover to what extent particular achievements can be mediated by cognitive processes of both lower and higher developmental forms. I, too, have argued that we will make little headway in understanding the impact of the environment on intellectual development until we clearly specify how particular environmental inputs to the organism influence specific cognitive processes. These areas of agreement, as well as others, should become even more apparent as I proceed with this presentation.

However, there does appear to be some differences between Uzgiris' and my own views concerning intellectual development and the problem of mental retardation. Some of these differences are subtle and some are not so subtle.

In reading Dr. Uzgiris' paper, I found myself often troubled by a general thread running through it and certain innuendoes stemming from this thread. Although informing us of the distinction between a genuine interactionist view of intelligence and a pseudo-interactionist view, there is actually very little in Dr. Uzgiris' presentation with which a true interactionist could be comfortable. In my opinion, much of Dr. Uzgiris' presentation reveals a profound disrespect for the biological, constitutional and/or genetic integrity of the human organism. Indeed, one looks in vain for the nature side in Uzgiris' discussion of the nature-nurture issue. Furthermore, rather than presenting any truly interactionist view, we have received a presentation which once again presents the importance of environmental factors without once being concerned with the genetic and/or constitutional nature of the organism on which these environmental events impinge. I'm very much afraid that Dr. Uzgiris has demarcated the world between bad guys and good guys. We immediately can sense who the bad guys are by the perjorative manner in which Uzgiris employs such terms as "predeterministic", "biological heritage", "genetic determinants", and "hereditarian bias". Not only do those with a biological bias believe wrong things, they also cause bad things to happen to children and to our society. Furthermore, Dr. Uzgiris finds her hereditarian bogeymen lurking everywhere. While I too have taken exception to those who espouse a simple psychometric approach, it is certainly an error to assert that "The psychometric approach has taken the slower rate of progress to be genetically predetermined and indicative of a lower level of intellectual functioning throughout life." Some proponents of this approach have had a hereditarian bias, while others have not. Binet, who must certainly be one of

the fathers of this approach, had a markedly environmentalistic bias. He was a leader in the mental orthopedics movement and spoke specifically of the intellect as a garden whose final fruits would be determined by the nature of their cultivation. In this same vein, Uzgiris' assertion that "current practices of institutionalization and special classes derived from the view of predetermined development" is simply an historical inaccuracy. As will be pointed out below, these practices were a direct outgrowth of a misplaced optimism concerning the value of remediation espoused by rather hard-core environmentalists. Furthermore, Uzgiris' suggestion that my views concerning the etiology of familial retardation stands in the way of "wiping out urban slums and establishing preschool education programs" strikes me as a rather gratuitous extrapolation from my position that people's genetic inheritance is an important factor in the phenomenon of intellectual variability.

Frankly, I have considerable difficulty identifying those reactionary warriors of the predeterministic or the hereditarian position to which Uzgiris alludes. Certainly no such individuals are to be found among the major figures contributing to our current thinking concerning intellectual development. One can, of course, find a shrill and misguided individual or two beating their breasts about the racial inheritance of intelligence issue and also a more benign small group of neo-Gesellians. Beyond these individuals, I see nothing more than a relatively small group of workers, for the most part confined to the behavior genetics camp, who feel that we shall never understand the impact of environmental events unless we simultaneously consider the inherent nature of the organism. I number myself among this group and I hardly think that such a position is a blasphemous one. On the contrary, I feel that it is central to the interactionist's position. It is on this issue that Dr. Uzgiris and I most markedly part company.

Not only do I insist that we take the biological integrity of the organism seriously, but it is also my considered opinion that our nation has more to fear from unbridled environmentalists than they do from those who point to such integrity as one factor in the determination of development. It is the environmentalists who have been writing review after review in which genetics are ignored and the concept of capacity is treated as a dirty word. It is the environmentalists who have placed on the defensive any thinker who, perhaps impressed by the revolution in biological thought stemming from discoveries involving RNA-DNA phenomena, has had the temerity to suggest that certain behaviors may be in part the product of read-out mechanisms residing within the programmed organism. It is the unbridled environmentalist who emphasizes the plasticity of the intellect, that tells us one can change both the general rate of development and the configuration of intellectual processes which can be referred to as the intellect, if we could only subject human beings to the proper technologies. In the educational realm, this has spelled itself out in the use of panaceas, gadgets and gimmicks of the most questionable sort. It is the environmentalist who suggests to parents how easy it is to raise the child's IQ and who has prematurely led many to believe that the retarded could be made normal, and the normal made geniuses. It is environmentalist who has argued for pressure-cooker schools, at what psychological cost, we do not yet know. Indeed, the dangers of inducing children to produce cognitive achievements through the use of inappropriate cognitive processes has been pointed out by both Uzgiris and Piaget, and I heartily agree with them. All that I am asserting here is that a naive or premature environmentalism is just as erroneous as a naive or premature hereditarianism.

To many of you familiar with my work in mental retardation, it may surprise you to find me to be so outspoken in my criticisms of the environmentalist. Following my paper in Science (Zigler, 1967b) on the etiology of familial retardation, I was criticized for being too genetic in my approach by Wortis (1967). More recently, Zeaman (1968) summed up my work as representing a motivational theory of mental retardation, which, given the nature of my research to date, would indicate that I believe that retarded intellectual functioning was solely the product of particular experiential events. I am afraid that my position concerning the behavior of the retarded is not nearly as simple as these two commentators would like to believe. I have never espoused an either-or approach on these issues. Rather, it has been my view that the emitted behavior of all individuals reflects both genetic and environmental factors. I, also, believe that any behavior emitted reflects at least two organismic systems, one the cognitive, and the other, the motivational and emotional. My own reading of the evidence leads me to suspect that these two systems are differentially open to the influences of environmental events. I do believe that the motivational and emotional system is more influenced by environment than is the cognitive system; but, even in this system, I realize the importance of genetic factors. Furthermore, and here I see certain agreements with the views of Uzgiris, I would view each of these systems as being made up of sub-systems differentially influenced by genetic and environmental determinants. I would think that this general approach qualifies me as an interactionist. The fact that both the biological and the experiential enter my thinking is attested to by critics of my work who have argued alternately that I am too biological or too experiential.

I have made this point about my own coloration because I was sensitized by Uzgiris' argument concerning the criteria that must be met to be labelled an interactionist. As you might guess, I found in her criteria a more than slight environmentalistic bias. She informs us that "A truly interactionist position would postulate that at any particular moment, environmental factors exert their effect on the organism as it exists at that moment (the product of all previous interactions between it and experience it encountered), not on some original state of the organism, and the very course of development is constructed in the process." What is crucial here is that an experience impinges not only on a genetic base, but also upon the residue of all of the other experiences of the individual. This of course emphasizes even further the experiential history of the individual and is surprisingly reminiscent of the concept of "setting conditions" which has become so popular with members of the radical empiricistic school workers, by the way, whose theoretical orientation is not only anti-genetic, but, anti-cognitive-developmental as well. What Uzgiris has failed to emphasize is that in an interactionist position, the gene is viewed as not only mediating the initial experience but every experience throughout the course of development. In locating true interactionists, Uzgiris points an approving finger at Hunt and the behavior geneticists. Although I have the profoundest respect for our chairman, Professor Hunt, both as a person and as a scientist, I must confess that I have certain misgivings about classifying him as an interactionist. Either by design or his emphasis, Professor Hunt has become a champion of the role of experience. I find rare those instances in his reviews or his research where phenotypic behavior is viewed as the product of an interaction between an environmental

event and a genotype. By the same token, I find no instance in Uzgiris' review of any study in which the impact of environmental events has been found to differ as a function of the genetic structure of the individual. Human behavior genecticists have been reporting such studies for some time and several can be found in Vandenberg's compendium on Human Behavior Genetics (1965). I am afraid that for the most part both Hunt and Uzgiris give lip service to the importance of genetic endowment in much the same way that the hereditarians they criticize gave lip service to environmental factors.

I do agree with Uzgiris that America's behavior geneticists are true interactionists. Indeed, the criteria for interactionist thought were succinctly presented by the behavior geneticist Lush over 30 years ago: "Every characteristic is both hereditary and environmental, since it is the end result of a long chain of interactions of the genes with each other, with the environment, and with the immediate products at each stage of development" (Lush, 1937, p. 77). With this as a given, I would like to spend the remainder of my time balancing Uzgiris' presentation with some material highlighting the importance of genetic factors in the interactionist's equation.

That genetics, with its essential commitment to the phenomenon of variability, has been so little employed by workers committed to the investigation of individual differences is an interesting matter in its own right. The historical factors and the particular values of our society that gave rise to this state of affairs have been discussed by Burton (1968), McKee and Honzik (1962), Zigler (1967a), and Spuhler & Lindzey (1967). That the genetic approach can bring new breadth to our thinking can be seen in examining the cross-cultural social anthropological evidence. Differences in behavior uncovered in such investigations are almost exclusively attributed to external

culture and experiential differences. As McKee and Honzik (1962) point out, the assumptions that cultural variation reflects only environmental variation is extremely dubious if one suspects that the societies concerned represent different genetic pools.

An extremely telling indictment of an overly environmentalistic approach to behavior can be found in the recent work of Thomas, Birch, Chess, Hertzig and Korn (1963). These workers correctly point out that even the developmental-cognitive approach, which many feel emphasizes the genetic aspects of behavior, actually addresses itself to how a developmental level or stage is important in structuring the individual's reactions to his environment. Thus, the concept of developmental level or stage is more concerned with general laws of responsiveness and addresses itself to the sequentiality in which various systemic organizations make their appearance rather than to the problem of individuality or uniqueness of functioning.

A truly genetic approach requires one to focus upon the initial biological characteristics of the individual as significant factors in determining the development of psychological individuality. Thomas, et al., have themselves isolated a number of early-appearing and persisting reactivity patterns which appear to be responsible for variations in behavior when environmental factors remain constant. These authors have pointed out that the tactics utilized by socializing agents will have different behavioral results depending on the nature of the child to whom they are applied.

The need for rescuing this genetic approach from what Bell (1965) refers to as its "excommunication" has been asserted cogently by Hirsch (1963) who makes a convincing case for the central role of genetic factors in the understanding of human behavior. With Hirsch (1963), I feel that we will make little headway in understanding individual differences in intelligence and many other

traits unless we incorporate into our thinking the fact that a major component of such differences reflects the inherent biological properties of man. We can all agree that no genotype spells itself out in a vacuum, and that the phenotypic expression is finally the result of environment interacting with the genotype. However, as Hirsch (1963) has noted, we can no longer make the "gratuitous uniformity assumption that all genetic combinations are equally plastic and respond in like fashion to environmental influences. ...Without an appreciation of the genotypic structure of populations, the behavioral sciences have no basis for distinguishing individual differences that are attributable to differences in previous history from those that are not, and no basis for understanding any differences whatsoever where there is a common history" (p. 1042). All of this suggests that the nature-nurture issue is far from dead. While it is true that the nature-nurture controversy has abated considerably, this does not mean that it was ever resolved in any very fruitful way. What appears to have happened in American psychology in the past two decades is that a consensus was reached that behavior was ultimately the product of some "complex interaction" between these two sets of forces. Following this consensus, the great majority of American workers continued to give nod at biogenetic factors while attending almost exclusively to those environmentalistic factors which are unquestionably important determinants of human behavior.

An example of this can be seen in an important recent contribution to the literature on socialization (Bandura & Walters, 1963) in which the authors, after noting the importance of genetic and constitutional factors, make the decision to ignore them on the grounds that "...until further advances in biochemistry and psychopharmacology have been made, there is more to be gained by studying the role of undoubtedly important social-learning variables in

personality development than by seeking to establish relationships between constitutional factors and personality characteristics" (p. 29). Uzgiris appears to subscribe to this same point of view.

The pragmatic dangers involved in this practice of identifying the part with the whole has been pointed out by Bruch (1954). Furthermore, the recent work in animal and human behavior genetics and in ethology (Lorenz, 1952, 1965, 1966; Scott, 1957, 1963; Vandenberg, 1965) is highly relevant to the concerns of the student of intelligence. These workers are further along than many appreciate in relating specific behavior to particular interactions between genetic and environmental factors, and in unraveling the mystery of how much of the variance in certain behavior can be attributed to each set of factors. Their efforts have been greatly facilitated by developments in basic genetics, which has witnessed a movement away from the early Mendelian emphasis on the activity of specific dominant and recessive genes in favor of quantitative and/or polygenic models of inheritance, as well as by the development of new statistical approaches (Cattell, 1965).

The relationship between biogenetic and environmental factors is surely complicated and remains baffling. We get some conception of its complexity in the many possible explanatory models that have been enumerated by Cattell (1965). By what route, for example, should genetic characteristics be said to influence environmental determinants of behavior? One possibility is that a person's genetic makeup leads him to create a particular environment for himself. A plausible case for this interpretation is made by Cattell, Blewett and Beloff (1955) in arguing that people who are aesthetically sensitive--a trait they believe to have a significant inherited component--tend to create a protected, unambitious, aesthetically soothing environment for themselves. Another

possibility is that genetically determined characteristics evoke distinctive responses from others and thus elicit rather than create a special environment; an example here is that aggression is often met with aggression (Cattell, et al., 1955). Moreover, where we believe we can rightly judge how constitution would affect environment, some entirely different model may turn out to be more appropriate. For instance, the frequency of crying outbursts in infants under ten days of age has been found to be correlated positively with intelligence at three years of age (Karelitz, Fisichelli, Costa, Karelitz & Rosenfeld, 1964). We might ascribe this relation to the greater attention, handling and general environmental stimulation which the crying elicits at what may be a critical period in intellectual development. Yet we need to consider the possibility that crying in infancy and later intelligence are separately influenced by the same polygenic determinants.

However genetic factors spell themselves out in behavior, a considerable body of literature now exists indicating that such factors are important determinants of individual differences in behavior in infants and young children. As noted by Thomas et al. (1963), such differences have now been found in such specific discrete areas as "sensory threshold (Bergman & Escalona, 1949), motility (Fries & Woolf, 1953), perceptual responses (Witkin, Dyk, Faterson, Goodenough & Karp, 1962), sleeping and feeding patterns (Escalona, 1953), drive endowment (Alpert, Neubauer & Weil, 1956), quality and intensity of emotional tone (Meili, 1959), social responsiveness (Gesell & Ames, 1937), autonomic response patterns (Bridger & Reiser, 1959; Lipton, Steinschneider & Richmond, 1961), biochemical individuality (Willians, 1956), and electroencephalogenic patterns (Walter, 1953)" (p. X). While these

differences may in part result from prenatal environment (Pasamanick & Knobloch, 1958; Pasamanick, Knobloch & Lilienfeld, 1956), they certainly indicate that individual differences potentially important for later social behavior are present at birth or shortly thereafter.

Muller (1958), a geneticist considering social behavior from the viewpoint of evolution and the survival value of particular traits, has shown how particular behavior tendencies could have been transmitted genetically along family lines and thus argues that much of the variance among individuals may be due to genetic factors. Such human behavior geneticists as Cattell (1965); Cattell, Stice & Kristy, (1957); Eysenck (1954), and Gottesman (1965) have provided considerable evidence that tends to support this interpretation. At the infrahuman level so much evidence of the importance of genetic factors has accumulated that its importance is no longer questioned, and researchers have been able to turn to effective study of the specific mechanisms mediating the genetic effect (Freedman, 1958; Fuller & Thompson, 1960; Hall, 1938; Lindzey, Lykken & Winston, 1960; Scott, 1963).

Disregard of possible genetic influences has been most often associated with a learning theory approach--an association which, though quite unnecessary, has historical roots in the environmentalist exuberance of Watson. Gardner (1965) has pointed out that some aspects of psychoanalytic thought have also led to underemphasis of genetic factors. Here the history is ironical, since Freud often pointed out that the emergence of particular patterns of defense mechanisms in particular individuals may be dictated primarily by heredity. That a commitment to psychoanalytic thought and concern for cultural variation are perfectly compatible with a lively awareness of genetic influence can be seen, to cite one example, in an interesting paper by Erikson (1951) dealing with sex differences in play behavior.

Some data have been presented indicating a relatively direct link between particular chromosomal properties and general behavioral status (Gibson & Pozsonyi, 1965). In the social anthropological literature, we find that certain African infants evidence marked motoric precocity (Geber, 1958; Geber & Dean, 1957). This precocity has been attributed to cultural origins, i.e., to early experience resulting from culturally determined attitudes of the mother, but a genetic interpretation appears just as plausible, especially since the precocity is seen in nine-hour old infants.

Related to these genetic concerns is a concept that has become extremely controversial in the literature on intelligence, namely capacity. With Maher (1963), I believe that the concept of capacity has considerable heuristic value for workers interested in intelligence. By intellectual capacity, I have in mind something akin to Hebb's (1949) intelligence A, i.e., an innate potential for the development of intellectual functions. Those who have argued that the capacity notion is a useless or erroneous one, e.g., Chein (1945), Ferguson (1954, 1956), Liverant (1960), Spiker and McCandless (1954), appear to be invariably committed to an environmentalistic or learning orientation. The interesting, if somewhat overdrawn, criticisms of the capacity concept need not concern us here. For our purposes it is sufficient to assert that the capacity notion has value for workers in the area of intelligence in regard to organizing what we grossly observe and in aiding us in the construction of a broad theoretical framework for the psychology of intelligence.

There has been an interesting, if unconvincing, effort to view intelligence, and thus mental retardation, as a matter of acquired skills and transference phenomena in the classical learning theory sense (cf., Ferguson, 1954, 1956). It is of interest to note that although Ferguson appears to abhor a biological

concept of intelligence, he must nevertheless fall back upon it in dealing with those early learnings which do not reflect transfer effects. In addition, his treatment of transfer as a uniformly manifested phenomenon overlooks differences in ability to transfer from one task to another which may very well be a reflection of the biological capacity that the learning approach denigrates. A capacity notion is indeed superfluous within such a context since individual differences are ultimately explainable in terms of different learning experiences.

Given such an orientation, we can derive the optimistic view that complete control over learning experiences would do away with individual differences and, thus, mental retardation, at least of the non-defective variety. Such a view, though appealing, flies in the face of what has grossly been observed, from Itard (1932) onward. The most Herculean efforts of teaching and training have not resulted in marked change in the intellectual level of most retardates. Thus, at a purely empirical level, we can agree with Maher (1963) that the capacity concept has value as related to "the differences between individuals in rate of acquisition of responses under similar learning conditions." Such a concept necessarily implies the existence of structural differences between individuals and is incompatible with a psychology of the empty organism" (p. 250). It is in this last sentence that we see the theoretical value of the capacity concept since it forces us to conceptualize individuals as biological organisms innately differing in respect to the potential manifestation of a multitude of traits. Thus the concept of capacity is intimately related to the concept of the genotype that we encounter at the biological level.

The environmentalist, while giving lip service to biological capacity,

treats human behavior as the direct outgrowth of an infinite number of experiences. When faced with the troublesome observations that not all environmental events have the same impact, that the same experiences result in different outcomes in different people, and that the same experiences result in different outcomes at different times in the life cycle of the same person, the environmentalist takes refuge in postulating unspecified experiences which have altered the experiential effects of interest, maturation, and most recently "setting conditions." Within this context it is understandable why the critical period notion, provided by recent work in ethology, has had an appeal for many such thinkers. However, such arguments and concepts serve primarily to evade the central problem; namely, exactly how do various experiences affect cognitive development and how might particular experiences differ in their effect on organisms having differing biological capacities.

It is of interest to note that one environmentally-oriented theorist (McCandless, 1964) has argued that although heredity and environment interact in the production of intelligent behaviors, we need only concern ourselves with environment since "we can do something about environment." This approach is reducable to a uniformity position in which the manipulations of environments are expected to have constant results. Within such a position, the question is not raised as to the possibility that children with particular capacities will need specific environmental events in order to maximize their cognitive development. As noted earlier, the one group that has taken seriously the matter of the nature of the interaction between genotype and experiences in producing certain behaviors (phenotypes) has been the behavior-geneticists. At the infrahuman level, these investigators have presented incontrovertible evidence that the effects of particular experiences and the behaviors to which

they give rise depend upon the biological nature of the organism (Fuller & Thompson, 1960; Gottesman, 1963; Hirsch, 1963).

McCandless (1964) is correct in pointing out that the nature-nurture controversy is meaningless in the sense that "when each of two conditions or states is required to produce a phenomenon...neither can be said to be more important than the other." However, as indicated above, I feel with others, e.g., McClearn (1962), Penrose (1963), that the attempt to isolate the role played by each factor, and the exact nature of the interaction between the two roles, is a meaningful and worthwhile enterprise. Again, efforts here have been hindered by the lack of a theory of intelligence and theoretically derived intelligence tests. When the effects of nature or nurture are being assessed, the research effort typically involves the utilization of our standard intelligence tests. However, as I have noted on numerous occasions, such tests reflect both intellective and non-intellective components. Thus, a change in the I.Q. following some environmental manipulation may be attributable to a change in non-intellective rather than intellective factors. The most obvious examples here would be the effect of coaching (Jones, 1954) or altering the relationship between the tester and the testee (Sacks, 1952).

Barring the existence of tests of intelligence derived from theories of cognitive development such as Piaget's, many workers will probably continue to use our standard tests of intelligence. I see little harm in such use providing we conceptualize such tests not as representing the essence of intelligence but rather as useful, if far from perfect, indicators of cognitive functioning. Given the distinction between process and content, our standard intelligence tests do get at process, albeit not perfectly. The success that such tests have had in predicting non-test behaviors of an intellectual sort reflects the fact that these tests are tapping cognitive structures or

processes that come into play across a wide variety of tasks. On the Stanford-Binet, when we ask a child what's foolish about the statement: "Bill Jones' feet are so big that he has to pull his trousers on over his head," or, on the Wechsler Adult Intelligence Scale, when we ask an adult, "In what way are a fly and a tree alike," we are not interested in feet and trousers, or flies and trees (contents). We are interested in the sum-total of logical operations, e.g., analyses, abstractions, generalizations, (processes) that the person employs in solving specific problems with which he has never before been confronted.

Clear evidence that standard intelligence tests get at cognitive structures can be found in numerous studies that have reported substantial correlations between the mental ages derived from such tests and the levels of cognitive functioning defined by performance on tasks employed by structure or process-oriented cognitive theorists. Thus, the mental age a child achieves on a test such as the Binet or the Wechsler that broadly samples intellectual functions, may be used as a gross indicator of his intellectual level. But again, caution is in order. One must remember that the obtained correlations between these two types of indices have been far from perfect. That a relationship exists appears to be more a matter of luck than any theoretical strategy. It is readily apparent that the I.Q. score obtained reflects many factors having little to do with central cognitive processes, e.g., the unreliability of the test; the test-taking set of the testee; content contamination, which allows particular experiences rather than cognitive functioning of the testee to determine the correctness of his response; and the relationship between the tester and the testee.

Once a test is viewed as a product of an interpersonal relationship and

the test score as reflecting a myriad of factors, we can begin the task of determining what responses to attribute to what factors, and continue to revise and administer standard tests of intelligence in such a way as to make them become even better indicators of cognitive functioning. In such a process the validity of tests would have much more to do with the construct of intelligence defined by the cognitive approach many of us have been advocating than it would have to do with the predictive criterion that we are presently employing.

Clarity could be brought to this area if we employed pure tests of cognition as well as tests which were non-cognitive in nature but were related to the individual's general level of adaptation. We could then determine the effects of particular environmental events on these two types of processes, both of which are important to the person's social functioning. McClearn (1962) has also noted that improvements in test construction would be of great aid in genetic studies. In the absence of pure tests of cognition, studies attempting to investigate the role of nature and nurture must continue to employ our standard intelligence tests as though they were measures of intellectual capacity alone. Since these tests have proven to be reasonably good indicators of intelligence, however it is defined, there is little danger, provided the investigator is aware of the error he is introducing. In this same connection, many investigators have interpreted the fact that an individual's I.Q. often does not remain constant throughout his life span as evidence that environmental factors at various points in the life cycle are changing the relative intellectual ability. That such an interpretation is less than parsimonious has been noted by several investigators (A. D. B. Clarke, 1958; Jones, 1954; Windle, 1962). The two erroneous assumptions involved in this interpretation are: (1) our standard intelligence tests are perfectly reliable, error-free

instruments completely impervious to the effects of any factor other than cognitive functioning; and, (2) the perfectly smooth, negatively accelerated mental growth curve which underlies the standardization of our tests actually reflects the pattern of intellectual growth for every single individual we test.

Let us first examine the purely statistical factors that literally guarantee the inconsistency of the I.Q. As Shapiro (1951) has pointed out, on a test with a mean of 100 and a standard deviation of 16, for every three children making a score of 100, on re-test one child would obtain an I.Q. of above 107 or below 93 because of errors in measurement. These same errors would result in one out of every 10 children scoring above 112 or below 88 on the second test. Further changes in the I.Q. over the life span are guaranteed if the test employed has varying standard deviations at different ages, as does the 1937 revision of the Stanford-Binet. Given the standard deviation of 12.5 at age 6 and 20.0 at age 12, Tizard (1953) points out that an I.Q. of 74 at age 6 is equivalent to an I.Q. of 60 at age 12. If one attends only to these absolute numbers, the interpretation here would be a startling drop in I.Q. when actually there had been no change at all in the person's relative position in the population distribution. It is possible to control for these changing standard deviations by employing the McNemar (1942) corrections, a practice rarely encountered in I.Q.--change studies involving the 1937 Stanford-Binet. Other factors which must also be considered when interpreting changes in the I.Q. include the statistical phenomenon of regression to the mean, which is especially pertinent to children initially testing either very high or very low; test practice; incorrect testing which includes a variety of poor testing practices; changes in test composition; coaching; and, the attitude of the testee.

Another factor, which has been noted by certain investigators (A. D. B. Clarke, 1958; Jones, 1954; Laurendeau & Pinard, 1962), but which has received very little attention in most treatments of I.Q. changes, is that of individual variations in intellectual growth. The intellectual growth of any particular person is not accurately represented by our theoretical curve, but is probably more like physical growth which is characterized as proceeding at different rates at different times. As Jones (1954) has noted, even within an individual growth curve, different mental functions exhibit differences in the rate, peak, and total magnitude of growth and in general the varying patterns we encounter "admit of no easy explanation in terms of extrinsic factors." Even the theoretical curve of mental growth offers certain problems for the environmentally-oriented theorist. As Jones (1954) has pointed out,

> A theory of environmental influence must be able to deal with the fact that in terms of units other than mental age the growth of intelligence is not linear but with increasing age exhibits a decreasing rate of change. If environment were the predominant factor in mental growth, we might reasonably expect a positive rather than a negative acceleration in later childhood and adolescence, since individual development involves a multiplication of environmental contacts and also an increasing scope of response to the environment. (p. 635)

Bayley (1949) has summarized the clear evidence that mental scores do not always show a regular increase with age but may exhibit cyclical changes reflecting personal rhythms in intellectual development. Jones (1954) has suggested that these personal rhythms in the rate of mental growth are reflecting neurophysiological factors. Given such differences in the personal rhythm of development and the variety of factors noted earlier, the surprising thing is not the inconsistency of the IQ but, rather, the fact that it shows as much stability as it does.

In addition to the variety of methodological errors inherent in the

typical study investigating I.Q. changes, and the complexity of individual variation in intellectual growth, there are even further difficulties in the assessment of the interaction between heredity and environment. A major problem in determining the exact nature of the relationship is related to the fact that man is not an experimental animal. We cannot assign various genotypes randomly to various environments, nor can we guarantee random mating through the random assignment of marriage partners. Here again we must tease out the fact from procedures that are far from clearcut. It is somewhat appalling to realize how popular hereditability indices have become in certain discussions of intelligence. It is not uncommon to be informed that the variance attributable to hereditarian factors is precisely .68 or 87 per cent. Again, this practice has little inherent danger provided we are aware of the limitations underlying the calculation of such indices. I cannot improve upon Jones' (1954) enumeration of these limitations:

> (1) The proportional contribution of heredity and environment does not refer to the make-up of individual IQ's or to the general level of intelligence, but either to average effects upon individual differences or to differences between groups. (2) Existing studies are based on fallible and incomplete measures both of intelligence and of the environment; this fact should be remembered when the data are being manipulated to yield an apparently highly exact result. (3) Even if it is ever logically feasible to seek a single value for the effect of environment, the particular value reported in a given study may not apply in samples involving (a) a different environmental level, (b) a different hereditary selection, (c) a change in variability of either of the above factors, or (d) a change in any special conditions which may affect the interaction of these factors. (p. 633)

Despite the shortcomings of the nature-nurture work on intelligence, it is still possible to derive certain conclusions. Studies of parent-child resemblances in intelligence, sibling resemblances, a variety of types of twin studies, and studies on children in foster homes have, in my opinion, made it abundantly clear that inherited intellectual endowment is a much more important

factor in intelligence than most environmentally-oriented psychologists would have us believe.

It is difficult to see how the most biased environmentalist could refute the recent review on genetics and intelligence prepared by Erlenmeyer-Kimling and Jarvik (1963). These authors, looking at 52 studies involving some 99 groups and more than 30,000 correlational pairings, found that the median value of the reported empirical correlations closely approximated the theoretical value predictable on the basis of a genetic relationship. I would take some exception to the goodness of fit reported since the assumption of random mating seems to have been made. Given this assumption we would indeed expect a correlation of .50 between parents and children. But since we know that assortive mating results in a correlation between parents' intelligence of approximately .50, a purely genetic hypothesis would predict that the correlation between parents and children be .75. In fairness to Erlenmeyer-Kimling and Jarvik, we should point out that Jones (1954) has suggested that if statistical corrections for attenuation, etc. are made for reported parent-child correlations of .50, the corrected value would approach the .75 figure. The work of Cattell and Willson (1938) is pertinent to this argument since they employed a sample covering the entire range of intelligence and found a mid-parent-mid-child correlation of .70.

One must not of course forget the importance of environmental factors on manifest intelligence. The role of environment can clearly be seen even in those extreme cases where a known gene defect is the cause of mental retardation. In the case of genetically determined phenylketonuria, subnormal intelligence only occurs in that standard environment that provides phenylanaline in the diet of the affected individual. A specific change in the environment, i.e.,

withholding pherylanaline from the diet will prevent the occurrence of subnormal intelligence. Some 30 years ago Hogben (1933) stated the matter well when he asserted, "No statement about a genetic difference has any scientific meaning unless it includes or implies a specification of the environment in which it manifests itself in a particular manner" (p. 14). The following conclusion drawn by Erlenmeyer-Kimling and Jarvik (1963) from their review thus appears to be a perfectly appropriate one: "Individual differences in behavioral potential reflect genotypic differences; individual differences in behavioral performance result from the nonuniform recording of environmental stimuli by intrinsically nonuniform organisms" (p. 1478). Serious shortcomings in the work of those who have emphasized the environmental factor in intelligence have been their failure to isolate the particular environmental factors which affect intelligence, their failure to designate the exact process through which such environmental factors operate, and their disregard for whether all components of intelligent behavior are equally affected by these, as yet unspecified, factors.

An issue in the nature-nurture controversy of special pertinence to mental retardation is the role of environment in producing individual differences in intelligence as opposed to affecting the absolute achievement of man. It is one thing to assert that the environment may play a role in the individual differences found among man. It is another thing to assert that environmental events can make the individual with a normal intellectual endowment retarded, or for that matter, shift the entire range of intelligence in such a way that no individual would display that degree of intellectual impairment that we now label retarded.

This distinction was made by Thorndike (1905) who warned us against confusing two totally different things:

> (1) the power of the environment,--for instance, of schools, laws, books and social ideals,--to produce differences in the relative achievements of men, and (2) the power of the environment to produce differences in absolute achievement. It has been shown that the relative differences in certain mental traits which were found in these one hundred children are due almost entirely to differences in ancestry, not in training; but this does not in the least deny that better methods of training might improve all their achievements fifty percent, or that the absence of training, say in spelling and arithmetic, might decrease the corresponding achievements to zero. (p. 11)

We are all aware that different environments, e.g., rural vs. urban, racio-cultural, social class, are associated with differences in intelligence. To what extent such differences are reflecting environmental and to what extent inherited factors remains an open issue. The majority position seems to be nurture-oriented, the argument being that it is the social class or cultural environment which produces retardation. To state the matter more simply, the hereditarian asserts that one is in a lower socio-economic class because one is unintelligent, whereas the environmentalist asserts that one is unintelligent because one is in the lower socio-economic class. The possibility of a genetic factor in social class membership was not raised by Uzgiris although, as Spuhler & Lindzey (1967) have noted, it has considerable currency among behavior geneticists. With respect to possible genetic influences on social class differences, the egalitarian tradition of the United States has doubtless contributed to the absence of research and to the near-absence even of the discussion that might lead up to it.

Gottesman (1965) has recently published a valuable paper which helps fill this gap. He points out that social class differences are differences between populations rather than individuals and that whenever there is a sizeable degree

of reproductive isolation between populations, the relative frequencies with which the different forms of genes occur in their gene pools will differ. Basing his views on the clear fact of assortative mating within social classes and the evidence of definite genetic influence on some aspects of personality (see Vandenberg, 1965), Gottesman argues that some social class differences in behavior may rest partially on a genetic basis rather than on the wholly environmental basis often supposed.

Whereas contemporary environmental thinkers give lip service to genetic limits, their basic credo is one which emphasizes the person's almost unlimited plasticity in respect to the intellectual level that he can attain. There has thus arisen the notion of mental retardation as a major consequence of social deprivation. The popularity of this view is such that many professionals and most laymen believe that all children are capable of "normal" intellectual functioning if we but expose them to enough "cultural enrichment." Such a position may or may not be true. What is troublesome to the impartial observer is that with certain exceptions, most of the post-Sputnik enrichment projects which dot the psychological-education field are more directed toward proselytizing than toward testing explicit hypotheses concerning the complex interaction between specific environmental events and the growth of intelligence.

A matter of considerable import in testing such hypotheses is the magnitude of change that could be affected as a result of changes in the environment. Many investigators examining this issue have been relatively pessimistic in their conclusions. Woodworth (1941, p. 26f) has noted that certain investigators have concluded that relatively large differences in environment are required to produce any substantial change in the I. Q. McClearn (1962) has also pointed out that the magnitude of the difference in

I.Q.'s attributable to environmental factors, though statistically significant, has been so minute as to be practically trivial. Burks (1928) in a classic study in the nature-nurture controversy, reporting findings later confirmed by Leahy (1935), concluded that "home environment contributed about 17 per cent of the variance in I.Q.... The total contribution of heredity...is probably not far from 75 to 80 per cent." This investigator made the general summary statement that,

> Home environment in the most favorable circumstances may suffice to bring a child just under the borderline of dullness up over the threshold of normality, and to make a slightly superior child out of a normal one; but it cannot account for the enormous mental differences to be found among human beings. (p. 308)

However, in support of the environmentalistic point of view, one can find instances where rather marked improvements in I.Q. have been reported following some type of environmental manipulation. (The reader is referred to the review by McCandless [1964] for perhaps the strongest statement in favor of the environmentalistic position. One thinks here of the Iowa studies [Coffey & Wellman, 1936-1937; Skeels, Updegraff, Wellman & Williams, 1938; Wellman, 1932-1933, 1934-1935, 1937-1938, 1938a] in which rather sizable changes in I.Q. have been reported.)

Other studies (Smith, 1942; Wheeler, 1942) have indicated that when a geographic area is subjected to social improvement such as better schools and improved communication, there is a tendency for the I.Q.s of all the inhabitants to improve. Smith's study gives an especially clear indication of the interaction between heredity and environment since he found that differences between racial groups continued to exist, suggesting that groups with the greater capacity would take greater advantage of improved educational opportunities than would the less capable groups. Wheeler's (1942) study of Tennessee

mountain children reported by Uzgiris has also been of considerable interest. Testing over 3,000 subjects in 1930, he found that I.Q.s progressively declined from a mean of 95 at age 6 to a mean of 74 at age 16. Testing a new sample ten years later, he found a mean increase in I.Q. of approximately 10 points at every age level. However, he again found a steady decline with age, from a mean of 103 at age 6 to a mean of 80 at age 16. This continuing decline is a mystery in light of the absolute improvement which was attributed to the general improvement in environment. As Jones (1954) has remarked in relation to these findings, "It is a little surprising, however, that the rate of decline in IQ is not affected by the changes which have produced a generally higher level" (p. 658).

There has been a certain inconsistency in studies that have attempted to relate I.Q. changes to environmental factors. In certain instances, significant correlations have been found between some subjective rating of the "goodness" of the environment and increase in I.Q. (Newman, Freeman & Holzinger, 1937; Thorpe, 1946). But in other instances no environmental correlates could be found to account for changes in the I.Q. (Bradway, 1945; Jones, 1954). Jones has given some especially striking case histories of children who have manifested either marked increases or decreases in I.Q. without any apparent environmental factors being involved.

A continuing problem has been our failure to designate just what constitutes a good environment for optimal intellectual development. Except that there is some consensus that the American middle-class home represents the ideal, very little work has been done on this problem. A related matter, of course, is the problem of defining cultural or social deprivation. At best, the social deprivation concept has been loosely applied to certain

events in early childhood which in turn are characterized as antecedant to certain social behaviors. The problem is that there is little agreement as to either the early events or the resultant behaviors. Clarke and Clarke (1960) have suggested that the major dimensions of childhood deprivations are: social isolation; cruelty and neglect; institutional upbringing; adverse child-rearing practices; and separation experiences across a wide range of severity. However, even such factors as these would need much further definition and clarification.

One other aspect of the cultural deprivation concept merits illumination and perhaps rethinking. Although the environmentalist emphasizes the plasticity and therefore great potential of man, there also emerges a picture of man as some sort of automaton helplessly enmeshed in his culture. There is the implicit assumption that the environment or culture shapes the man.

I (Zigler & Child, in press) have written at some length on the significance of the passive versus active organism view for our understanding of all developmental processes. The insistence of so many investigators (cf. Harlow, 1953; Kessen, 1963; White, 1960) on treating the child as an active agent, playing an important role in his own development, makes it impossible for us to content ourselves with any social mold theory (Homans, 1950; Wrong, 1961). Considerable work has now indicated that even in infancy, the child has a certain integrity and is an active agent in his interchange with the environment. Uzgiris reports to us the work of Lipsitt which again emphasizes learning and the infant's susceptibility to environmental events. However, much recent work on infancy has emphasized the importance of the very nature of the newborn child in determining his behavior when confronted with specific stimuli. For example, Peiper's research (1963) has made it clear that the behavior of the newborn displays much more integrity than had previously been supposed.

Kessen (1963), recently reviewing research on infancy, has also emphasized that the underlying conception has been changing from that of an undifferentiated, passive recipient of stimulation to that of an active, competent organism in reciprocal interaction with the environment.

In respect to cultural determination, as Jones (1954) has suggested, it is just as reasonable to assert that man makes his culture, and that the type of culture he creates is consonant with his psychological make-up. Given the phenomena of cultural inertia, lack of perfect mobility in any society, and economic factors over which the individual has little control, the most plausible view would appear to be one in which the relationship between culture and man's behavior is viewed as a complex interaction rather than one in which a culture simply creates people in its own image. This point of view has also been expressed by Goodenough (1940) in refuting the argument that the lack of schooling of southern mountain children was the only reason for their depressed intelligence. In reference to our New England forebearers, Goodenough commented, "They made schools, and it did not require two centuries of residence for them to do so. Accordingly, I find it hard to accept the idea that the low IQs of the mountain children are to be explained solely on the basis of educational deprivation. One is forced to ask: Why were they so deprived?" (p. 329).

In general, since many of the studies relating environmental manipulations to changes in I.Q., particularly the Iowa growth studies, have been enmeshed in controversy, there has been a tendency to discount them. The reader is referred to McNemar (1940) for a comprehensive critique of the Iowa studies. In discussing the controversy concerning these studies, Jones (1954) has concluded, "At the present time disagreement exists as to the extent to which errors of measurement, of experimental procedure, and of statistical treatment

may be responsible for results which have been so enthusiastically advocated as evidence that mental growth responds promptly and permanently to educational influences in the nursery schools" (pp. 681f). Jones (1954) and others (e.g., Burks, 1939) have pointed out that certain environmental events such as nursery school attendance, while perhaps not affecting cognitive development per se, may influence a variety of traits which are directly amenable to environmental influence and that are as essential as intellect in social adaptation.

A variety of studies have indicated improvement in I.Q. scores following foster home placement in which the child characteristically moves from a culturally deprived to a more satisfactory environment. While these studies have been subjected to a variety of criticisms, taken in toto they do indicate that the intelligence quotient increases as a result of being placed in a "better" environment. Here again one wonders whether such improvement is reflecting genuine intellectual development or changes in those myriad factors which can also influence a test score. Since this entire issue is a value-laden one, I would like to be most explicit on this last point. To the extent that I.Q. scores are related to social adjustment, then any improvement in the I.Q., regardless of what factors such improvement is reflecting, is both meaningful and of value. The author is thus not attempting to make an anti-environmentalistic argument. Rather, he is attempting to (1) insure that the reader never loses sight of the biological components of intelligence; (2) point out that certain psychological functions may be more amenable to environmental manipulations than others; and (3) highlight the issues that must be resolved before we will fully comprehend how environment affects both the intellectual and non-intellectual components of behavior. This last issue is perhaps the most

intriguing one in developmental psychology. It is my view that it has been treated too lightly by those in mental retardation due to their focus on attempts to demonstrate that a gross change in environment results in a change in performance.

However, the view that, given a fairly standard environment it is extremely difficult to improve the quality of cognitive functioning, is consistent with the bulk of findings resulting from efforts to improve children's performance on Piaget-type tasks. Of course, familial retardates do not come from what we consider standard environments. Even with these children there is considerable evidence that a variety of techniques result in no great improvement of intellectual capacity. The audience is referred to E. E. Doll's (1962) excellent history of mental retardation for evidence on this point. Binet, with his concept of "mental orthopedics," and Itard, with his great faith in the possibility of improving the quality of intellect, set the tone of the philosophy for the early work with retardates in this country. After several years of employing a variety of techniques, many of which are today being rediscovered, it became apparent that this optimism was unwarranted. In the early days, training schools in this country were just that. They became custodial institutions only when it became apparent that many retardates could not be trained to a level that would make them self-sustaining in the society at large. It was at this point in time that a reaction appears to have set in, and the view became dominant that we could do nothing for retardates except provide them with a comfortable domicile. There is much for contemporary workers to learn from this marked swing in attitude toward the retarded. It suggests that undue optimism is dangerous since it breeds undue pessimism.

The point which must be re-emphasized is that this evidence does not mean that the retardates' social functioning cannot be improved since such functioning depends on a variety of factors that are non-intellective in nature. It is true that we are periodically confronted with findings that report amazing changes in the I.Q.s of retardates (see Schmidt, 1946). But such reports have not proven to be terribly reliable. After a lifetime of work with retardates, Penrose (1963) warned us to be cautious about such claims and concluded that,

> The most important work carried out in the field of training defectives is unspectacular. It is not highly technical but requires unlimited patience, good will and commonsense. The reward is to be expected not so much in scholastic improvement of the patient as in his personal adjustment to social life. Occupations are found for patients of all grades so that they can take part as fully and usefully as possible in human affairs. This process, which has been termed socialization, contributes greatly to the happiness not only of the patients themselves but also to those who are responsible for their care. (p. 282)

I find myself in general agreement that it is within this area of socialization that we can do a great deal to enhance the everyday effectiveness of the retarded. Given his genetic orientation (in both the biological and developmental sense) he concurs that it is difficult to alter intellectual structures, per se. It is of more than passing interest, however, that both Burks (1939) and Leahy (1935) discovered that personality and character traits were more influenced by environment than was intellectual level. Such findings bolster the argument that there are many factors subject to modification which are important in the determination of social adjustment. It is not rare to encounter individuals with the same intellectual make-up demonstrating quite disparate social adjustments. In respect to familial retardates, the question that has motivated the author has not been how to improve the cognitive

functioning of such persons, but rather how to maximize the adjustment of such individuals whatever their intellectual capacity may be. But it is at this point in my thinking that I, too, don the environmentalist's mantle--making this an appropriate point to end my presentation.

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