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AN ABSTRACT OF THE THESIS OF Lynda L. McDonald for the Master of Arts in Psychology presented July 25, 1973.

Title: A Theory of the Creativity-Intelligence Interaction: An Environmental Suppressor Variable

APPROVED BY MEMBERS OF THE THESIS COMMITTEE

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Jack Lind

In the commonly held view, creativity and intelligence interact in a mutually enhancing way. Their interaction, however, is assumed to be slight and relatively unimportant, and to find its ceiling at a certain IQ level. Beyond this IQ ceiling, no interaction is believed to occur, and the two variables are assumed to be independent. It is suggested that this view and those theorists who hold it do little to explain the reasons for the ceiling effect.

An attempt is made to devise a theoretical system which accounts for and explains the ceiling effect, as well as providing new ground for the synthesis of existing experimental data from a wide range of related fields. The theoretical system is based upon the hypothesis that an environmental variable acts to suppress increased potential for creativity accompanying increases in intelligence level, and that this variable is able in effect to cancel the higher potential for creativity which may exist among those above the ceiling level of intelligence.

The research is reviewed in search of any support for or critical refutation of the hypothesis and its corollaries, and sugestions are made as to the possible mediators of the suppressor-variable effect. It is concluded that the suppressor-variable hypothesis is a valuable one--one which, along with its supplementary hypotheses, provides a useful means of bringing together widely diversified bodies of research data, and accounts for the ceiling effect without violating logical and intuitive conceptions of intelligence and creativity.

A THEORY OF THE CREATIVITY-INTELLIGENCE INTERACTION: AN ENVIRONMENTAL SUPPRESSOR VARIABLE

by

LYNDA L. MC DONALD

A thesis submitted in partial fulfillment of the requirements for the degree of

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TO THE OFFICE OF GRADUATE STUDIES AND RESEARCH:

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CHAPTER I

INTRODUCTION

If any culture is to profit fully from its members, or any individual to derive maximum benefit from his own potential, then the nature of intellectual and creative abilities must be fully explored. It is clear that we can only enhance and develop these mysteries if they cease to be mysteries and become definable phenomena, however complex the definition.

The importance of intelligence has not been overlooked by our culture---as is well attested to be our burgeoning interest in and even obsession with "The IQ" in our schools, businesses, and private lives. From even a casual examination of the intelligence literature, it can be surmised that we are rather more interested in our lack of intelligence than in our possession of it. Nevertheless, we have certainly not neglected placing emphasis on the importance of understanding (and accelerating) intellectual abilities.

Our preoccupation with creativity is relatively young. J. P. Guilford (1950) made the comment that only 186 titles had been published in Psychological Abstracts in the twenty-three years from 1927 to 1950 which could be indexed as relevant to the subject of creativity. That situation has drastically changed in the twenty-three years <u>since</u> 1950. A great amount of research and speculation has been done about creativity since then, and it has become an important concern. Still, as preoccupations go, creativity holds a definite second place next to intelligence, and many feel that the conceptual revolution which brought its significance to light has not been revolutionary enough.

The revolution has not gone far enough because it is difficult--more difficult than we find it to be for intelligence--to specify the importance of creativity. Creative processes are simply less clearly related to successful functioning within our culture. What do they mean, in terms of potential benefit for the individual, or for the culture?

For the individual, Torrance (1962) has suggested that creativity brings mental health, full functioning, and better ability to cope with stress, difficulty, and problem solving. Barron (1968) expressed the same sort of conviction: that creativity is somehow related to health and vitality, to "courage, resourcefulness [and] flexibility... (p. vi)." In short, it seemed to Barron that creativity was a culmination of things good for individuals to be.

> There is a rather strong ethical tone to some of these preconceptions, as I have suggested. But I would urge here that we must avoid any implication that the healthy person psychologically must necessarily be a good person morally. For the most part it is probably a healthy thing to be rather well behaved, and as a rule we are in better health when we are cool and collected. . . But there are times when it is a mark of greater health to be unruly and a sign of greater inner resources to be able to upset one's own balance and to seek a new order of selfhood (pp. 3-4).

Although it is certainly not the intention of this paper to take a position as humanistic in tone as does this passage, we do not argue with Barron's assertion that creativity is good for individuals. Of course, there are many different convictions as strongly held as this one about what it is good for people to be. None of them, including Barron's, can be anything but beside the point if they suggest behavior patterns which are "good" for individuals, but bad for the survival of their culture. That is an absolute which must be basic even to individual good, as cultural survival is basic to individual survival. And creativity is not of overwhelmingly <u>apparent</u> value to a cultural organism. As B. F. Skinner (1972b) comments,

> Many accidental cultures have been marked by uniformity and regimentation. The exigencies of administration in governmental, religious, and economic systems breed uniformity, because it simplifies the problem of control. Traditional educational establishments specify what the student is to learn at what age and administer tests to make sure that the specifications are met. The codes of government and religions are usually quite explicit and allow little room for diversity or change (p. 162).

Although most cultural institutions have always been, and still seem to be, unaware of the advantages of encouraging creative behavior in their members, the advantages do exist, and it is time they were recognized, along with the very real dangers of conformity. Skinner continues:

> If men were very much alike, they would be less likely to hit upon or design new practices, and a culture which made people as much alike as possible might slip into a standard pattern from which there would be no escape (p. 162).

The principle means of survival in an evolutionary system have always been adaptability and flexibility. Species have been able to survive because they were able to adapt to changing environmental circumstances. Man has survived and flourished chiefly because of his great flexibility and his freedom from fixed, reflexive behaviors. This is an absolute, a fact of and an explanation for his continued existence on this planet. It is good, in one of the few senses of the word "good" which can be universally agreed upon: its applicability to solutions for the problem of survival.

There is no reason to believe that the value of flexibility is not as real at a cultural level as it is at the biological level. For every culture there is a changing environment, and ours is no exception. How are we to adapt to physical and social changes if we, as a group and as individuals, have destroyed our capacity for flexibility? As Torrance (1962) points out, the "future of our civilization--our very survival--depends upon the quality of the creative imagination of our next generation (p. 6)."

In the face of this biological absolute of adaptability, our own culture seems to be intent upon eliminating the differences among its members, and the flexibility within them. Friedenberg (1959) argues that Americans began to use their school systems as a kind of homogenization process very early, because of a need to unify their melting-pot culture--and they have never recovered from the tendency, now that the need for it is past. Thus, creativity, originality, and individual differences are seen as inequalities in need of stirring up, not as assets and potential for cultural growth, flexibility and survival.

From a cultural standpoint, then, there is no need or possibility of avoiding an evaluative quality in our interest in creative behavior. We can often assume that when a behavior can be placed along a creativity continuum, it can be evaluated. And that "creative" equals "good." Of course, there are limits as to how much individual eccentricity is valuable for a culture, but these limits can be allowed for if we incorporate into our criteria for creativity some qualification of applicability or appropriateness.

A more serious danger, to which we must constantly be alert, is the assumption that what is a good degree of individual variation and creativity for a culture is the degree naturally recognized and reinforced by that culture. Unfortunately, the evolutionary and accidental nature of the development of cultures usually precludes foresight. Any culture may respond to its present environment by shaping its members in a way which will eventually mean its own destruction. This seems especially possible in a rapidly changing culture, where the future demands maximum flexibility, but the present shaping processes and structures are the result of a more stable time. We cannot trust to the wisdom of the cultural status quo. It may be our job to instigate a process of change—to begin encouraging creativity in the members of our culture so that it can survive, in spite of its own adherence to the shaping of noncreative behaviors.[1]

I. PRESENT VIEWS OF THE CREATIVITY-INTELLIGENCE INTERACTION

The need for creativity among the members of our culture, and the additional need for an increased emphasis upon creativity and appreciation for it, are well recognized here. They are indispensible and vital. But it should not require argument to assert that intelligence is also necessary for the full realization of cultural and individual potential. The assertion seems superfluous. Yet, there seems to be little willingness among intelligence researchers to stipulate the importance of creativity, and vice versa. Their natural dichotomy of interests has become a polarity of viewpoints, and then a battlefront of values. Torrance (1962) went so far as to suggest that in its traditional conception,

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[1]We are here assuming that creativity and other behaviors can be reinforced or shaped by the cultural environment—an assumption which will later be demonstrated.

intelligence is a kind of conformity which is actually antithetical to creativity (although he did admit that both kinds of thought are necessary for survival).

Does it come to that? Are they, in fact, that different? As Barron (1969) has suggested, it would really be much more in line with our expectations if intelligence and creativity turned out to be very closely related--and if those we suspected of great intellectual gifts were also creative, without exception. This does not prove to be the case. Even Barron concludes that, although creativity and intelligence have a low, positive correlation with each other over most of their range, there is a ceiling effect in their interaction.

> Another way of putting this is to say that for certain intrinsically creative activities a specifiable minimum IQ is probably necessary in order to engage in the activity at all, but that beyond the minimum, which often is surprisingly low, creativity has little correlation with scores on IQ test (p. 42).

It is very widely accepted that the data from studies of the creativity-intelligence interaction are best described as a ceiling effect --although some theorists emphasize the independence of the two variables to a greater degree. But the fact of the ceiling effect is a difficult one to accept. For, if intelligence contributes to creativity at lower levels, why does it act completely independently at higher levels? It is not adequate to say that any amount of intelligence is simply enough intelligence for creativity. Whatever it is that intelligence provides which is necessary for creative processes, it could automatically be argued (at least) that this variable does not arrive in chunks of "enough" or "not enough"--that if some of it enhances creativity, more of it might continue to enhance creativity. Where there is a reason for an inter-relationship, it is essential that the reasons the relationship ceases to function be specified.

In spite of its difficulties, let us examine evidence of the existence of the ceiling effect. Most of the evidence we have to present provides strong support only for its first assumption: that the overall relationship between intelligence and creativity is a weak and positive one. For example, the highest correlation with IQ among the Getzels and Jackson (1962) battery of creativity tests is .38 ("word association"). Torrance (1959), in a review and summary of much of the creativity-intelligence correlational literature, reported that the median correlation between the two variables was .20, with the median among verbal tests of creativity with intelligence at .21 nonverbal creativity correlated at merely .06 with intelligence). In his own analysis of existing data, Barron (1969) estimated that the average correlation was .25 between IQ and imaginativeness or originality. In our own review of obtained intelligence-creativity correlations, few exceptions to the expected ranges of .20-.30 have been found (for typical examples, see: Saugstad, 1952; Schlicht, Anderson, Helin, Hippe, Listiak, Moser, and Walker, 1968; and Getzels & Jackson, 1962).

But there are exceptions to this .20-.30 range which seem to prove the second assumption of the ceiling-effect description: that after a certain IQ level, creativity is not enhanced by increases in IQ. For example, an Institute of Personality Assessment and Research (IPAR) study, using a group of creative architects as subjects, obtained a correlation of -.08 between creativity measures and the Concept Mastery Test (Barron, 1969). Two other groups of architects were chosen for comparison with the creative group—one randomly, and one matched for age, background and experience with the creative group. The creativity level of the creative group was higher, even though the mean IQ's of all of the groups were within one point of 130 (Wechsler Adult Intelligence Scale, or WAIS). These results seem to indicate a ceiling for increases in creativity with increases in intelligence at about IQ 130. The creative architects were of the same average intelligence as the other groups, and among themselves showed no net correlation between IQ and creativity.

However, the generality of these results is limited, not only because of the narrow range of creativity found in the group of what Barron describes as very "distinguished" architects, but also because of the artistic nature of their profession, which would automatically limit the lower ranges of their creativity at a relatively high level. It should also be pointed out that correlating ratings of professional creativity with intelligence may as a general rule involve significantly stronger threshold effects, because of the basic intellectual qualifications required for different professions. This may not be the case with "creativity" as opposed to "creative architecture" as the focus of attention. The professional creativity rating has been Barron's purview, and he therefore emphasizes the variability of ceiling levels. This variability in where the ceiling effect is to be expected in any particular group of subjects does not obscure the fact that ceiling effects are certainly evident in the work of Barron, and of IPAR.

Other investigators, who do not have this particular emphasis, place the ceiling at a more or less constant level of intelligence (for example, Terman and Oden, 1947).

> But the overwhelming body of research indicates that the ceiling effect does occur. Correlational studies of intelligence and creativity yield results in the neighborhood of $r = .2-.3$ when a wide range of intelligence levels is used. But when the intelligence range is narrow and the overall level is high, zero or negative correlations are the rule (MacKinnon, 1959; Holland, 1961; Taylor, Smith, Ghiselin, and Ellison, 1961--all reviewed in Taylor and Holland, 1964; see also Ripple and May, 1962).

The idea that intelligence and creativity are independent of each other above a certain level is supported to a certain extent by the literature dealing with creativity, intelligence, and achievement. Getzels and Jackson (1962) found that an extremely high-IQ group had achievement levels in various subjects no higher than those of a less intelligent (but still bright) group who were also high in creativity. The conviction of Getzels and Jackson seems to be that high creativity and extremely high IQ are equally effective, independent factors which enhance achievement. Their results were replicated, and similarly interpreted, by Torrance (1962).

Anderson (1960) formulated a view similar to that of Getzels and Jackson about achievement, with a few important exceptions. As tested and summarized by Cicirelli (1965), this view hypothesizes that "IQ has an effect on academic achievement up to a certain threshold IQ level, where further increases in IQ would have no further effect on achievement but where creativity would begin to have an effect (p. 305)." If achievement is seen as a measure of effectiveness, then at low levels of intelligence IQ is the critical determinant. At medium levels, both IQ and creativity interact, enhancing each other and increasing effectiveness. At high levels, creativity enhances effectiveness, while further IQ increases do not. This hypothesis would not validate the description of the creativity-intelligence interaction as a ceiling effect, since it only treats their interaction with respect to achievement. However, it does put emphasis upon creativity as a critical factor capable of altering intellectual effectiveness independently of intelligence (like Getzels and Jackson). Further, it harmonizes with Torrance's (1959, 1962, 1964, 1965) view that creativity is simply a different (and perhaps superior) kind of intellective process.

Cicirelli's extensive testing of the Anderson threshold hypothesis did not find, however, that it was supported by the evidence. This might have been expected, considering Hollingworth's investigations of so many years before (Hollingworth and Cobb, 1928). She had compared groups of gifted children with high (146) and very high (165) mean IQ's, and found significantly higher achievement in all different content areas among the group with extremely high IQ's. Further refutation of the Anderson hypothesis is reported by Cicirelli. He cites an unpublished work by Pielstick (1963), who found an actual reversal of the Anderson prediction. According to the Pielstick results, "The correlation between creativity and achievement decreased as IQ increased (Cicirelli, 1965, p. 304)."

Of what significance are the combined results of Hollingworth, Cicirelli, and Pielstick? Although they neither confirm nor deny the validity of the ceiling effect in creativity-intelligence interaction, they may indicate a direction we might take in searching for the factor <u>responsible</u> for the ceiling. They present us with a question: if achievement increases throughout all levels of intelligence, but is not continuously enhanced--and may even be negatively affected--by high levels of creativity, why should the highly intelligent child behave creatively? Certainly creative behaviors would not gain him a larger portion of the reinforcement accruing to achievement, and they might even lose that reinforcement for him. Perhaps, then, there is some social variable which is suppressing increases in creativity with increases in intelligence at a very high level.

II. THE SUPPRESSOR-VARIABLE VIEW

Observations by Getzels and Jackson (1962) and Torrance (1962) support the idea that a social variable suppresses creativity among the highly intelligent. They found that, all other things being equal, teachers prefer high-IQ students with low creativity to less bright students with high creativity---even if their achievement levels are equal. Further, Hollingworth asserted that mentally gifted subjects with IQ's over 150 have significantly more adjustment difficulties than subjects within the 130-150 range (Hollingworth and Cobb, 1928; and Hollingworth, Terman, and Oden, 1940). Apparently, at the 150-IQ level, the well-documented social advantages of high intelligence are superseded by some less familiar disadvantages.

The Getzels and Jackson, Torrance, and Hollingworth observations all hint at social pressures acting upon the highly intelligent to discourage or stifle their creativity. It seems to be the result of something more than chance that the ceiling level in creativity increases among the highly intelligent finds such a close analogy in the ceiling level of optimal social adjustment. It is impossible to avoid the suggestion that mounting social and interpersonal difficulties, expectations, and pressures might singly or collectively constitute a suppressor variable which inhibits increased creativity among the highly intelligent.

A hypothetical system based upon the suppressor-variable concept would be well able to account for the ceiling effect in the creativity-intelligence interaction. Increases in intelligence could produce increases in creative potential, but at the same time precipitate slowly accelerating increments in the effects of certain socio-environmental variables having a tendency to decrease creativity. At some level, these socio-environmental factors, labeled together under "suppressor variable", would produce effects equal to those of the increased creativity potential. From that IQ level upward, there would be no net positive correlation between intelligence and creativity. A ceiling would have been reached. Modifications of this hypothesis can account for the low positive correlations between intelligence and creativity, as well.

What are the options open to us if we do not accept the suppressor-variable hypothesis? We can choose among a number of closely related points of view which are currently in vogue:

1. Creativity and intelligence are essentially independent factors of intellective ability (Guilford, 1967; Wallach and Kogan, 1965a, 1965b; Galanter, 1967; no specific ceiling referred to).

2. Creativity and intelligence are slightly correlated, but mainly independent, factors of intellective ability (Torrance, 1962, 1965; Getzels and Jackson, 1962; all placed here by virtue of their findings; no ceiling referred to).

3. Creativity and intelligence are correlated over their lower ranges, and uncorrelated above a specifiable or variable ceiling (Barron, 1968, 1969, and others).

The first and second viewpoints are derived essentially from Guilford's (1967) model, and assert that intelligence as it has traditionally been viewed is convergent, conforming and reproductive in nature--while creativity is essentially the opposite: divergent, constructive, and non-conforming.[2] Both viewpoints, then, point to what Torrance (1962) called the "antithetical" quality of creativity and intelligence. The qualification that slight correlations may be found is added by those investigators who happened not to obtain insignificant correlations in their research. Both of these positions have two difficulties in common.

First, if the two "ability factors" are so vitally opposite in nature, how can it be that they occasionally coincide in a single individual? Wallach and Kogan (1965a, 1965B) and Getzels and Jackson (1962) did find groups of children high in both creativity and intelligence, and the undeniable reality of men like Einstein must in some way be dealt with. Further, the correlations obtained by Getzels and Jackson between intelligence and the creativity-test battery were greater than those obtained among the creativity tests themselves. Although intelligence did not account for most of the variance in the creativity tests, it was, nonetheless, the most constant indicator of creativity in a wide variety of test contexts. How, then, can we say that the two variables are independent?

Second, although Getzels and Jackson emphasize heavily the differential environmental phenomena in the homes of creative and noncreative

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[2]Convergent thought is understood to be a process in which one single, correct response can be derived from the information given, while divergent thought is a process where the information presented is elaborated or extended in any number of possible and satisfactory directions.

children, and discuss at length the creativity-stifling aspects of our culture, they seem not to recognize the implications of these environmental variables. It should be clear that creativity may be largely environmentally determined, from this data. Yet the authors, in such theoretical positions as they take, treat creativity always as simply a different aspect of intellectual ability. Wallach and Kogan seem to avoid implications of their data, as well--findings of important personality distinctions between creative and noncreative groups, for example.

Although the third position avoids many of these difficulties by introducing the ceiling concept, it has one major problem: it does not provide an explanation for the ceiling phenomenon. All of the positions mentioned would no doubt have to adopt some kind of ceiling-effect qualifications, yet the others are no more able to explain it.

And this is the great failing of all of the work that has been done in the area of creativity research. There have been many attempts at describing the interaction of creativity and intelligence in terms of more or less statistical concepts (independence, ceilings, etc.). But there has been no systematic theory proposed to account for the phenomena once they have been described. We have been told that creativity and intelligence react in a certain way. But we have never been told, in more than hints and implications, why it is that this is their manner of interaction.

III. OBJECTIVES

In this paper, then, we have two primary purposes: first, to develop a theoretical system which can to some extent explain the data resulting from the interaction of creativity and intelligence, and to use that system to construct predictions of the relationship of creativity and intelligence to other variables; and, second, to arrive at a hypothesis for the creativity-intelligence interaction which is in better logical and evidential harmony with the entire body of the literature than are any descriptive systems at present.

It is proposed that the data already in existence, if brought into a consistent and logical synthesis, would provide considerable support for and delineation of a new theory of creativity and intelligence and fulfill the primary objectives we have stated. At very least, an attempt at such a synthesis would sharpen the focus of experimental efforts, which are at present scattered in many different directions--showing a strong tendency to be redundant in exploring certain fields which would provide little new information even if decisively defined, while being strangely lacking in other areas which seem to be of critical significance.

The suppressor-variable hypothesis we have suggested will be the tentative foundational structure of the new theory of creativity-intelligence interaction. We shall use it as a guide--for a fresh organizing principle for existing data--and hope that it reveals nuances in the evidence which have not as yet been appreciated. We also expect that the organizational principle itself, the suppressor-variable hypothesis, will lead us to suspect structure where none may be visible--that we can be led to look at important data which may not as yet have been considered relevant to the creativity-intelligence investigation.

We intend to follow the following general pattern in organizing our efforts:

1. We shall define intelligence, in order to provide ourselves with some valid basis for accepting intellectual measures as significant to our study, and in order to point out, in the very nature of intelligence, factors which support or contradict our hypothesis.

2. We shall define creativity, in order to provide ourselves with the tools for comparing its processes to those of intelligence; and we shall examine whether or not it can be treated as a global behavior pattern, or must be seen as a collection of environmentally determined responses. Throughout this section, our concern will be to show whether or not there is any characteristic intrinsic to creativity which would make it incompatible with intelligence.

3. We shall compare creativity and intelligence at a cognitive level, attempting to show that they have a common basis in associational processes--but also searching for the variables which differentiate between creativity and intelligence in an individual.

4. We shall discuss arousal or excitation, exploring the possibility that increased arousal levels among the highly intelligent might either facilitate or inhibit creativity among them.

5. We shall present evidence on inhibition, demonstrating the close coordination of excitatory and inhibitory processes in all higher cognitive operations, and attempting to discover if innate capacities for inhibition and/or delay of gratification can be responsible for the suppressor variable.

6. We shall investigate the literature on anxiety, intelligence, and creativity, still in a search for variables which might be responsible for the suppressor-variable effect--which might discriminate between creativity and intelligence.

7. We shall discuss "styles" of perception and cognition, the more or less stable patterns of information intake we might expect among the highly intelligent, and the possibility that these might interfere with creative processes.

8. We shall look at specific environmental effects capable of mediating a suppressor-variable effect--those social or cultural stimuli with a high probability of occurrence among the highly intelligent which might tend to inhibit creativity.

9. We shall present a concluding statement of the suppressor-variable hypothesis--attempting to derive as many implications and/or predictions from it as possible, and to form these implications and predictions into a theory of the creativity-intelligence interaction.

CHAPTER II

INTELLIGENCE

E. G. Boring once provided us with a definition of intelligence which has survived fifty years not so much by its own dubious merits as by the faults of its various competitors. "Intelligence," he asserted, "is what the [intelligence] tests test (Boring, 1923, p. 35)." This definition is operational, at least in spirit, but serious students of the nature of intelligence have never been able to find complete satisfaction within its circularity. Boring's statement may still be the best we can say about what intelligence is, but we can at least attempt an understanding of the meaning of intelligence from several and various other points of view--with the hope that we can provide for ourselves an intuitive and logical basis for Boring's definition.

Matarazzo (1972) points out that in the process of defining any theoretical concept, several major steps must be taken. First, we must grasp for the subjective essence, the intuitive and personal meaning of the concept. Then must come a stipulative definition, followed by a low-level operational definition. A thorough research effort at that point results in concurrent validation, exemplars, or correlates of the concept, which lead to a second operational definition of considerably more predictive power than the first. This process continually repeats itself, and new evidence in validation or refutation constantly causes changes or elaborations in the operational definition. It is only after much reshaping and confirmation that the definition has a high degree of reliability.

Historically, it was for Alfred Binet to posit the first stipulative definitions, derived from his own, intuitive grasp of what we all mean when we speak of intelligence. These are some samples of his attempted definitions, over a period of eighteen years:

> Intelligence, that is to say, reasoning, judgment, memory, the power of abstraction (as cited in Matarazzo, 1972, p. 65).

> That which is called intelligence, in the strict sense of the word, consists of two principal things: first, perceiving the exterior world, and second, reconsidering these perceptions as memories, altering them and pondering them (as cited in Matarazzo, 1972, p. 66).

> The tendency to take and maintain a definite direction; the capacity to make adaptations for the purpose of attaining a desired end, and the power of autocriticism (as cited in Terman, 1916, p. 45).

> A subject has the intellectual development of the highest age at which he passes all the tests. . . (as cited in Matarazzo, 1972, p. 67).

With this last statement, Binet moved from the realm of the intuitive expression to an operational definition. He suggested in it that intelligence is indicated by the level of the tests passed. That is, intelligence is what intelligence tests test.

All operational definitions have the same advantages and disadvantages, and this one is no exception. If other psychologists can agree that their intuitive definitions are satisfied by the kind of test Binet proposes to use as the operational criterion, then they can agree upon the criterion. For many years, intelligence tests have been used as if this kind of agreement had already been reached. In actuality, however, there has never been a solid agreement among theorists about the intuition-level definition of intelligence. The foundational steps have been hastily and badly built, and simply do not support the superstructure of the operational definition. In this chapter, we shall investigate three of the areas of division which have prevented consensus about the theoretical definition of intelligence in the years since Binet: the problem of the uni- or multi-factorial nature of intelligence, the problem of the hereditary or environmental dominance of intellectual development, and the problem of whether or not higher mental processes are qualitatively different from lower ones.

I. THE NATURE OF INTELLECTUAL FACTORS

While the intelligence test rose to a high level of popularity, it remained a matter of disagreement whether or not intelligence was a unitary "thing" to be measured, at all. If intelligence is not a unitary phenomenon, we might certainly ask ourselves what it is that the intelligence test measures, and question the validity of its measurement.

Guilford (1967) argued that intelligence was not unitary at all, and that it should be considered only in terms of individual and independent factors. He had broken away from the Spearman group, who were convinced of the existence of at least a general factor of intelligence. The general factor was assumed to appear in every intelligence test, although each test would be affected by its tapping of different specific ability factors (Spearman, 1904). Guilford disagreed.

The proof that there is no general factor of intelligence must begin, Guilford (1967) suggests, with the indisputable variability of abilities we find within individuals. Even retardates can have significant musical ability, and subjects can show unusual competence at remembering colors only to be deficient at sentence memory. Another support for placing emphasis on the factorial nature of intelligence is the evidence that during childhood growth and during mental decline of old age, various aspects of intellectual functioning increase or decrease differentially. Guilford also alludes to the lack of perfect, unitary correlations among intelligence tests. No one, of course, has ever been able to argue successfully that intelligence has no factor character at all--that there could not be some aspects of intellectual ability more easily manifested in one test than in another. Guilford, however, places a heavy emphasis upon the factorial nature of intelligence, to the exclusion of any general factor.

The very basis of Guilford's Structure-of-the-Intellect (SI) model is his factor analysis of data from many administrations of ability-test batteries. When analyzing data according to the content of the tests involved (verbal, figural, etc.) he seemed to find factors in the data corresponding to these content areas. On the other hand, factors also seemed to appear when the analysis was based on the kind of cognitive operation performed on the test, or the kind of product achieved in the answering process. Guilford thus had three different dimensions through which he could successfully factor-analyze intellectual processes--three continua along which to arrange specific abilities. He could not resist the temptation to put his three continua into a three-dimensional form (a cube) alleged to represent the total human cognitive structure, and to call each of the 120 cube cells an intelligence "factor." The descriptive definition of each cell-factor was an arbitrary result of which three of the dimensional factors intersected in that cell. The cells were thus assumed to be the elemental factors of the intellectual processes. Many of these specific factors (about eighty-two) had been or have been discovered and accounted for by Guilford, but only twenty-three are claimed by other investigators (Cronbach, 1970). Some are merely assumed to exist as actual factors because the cube model predicts them, and others have been found to be dual instead of unitary in nature as they had been assumed to be. Twenty-four factors correspond roughly to "creativity," and thirty to interpersonal or social response patterns—two ability areas which have classically been excluded altogether from the intelligence question.

Guilford's SI system is arbitrary, unwieldy, and overwhelmingly complex. But that should not be the basis of its out-of-hand rejection, if the model is really an accurate picture of human cognitive structure. There are, however, some valid reasons for caution in regard to the model, not the least of which is that the system is based on the validity of the factor-analysis technique.

Cronbach (1970) comments: "Guilford's factor analyses are designed to fit the data to his hypothesis; they do not tell whether his complicated scheme is necessary (p. 339)." In point of fact, suggests Cronbach, factor analyses may conceal the true nature of the problem:

> Guilford's is a fine-grain analysis, and fine-grain analyses are not necessarily useful. The photoengraving process of the newspaper breaks a photograph down into minute gray and black dots; when we look close enough to see that detail, we lose all sense of what the picture is about (Cronbach, 1970, p. 341).

Matarazzo (1972) is perhaps even less kind, implying that results on the order of those Guilford obtained with intelligence-test data have been duplicated with random, fictitious data, using his methods. Matarazzo argues further that even if the factors Guilford derived from his data were "real," they would not necessarily correspond to the title he attached to them. There is no way of ascertaining that a test known to load very heavily on a single factor is actually testing any certain ability. We can only assume that any ability is represented by our factor loading because it has a sort of face validity in respect to that factor. So, with Guilford's system, we may well have come full circle to the original problem: how do we know that what we test is what we think we are testing?

Of course, in spite of all objections to Guilford's approach, there can be no denial that there is a factorial character to intelligence, as even Binet was well aware:

> What we call our mind, our intellect, is a group in internal events very numerous and very varied, and. . . The unity of our psychical beginning should not be sought elsewhere than in the arrangement, the synthesis--in a word, the coordination of all these incidents (as cited in Matarazzo, 1972, p. 66).

Binet, although allowing for the existence of factor trends in the phenomenon called intelligence, placed emphasis upon the interaction of those trends, rather than their independence. This is one way to solve the factor problem. We can take a point of view outside the organism, and decide that we are interested in the total relative efficiency of the individual, as compared to his peers. An individual's abilities may come in factor packages. But even if they do, the great difficulty of constructing tests which tap one factor and only one, demonstrates that human beings seldom use one factor at a time. Human interaction with the environment is much more complex than that. It does not seem unreasonable, then, that a test with several well-chosen subtests could be quite an accurate reflection of an individual's total relative efficiency and adaptability within his environment. As Wechsler (1958) writes,

> Intelligence, operationally defined, is the aggregate or global capacity of the individual to act purposefully, to

> think rationally and to deal effectively with his environment. It is aggregate or global because it is composed of elements or abilities which, though not entirely independent, are qualitatively differentiable. By measurement of these abilities, we ultimately evaluate intelligence. But intelligence is not identical with the mere sum of these abilities, however inclusive. . . The ultimate products of intelligent behavior are a function not only of the number of abilities or their quality, but also of the way in which they are combined, that is, their configuration (p. 6).

In support for his position, Wechsler points out that anyone with a clear excess of one particular ability is not really much better off, in terms of his total relative efficiency. The standard intelligence test should generally be able to avoid hypersensitivity to such excesses, and remain an adequate indication of the individual's overall probability of successful environmental dealings.

Is intelligence not, then, a "thing"? We have argued for the general validity of the intelligence test, even should intelligence prove not to be a unified entity, but rather an interaction of factors. But must we accept Guilford's argument that it is <u>only</u> factors and nothing more, however interactional?

Evidence produced by Alexander (1935) indicates that the answer to that question is very complex. He found that intelligence test results could be largely explained by the presence of a common factor, but not entirely so. There were also evidences of several broad, fairly-independent factors (verbal, practical, and so on), but these were definitely correlated with each other. Presumably, this intercorrelation accounts for the substantial intercorrelations among intelligence tests. In addition to the broad factors, some of the variance in Alexander's data could only be accounted for by introducing some kind of non-intellective, perhaps motivational, variables. Both Spearman- and Guilford-type premises were in part supported, although the general factor idea came out a little better than we might have expected. It seems that when we administer an IQ test, we are measuring some entity, plus some entities (factors) and their interaction, plus some personality or motivational variables.

Although we have attempted to find a workable synthesis of the unitary and multiple factor theories, it is just possible that when we discuss whether or not intelligence is a unitary entity we are missing the point entirely. Perhaps our treatment of the second major area of controversy in intelligence theory will show what exactly can be used among the evidences we have already examined.

II. THE BASES OF INTELLIGENCE: HEREDITY AND ENVIRONMENT

This second kind of fragmentation faced by intelligence theorists is the source of most of the controversy engendered by intelligence research over many years. No matter what our position on the question of factors, it is inevitable that we ask ourselves what is the original source of intelligent behavior. At some level that source must be physiological in nature, and if this is true, then physical heredity becomes an inescapable concern. It is of great significance in our understanding of intelligence to be able to separate its hereditary and environmental aspects.

Again, we are dealing with a problem having two-fold implications. If there is a question about the hereditary/environmental composition of intelligence, then there is a question about the very nature of intelligence—about its theoretical or stipulative definition. Those who believe that intelligence is based in hereditary, biological phenomena must necessarily have a different intuitive understanding of it than have those who believe it to be the result of environment and learning. But then, if there is a question about theoretical definition, there is also a question about the operational definition which should result from theory.

The past fifty years have seen a fierce and open war, where correlation coefficients have been thrown swiftly back and forth between those who believe that IQ tests test environmental effects upon the individual and those who believe that they test hereditary effects. These two factions have perhaps made one significant error. They have both concentrated their efforts on proving or refuting the operational definition of intelligence (the IQ), without ever having come to agreement on a theoretical definition. Perhaps it was at the first step that they should have begun.

In fact, it has been suggested that many of the difficulties psychologists have been having with the concept of intelligence stem from the fact that they are talking about two very different kinds of intelligence: the innate, and the learned (Hebb. 1949). Those who have made clear this distinction at the level of the theoretical definition, have had, it seems to this writer, much better success in accounting for the data and moving on logically to the operational level.

Piaget (1967, 1969) has not really been involved in the mainstream of the intelligence investigation, except as it played a part in his understanding of early cognitive development. His ideas have proved most helpful to others, however, in spite of the fact that he himself had no need to fit his ideas of intelligence into existing theories, nor to move on to operational criteria which in any way differentiate individuals according to intelligence.

In brief, the Piagetian system sees the neonate as an organism capable of exercising "hereditarily determined sensory and motor co-ordinations that correspond to instinctual needs (1967, p. 9)." These are the rudimentary sensori-motor schemata or cognitive structures, which enable the infant to assimilate sensory information. But as the child performs these basically rigid behaviors, generalizing them to stimuli for which they were not originally responses and modifying them through the coordinating effect of repetition and the shaping of environmental contingencies, he is doing something more than assimilating. He is accomodating his own system to his environment, elaborating and building upon the rudimentary schemata.

> These elementary schemata are then differentiated into new motor systems (habits) and new perceptual organizations. The point of departure for this differentiation is always a reflex cycle. This cycle does not, however, merely repeat itself. It incorporates new elements and together with them constitutes broader organized totalities by means of progressive differentiation (1967, pp. 10-11).

For Piaget, "intelligence" is manifest in any behavior which is instrumental in achieving a pre-established goal. Thus, it is only the mental structure and activity which result from the interaction of the child's environment and hereditary schemata that can be called intelligence. The hereditary capacities themselves are not intelligence, but presumably they have significant influence on the intelligence which does result. Here, our interest in Piaget reaches its limit, for he has no particular interest in understanding individual differences.

This idea that intelligence is a product of the differentiation of innate potential through interaction with the environment is partly adopted by Cattell's (1963) theory of abilities. Cattell differs from

Piaget in that he labels both potential and manifest abilities "intelligence." But he does distinguish between them. His "fluid" intelligence is almost literally a reservoir of innate, neurological, and physiological capacity which is basic to all intellective behavior, but is dominant in those behaviors not specifically learned—that is, not having the nature of a skill. "Crystallized" intelligence, on the other hand, is intellective behavior in which a skill has been learned in specific response to a stimulus situation. Crystallized abilities do have some relation to the individual's original fluid intelligence, but they are more importantly affected by environmental and/or cultural influences. Furthermore, it is Cattell's contention that fluid intelligence can and does continue to affect behavior throughout the life of the individual. The traditional intelligence test is, therefore, a mixture of fluid and crystallized abilities, and Cattell feels it unsuited for the task of defining intelligence. He argues that tests can be constructed which will tap each kind of intelligence: a "culture-free" test for fluid ability, and many tests specifically responsive to various crystallized abilities (factors).

Cattell's system, if accepted, would account quite well for existing data, making allowance for factorial as well as general characteristics inherent in intelligence, and at the same time coming to some kind of terms with the heredity/environment problem. There are, however, several difficulties in accepting his conclusions.

First, certain very specific abilities do seem to have very little relation to over-all reasoning ability or intellectual efficiency (for example, rapid color-naming; see Cronbach, 1970—or foreign language achievement; see Guilford, 1967). There is no difficulty here since these phenomena are in harmony with Cattell's de-emphasis of fluid intelligence as a determinant of the crystallized abilities. But many specific abilities are strongly related to more general intelligence measures, and that is the source of the difficulty. It would seem that Cattell does not give enough weight to the possibility that specific abilities may be quite dependent upon the general fluid ability. In this respect, a modified version of the Cattell theory, like the hierarchical model proposed by Cronbach (1970) and synthesized from the systems of Cattell, Vernon, and others, would be more acceptable.

A second objection to the Cattell system is that it is somewhat difficult to accept his assertion that a test can be truly culture-free —that is, that a test can be a pure and direct measure of fluid ability. It is difficult to conceive of <u>any</u> situation in which absolutely no social learning or concept learning would have effect—even more difficult when one considers that a test of fluid intelligence would have to be without associations to previously learned-about stimuli for all, or almost all, of the people tested. As Piaget (1969) considers learning, every stimulus-assimilation process is modified or elaborated in some way by experience. Acquired strategies for dealing with the environment are most likely to be interwoven in a highly complex way and drawn into even very novel situations by their interrelationships. In sum, it is difficult to accept the contention that bare, fluid intelligence is any where exposed for us to see and test it.

Hebb (1949) seems to provide an answer to these particular problems with a few significant differences in the structure of his theoretical system. He differentiates "intelligence <u>A</u>," which is one's innate, physiological capacity for intelligent behavior, and "intelligence B," which is "the functioning of a brain in which development has gone on, determining an average level of performance or comprehension (p. 294)." In all persons except the neonate, development has occurred, and any overt manifestation of intellective behavior must necessarily be classified as intelligence B. Even in the neonate, the variables responsible for intelligence A, while not contaminated by experience, are most probably inaccessible to observation. Such things as ease and strength of neuronal associations, potential number of associations, neural metabolism, reticular activity, vascular efficiency, and neuron-count, which might well be the independent variables represented conceptually as intelligence A, have no known relation to infant behavior. Thus, they are of little value as measures at this time, and intelligence A remains out of our reach.

Intelligence B is the interaction of the individual's potential for thought and reason with his learning and environment. A high degree of intelligence A is a necessary, but not sufficient, precursor of high intelligence B. Intelligence B can be externally limited by disease or poverty of stimulation. But in most cases, human beings have very similar stimulation levels during development, and minimal brain damage (see Hayes, 1962, for supporting arguments). And because of the high probability that individuals have similar learning opportunities in the most basic and essential aspects, intelligence B should be in approximate proportion to intelligence A in almost all individuals, and provide a valid indicator for it.

> It has not been realized that if the effects of early experience are more or less generalized and permanent one can concede a major effect of experience on the IQ and still leave the IQ its constancy and validity as an index of future performance (Hebb, 1949, p. 295).

It is important to point out the phrase "if the effects of early experience are more or less generalized." We cannot necessarily expect individuals of radically different cultures to perform at the same level of efficiency on a test designed by one of their cultures. Nevertheless, within any culture it is possible to find an individual's intellectual efficiency relative to his peers. That does not mean that any test will be free of cultural bias, or that any manifest ability is untouched by environmental influence---but it does mean that an individual can be validly tested by his own culture, and perhaps in his own cultural idiom by someone from another culture.

In order to get the best possible prediction of an individual's probability of success in a future environment, one should tap the behaviors most directly reflective of intelligence _A_. That means that an intelligence test should avoid areas where there is a high probability of overlearned responses (which represent grossly unequal learning opportunities) and certain other abilities which correlate badly with general mental efficiency---much for the same reasons Cattell wished to avoid them. In the present framework, however, we are attempting only to get the most accurate indication possible of intelligence _A_: we do not purport ever to achieve direct measures of intelligence _A_.

The Hebbian system allows us to consider essentially all manifest intelligence as learned behaviors and thus is able to handle those theorists who continually emphasize its cultural nature. Attempts at calculating the number of IQ points attributable to environment are really quite beside the point, since it is very likely that _all_ our manifest intelligence (IQ points) is learned. Judd (1928) is no doubt justified in criticizing this kind of activity. He also points out that much of what individuals are able to learn is the result of their capacity for symbolization and language. Language and symbols are a means for cultures to accumulate learning and to pass it on to the next generation of individuals. Without them, we should have to rediscover basic principles during each generation, and would appear much less intelligent than we now do in the tests designed for our civilization. In this sense, many IQ points in any individual could be attributed to the Arabs, who gave us a number system, or to other previous cultures, whose learning was efficiently passed on to us through the medium of interpersonal communication.

Hayes (1962) makes the same point, arguing that thought is possible without language, but such thought is unlikely to be highly productive. Language enables men to process and manipulate, store, and use information they have not individually acquired through experience, but have had accumulated and given to them by present and previous cultures. "The efficiency and flexibility of language appear to be essential for anything approaching even the simplest of recent human cultures . . . (p. 327)." Most of any individual's adult knowledge, then, is culturally determined and transmitted, some of it is gained through personal experience, and all of it is learned.

Intelligence is a concept invented to account for differences between individuals within a century, within a culture. Although we may wish for sophistication in our understanding of it (and can reach some sophistication through consideration of cultural variables), it remains a concept tied to the problem of individual differences within a culture. It is most productive as an abstract representation or label for differential efficiency at learning certain behaviors among individuals with roughly the same learning opportunities. In Fisher's (1969) proposed definition (at an intuitive level), "Intelligence refers to the effectiveness, relative to age peers, of the individual's approaches to situations in which competence is highly regarded by the culture (p. 669).

It should be pointed out that this intuitive definition could quite easily rely upon the intelligence test as its operational criterion. This time, however, in accepting the intelligence test as the operational criterion of intelligence, we have included consideration for the fact that it is a cultural construction, which will test an individual's ability to respond to cultural demands.

The cultural demands of an intelligence test are more intellectual than behavioral, and more molecular than molar. Interestingly enough, these molecular measures do tap abilities which allow the individual to meet cultural demands at a more molar level. This is demonstrated by healthy correlations between IQ scores and measures of scholastic achievement (Terman, 1925; Hollingworth and Cobb, 1928; Cicirelli, 1965; and Feldhausen and Klausmeier, 1962), popularity (Jacobs and Cunningham, 1969), responsibility and morality (Jaggers, 1934; Unger, 1964), persistence (Miles, 1954; Terman, 1925), and delay of gratification (Melikian, 1959; Mischel and Metzner, 1962).

There is, for all of its appeal, something missing from a definition like Fischer's: some indication of the source of the individual's competence, and, therefore, any implication about whether he is judged intelligent because he happens to be good at the skills his society demands at the moment, or because he is able to ascertain demands and meet them in a variety of possible ways. It is our argument that the intelligent person is not necessarily innately better at what intelligence tests test than he is at any number of other tasks. If he has developed these skills, it is because he is not only more effective at reading cultural preferences, but also because he is better able to channel his behavior in efficient ways on a variety of tasks. We do not accept the suggestion that intelligence as it is culturally defined, does not reflect intelligence A, or reflects only an arbitrarily selected aspect of it.

III. MID-COURSE SUMMARY

We have now discussed two controversial areas which have been instrumental in preventing a theoretical concensus upon which an operational definition for intelligence could be based. We have examined the unitary- versus multiple-factor controversy, and the nature/nurture controversy. For the purposes of this paper, a summary at this point, before we embark upon the third area of controversy, will provide clarification of our position. It will provide us the tools for handling further suggestions. We shall try to synthesize the points of each theory which are salient to our hypothesis, and we shall rely heavily upon Hebb.

Intelligence can be described as a biologically based potential (derived from inheritance or prenatal environment) for efficient, competent interaction with the environment--an interaction which produces learning. It is this learning which is the sum total of manifest intelligence, and which is tapped by an intelligence test. Manifest intelligence has a largely general-factor character because of the dependence of many different specific abilities upon the same biological variables (intelligence _A_), but any individual may have varying degrees of ability on different tasks (either because of learning or physiology). In fact, a few tasks (musical ability, language-learning ability, and so forth) may be little related to the basic or general intelligence, but may rather be attributed to some other specific physical variable.

Further, we argue that although intelligence tests measure only intelligence _B_, the nearly universal opportunity for essential levels of stimulation and learning within a culture provides that manifest intelligence will be a satisfactorily valid indicator of innate intellectual potential.

This theoretical definition of intelligence, synthesized from various conceptual viewpoints, could form the basis for accepting the intelligence test as an operational criterion, if it were widely agreed upon. It asserts that although the test is not free of cultural influence, it is a valid indicator for individual effectiveness relative to peers within the culture and provides a good reflection of intelligence _A_. That is all the support one needs for use of the intelligence test as an operational criterion—except that until many theorists can agree upon this matter, proper refinement of our definition is impossible.

IV. THE PROBLEM OF HIGHER MENTAL PROCESSES

Higher mental processes are the third traditional area of controversy in intelligence theory (although the controversy has recently become roughly a consensus). The problem here is the distinction between "simple" learning, and processes of a very abstract, complex nature. They appear, from an extra-cognitive viewpoint, to be quite different processes, and it has long been a matter of interest whether or not they relied upon the same mechanisms---especially since individual differences in capacity for the more abstract operations might be the source of differences in intelligence.

The classical point of view was that higher mental processes involved neurological mechanisms qualitatively different from lower ones. It was assumed that basic processes were mediated by simple associational events, or habits, while the higher processes must have some obscure (and probably innate) structural requirements without which their functioning was not possible. It was these complex cognitive or neurological structures which were thought to be the basis of intelligence and of individual differences in intelligence.

Hebb (1949) and others (Thorndike, Bregman, Cobb, & Woodyard, 1926; Hayes, 1962), placed themselves in strong disagreement with the traditional explanation. They all argued that all human, or even animal, thought processes can be explained in terms of simple associational structures. There are many supportive evidences for this position. After thorough comparison of human to animal subjects, Hebb was forced to conclude that "there is no evidence to support the idea that learning _in general_ is faster in higher species---even at maturity (Hebb, 1949, p. 115)." This equality in the ability to learn among species suggests that there is a simple, universal character to all learning---perhaps even a basic universal element, like the association. Hayes (1962) reviews many other evidences in support of this conclusion.

From the widely held conviction that higher mental processes are not qualitatively different from others, we deduce that these high-level functions must be made possible through the sheer number of associational elements or the complexity of their simple inter-associations. Thorndike, et al. (1926) add:

> 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).

A further evidence for the suggestion that the elements of all thought are associational in nature is also indirect. It is found in the construction of computer simulations of mental processes. Newell, Shaw, and Simon (1958), in a simulation of human problem solving, distinguish the elements necessary for a system capable of solving problems. There must be symbolized information (memories or associations), primitive information processes (presumably, the various fixed methods by which new associations can be formed), and programs (definite set of rules for use of primitive associational processes).

Hayes (1962) points out that "only a few kinds of logic units or operations are needed in the most powerful computers (p. 322)," so that higher mental processes need not be different (and more difficult) kinds of operations, but simply recombinations of elemental ones. He also likens human "innate intellectual capacity" to a computer's memory banks, and human "manifest intelligence" to its program (or education) of operation (p. 322). It is apparently possible to construct a computer capable of fantastically complex tasks using simple elements of an associational nature, and this gives credence to the argument that the same kind of structures and operations may occur in human beings.

No matter what the complexity or storage capacity of a computer, its functions are based upon simple associational elements and their interaction. As a result, in the simple storage of a bit of novel information computers would not be expected to differ significantly in the speed or efficiency of storage. However, there might be significant differences between computers in the total number of elements which could be stored, and in the efficiency of their recombination and their recall in relation to new, relevant problems. An analogous dichotomy can be observed in human memory processes.

Rote memory performance (that is, the recall of simple information with no meaning outside itself, like nonsense syllables) fails to distinguish bright from dull subjects (Cronbach, 1970). This may simply reflect the universal level of difficulty in the formation of simple associations with little or no reference to other knowledge. Rote memory probably reflects the simplest kind of learning, and it is therefore no surprise that most individuals find it equally difficult.

Other memory processes are a somewhat different matter. Some individuals seem to be significantly more efficient than others at non-rote memory (learning and recall of meaningful information), and this variable efficiency is constantly found to be correlated to intelligence-test scores (Matarazzo, 1972; Pollert, 1969). The source of individual differences in non-rote memory is difficult to ascertain. Thorndike, et al (1926) related higher intellectual functions to the <u>number</u> of associations. Presumably, non-rote memory does depend upon a high number of associations for its functioning. Perhaps the possible number of associations is limited by an inherited potential for inter-neuronal connections. On the other hand, the mediator of better memory capacity may be some efficient manner of associational inter-connection which makes informational elements more available for recall. Highly intelligent subjects seem to have a tendency toward holistic organization of perceptions, and this kind of organization is known to be conducive to memorization in any one (Gardner, Jackson, & Messick, 1958; Beck, 1968; Saugstad, 1952). All that is known for certain is that individual capacity for recall of associations is a critical factor in intelligence, and basic to all human problem solving (Guilford, 1967)--the exact means of efficiency in recall, whether number or kind of associational connection, is in need of determination.

In addition to the mechanics of association, there may be other factors which are crucial in memory processes. There is some evidence, for example, that certain motivational factors may mediate individual differences in memory and intelligence. Hayes (1962) argues, in fact, that the only heritable influence on the potential amount and complexity of learning is motivational. Motivational variables are known to enhance memorization (for example, Weiner, 1966). We shall treat this area of interest at a later time.

Memory, of course, does not represent the only kind of associational process. These processes are involved in the development of perceptual systems, symbolization, discrimination, and generalization. Although a detailed theory of the associative nature of intelligence is not developed here, it should be emphasized that all of the mechanisms postulated for differentiation among individuals along a continuum of intellectual efficiency are reducible (at least in theory) to very simple, bio-cognitive elements on the order of the elemental association. According to most theorists, there does not appear to be any need for more complex mechanisms than these to be invoked in any explanation of intelligent behavior--although the interaction of these machinisms will no doubt prove to be of great complexity.

It is hypothesized that some innate physiological efficiencies at interrelationship, organization, and/or categorization of simple associational elements are the basis of intelligence—with the possible inclusion of some important innate motivational or arousal variables. Hebb has constantly stressed the equipotentiality of such associational structures, and we wish to stress it again in a slightly novel application.

It would not seem likely that the basic processes of higher intellectual activity, having such an elemental nature, would be strongly predisposed in very specific directions. For if all human thought is reducible to simple elements, those elements are obviously capable of recombining in many different ways. This is testified to by the interindividual variety of cognitive styles, and the intraindividual flexibility of behavior in human beings. It has been argued that higher mental processes are simply an elaboration of simple ones through learning and experience. Although almost all individuals are able to interact properly with their environment to the extent that they develop basic abilities for memorization, generalization, discrimination, and so on, the direction in which their more highly developed cognitive activities tend to differentiate may be determined to a significant extent by the particular environment of the individual.

Because the highly intelligent person very likely has innately superior processes of associative recombination and coordination, it seems likely that he would tend to be more efficient than the average at almost any intellectual activity demanded of him. It has been shown that highly intelligent subjects have fairly consistent, across-the-board superiority in the culturally approved sub-tests of traditional intelligence tests. We are merely arguing that were our culture different, these same individuals would also be different—that their cognitive processes are not only more efficient, but that they also have a greater potential for adapting to and meeting the demands of their environment.

V. FINAL SUMMARY

This ends our attempt at constructing a theoretical system upon which to base an operational definition of human intelligence. We now have several justifications for accepting the intelligence test as our operational criterion, as long as the sample population is specified and certain other qualifications are borne in mind:

1. Tested behavior is learned behavior, but it is reflective of an innate potential (intelligence A).

2. Tested behavior is culturally influenced, but significant nonetheless in determining the cognitive efficiency of individuals relative to their peers.

3. Tested behavior is behavior selected by the culture as important within that culture, but highly intelligent people should have the same relative potential capability at most other, unselected tasks as they demonstrate on the intelligence test, if the tasks are of a cognitive nature. This potential may, however, be undeveloped.

Because of these justifications, we should be able to deal with intelligence-test data as a valid manifestation of the collection of phenomena we mean when we speak of intelligence (intelligence A). We shall devote no further efforts either to justification or to qualification of our definitions of intelligence. Those we have already recognized will from this point on be assumed.

It is important, however, that more than justifications and qualifications be retained from this chapter. We have also tried to get at the essential nature of intellectual processes. Every understanding which it is possible to glean from what we have discussed about the workings of intelligence will be needed, if we are to deal with the interaction of intelligence with other variables within the individual.

We now move on to our second major area of concern: creativity.

CHAPTER III

CREATIVITY

According to the step-by-step method of definition we have been following in this paper (Matarazzo, 1972), we should try to establish for ourselves a generally acceptable, intuitive definition for creativity. On the face of it, this task would seem a great deal more difficult for creativity than for intelligence. The highly subjective nature of typical working definitions of creativity is demonstrated by the following anecdote:

> (At) a leading Midwestern University. . . an old, experienced teacher and scholar said that he tried to encourage originality in his students. In a graduate course, he told the class that the term paper would be graded in terms of the amount of originality shown. One school teacher in the class was especially concerned about getting a high mark in the course. She took verbatim notes, continuously and assiduously, of what the learned professor said in class. Her term paper, the story goes, was essentially a stringing together of her transcribed lecture notes, in which the professor's pet ideas were given a prominent place. It is reported that the professor read the term papers himself. When the school teacher's paper was returned, the professor's mark was an A, with the added comment, "This is one of the most original papers I have ever read" (Guilford, 1950, p. 448).

Originality has long been one of the most widely agreed upon synonyms for creativity, and obviously it can have validity problems of its own. Fortunately, most creativity researchers are able to be more consistent and empirical than are college lecturers and high school teachers. But it has proved difficult, even for them, to decide upon criteria by which to define creativity. Here are some of their efforts. . .

I. ATTEMPTS AT AN INTUITIVE-LEVEL DEFINITION

From philosophers, thinkers, and creative artists has been culled a general pattern of agreement about the nature of creativity. In part, creativity is perceived to be an awareness of new, hidden relationships—an awareness made possible only by past discipline and learning which are reapplied to a present situation (Koestler, 1970). For Poincaré, creation

> does not consist in making new combinations with mathematical entities already known. Any one could do that, but the combinations so made would be infinite in number and most of them absolutely without interest. To create consists precisely in not making useless combinations and in making those which are only a small minority. Invention is discernment, choice (Poincaré, 1952, p. 35).

Stephen Spender (1952) stresses concentration, and memory which is task directed—that is, a memory for sights and sounds and smells which could have possibilities as future elements for creation. He also calls upon "inspiration," a nebulous word which might be translated as the sudden appearance of a good (selected and useful), novel idea. Einstein alludes to "combinatory play [which] seems to be the essential feature in productive thought" and adds that this play is "aimed to be analogous to certain logical connections one is searching for (Einstein, 1952, p. 43)."

Creativity, then, according to some of those most generally recognized as being unquestionably in possession of it, is the remembrance of past learning or experience, recombined (playfully?), and selected for usefulness, often with concerted attention toward a predetermined goal.

Frank Barron (1952, 1958, 1959, 1968, 1969) was one of the first psychologists to concentrate his efforts on outlining a definition of creativity, in terms as precise as possible, and exploring its implications. His intuitive-level definition is a composite, derived from sources like the ones we have just touched upon. It suggests that originality is the sine qua non of creativity (Barron, 1969). Others (Brogden and Sprecher, 1964; Taylor and Holland, 1964; Getzels and Jackson, 1962) have concurred. Originality is well established as a criterion of creativity. It is a very useful concept, because it can quite easily be operationally defined: originality is merely the unusual, as "specified statistically in terms of incidence of occurrence (Barron, 1969, p. 25)."

But most investigators also agree with Barron that originality, in itself, is not enough to provide clear identification of creativity. An original response "must correspond to some extent to, or be adaptive to, reality (Barron, 1969, p. 25)." Thus, mere eccentricity is not creativity--it must have some selective process performed upon it; it must have some value to justify itself, much as suggested by Poincaré (1952; see also Brogden and Sprecher, 1964).

This evaluation of originality as to its usefulness and adaptiveness is the main area of controversy in creativity research. Originality is easily accepted as an operational criterion of creativity. But it is much more difficult to define and measure the ways in which originality should be useful or selected. The difficulty is two-fold: it is hard to determine what criteria individuals should impose on themselves, and therefore what behavioral correlates should accompany originality in the creative person; and it is difficult to judge the level of creativity represented by any individual on a task which we wish to make an operational criterion of creativity.

The questions about the selective aspects of creativity have not by any means been resolved. Mainly in this area have arisen disagreements among theorists about the theoretical or intuitive-level definition of creativity—and disagreement is certainly plentiful. From each theoretical faction in the controversy has come a different test of creativity, and as a result many different tests have been adopted as operational criteria for creativity.

Because of this hopeless splintering, we shall tentatively accept all of the creativity tests as operational criteria of creativity, and shall later present some justification for such a leap of faith. Accordingly, ignoring the chaos at both the intuitive and operational levels of definition, we shall move on to the third level.

II. VALIDATIONS, CORRELATIONS, AND THE UNITARY CONCEPT

When he had a rough personal definition of the object of his interest, Binet put together a test, and went out to see if it would work as an operational criterion. In validating the test, he compared its results with pooled judgments of teachers about the "intelligence" of his subjects. Having met with considerable success in that kind of comparison, he at least tentatively accepted his own operational definition.

This kind of process is the third step in the task of definition suggested by Matarazzo (1972), which he calls a step of validation, correlation, and the discovery of exemplars. It is not unique to intelligence research, but has also gone on in the field of creativity investigation.

Two methods have been used with particular frequency to deal with validating criteria for creativity (Torrance, 1964). The first method is identical to one used by Binet. A test is chosen as the possible operational criterion of creativity. It is administered, and high- and low-scoring groups are selected on the basis of its results. Then, these groups are examined to see whether or not there are any behaviors they emit which can be identified as creative (besides those measured directly by the test), and which also distinguish the high and low groups from each other. The second method is to choose some behaviors as criteria for creativity, to construct groups on the basis of these "creative" behaviors, and then to see if there is any other test or tests which will differentiate between these groups.

Many tests have been found to differentiate acceptably between groups in one of these ways. But when these many tests of creativity are compared to each other, we find an alarmingly weak relationship among them. Whereas well-accepted tests of intelligence (the Stanford-Binet, WAIS, Army General Classification Test, Army Alpha, Army Beta, and Peabody Picture Vocabulary Test) have correlations on the order of .7-.8 with each other (Matarazzo, 1972; Wechsler, 1958), creativity tests (none of which could actually be classified as "well accepted") have intercorrelations of around .2-.3 (see Getzels and Jackson, 1962; Barron, 1969).

Correlations such as these are high enough in some cases to propose that there is a relationship between variables affecting two different creativity tests, but they are hardly high enough to indicate that we are measuring the same entity when we administer the two tests. Especially in the light of creativity's weak but fairly consistent positive correlation with intelligence, it seems foolhardy to cling steadfastly to the idea of a unitary and generalized "creative response tendency," independent of intelligence (Wallach and Kogan, 1965a, 1965b).

Can we find analogue to the factor theory of intelligence research in creativity? A factorial aspect to the creativity phenomenon seems likely, at very least. Each test of creativity has a correlation so low with most others, under most conditions studied, that it must be tapping fairly independent variables. It is possible that creativity is so "factored" as to be nearly situation specific--or perhaps task specific, with high sensitivity to situational stimuli.

There has, however, been little discussion of a factor theory of creativity. Perhaps this is because from the very outset there has been less assumption that creativity is a single, innate, unitary thing. Very few have suggested that one is "born" with creativity, in the sense that one might be born with intelligence. There has been no noticeable tendency (perhaps because of the Sisyphian nature of such a position) to suggest that any one measure of creativity can define it. Although there is always the stated or unstated assumption that creativity is a phenomenon unitary enough to be used to distinguish those who have it from those who do not, at least in relative terms, the emphasis has always been upon broad-based and varied assessment in such a task (Torrance, 1962)--as if there were not one creativity, but many.

Actually, if creativity tests all showed .2-.3 correlations with each other and factored out upon multivariate analysis into several semi-independent factors, we should have a clear basis for theorization, although it would be different from theories constructed up to now. But there are other data which constructively complicate the issue.

Some creativity measures, although rare in number, show quite good correlations with each other. Barron's Independence of Judgment Test correlated .7 with teacher ratings of creativity, Mednick's Remote Associations Test (RAT) correlated .7 with ratings of creative writing, and biographical information has several times shown relationships of .30-.55 with other creativity measures (all referred to in Taylor and Holland, 1964).

Some of these atypically strong relationships are fairly easily understood. The RAT, for example, is a measure of the subject's ability to solve a three-part verbal relationship problem, where the answers are words not in common usage. It stands very much to reason that if a person has free access to uncommon words for use in problem solving, he has a much better chance of being a good creative writer.

But the example of the intercorrelation of the Independence of Judgment Test with teacher ratings, even making allowances for the unreliability of the ratings, is not merely the result of a relationship between variations of a single task. Presumably, creativity, as seen by the teacher, and independence of judgment have no logical relationship to each other--unless it is through their common relation to creativity. Here, then, is evidence of some kind of generalization of creative responses, either from one task to another, or from one level of behavior (cognitive independence) to another (overtly behavioral, rated creativity).

Another kind of complication in deciding whether or not creativity measures are independent or tap a global phenomenon is found in the work of Wallach and Kogan (1965a, 1965b). Although half-convinced at the outset that the most consistent variable measured by creativity tests was intelligence, they decided to alter typical testing conditions and to look for some situational variable which might be able to save the unitary-phenomenon concept of creativity---an idea too attractive to be easily abandoned. They reasoned that a typical "test" situation, with its constrictions and tensions and pressures, could not possibly allow subjects to show their full creative capacity, since creativity is closely related to play, experimentation, and spontaneity. Therefore, they administered their tests in a "creative" situation. Subjects were not placed in large groups, there were no time limits, and the academic trappings which are usually found in a test situation were removed to the fullest possible extent. With great efforts and precautions, conditions were made to resemble a "game" situation. Teachers and principals were not involved, the word "test" was not mentioned, and young ladies administered the tests.

The results of these manipulations were rather surprising, even when due account is taken for their limited generality. Correlations among the creativity measures was .4. This is unusually high, considering the number of tests involved (five)---even though four of the tests deal with word usage, and three of these are extremely similar in nature. One of the tests, however, is a visual test, and although the authors do not mention its specific reliability in relation to the other tests, we shall assume that some generalization was evident across tasks. It is also very likely that the change in test conditions, rather than providing a truer measure of creativity and thus higher correlations between tests, presented a wide variety of play "cues" and in that way increased the generalization of creative responses.

The rest of the Wallach and Kogan results do not bear directly upon the question of creativity as a unitary phenomenon, but they do suggest caution interpreting the high correlations obtained among the creativity measures. Curiously enough, the high correlations were not the result of an across-the-board increase in the creative output of all the creative subjects--although this might have been expected to be the case. The data showed that other variables were interacting with creativity level in determining the responsiveness of individuals to the playful atmosphere. An extraordinarily low correlation was obtained between intelligence and the creativity measures in this situation (replicated by Galanter, 1967).

Under ordinary conditions, we can expect correlations of .2-.4 between creativity and intelligence--if the sample has a fairly broad IQ-score range (Ripple and May, 1962). Since the magnitude of the correlation between creativity and intelligence has been decreased, there are two possible explanations for the effect of the situational manipulations. Either the high-IQ children are less creative in this situation, or the low-IQ children are more creative in it than is usually the case. It does not seem very likely that any group would be less creative because of a non-test, untimed, non-authoritarian atmosphere. That leaves the possibility that the low-IQ children were more responsive to the creative context of the testing than the high-IQ group. Why? Three suggestions might be extended: (a) high-IQ children were better able to interpret the situation as it actually was, a testing procedure, and were less free to respond unusually creatively than were the less observant children, (b) high-IQ children were less able to change their behavior patterns in response to a quick change of cues, having a more internalized complex of test responses, and (c), originality was the only criterion used to determine the creativity level of the test responses; it is to be expected that originality would be much easier for less-bright children to produce than would adequate selective processes. In the area of selection, high-IQ children would have their clearest advantage in responding creatively, and this would not be represented at all in the data.

Although there is not enough information available to answer the questions raised by the results of this study, they do provide an indication that when we are studying interrelationships among creativity measures, it is unsafe to ignore other variables which might be distorting the results. In this case, the modifying variable was intelligence, and intelligence has often been inadequately treated in the research of creativity. Fortunately, Wallach and Kogan did not simply match for intelligence, and thus lose information about its effects, but rather divided their sample into a two-by-two matrix to deal with differential levels of both creativity and intelligence. Without such procedures, it will be impossible to make logical sense of the creativity field. Answers we may get to the question of the unitary quality of creativity would have the possibility of being based on unsound presuppositions.

We have seen, then, some evidence that creativity is a global phenomenon, but we do not know for certain how reliable the evidence may be. There remain, however, other kinds of evidence of the global quality of creativity. These data are the correlations between creativity tests and tests of personality traits or patterns. They are another example of exemplar or correlate validation, and have been vigorously accumulated, in spite of the tentative nature of their foundation. Almost all measures assumed to be assessments of creativity have been thoroughly correlated with personality traits. The only compelling aspect of this evidence is the consistency with which certain personality patterns have been found to correlate with creativity, no matter which creativity measure is used.

Every human being who is not profoundly retarded is capable of creative behavior, just as he is capable of intelligent behavior. But some individuals, if not more creatively capable than others, are at least more frequently found behaving creatively. There seem to be certain cognitive and personality characteristics which have a higher-than-average probability of occurrence in these people--whether because these characteristics are conducive to creativity, or because they are codependent variables with it.

Cocharacteristics of a cognitive type are good memory, cognition, evaluation, convergent productions, and especially divergent productions, to follow Guilford's general system (1967). More specifically, Taylor and Holland (1964) express the covariables as originality, redefinition, adaptive and spontaneous flexibility, associational, expressional, and ideational fluency, elaboration, and evaluation (see also Barron, 1968, 1969).

Cocharacteristics bearing a closer resemblance to personality traits include: "independence, self-sufficiency, tolerance of ambiguity, 'feminine' interests, and professional self-confidence (Taylor, 1964, p. 180)." Creative people have also been suggested to display an abundance of fantasies, play, humor, problem- and pattern-awareness, autonomy, judgmental independence, stability, dominance, self-assertion, preference for complexity, self-acceptance, adventurousness, curiosity, and self-control (Taylor and Holland, 1964; also Barron, 1952, 1958; Day, 1968).

Most of the personality "traits" correlating with our creativity construct are difficult to measure with any accuracy, and are not known to be stable or generalized over situations, even if they could be accurately ascertained (Mischel, 1971, pp. 147-150). Therefore, they are themselves in urgent need of clear operational definition and have difficulty in supporting the weight of some additional undefined construct. Nevertheless, they do form part of a clear and logical pattern, which is consistent over many tests for differentiating creativity according to "personality" variables. It is necessary to deduce as much from this kind of evidence as it is possible to deduce. Even their unreliability and situation specificity can be used as a sort of information--for when, in spite of such unreliability and specificity, we come upon such clear patterns, they are very likely to indicate something, although it may not be what we expect.

Although the personality-test correlations with creativity measures are evidence of a global, unitary pattern of creativity, they, too, are insufficient proof that such a pattern reflects the actual case.

We have come near to the limit of the usefulness of correlational data in creativity research. It is clear that they have given us signals about fruitful areas of search, but they can never answer in a final way the question of the molarity of creativity. Nor can they be situationally specific enough to explain why creativity comes about. Nor can they be cognitive enough in approach to explain what processes are at the basis of creative responding. For these questions, we need other tools.

III. CREATIVITY TRAINING AND THE UNITARY CONCEPT

Experiments in the training of creativity offer an excellent source of information about whether or not creativity is a global phenomenon. If we can discover environmental manipulations which result in creativity, we shall be in a position to make valuable guesses about how it is that creative behaviors come about in nonexperimental situations. Furthermore, through training studies it is easiest to ascertain how extensively creative behaviors are interrelated--that is, whether specific ones are generalized to other tasks or situations, whether other responses result spontaneously from the acquisition of one creative response, whether very specific behaviors generalize to more molar behaviors, and so on. If these kinds of generalizations do occur, they will be substantial evidence for the global, unitary character of creativity.

Unfortunately, the complexity of these problems as well as the different possible levels of generality, the huge variety of possible discriminative stimuli, and so on, demands that a rigorous and systematic body of research be compiled which deals with each possible combination of various types of variables. And this quantity and quality of research have not yet been achieved. The studies which are presented here are an example of the inadequacies of the present status of the research--not so much by their individual weaknesses as by their collective ones. They indicate, nevertheless, some critical points of interest and significance, to which we shall call attention.

Maltzman and his associates, for example, (Maltzman, 1967; Maltzman, Simon, Rasking, and Licht, 1967) have made many closely related studies in the training of original verbal responses. Their method, for the most part, has been to demand several different sets of responses to the same set of stimulus words---a procedure designed to encourage the practice of creative responses without introducing any reinforcement into the training. For their consistency and concentration of study, they are to be commended, although this writer is of the opinion that no clear reflections of reality will be obtained from situations in which contingencies are the variables held constant. Appropriately enough, in the light of our reservations about this approach, it has met with very mixed success.

For example, Simon, Lotsof and Wycoff (1966), using the Hatzman technique on fourth, fifth, and sixth graders, reported that their subjects emitted no more original responses to a new list after one day of training, although they did show results after three days of training. The authors point out that college-age subjects do show learning effects after only one day.

McDonald and Martin (1967), with a slightly different technique, used verbal reinforcements to shape original associations. They were successful, although there was a differential ease of training, depending upon whether subjects were high or low in creativity at the time the training was initiated. However, when the subjects were administered the RAT, a test of creativity which depends upon the use of novel word associations, <u>no transfer</u> (generalization) was observed. Simon, et al. (1966) also had difficulty in obtaining generalization of word-originality training. They found some transfer to measures of personality traits correlated with creativity, but only in older subjects, and only after fairly extensive training.

From these two studies we infer that training, when concentrated on very molecular tasks, does not transfer very well--even to other tasks at the same, molecular level and of a closely similar type. Although Simon, et al. (1966) obtained some transfer tendencies to more molar behaviors (personality trait tests), their results can be held somewhat in doubt, given the McDonald and Martin (1967) failure to obtain generalization across much more closely related tasks, and the tentative nature of personality assessment.

Levy's (1968) training procedure included both the specific reinforcement of certain responses and a more molar method of creativity training, based on modeling and role identification. He also tried to measure the effects of this multidirectional technique upon both molecular and molar behaviors. The best combination of training methods for both molar and molecular task performance was a composite of reinforcement for specific original responses, modeling of specific responses, a general "role" model, and praise to the model in the presence of the child--and this combination did have significant results.

Results strikingly similar to these were found by Brown (1965) in his study with adult subjects. Again, the composite training method worked best. To summarize his method in slightly more operant terms than those used by Brown, creativity and conformity were introduced to the subjects in the form of animal symbols--which were used to evaluate, reinforce, or punish behaviors as they were emitted by students in the classroom. At the same time, strong emphasis was placed by the teacher on challenging clichés, supporting new ideas, and so on. The training procedures were quite significantly effective.

In both of these studies using composite training techniques, one critical limitation stands out clearly: although subjects were well capable after training of emitting creative responses of both a specific and a general nature, they did not manifest <u>any</u> significant change until given some extra cue that the kinds of responses learned or learned about during training were now appropriate in a different situation. In both studies, subjects demonstrated significantly increased creativity only after they had been asked to take the posttraining tests as their <u>models</u> would take them. Presumably, this suggestion brought the rewards observed by the subject to be accorded to the model into the subject's contingency expectations.

To clarify this phenomenon, Brown (1965), whose study included animal-symbol models of both creative and conforming behavior patterns, asked some of his subjects to take the posttraining tests as would their conformist animal model. Surprisingly enough, Brown found that under these request conditions, subjects could appear significantly more conforming on the tests--their behavior actually did closely approximate the conforming animal model's. Although they had constantly heard criticism of this model, and praise of his polar opposite, subjects had equal command of and replicative powers for the behavior patterns of both of them. This presentation of cues to the subject for which behaviors are expected is labeled "triggering" by Brown. Evidently, in the process of modeling and reinforcement, subjects "learned" all of the new responses, performed none of them spontaneously, but performed either kind well when "triggered" to do so.

In another striking set of findings, Renner (1970) demonstrated generalization of creativity training at a molar level to completely dissimilar global tasks. College-age subjects were presented with lectures on the novelty and complexity of art accompanied by slides of modern paintings. From pretest to posttest subjects showed, as might have been expected, significant increases in tolerance for novelty and complexity in visual stimuli. More significantly, experimental subjects also showed more tolerance for novelty and complexity in musical stimuli. Finally, and most significantly of all, experimental subjects performed more creatively than controls on a test of verbal divergent thinking (a Guilford creativity test).

These results are doubly impressive because, to all appearances, learning and transfer of global, creative behaviors was accomplished without need of a triggering stimulus. Of course, it is very possible that the constant emphasis on art throughout the treatment and testing procedures provided a very subtle discriminative stimulus for "arty" responses. What is important is that even if a stimulus was present to trigger the creative responses, it was not an outright and explicit command. People can evidently be taught to act creatively in appropriate situations; they need not always release creative behaviors a package at a time, on cue, like vending machines.

There is indication in this study that the only discriminative stimulus accutely necessary to the subject is some manifestation of a "creative" atmosphere, in which creative responses appear likely to be reinforced. This was what Wallach and Kogan (1965a, 1965b) attempted to achieve, but they were probably less successful than slides and art lectures and so on. Artistic training may in itself be more effective in generalizing creative behaviors, because of the fact that the complex of behavior patterns (or role) it represents is very familiar to us. Our familiarity might be the result of direct artistic training or of consistently modeled behaviors (artist-acquaintances, or media representation of the artistic life style), but it can be easily assumed that we know how to behave "artistically" in the proper setting, whether or not we have ever performed any specific behaviors before.

What pattern are we to make of these few examples of creativity training projects? Clearly they do not sample widely or deeply enough to tell us much for certain (and this is not a weakness limited to our sample). But we have seen enough to hint at several general principles.

First, it has been often demonstrated that in any particular situation a tendency toward creative or original or uncommon responses _can_ be learned, and so also can a tendency toward common responses (Brown, 1965; McDonald and Martin, 1967). The ease of this learning is dependent, however, on several variables.

For example, in a procedure shaping an uncommon response tendency through reinforcement, learning was stronger in older subjects (Simon, et al., 1966). However, when an uncommon response tendency is enhanced by a change in discriminative stimuli (setting), lower-IQ subjects seem more responsive (Wallach and Kogan, 1965a, 1965b; Allen and Levine, 1968). Mental processes can be assumed to be more efficient in both older and brighter subjects, but these groups show opposite tendencies in the studies presented. It is possible that the acquisition of new behaviors is easier for individuals with better cognitive development and differentiation, but that those behaviors already acquired are more strongly internalized and durable in the more highly intelligent. However, other interactions make such a simple explanation unlikely. Intelligence and pretreatment creativity levels are known to interact significantly, although it is not known why this is the case (Wallach and Kogan, 1965a).

The second major generalization to be derived from the studies we have reviewed is that the molecular/molar nature of the creative behaviors trained has a strong effect on the effectiveness of the training. Specific training of very specific creative behaviors, like novel word association, seems not to generalize successfully to any other behaviors, whether they are global or specific—even when they are strongly similar to the learned originality task (Simon, et al., 1966; McDonald and Martin, 1967). If this were all we knew about creativity, we should have to surmise that each minutely specific creative behavior must be individually learned, and is strongly resistant to generalization.

However, the picture is entirely different when we consider training procedures comprised of both molar and molecular elements. If a training process includes reinforcement of specific tasks of originality as well as modeling of many creative behaviors and/or a presentation of creativity rules to follow, the learning of specific, original responses is readily learned. Under these same training conditions, learning may generalize to other, more general behaviors, if discriminative stimuli are present to encourage transfer. With an appropriate stimulus situation, learned creativity at a completely global-behavioral level can transfer to other equally general behaviors. In summary, it seems most efficient, in terms of ready learning of both specific and generalized creative tendencies, to incorporate into the training many different shaping and modeling and cue-presentation techniques. Until our culture has progressed to the point where almost all activities can be acceptably considered fair territory for creativity, it may also be important to involve in the training situational stimuli which are clear cues that "creativity is spoken here," such as music and art.

IV. SUMMARY: A RETURN TO DEFINITIONS

Is creativity global in nature? The answer seems to be that it can generalize, if enhanced by broad-spectrum training and if presented with appropriate discriminative stimuli. It is our guess that creativity tends naturally to generalize but is extremely sensitive to discriminative stimuli. Broad spectrum training provides that more stimuli inherent in a novel situation will be functional as cues for creative behavior--and cues which are verbalized, or represented as symbols, will make generalization even easier. It is this reliance upon discriminative stimuli for the generzlization of previously learned, creative responses which gives them the appearance and the pattern of task-specific or situation-specific behavior, even though they may always be well within the subject's capabilities.

It probably happens that those individuals reinforced and not punished by their environment for a certain creative behavior pattern are also usually reinforced for other creative behaviors, but not <u>all</u> others. The demands of the environment would shape different profiles for each individual, but on the whole, environments would tend to be accepting of a certain degree of originality for each class of behavior. We suggest that any behavior, if novel or deviant or original to a certain degree, would be unacceptable to a culture--with some qualifications (deviant art is more acceptable than deviant theology).

When we put a subject in a laboratory, and train him to respond originally or creatively in one particular way, we are probably not causing a revolutionary change in his general pattern of expectations about his environment's reward or toleration of originality. Thus, it should prove to be no surprise that his specifically trained original behavior does not transfer to other situations. If, however, we use a composite approach in his training, and especially if we train not only specific behaviors but also whole behavior patterns (both cognitive and social), and if we provide cues that the training situation can be compared or generalized to a new situation, we may actually change his reward expectation for a whole complex oforiginal behavior--and we shall see generalization. Our subject will have learned to expect his environment to accept and reinforce a higher level of originality than was his previous expectation.

It may prove helpful to look to this concept of the acceptable "level of originality" for a solution to the problem of defining creativity. Rather than seeking to find creativity, the entity, we can think of creativity as a continuum, along which almost any behavior could be placed. Any single response could thus be evaluated in terms of its degree of creativity. Since creativity has been roughly agreed to be represented by originality plus quality or utility, a creative response could be evaluated statistically for its frequency of occurrence and then by consensus as to its value. By the same token, a creative individual could be distinguished by the rate of statistically rare but subjectively useful and valuable behaviors he has emitted. This kind of approach could not only standardize and objectify differentiation of creative individuals, but it would also help us get at (or by) the problem of what creativity is and whether or not it is global.

Accepting the hypothetical and stipulative definition of individual creativity as the rate of statistically uncommon but judged-to-be-valuable responses, of any sort, we can now return to the problem of the operational definition of creativity with a better perspective for solving it.

Creativity is the measured degree of originality and usefulness in any set of responses, relative to peers, for any kind of behavior examined. Any creativity test, then, can be used as an operational criterion for creativity at that task--as long as response originality is taken into account. Measures of fluency and the ratio of original responses to the total number emitted are other ways to approach creativity assessment, and each has its merits: these will be marginally accepted. We shall thus accept most measures nominated as tests of creativity, but only in regard to the particular behaviors they require. And we shall not expect that these measures and the responses they represent will be strongly related to any other response tendency, especially in an experimental setting.

CHAPTER IV

AN ASSOCIATIONISTIC APPROACH TO CREATIVITY AND INTELLIGENCE

We have tentatively defined creativity as the production of statistically uncommon, but valuable responses. This definition is practicable in the examination and evaluation of overt behavior, but it is of little use in investigating how it is that creative responses come about. By this we mean both the socio-environmental "how" of creative behavior (that is, its external causes), and the cognitive "how" (that is, its internal causes). This chapter will deal with the cognitive processes of which creative behaviors are the products, and suggest possible explanations for the way in which they have their effect. It will also compare creativity, at a cognitive level, with intelligence --in an effort to show whether the two variables have any intrinsic antagonism, or, to the contrary, any essential cognation.

I. NOVEL ASSOCIATIONS AND SELECTION

For our purposes, Donald Campbell (1960) provides the best model of the fundamental nature of creative thought processes. It is not only a model of creative thought, however. It is also a sketchy conceptualization of all higher thought processes.

Campbell conceives of intelligent behavior as a manifestation of an organism's freedom from direct stimulus control--the introduction of mediating processes between the environment and the organism. With increasing complexity, species become increasingly better able to deal with and receive information from their environments without direct physical interaction (see also Hebb, 1949; Piaget, 1967; Gibson, 1970; and Diamond, Balvin, and Diamond, 1963, for corroborating views).

But the development of complex systems for indirect manipulation of the environment depends strongly upon the influence of that environment. An organism at the primitive level emits random locomotion, receives direct sensory input, and has its random behaviors "selected" by that input--usually in a very simple, approach/avoid manner. This is what Campbell calls "blind variation and selective retention." When an organism is capable of somewhat less direct methods of sensory input, it has merely acquired a means of representing to itself a sensori-motor exploration of its environment. It has a way of obtaining information without direct manipulation. With each increase in a species' neurological complexity and each process of environmental selection, the species becomes more adept at internal, representational manipulation of its environment. At the highest levels of development, we call this "intelligent behavior" (see also Hayes, 1962). Campbell elaborates:

> At this level there is a substitute exploration of a substitute representation of the environment, the "solution" being selected from the multifarious exploratory thought trials according to a criterion which is in itself substituting for an external state of affairs. Insofar as the three substitutions are accurate, the solutions when put into overt locomotion are adaptive, leading to intelligent behavior which lacks overt blind floundering and is thus a knowledge process (p. 384).

Innate intelligence (that is, intelligence A) and learning interact in this process, and have their effect on the quality of the product. Individuals may differ "in the accuracy and detail of their representations of the external world (Campbell, p. 391)." Presumably, with greater intelligence a greater intake of sensory information can be accomplished, and with less distortion and better retention than might occur in a less efficient system. At the same time, learning experience multiplies the amount of stored information, which, among other things, enables the organism's internal representation of the environment to be garnished with greater detail and accuracy. Learning also increases the number and range of responses possible to the organism. Most importantly, the informational elements acquired in learning are essential building blocks for random recombination--the recombination which is half of the creative process.

When various random associations are made, they are subject to many selection criteria. This is fortunate, for the number of useful solutions is probably just an infinitesimal minority of the total possible associations (as Poincaré, 1952, observed). The primary selective criterion is, of course, that an associative combination be an appropriate "fit" to the environment--that is, that it be realistic, to use the term in a slightly unusual and literal sense. But other criteria also operate on combinations--criteria derived from experience in problem solving, rule verbalization, and so on. Some individuals should be better at maintaining simultaneously more such selection criteria, thus increasing their "likelihood of achieving a serendipitous advance (Campbell, p. 391)." Logically, highly intelligent people should be better able to accomplish such a feat.

What Campbell describes, in short, is really a matter of happy variations, or associations, arising partly by accident, partly because of the cognitive furniture of their host. Certain of these associations or combinations are chosen as solutions because they meet the criteria of the problem-solving situation, or because they meet the criteria of an artistic product.

This point of view is not incompatible with more extra-cognitive approaches. Barron or Taylor might see Campbell's creativity as a process of producing original or creative products (novel associations) which is enhanced by intelligence and sensitivity to the environment (accurate representation of reality as stored information), by learning (which provides a large number and variety of elements available for association, producing large possible numbers of useful associations), and by flexibility (a lack of selection criteria which might inhibit the appearance of useful novel associations). If creative products are overtly evident in any individual, it is very likely that these variables are in effect.

Campbell's model is of course quite consonant with simple association theory, as it has been described in relation to intelligence. We have argued that human thought processes can be reduced to a few basic operations, of which the most basic is (or is something very like) the association. The associative model of thought and intelligence seems tailor-made for Campbell's theory of creativity.

In fact, if all human thought processes are based upon simple elements and operations, and if creativity can be reduced to analogous or identical elements and operations, what distinguishes creativity from intelligence? If we speak of intelligence, we must start with neurological efficiency and innate potential, but we end up by evaluating an individual's efficiency and adaptability relative to his peers. Efficiency and adaptability are obviously the result of rich potential for random recombinations and effective selection criteria. Is creativity, can it be, distinct from these processes?

For Campbell, creativity is not something in a different category from other kinds of thought--it is simply another high-level process of variations and selection at an extreme point on the creativeness continuum. To be more specific, a creative association is simply a random association which has a lower chance of statistical occurrence in the general population than other associations, but which at the same time survives adequate selection criteria.

Since "intelligent" associations and their resultant behaviors are by nature adaptive and realistic, the only distinction that remains to creative products is their originality. The factors conducive to the process of random variation and selective retention are, among others, retention and usability of past encountered solutions, and good representation of the environment, with detail and accuracy.

Any one of these factors is basic to intellective processes as well. Increases in any one of them should cause, or enable, increases in both intelligence and creativity to occur.

In point of fact, it is difficult to understand why intelligence and creativity do not precisely coincide. Barron (1957) points out one aspect of this paradox:

> If one defines originality as the adaptive and unusual, and if one defines intelligence simply as the ability to solve problems, then at the upper levels of problem-solving ability the manifestation of intelligence will be also a manifestation of originality. That is to say, the very difficult problem which is rarely solved requires by definition a solution which is original (p. 735).

If it is true that difficult problems are solved only with the help of creativity, then it is doubly true that creativity is only possible with the facilitation of intelligence. Zaragueta (1953) suggests that intelligence is creative elaboration, and creativity is the expression of that elaboration. This suggestion fits quite well with the evidence we have presented to this point. Each improvement in the biological efficiency of an individual would result in better cognitive systems of retention or recombination or selection. This superiority would be overtly manifest as intelligence _B_. But since the variables enhancing intelligence _B_ would be those very variables enhancing creativity, intelligence _B_ would be creativity--creative elaboration, expressed. Why is this not exactly the case? Why are intelligent behaviors not equivalent to creative behaviors? The paradox of their inequality is the essential stone in the theoretical foundation of this paper and it is important that we now examine the experimental literature for some clue to its solution. We must search for some factor which differentially influences intelligence and creativity, or some indication that the assumptions we have made about their bases are incorrect.

II. MEMORY AND SETS

As we saw while attempting a definition of intelligence, most theorists and test constructors agree that memory is a primary factor in intelligence, vitally important in every intellective operation. Guilford (1967) saw it as fundamental to all problem solving and even to creative processes. Many investigators have found relationships between memory and creativity. Therefore, memory seems to wind itself through both creativity and intelligence, and should be examined as a possible source of their differentiation.

In a study with rats, Bruner, Mandler, O'Dowd and Wallach (1964) found that only those subjects which had been overtrained in a maze task showed the ability to transfer or generalize their learning to a second task which was the exact opposite of the first. Although the transfer was affected by motivational factors, no transfer occurred under any motivational state unless the subjects had first overlearned the task. Apparently, the animals had to have the associative elements they needed to manipulate firmly fixed in their memories before a novel, or "creative," solution could be arrived at.

One examination of the relationships between intelligence, creativity, and memory was carried out by Pollert (1969). He hypothesized that memory was important to creativity, and his results supported this hypothesis, with one predictable exception: two rote memory tasks showed no relation to non-verbal creativity and only a very small relation to verbal creativity and intelligence. Rote memory is known to be quite independent of intelligence tests (Cronbach, 1970). If it is also independent of creativity, our hypothesis about the common origination of intelligence and creativity can only be supported.

Pollert also found that all other measures of memory (that is, meaningful memory, like memory for verbal details, objects, number, and color) were significantly correlated both to intelligence and to at least one of the measures of creativity. Pollert's conclusion was that "most divergent thinking involves the manipulation of information retrieved from memory or storage in addition to external stimuli (p. 155)."

Supporting evidence of the facilitating effect of memory upon creativity is to be found in the work of Kerr and McGehee (1964), who obtained correlations of .16, .53, and .30 between creative temperament profiles and several different memory tests (the last two correlations are significant). These results are especially interesting because they seem to relate memory, which could only exert its creativity-facilitating mediation at a cognitive level, to a global manifestation of creative behaviors. Thus, creativity does seem to be dependent upon intellectual, as well as socio-environmental, variables.

Of course, it is reasonable and logical that a good memory should increase one's potential for creativity. Campbell's model of creative thought provides a significant role for the storage and availability of elements for recombination—without these elements, creativity is not difficult: it is impossible. McKellar (1957), even while taking a more phenomenological approach to creativity, is strong in his emphasis upon the importance of stored perceptions, or "memories." As he puts it, "no imagination can occur that is not composed of elements derived from actual perceptual experience (p. 23)."

The anecdotal evidence for the importance of memories of past experience in creative processes is almost limitless (Ghiselin, 1952), and it is supported by empirical evidence and reasoned argument. But is there not also reason to suspect that memory could be an obstacle to creativity? If associational elements are too easily accessible, might they not conflict, block each other, or result in the domination of old solutions in new and inappropriate contexts? Can variation be limited by retention, and memory be a handicap in creative problem solving?

Saugstad (1952) attempted to unravel the problem with an experiment on incidental memory and problem solving. He distinguished between a holistic, Gestalt-type memory and an "incidental" memory—a memory of isolated, concrete items. His results were more puzzling than enlightening. Task-related memory had no relation to problem-solving ability (the Pearson r was +.001). Incidental memory, on the other hand, showed negative correlations with problem solving and with school grades. These correlations were only significant for boys, and within the boys, incidental memory seemed more of a handicap among language majors than among physics and mathematics majors.

Although the entire meaning of these results is not clear, they do provide an indication that some kinds of memory (not necessarily those correlated with intelligence or achievement) can interfere with problem solving of a creative nature. But since there was a significant sex difference in the effects, it is impossible to avoid motivational considerations.

There are other studies which indicate that the problem is not as simple as we should like it to be. The overlearning study of Bruner, et al. (1964) reconciled previous studies by indicating both that overlearning (or good memory and usage of information?) was a help to flexibility in problem solving, and that it was a hindrance. The reconciliation was accomplished only by consideration of motivational variables.

Conceivably, problem solving could represent either creative or intelligent behavior, or both. It is therefore possible that in having some negative effect on problem solving, a memory variable is differentiating between creative and intelligent cognitive behavior. The most likely possibility is that such a variable covaries with what we know as intelligence, while it inhibits creativity. Whatever the nature of the memory variable, it could provide some answer for the problem of the apparent semi-independence of creativity and intelligence. But up to this point we have been discussing "memory" in a very simplistic manner, and we do not have the conceptual tools for handling it differently. Memory is not a simple association. At very best (that is, in the simplest possible case) it is a multiple association, and it is probably merely an aspect of complex cognitive systems from which it can be only arbitrarily isolated. Until we have some idea of what those systems might be, it will be hard to understand whether memory, as a "set" which inhibits or enhances future solutions, effects the differentiation between creativity and intelligence. Therefore we must embark on an elaboration of our understanding of cognition.

III. HIERARCHIES OF NOVEL ASSOCIATIONS

We have described a general associational system for dealing with creativity and intelligence, following the recombination-and-selection model of Campbell (1960). Campbell's model, however, never reaches specificity when dealing with cognitive constructs, and we should like to develop a more precise understanding of them. One particular aspect of the recombination model serves as a critical point in another hypothetical system. This system is Mednick's (1962), in which a more directly cognitive approach is taken. The critical point in common with both systems is illustrated by this perceptive introspection of Poincare (1952):

> Among chosen combinations the most fertile will often be those formed of elements drawn from domains which are far apart. Not that I mean as sufficing for invention the bringing together of objects as disparate as possible; most combinations so formed would be entirely sterile. But certain among them, very rare, are the most fruitful of all (p. 36).

The originality of which creativity is composed can be defined as an associative combination of statistical rarity. But it is the reasons such rare combinations occur (when they do occur) that hold particular fascination for Mednick. He believes an association to be rare because it associates two very unlikely fields (see also, Bronowski, 1958). A novel recombination is born not of two novel elements, but of two "ordinary" elements which have a very low probability of co-occurrence. Together they are, to use Mednick's terminology, a "remote association."

A remote association can be defined according to a norm population, or within an individual. If an association has a probability of .0002 of occurrence among a random sample of Milltown, Ohio residents in response to the word "rock," it can be described as original. If it is an appropriate or useful association, it might also be described as creative. Unless the individual producing the response is highly deviant from and incompatible with his social milieu, that response will have a fairly low probability of occurrence within him as an individual, as well. It will probably not be the first response he emits: it is a remote association. The reason it occurred in his mind, and not in someone else's, is that his remote associations are somehow more accessible than others'.

The probability of occurrence, within an individual, of a particular association depends upon its remoteness. For Mednick, "remoteness" is directly proportional to the number of other associations arising in response to a stimulus before this particular association occurs. If each association possible for a certain stimulus were arranged along a continuum, with the least likely responses at the outer edge and the most likely ones at the inner edge, we should have a picture of our subject's "associative hierarchy" for that particular stimulus. If we had plotted each response after one thousand stimulus presentations, we might have half of a normal-shaped curve of probability along the continuum. This curve would tell us the probability of any response for our subject to this stimulus. But probability, in this case, is really equivalent to the degree of dominance of a response. It should be readily seen that two individuals could have exactly the same words in their response hierarchies to a certain stimulus, but have very different overt response patterns because of the different profiles of dominance or probability they would show for the events in their hierarchies.

In any individual whose associative hierarchy is "steep," there are a few well-used responses to a certain stimulus which dominate all others. The remote associations rarely get the opportunity to come into play, and typical responses could not be called creative. In a "flat" hierarchy, on the other hand, the high-probability responses are less dominant, remote associations have almost as high a probability of occurrence as ordinary ones, and creative behavior frequently occurs.

Mednick argues that individuals could have steep but deviant hierarchies, and he includes these people within his definition of creativity. Their inclusion is an artifact of our definition of originality as a low statistical probability of occurrence within a population sample. It is a useful definition, because of its clarity and ease of assessment. But as we have seen, selective criteria and usefulness in creativity are also important. And there is still another significant characteristic of creativity which is not included in measures of originality.

Usually, those capable of responding "originally" have such a capacity because of a wide-ranging variety of possible responses (that is, a flat associative hierarchy). It is because of this that they are able to be flexible; in another situation, with different stimuli, the individual will be capable of yet another original and appropriate behavior. On the other hand, the person with a steep but deviant hierarchy is capable of only a few dominant responses—he has no appropriate behaviors in many stimulus situations, and will be rigid, not flexible. If he is fortunate, his deviant dominant responses will be appropriate and culturally acceptable, or even desirable, and he will be called "creative" (by Mednick, and perhaps by his culture)—although his creative life may be short-lived, since he is capable only of repeating his first successes (this is recognized by Mednick). If he is less fortunate, his deviant behaviors will be inappropriate or unacceptable to his culture, and he will be labeled "neurotic." But this "neurotic" behavior is no more inflexible or unadaptable than that of the rigid "creative."

Thus, it seems careless to leave unqualified our dependence upon statistical rarity, even with the qualification of usefulness. Creativity is desirable not only because it introduces novel events into a culture, but also because it provides flexibility for the individuals within the culture. If we lose sight of its real evolutionary value, we shall be the victims of our own definition. Therefore, flexibility must be intrinsic in our understanding of creativity (Guilford, 1967).

Mednick finds considerable support, however, for his contention that creative responses are the product of a flat associative hierarchy. For instance, Bousfield, Sedgewick, and Cohen (1954) found that the total number of associations made by a subject to stimulus words was strongly and inversely related to the rate of associations. Since the creative subject's responses to any stimulus should have roughly equal dominance, Mednick had concluded that "the high creative subject (flat hierarchy) would respond relatively slowly and steadily and emit many responses while the low creative subject would respond at a higher rate but emit fewer responses (Mednick, 1962, p. 223)." And such appears to be the case. This position is more supported than refuted by widespread findings of higher verbal and/or ideational fluency among creatives (Barron, 1968, 1969; and others).

If we admit that associations do have a continuum of probability in response to a certain stimulus, and that this continuum, in a very abstract sense, has certain properties which differ from individual to individual, then we have merely described a very simple associational process, taking place on an elemental level. What leads us to believe that these very fundamental phenomena have any analogue in overt creative behavior?

In a study of his own, (reported in Mednick, 1962), Mednick found that groups of research scientists rated high or low in creativity could be very easily discriminated by the relative frequency of stereotypical responses they gave to stimulus words. The low-creativity group responded more stereotypically on eighty percent of the words.

Mednick's Remote Associations Test (the RAT, which has already been described in this paper) has also found substantial validation. Mednick reports that RAT scores correlate significantly ($\underline{r}$ =+.7) with rated creativity of students in a design course, a nonverbal task situation--this result, in spite of the strongly verbal nature of the RAT. The test also correlated +.31 with the Originality Scale of IPAR, and

-.31 with the Crutchfield Conformity Score. High RAT scorers proved to be significantly more "liberal" on questions of sexual morality and women's rights, and scored higher on the scales of the Strong Vocational Interest Blank which are most strongly correlated to other measures of creativity and which are also relatively uncommon interests in the normal population (for example, artist, psychologist, physician, mathematician, and author-journalist).

Many criticisms have been leveled at Mednick's RAT as a creativity measure. Some are more telling than others. Arguments about whether or not the RAT is a measure of convergent or divergent thinking (Taft and Rossiter, 1966) are aimed at a straw man. Quite clearly, the RAT is a test of convergent thinking in the true sense of the word, "convergent." Three stimulus words are given, and one "right" word is given in response. However, convergent processes have been accepted and agreed upon (Taylor and Holland, 1964) as part of the total creative process, and proof that the RAT is a convergent test is not proof that it is not a measure of creativity.

Arguments that the RAT is an originality test (Hood, 1969) are perhaps equally beside the point. If originality is not creativity (and it is not), then it is at very least a major and vital prerequisite. Without original responses upon which the selective criteria can operate, no creativity is possible. At any rate, some task-appropriate selection of the original responses is also required in the RAT. Perhaps Hood (1969) is objecting in part to the very specific nature of the RAT when he accuses it of measuring originality instead of creativity; no global behavior is measured by it, nothing even close to real-life gross behaviors is elicited. On the contrary, what we see in the

RAT is something just about as close to the original neurological association or "ingram" as we can possibly obtain from measures of overt behavior.

But in a very real sense, the elemental quality of the RAT is what makes it a valuable measure. Here it is possible (not proven, but possible) that we have a measure of the very basic processes which enable more molar creative behavior to occur. If, as we believe, the elements of creativity and intelligence are identical, then the RAT should measure intelligence as well. Its detractors have argued that this is exactly what the RAT does measure, and that that factor should be eliminated from it. But if the natures of creativity and intelligence are intimately related to elementary associational processes, we cannot have a test of one at that level without tapping the other. Thus, it should not prove surprising that the RAT correlates .62 with the Otis IQ, and .66 with the full-scale (verbal and quantitative) Scholastic Aptitude Test (SAT) scores of undergraduates (Gamble and Kellner, 1969). Schlicht, et al. (1968) argued that since the RAT is a verbal test, a non-verbal intelligence test would show no relationship to it. Although they found that the correlation was lower than usual when creativity was compared with the Cattell Culture-Fair test of intelligence, it was still significant (+.36 for males and +.30 for males and females).

The Mednick measure of the availability of remote associations thus seems to be tapping a mental process closely related to intelligence, and essential to creativity. But it seems to indicate at the same time some divergence between the two phenomena at the lowest of levels. And so far, we have no information to help us to understand how that can be.

The clue lies in the independent variables which Mednick hypothesizes facilitate remote association. He suggests that remote associations are the immediate results of a flat associative hierarchy, an abundance of associative elements, serendipity, the ability to select the creative combination, and cognitive mediation between and among associations. We have already discussed how a flat associative hierarchy can facilitate novel associations. It frees the mind from the domination of common responses and gives a higher probability of occurrence to more original responses. Some of the other facilitators of remote associations (the number of associative elements, serendipity, and the ability to select the creative combination) are all strongly similar to those Campbell suggested, and provide obvious and logical benefits for the appearance of remote associations.

It is the idea of cognitive mediation which is our stepping-stone toward a more complex conceptualization of creative thought. Mediation provides a way of looking at the elicitation of novel responses as something more complex and more believable than a simple S-R chain. There must be some way for associational hierarchies to affect each other---otherwise, only the dominant associations would ever be elicited, even in creative people, and especially on a first trial (as in "insight" learning).

Mediation is a process in which "the requisite associative elements may be evoked in contiguity through the mediation of common elements (Mednick, 1962, p. 222)."

Although he almost laboriously avoids any differentiation of this associational process into levels, or vertical hierarchies (in associative hierarchies, elements are presumed to be peers, horizontally arranged), vertical differentiation is exactly the path down which Mednick's mediation idea leads.

If remote associates appear to be more accessible in a certain person, a claim that he has a "flat associative hierarchy" is no explanation of his behavior. Why does he have a flat hierarchy? And what does the term mean, aside from its applicability in describing behavioral data? Mediation provides one of the answers to these questions. Simply stated, an individual with a flat hierarchy must have many interconnections between hierarchies--many elements common to more than one hierarchy which can call the others into play. This provides a means for relatively unexpected associations to be made; remote associative elements can be made available through mediation.

If this argument is sound, there should be some relationship between facility at cognitive mediation and facility at providing remote associations. Higgins and Dolby (1967) attempted to test the existence of the relationship between mediation and remote-association abilities. The authors devised a learning task in which pairs of words (half of them related to each other by a common, but unstated mediator; half of them nonmediated, but not unrelated) were learned. In spite of some methodological problems which tended to act against them (like the subject's awareness of the mediation sequence), the results were significant. With the learning of the nonmediated pairs held constant, RAT scores correlated significantly and negatively (-.312) with errors made in mediated-pairs learning. Although this study has limitations and needs very much to be substantiated by additional and perhaps more sophisticated evidence, it does provide some very interesting support to the idea that the process of mediation of simple concepts or symbols (like words) is significantly related to the flexible and creative nature of associational processes.

In their attempts to train uncommon word associations, Simon, et al. (1966), based their work, as we have seen, upon that of Maltzman (1967). One incidental effect they discovered was that uncommon stimulus words were most frequently followed by uncommon response words, while the opposite trend resulted from common stimulus-words presentation. They could interpret this result as supportive of Maltzman's argument that "common" and "uncommon" represent two separate associational classes, and that since associations are stronger within any class, common stimuli result in common responses while uncommon stimulus result in uncommon responses. It seems to this writer, however, most inefficient for so many (an unspecifiably large number) associative elements to be stored and interconnected in classes according to their functional and logical relationships, but at the same time form a class (that is, have direct connection) with every other element of their comparative level of commonness or rarity. Some kind of search operation, at a higher level of cognitive organization, capable of sensitivity to the commonness dimension, is far easier to envision than this duplication of elements in many separate classes. Even if each element needed to have a kind of chemical code or tag (perhaps dependent on rate of usage), the complexities of such a system of high-level searching are far less overpowering than those of Maltzman's hypothetical system.

The idea that cognitive processes differentiate vertically into levels of operation is inevitable. As Neisser (1967) comments, without allowing for some kind of executive process, "we must think of every thought and every response as just the momentary resultant of an interacting system. . . (p. 293)"--a system whose elements are at a single level and whose organization is determined only by associational "habit" strength.

Of course vertical differentiation really serves both vertical and horizontal mediation. It is difficult to imagine many levels of control without interaction of the levels, and when there is interaction of levels, there are alternate pathways to different parts of the same level--that is, horizontal mediation. It is in theories of such mediation that we may find the reasons for differential dominance of common or uncommon associations.

On the other hand, assuming there are levels of command in human cognitive systems, the associational processes must be basic to all these levels and to all of their interaction. It must be basic to higher thought processes; it must be basic to creativity. The remote-associations model does predict and find creativity at the global level as a result of flat associative hierarchies. Therefore, even when we move to a more complex view of cognition, we may find that principles applying to simple remote associations also apply to more general cognitive systems. But it is as we begin to speak of the interaction of various associative hierarchies at various levels that we suspect the major differentiation between creativity and intelligence will be discovered.

IV. HIGH-LEVEL HIERARCHIES

Schroder has hypothesized a "conceptual systems theory" (outlined in Schroder, Driver, and Streufer, 1967) which is an attempt to describe the individual's interaction with his environment as a function of his information-processing abilities and his cognitive structure. Karlins (1967) has summarized the Schroder theory in this way:

> Over a given range of stimuli, information-processing ability varies among individuals and is measured in terms of its integrative complexity. Higher integrative complexity refers to a greater number of perceptual categories for receiving information about the world, and more conceptual or combinatory rules for organizing such units of information. Structural complexity is described as varying along a continuum that represents gradation in integrative complexity (p. 264).

From this viewpoint, Schroder and his colleagues generate several specific predictions, which Karlins outlines and draws into comparison with predictions generated by Mednick.

Because of their greater integrative complexity, some subjects will be able to make a broader range of intercategory combinations--more "remote" classes of information will be able to be brought together. These same subjects, whom we might call the "Schroder" creative subjects, should have bonds between different categories which are of more even strength than those of less integratively complex subjects. It is argued that more statistical uncertainty is generated by wide as opposed to narrow but in-depth sampling of information categories, and that integratively complex subjects would be more able to handle such uncertainty--that is, to tolerate it. Of course, uncertainty might not be generated in one's exploration of one's own information categories in memory because their content would not be unfamiliar. But if the subject were to sample information from an external source (such as the resources of a computer) this uncertainty aspect would come into significance--and in any case, wide search patterns might be expected of the Schroder creative subject for other reasons

Karlins does not mention.

The similarity of the Schroder and Mednick systems is quite striking. Each predicts a sort of hierarchy basic to cognitive structure. Each predicts that among creative individuals, such hierarchies of association will be flat, and large numbers of associations will be more likely to occur.

The significant difference between the two hierarchies suggested by the two theorists is the level of abstraction or generality of the members of those hierarchies. The elements of the Mednick hierarchy are simple word (or idea) responses; the elements of Schroder's are idea classes or categories which could themselves conceivably be hierarchies (for example, "religion," or "sickness").

It appears, then, that Schroder's system is the outside of a Chinese-puzzle nesting of hierarchies within hierarchies, with the same principles of causality operating to produce creativity and originality at each level. At least we know that the principles Schroder assumes are believed to be the same at a very much lower level (Mednick's). What levels may lie between Mednick and Schroder, or within them or beyond them, and what <u>their</u> rules may be, is not indicated.

It might be expected, or rather it might even be hoped, that measures of Mednick's creativity and Schroder's creativity would show a strong relationship. The results of research efforts to the present, however (Karlins, 1967; Karlins, Lee, and Schroder, 1967) have not been easily subjected to synthesis.

Part of the difficulty of comparison rests in evaluation of the test used by Schroder and Karlins to discriminate between groups high and low in "integrative complexity" (Schroder creativity). The Paragraph Completion Inventory (PCI) has no obvious, direct relationship to the Schroder theory, and is so open-ended that scoring of it may be a largely subjective matter.

A study was made, however, to validate the use of the Schroder PCI in separating creative from noncreative subjects (reviewed in Karlins, 1967, and in Karlins, et al., 1967). Karlins made one further assumption before preceding with the validation: that a subject's recorded use of the resources of a computer in a complex problem-solving task would be closely analogous to the unrecordable uses he makes of his own cognitive information resources. We have already mentioned one qualification that assumption should include, but it does seem reasonable that some relationship would exist between the ways in which one uses one's own memory "banks" and those of a computer.

The results of the experiment were that the Schroder PCI did discriminate between flat-hierarchy creative sources (as determined by the fact that many of the computer's categories were tapped, and questions were more broadly distributed among them) and steep-hierarchy noncreative subjects. The RAT scores of the same subjects did not discriminate significantly, although correlations were in the predicted direction. Abilities tapped by the RAT and the PCI were mutually enhancing; the highest number of categories searched and the highest breadth-of-search scores were found among subjects in the group high on both the RAT and the PCI.

Although we may not agree with Karlins' assertion that no higher-order integratory mechanisms are necessary for remote association, his general explanation for the weak relationship between the tests could be tentatively accepted. He argues that Mednick's associational theory may apply only to the lowest levels of a creativity scale, and "that both conceptually simple and complex individuals may be associatively creative, but that only structurally complex individuals are integrative complex [that is, Schroder-creative] (p. 267)." His position does not provide an explanation for the possible existence of subjects high on the PCI and low on the RAT, although there may be other, unrelated variables accounting for them.

In substance, we have already argued that the RAT should be a measure of fundamental potential for creativity—that it only begins to discriminate between creativity and intelligence, and that some further kind of complexity is the probable source of their distinction. This argument is supported by the finding that the number of categories searched in the computer-use task is significantly and positively related to Guilford's Uses For Things creativity test, but unrelated to the Wonderlic IQ (Karlins et al., 1967, p. 166). At one of the most open-ended, global tasks which might be imagined ("build a hospital and use the computer to get information for it"), the style of association-formation seems not to be significantly related to intelligence. Somewhere between the first box and the last box in our Chinese puzzle (or in what we can see of it), lie some answers for our questions about the creativity-intelligence distinction. We shall try to look between.

V. COGNITIVE DIFFERENTIATION

We can now quite safely assume that both creativity and intelligence have their foundation in simple associational processes. But we also know that in some way, intelligence and creativity become differentiated from each other—otherwise, it would not be possible for variables to have effects on one and not the other.

It is our task, then, to try to outline possible processes of cognitive differentiation between creativity and intelligence. We shall follow, in a rather theoretical path, the differentiatory process from the associational stage to its end product in executive-type cognitive operations. We shall divide our efforts into two sections roughly equivalent to low-level, primary differentiation and high-level, secondary differentiation. We see these levels as more or less analogous to two structures hypothesized by Schroder to be necessary for cognitive complexity: perceptual categories, and combinatory rules.

Perceptual Categories

Although the brain probably has approximately fixed kinds and numbers of neural interconnections and limits to growth and differentiation placed upon it by metabolism and blood supply (Hebb,1949; Rimland, 1960), and possibly a few innate structural pathways or a rudimentary organization (Hebb, 1949; Piaget, 1969; Milner, 1970), its various activity areas are quite probably within themselves undifferentiated and equipotential (Hebb, 1949; Diamond et al., 1963; Bennett, Diamond, Krech, and Rosenweig, 1964; Milner, 1970). It is the task of learning and experience to provide differentiation—and to provide a differentiation which corresponds to some extent with an external reality.

> The process of perceptual learning must be thought of as establishing a control of association-area activity by sensory events (p. 123). . . We can then regard the stage of primary learning as the period of establishing a first environmental control over the association areas, and so indirectly, over behavior (Hebb, 1949, p. 125).

Hebb's theoretical system traces the development of higher cognitive processes from the first elemental neural associations. When neural events corresponding to sensory stimulation are simultaneous, they become related to each other.

> It is proposed that a repeated (simultaneous) stimulation of specific receptors will lead slowly to the formation of an "assembly" of association-area cells which can act briefly as a closed system after stimulation has ceased; this prolongs the time during which the structural changes of learning can occur and constitutes the simplest instance of a representative process (image or idea) (Hebb, p. 60).

Of course, as Diamond, et al. (1963) point out, the process of neural differentiation cannot be simply the result of intercellular facilitory associations. In order for the organism to be capable of intricate and environmentally appropriate behavior, it must learn to discriminate among stimuli. Discrimination is learning to be responsive not only to what a stimulus is, but also to what it is not. Our sensitivity to a unique element of a stimulus must elicit some inhibition of our response to previous and similar stimuli if we are to be able to respond to the new stimulus differentially. Therefore, neural associations must have an inhibitory as well as a mutually excitatory nature. This kind of inhibitory process makes discrimination possible.

Inhibition is also the process which enables generalization to occur, and generalization is extremely important in the development of cognitive processes.

In most cases, generalization is taken to be a state of being insensitive to distinctions between stimuli (Deese and Hulse, 1967; Tempone, 1965). But this need not be the case. In a study by Gardner (as reported in Gardner, Holzman, Klein, Linton and Spence, 1959), subjects were asked to categorize objects. It became apparent that subjects who generalized more broadly were not simply less aware of distinctions than were the other subjects. On the contrary,

> . . . some broad-range Ss noticed many subtle differences in objects in the Sorting Test. The essential difference between the Ss at the opposite poles seemed to be in the degree to which they were impelled to act upon or ignore an awareness of differences (p. 39).

In any philosophical or linguistic usage of the word "generalization" it is understood that one ignores an awareness of distinctions for the purpose of making use of similarities along a certain continuum. Indeed, if any population of elements is to be available for recombination along more than one continuum, their distinctions must first be learned. Then, according to the purpose of the moment, any number of generalizations can be made, each of which is constructed among the elements having the quality selected for attention--all other qualities, all other distinctions, are ignored.

In the development of basic perceptual categories, or any other cognitive classificatory systems for that matter, the process is the same. First, there is the generalization of ignorance. Similar stimuli elicit similar responses and facilatory associations are formed. Then, there is discrimination. The organism learns, consciously or unconsciously, to inhibit old responses to similar stimuli; new responses are made and are contingent upon certain specific aspects of the stimulus. There may be finer and finer discrimination, in which smaller and smaller parts of an originally broad-range response pattern are eliminated in the presence of specific cues. But the organism would be hopelessly fragmented and specific if it were not then possible to generate new, "aware" generalizations--generalizations which can be more and more inclusive, reaching a high level of abstraction.

These generalizations are expected to be flexible in nature, because of heavy overlapping. They involve temporary inhibition of responses to stimuli having a certain quality. One response can thus be involved in many completely different generalizations. If such were not the case, no flexibility would be found in human beings, and so it must be assumed to be a natural tendency--a tendency of vital significance in cognition.

It is no wonder that Harlow is reported (in Diamond, et al., 1963) as making the comment that "all learning and all thinking may be regarded as resulting from a single fundamental operation, the inhibition of inappropriate responses or response tendencies (p. 287)."

Are capacities for discrimination and generalization related to intelligence?

Hebb (1949) indicates that one locus of individual differences in neurological efficiency is the differences in potential for discriminatory acuity. There is additional evidence that intelligence is the result of or a covariable with efficient discrimination processes, however. Tempone (1965), with a technique borrowed from Mednick and Lehtinen (1957), studied visual discrimination abilities and their relationship to mental age. Subjects divided into high, average, and low mental-age groups according to the Pinter General Abilities Test were significantly different in their performance on the discrimination problem at better than the .05 level of probability. Children with higher intelligence were better at visual discrimination, and made fewer errors through over-generalization.

In a study with mental patients, Desai (1960) correlated scores from Raven's Progressive Matrices Test (which is, according to Matarazzo, 1972, substantially but not overwhelmingly correlated with the

WAIS) and Epstein's Over-Inclusion Test. Correlations between the intelligence test and the number of errors due to over-inclusion (-generalization) was negative and significant (beyond the .01 level of significance). Desai comments that the sample was higher in average intelligence than the norm, and had a smaller standard deviation; it is very possible among a normal population, correlations between the two tests would have been even higher. High-IQ subjects, then, show a significant tendency to avoid errors of generalization---to avoid the kind of generalization which would show a lack of discrimination, rather than an overriding of discriminations.

It is of supplementary support that Spotts and Mackler (1967) found a significant positive correlation (p less than .01) between Otis IQ and field independence on both the Embedded Figures Test and the Hidden Figures Test. Highly intelligent subjects may be able to "discriminate" a figure from its field. And Kerrick (1956) found that high-IQ subjects used the Osgood Semantic Differential Scale more fully than did low-IQ subjects, relying less on extreme positions and more evenly on all positions (level of significance for the difference was better than $p = .01$). Thus, high-IQ subjects show a tendency to use finer discriminations in evaluation, as in many other kinds of tasks.

Furthermore, we know that intelligence is strongly related to memory processes, and Helson and Cover (1956) discovered that subjects perform better on memory tasks when items to be memorized are presented in more specific categories. They suggest that over-general categories allow too much interference and impair memorization. We suggest that the facility for memorization found among highly intelligent subjects may be the result of better discriminations and better category systems would be of little help in the learning of nonsense syllables or other rote memory tasks, and these would not be expected to be (and are not found to be) any easier for the more intelligent subject than for his less intelligent peers (Cronbach, 1970).

All of these evidences indicate that high intelligence is, or brings with it, a broadly based efficiency at making discriminations. They also suggest that when intelligent subjects regeneralize (speaking now at the level of the formation of the most elementary perceptual and conceptual categories) their categories are narrow and maintain a high level of discriminatory potential. For more abstract purposes, a higher order of generalization, with each of these categories as an element in the new generalization, is constructed. It is hypothesized that a higher potential number of both facilitory and inhibitory associations in the highly intelligent subject make him capable of maintaining more discriminative and fine-grained categories, and perhaps enable him to construct more levels of "aware" generalization than an average person might have over the same total difference in level of abstraction.

These kinds of processes begin at a level so basic as to be beyond our intuitive understanding. By the time we reach the level of activity represented in the RAT, for example, many differentiation/generalization processes must have already gone on. The subject must at that level be capable of integrating his sensory input, his linguistic training, and so on. In fact, the cognitive structures which enable a subject to do well on the RAT are probably at a higher level than is represented by the simple-word items of the test. Mediation, after all, is accomplished to a great extent by higher mechanisms; it is the generalization of the test items into higher categories that makes other associations to them accessible.

But Schroder's system is argued to be a very high-level analogy of Mednick's. What, then, is the difference between what has here been proposed and Schroder's system of proposals? Very little, except that it is here argued that the total capacity in an individual for such complexity of differentiation and integration is in actuality his potential for intelligence (that is, that his complexity capacity is his intelligence A). Schroder, on the other hand, believes this complexity, at its most global and social level of manifestation, to be creativity. He comments:

> When personality structure is taken as the anchor for viewing behavior, then we are focusing upon creativity--the ability to generate diversity and conflict, to evolve alternate organizations or integrations of diverse perceptions and decisions (Schroder, et al., 1967, p. 11).

Schroder's (and Karlins') assumption is that higher integrative complexity is necessary to and sufficient for the "alternate organizations" which are equivalent to creativity. And of course, we could not disagree in the assumption that creativity is the ability to generate "alternate organizations or integrations."

Bruner (1964) provides an exciting argument which is partly supportive and partly elaborative of this position. His is a kind of "one-better" approach. Creativity is envisioned to be simply a high-level sort of generalization (where generalization is what he calls "generic learning"). This higher level of generalization can be called "generic coding," although Bruner admits that the difference between generic learning and generic coding is one of degree or level only. Generic processes are compared to an emptying operation, in which the abstraction is a "contentless depiction of the ideal case, empty in the sense that geometry is empty of particulars (p. 307)." If this is creativity, then creative works of great moment and magnitude are merely products of generic processes at still a higher level--a reabstracted, generic "supercoding." Comments Bruner:

> It seems to me that the principal creative activity over and beyond the construction of abstracted coding systems is the combination of different systems into new and more general systems that permit additional prediction. It is perhaps because of this that, in Whitehead's picturesque phrase, progress in the sciences seems to occur on the margins between fields (p. 308).

This is an attractive Jacob's Ladder, but as a theoretical system it presents almost as many questions as solutions. If creativity depends entirely upon the individual's ability to construct viable generic systems, why is it not directly dependent upon intelligence? Examine the multitude of ways in which highly intelligent subjects prove themselves capable of such system construction: they are superior at memory tasks, information storage, discernment of similarities, recall of remote associations, vocabulary, symbol manipulation, arithmetic operations, and general comprehension (Wechsler, 1958; and others). Any higher systemization of categories and generalizations must certainly depend upon systems like these, as well as a high potential for fine discrimination; we have consistently seen that the higher systems are derived from the lower (Karlins, 1967). It seems impossible that the causes of the problematic creativity-intelligence divergence can have any basis in only these kinds of structural potential. The answer must have something to do with the direction in which such structures are put to work, or their management by other structures. That is where we must next search, in hope of finding our suppressor variable.

Sets and Combinatory Rules

We are now interested in higher-level patterns of response tendency which either direct differentiation at lower levels, or direct the use of differentiated structures, or are themselves the products of differentiation processes.

First, let us examine sets.

We have already, in another context, discussed sets. But they hold new relevance at this point in our discussion because they are phenomena which seem to guide or direct behavior--presumably according to principles previously derived from learning and experience.

If more intelligent persons were more readily able to acquire principles by which to direct their behaviors (and this is not an unreasonable suggestion), they might be more susceptible to sets or negative transfer to new situations. Are they more prone to negative sets than others? The research data is a chaos on this question.

One significant positive correlation between an intellectual measure (the Miller Analogies Test) and a measure of rigidity which has questionable validity was obtained by Kapos and Fattu (1958). Two insignificant positive relationships are reported on the same variables by Kapos and Fattu (using the Scholastic Aptitude Test), and Galanter (1967).

Two researchers report insignificant negative correlations between academic achievement and rigidity (Galanter, 1967; Davids, 1956), and Lester (1966) found a negative correlation between the Otis Quick-Scoring Test and resistance to extinction. These three results are confirmed in part by Rokeach's (1948) finding that the Einstellung effect, which is really a negative set, was negatively correlated with the Stanford-Binet.

These studies provide proof that if there is any relationship between manifest rigidity or negative sets and intelligence, it is not a simple one, and not a strong one, and more probably negative than positive. The best indication of the real state of things is probably provided by Duncan (1959) in a review of problem-solving research. He concluded that more intelligent subjects are more likely than the average to be good at problem solving and at overcoming negative sets—while they are at the same time possibly able to benefit more than their peers from positive sets.

It should be obvious that mere susceptibility to transfer would not be of any net significance, since negative effects would tend to cancel out positive effects, and so on. What does make a significant difference is how one handles the sets, and Duncan reports that highly capable subjects seem to get more out of pretest directions, and to be able to overcome negative sets if necessary. They are neither more susceptible nor less. They simply are able to manipulate their own sets to greater advantage.

It may turn out that what we are examining here are examples of generic learning derived from more concrete learning. In essence, an individual may recombine categories evolved from sensory information to form a higher generalization which can tell him how to deal with that information. For example, Harlow (1949) was able to train his monkeys to acquire a generic learning set for dealing with their negative, task-to-task set. This point of view about sets is, of course, very much consistent with Bruner's (1964) position.

But Bruner's explanation of creativity as an increasingly abstract process of generalization--up and up and up--cannot be sufficient to explain all cognitive systems. It is no more sufficient to that task than a radical associationist view which posits a process going out and out and out. It may be profitable to try another tack altogether.

A slightly different way of examining the problem is to use Neisser's (1967) analogy between sets and schemata, and to look at them both as combinatory rules--rules for searching and recombining existing categories of information. When schemata have been developed for dealing with information, even though they are themselves derived from it, they act upon it, transforming it (Posner, 1965; Neisser, 1967) in ways that make it more useful. These schemata are generalizations for a purpose, and it is conceivable that they might have varying directional tendencies. Not all schemata are in the direction of greater abstraction; they could perhaps be capable of organizing information toward exactly opposite ends. Neither is there any need to see them as endlessly nested upward. There may be a completely different kind of organization at some cognitive level.

We have already hinted that to explain cognitive processes fully we must call upon something like an executive computer program, whose heuristics are like operational sets. Neisser (1967) suggests:

> Some programs may even have a hierarchical structure, in which routines at one level can call those which are "lower" and are themselves called by others which are "higher." However the regression of control is not infinite; there is a "highest," or executive routine which is not used by anything else (p. 296).

Neisser clarifies the fact that human "executive" program must (presumably) be capable of being derived from experience and learning, and must also be capable of self-modification. It is still, however, relatively independent of the vicissitudes of everyday learning experience, and it exerts a definite shaping force on all products of the neural "computer." Conceivably, it could direct operations working at a higher level of abstraction without itself being at a lower level of control--that is, without itself being used by the more abstract generalization or operation.

Of course, we have no present proof that such systems exist, but they seem to be the only logical way out of the "up-and-up-and-up" dilemma. Further, there are clues to their existence, and to the possibility that they are closely related to the creativity-intelligence problem.

Riegel, Riegel, and Levine (1966), in basic agreement with the Mednick and Schroder hierarchical models, set out to discover something more than the models suggested about hierarchical differentiation. They tested the hypotheses that: (a) creative subjects have flat associative hierarchies, and (b) creative subjects have different patterns or classes or responses to a stimulus. Fourteen tasks were presented to the subjects, and divided for analytical purposes into five categories: imitative, logical, grammatical, infra-logical (that is, physical), and free associational.

As expected, high-creativity subjects showed greater differentiation on the free associational task. They also showed greater differentiation on all of the other tasks, except for grammatical and infra-logical. In these two categories, low-creativity subjects had greater differentiation. Over all categories, highly creative subjects used significantly more terms of logic and relations in description, while the non-creative used significantly more terms of functions, parts or attributes. Thus, the task categories which were highly differentiated for each group seemed to represent the kinds of approaches taken by that group to all tasks.

What this study appears to demonstrate is that individuals are not merely highly differentiated, with flat hierarchies, or little differentiated, with steep hierarchies. Karlins had stipulated (1967) that individuals could have different interest or ability areas, in which they could show opposing patterns of differentiation (flat or steep). But what we see here is not merely a difference in hierarchy slope, or a difference in interest area or ability field. What we see is evidence that differentiation can be invested in the direction of either the concrete <u>or</u> the abstract--that individuals can have schemata which direct their behavior (and their processing of information) in stylized directions, among whose possibilities are abstraction or concreteness.

VI. CREATIVITY AND SCHEMATA

Is there a creativity schema or schemata? If so, what is its (their) nature? The presence of creativity schemata can only be guessed at, but if they exist something can be predicted about their nature. First, they will be the perpetrators of the creativity/intelligence divergence--combinatory rules which depend upon cognitive potential (intelligence) for complexity, but do not automatically coexist with that complexity. Second, they will be highly placed schemata, responsible for either wide-ranging or specific areas of creative behavior. Third, they will be capable of directing the highest levels of abstract processes, as well as facilitating them.

Fourth, they will be schemata embodying the principle of flexibility, capable of generating systems and regenerating them quickly, and intrinsically tending toward remote association. Fifth, they will come into existence through some facilatory environmental effect--some pattern of reinforcement of past behaviors. Sixth, in their absence, other kinds of schemata, also capable of abstraction but oriented toward concrete information processing, concrete sensory input, and factual manipulation, will be developed--these, too, will arise because of an environmental selection process. Seventh, any conceivable ratio of creative/noncreative schemata is possible, with the pattern of their proportional dominance drawn along existing lines and patterns of environmental selection.

We need now to look at some research data which can provide confirmation or qualification of these postulates.

For example, there is evidence that creative behaviors are learned best not specifically, but as a class of responses. As we saw in the creativity-training review, Levy (1968) obtained his best training of creative responses when composite techniques were used, including a role model, which presumably acted as an organized system of rules for the benefit of the child. Furthermore, transfer to novel contexts occurred only when this model was evoked. The subjects could incorporate the model as a schema for temporary behavior regulation, although they had not yet internalized it. Brown (1965) and Renner (1970) obtained results which also clearly supported the idea that creative responses are best learned and transferred as a group, especially if some verbalized rules or symbolizations are involved.

Additionally, the general tendency of creative individuals to show predictable patterns of interpersonal, problem-solving, and preference behaviors indicates the possibility of modes of cognitive organization which are consistent through several levels of abstraction and which pivot around the principles of flexibility and originality. These modes could well be the creativity schemata. And thus evidence does appear as to their existence, their "rule"-like character, the breadth of their effect, and their dependence upon flexibility; our first three postulates have support.

There are other evidences in support of the fourth postulate and those after it. While creative people may be equally capable of spontaneous—or slightly triggered—emission of both common and creative responses, noncreative people have a great deal more difficulty exhibiting creative behaviors. Riegel, et al. (1966) found, for instance, that high-creativity subjects used fewer logical responses than low-creativity subjects on the free association task. But when asked to produce logical responses, they were well capable of doing so, and used many associations frequently used by low-creativity subjects. We find a similar phenomenon in the results of Mednick's (1962) RAT, where the first associations emitted by high-creativity subjects may be very common ones, but uncommon responses are quick to follow, and almost equally available.

McDonald and Martin (1967) found that low-creativity subjects were exceedingly difficult to train to uncommon word associations, while highly creative subjects (whose typical mode of response was originality) were easily shifted to making common responses by means of verbal reinforcement. When readministered the RAT after "commonality" training, they showed no decrement in their scores. Flexibility twice demonstrated.

Duncan (1959) noted that previous use of an object in a commonplace manner inhibits unusual use of that object later. On the other hand, previous unusual use of an object does not create a "functional fixedness," and subjects who have been so exposed to originality are later able to use the object in its customary manner. Creativity was in a manner of speaking "induced" in these subjects, by exposure to novel uses (or various uses) of an object. Presumably, subjects who come to our attention already creative have internalized some way of looking at objects and ideas in flexible and various ways--this is their schema.

We point to a study by Eisenstadt (1966), in which it was found that on insoluble rebus puzzles, highly creative subjects gave up an average of twenty-two seconds sooner than the group low in creativity. This difference was not due to the low efficiency or interest of the highly creative, because on the soluble puzzles they achieved as many solutions as their peers and did so in a shorter time period. It can only be suggested, in the light of the other studies reviewed here, that the highly creative subjects had a capacity for quick changes of strategy which enabled them both to arrive at a proper solution more rapidly, and also to avoid persistance at methods which did not, or could not, be successful.

All of these experiments hint at the overwhelming importance of flexibility to the highly creative individual. He seems to be able to move back and forth among strategies with little difficulty. He must, as the fourth postulate suggests, have means of rapidly generating and regenerating systems of operation. And the most likely form for that sort of capacity is a kind of high-level schema or executive program.

It might be argued that creative or noncreative schemata are simply opposing tendencies of recombination--one toward novelty and the other toward commonality, or one toward abstraction and the other toward concreteness. But the evidence reviewed argues to the contrary --that creative schemata include both the common and the uncommon, the abstract and the concrete, while schemata directing the less creative person's behavior are not so multifaceted. With this suggestion in mind, it should be a simple matter to decide which kind of cognitive organization is most desirable and adaptive.

Another principle evident from the data is that creative or noncreative schemata are affected socio-environmentally. All training procedures were able to show some effects, although results were not as easily achieved as might have been expected. Therefore, postulates five through seven find support. The appearance of even this degree of responsiveness to reinforcements and modeling treatments implies that the original schemata are constructed in response to environmental effects.

What do the data tell us about intelligence and the creativity schemata? Let us reexamine the Wallach and Kogan experiments (1965a, 1965b). Boys were asked to divide a group of fifty objects into categories of their own construction, according to which ones "seemed to belong together." They were then asked to express their grouping rationale, and these rationales were analyzed. Possible categorizing principles were assumed to be physical-descriptive (for example, "hard objects"), conceptual-inferential ("for eating"), and relational or thematic ("getting ready to go out").

The results of analysis showed that high-creativity subjects, no matter what their intelligence level, tended to use both inferential and thematic categories. Boys high in intelligence but low in creativity showed a strong dominance of inferential categorization, while boys low in both used thematizing almost exclusively.

In order to discover whether the highly intelligent boys who were low in creativity were simply incapable of thematic organization, the authors administered a test of that particular ability. When they had no choice (and very possibly, failing any assigned task would be odious to this group) these boys could thematize as well as their intellectual peers who were also creative.

Wallach and Kogan (1965a) conclude:

> In sum, creative boys seem able to switch rather flexibly between thematizing and inferential-conceptual bases for grouping; the high intelligence-low creativity boys seem rather inflexibly locked in inferential-conceptual categorizing and strongly avoidant of thematic-relational categorizing; finally the low intelligence-low creativity boys tend to be locked within thematic modes of responding and relatively incapable of inferential-conceptual behavior (p. 363).

Evidently, the children low in creativity were limited by that lack, and those low in intelligence were limited as well--even though they were creative. Only the high intelligent children, then, were fully capable of flexible behavior, but they tended to avoid it strongly if they were not also creative.

VII. SUMMARY

On the basis of the research we have reviewed, we can make a few tentative suggestions. In understanding creativity the emphasis, it would seem, needs to be placed upon the originality of behavior at an associational level and the flexibility of behavior at the level of higher cognitive structures. While it is very possibly true that the great products of creative thought are high-level abstractions, this may not be so much because of the nature of abstractions (for surely abstractions can sometimes be uncreative) as because of the nature of creativity (creativity provides a flexibility of recombination which is conducive to workable abstractions). What we find about intelligence is that it tends toward abstractness; but unless creativity is also present, the individual can apply abstractions rigidly. Such a person would be less likely to come upon a highly creative solution to abstract problems; he would also show less flexibility at the level of social behavior. His noncreative schemata, not being flexible and inclusive of creative patterns, would limit him in spite of his capacities for abstraction and complication.

Highly intelligent persons, because of the complex character of their cognitive organizations, undoubtedly have much greater benefit to derive from creative cognitive schemata than most others. Their larger informational stores, more accurate representations of reality, and larger potential number of interneural associations (as hypothesized) would mean that flexible schemata could lead in them to creative production of considerable significance.

But the style of individual's schemata is very likely due to environmental selection, and the highly intelligent person may not be "selected" for creativity. What has happened? He must have a complex cognitive system, or he would not be intelligent; it must be assumed that the cognitive complexity is channeled into noncreative directions. Perhaps the storage and manipulation systems for information handling are highly differentiated, but "programmed" toward specific, rather than flexible, ends. Perhaps abstraction categories are very complex, but do not have a large overlap, so that rapid and flexible construction of many different generic systems is not possible—categories could be highly differentiated, but have a minimum number of interconnections at a very high level.

Whatever its exact mechanisms and derivation, it is easy to see how such a cognitive state might be environmentally determined. Generalizations which were task relevant might be rewarded by the environment, and reinforced in a physiological sense. Connections would thus be established between categories in terms of specific, task-relevant characteristics. Future generalizations would have no interconnections except these to use as mediators—no way to "call" for elements according to some other criterion. Other generalizations might even have been environmentally punished, so that specific schematic impediments were set up against non-task-related generalization. The schemata derived from environmental contingenies during childhood might continue to direct the behavior of the adult.

But training resulting in the severe limitation of generalizations at a high level would be difficult, for overlap is very natural within the human neuro-cognitive system. And if the cognitive schemata we have described are to fill the role of the suppressor variable for which we have been searching, some explanation must be found for why highly intelligent children--who have a natural associative abundance and cognitive complexity--should be directed by their environments toward schemata antithetical to their own tendencies. For this problem, we still do not have the answers.

CHAPTER V

AROUSAL

In Chapter II the suggestion was made that what is testable of an individual's intellectual efficiency--that is, his manifest intelligence--is merely a collection of learned behaviors which we suppose to reflect his innate, biological, potential intelligence (Hebb's intelligence A). It was also argued that the basic elements of cognitive processes are very simple operations, based upon simple associational elements. Intelligence, then (intelligence A), is due to biological limitations upon the number of potential associations, the acuity of perceptual "equipment," metabolism, or other unknown variables (Hebb, 1949).

In this chapter and in the following one, we mean to explore intelligence and its biological variables, and their relation to creativity--in order to investigate why it is that highly intelligent children may develop tendencies away from creativity, in spite of their natural predilection toward it at a cognitive level. We wish to determine if there is a physiological phenomenon covarying with intelligence (or perhaps even responsible for it) which is also intrinsically in opposition to some aspect of creativity.

I. INTELLIGENCE AS MOTIVATION

Keith Hayes (1962), has written a fascinating and original summary of his own theory of intelligence which is of utmost relevance to our present discussion. He posits that manifest intelligence is completely learned behavior (to which we have already agreed), that potential intelligence is innate, and that the hereditary basis of potential intelligence is in a cluster of "tendencies to engage in activities conducive to learning. . . referred to here as experience-producing drives (EPD's) (p. 337)."

With his invention of the term "EPD," Hayes is referring not to something exactly equivalent to a "drive" in the classical sense, but rather to innate tendencies very closely similar to the curiosity, exploratory, or manipulatory drives postulated by many researchers (for example, Hebb, 1949, 1964; Berlyne, 1960, 1964a, 1964b, 1964c, 1965; Harlow, 1965; Day, 1968). And Hayes is also arguing that these drives, and they alone, are responsible for individual differences in learning and (ultimately) in manifest intelligence.

Hayes discounts any other strong possibility as the hereditary carrier of intelligence differences. He argues that fully established, inherited, structural differences in individuals' higher mental functioning could not possibly mediate intellectual variations, since higher mental functions are merely elaborations of simple processes of which every one is capable and the young human brain is extremely plastic. All of this seems well substantiated by research, and evident. But he also argues that there is no upper limit on memory, inherent biologically, which restricts learning potential. This claim is less well substantiated, and must be tentative, at best. Obviously, at some physical level in some physical mechanism there is a heritable, structural difference which causes individual variations in EPD's, if Hayes is correct. Although EPD's are not "restrictive," they are heritable (and they must also be structural) differences. It is not clear why some potentially <u>restrictive</u> variable could not be physiological and heritable in some way analogous to EPD's. Of course, if Hayes is accurate in his suggestions about EPD's, there is no need to search for other mediating variables.

Hayes concludes his hypothetical exposition with the argument that higher animals and human beings have essentially the same capacity for learning. Individual organisms between or within a species vary in intellectual capability only because of their life span, their capacity for language and symbols and the culturally accumulated knowledge they transmit, or the strength of their EPD's. Within species---for example, among human beings---it is the EPD's which account for all variance in intellectual performance. Thus, EPD's in a very real sense, <u>are</u> intelligence.

In spite of the difficulties inherent in the construction of a theory with so few facts to work with and such ambitious goals, Hayes managed to produce a radical and original point of view which at the same time has great merit. He has made the critical distinction between intelligence as a phenomenon (which is a collection of learned behaviors) and intelligence as a biological source of heritable individual variation. He has also pointed out the vital importance of motivational variables as discriminators of intelligent individuals.

It has long been realized that highly intelligent people have significant tendencies toward high motivation. But the traditional interpretation of that trend is that motivation states are environmentally determined variables affecting academic achievement and thus one's performance on an IQ test (usually, adversely), and that they are significant but only partial determinants of a largely inherited intellectual potential. Hayes makes a dramatic change in emphasis, arguing that "true" or innate intelligence and motivation are intimately related, if not mutually inclusive.

Hayes' entire hypothetical structure, with its supposition that people (and even animals) are equally capable of complex learning and its restricted focus upon EPD's as the only source of human intellectual variation, seems difficult to accept. Perhaps we can avoid some difficulties by accepting only part of it.

First, it is agreed that intelligence and motivation (especially exploratory, curiosity and manipulative types of motivation) are intrinsically related. The possibility is also accepted that manifest intelligence is partially mediated by innate motivational differences, and thus that intellectual potential is partially equivalent to motivational "programming." What is not accepted is that EPD's are the sole source of human intellectual variance, or that there are no innate differences in information-processing capacities among human beings.

In place of the discarded portions of Hayes' theory, we make certain amendments of our own. We suggest that motivational differences and differences in intellectual capacity are correlated phenomena, not an identity. Although the increased motivation of the highly intelligent certainly enhances their learning and accumulation of knowledge, it is not the only or the primary "cause" of manifest intelligence. Moreover, we suggest that some kind of motivational state and manifest intelligence are both dependent variables resulting from the effects of an as yet unspecified independent variable. This independent factor may well be a biological structure (or a group of them) which determines to a large extent the levels of both motivation and information-processing capability. In fact, the intersect of these two phenomena—the detection of the possible location of a factor having these two kinds of effect—may be the eventual solution of the problem of the physical basis of intelligence.

We shall now attempt to support these amendments to the Hayes' theory with facts.

II. BEHAVIORAL EVIDENCES OF MOTIVATION

What evidence is there that motivational differences accompany differences in intellectual ability? Much of the evidence exists in anecdotal form. Eiduson (1962) reports from her study of forty eminent research scientists the consistent pattern of a drive toward work among them—a drive which seemed to center around a desire to "find out" things. In a historical study of eminent men, Cox (1926) found that even through indirect means of information about them, she could clearly detect among these men a tendency toward both broad and intense intellectual interest.

These data might be argued to depend more upon creativity than upon intelligence. But Terman's (1925) Gifted Group is clearly a group defined by means of intellective performance, and not creativity (in fact, this is one of the criticisms most often leveled at it; see Burt, 1961). But analysis of responses on questionnaires, parent and teacher ratings, and so on, gave strong support to the idea that these intellectually gifted subjects had high levels of interest and motivation. They showed significantly more desire for leadership, devotion of effort toward future goals, perseverance, persistence, and desire to excel. They maintained twice as many collections of things as subjects from the control group, and were very clearly superior to less able children in their desire to know about things (see review in Miles, 1954).

All of these characterizations can be summarized in two general attributes: interest and perseverence. There has never seemed to be any question among researchers who have observed large numbers of intellectually gifted children that such children were more intellectually curious and persistent than their peers.

One qualification needs to be made here, however. Highly intelligent children have not been found to be universally more curious about all things (Day, 1968). There may be a directional variable involved. Evidence on this point can be gathered and implied from the work of Getzels and Jackson (1962) and Wallach and Kogan (1965b), although these authors do not address themselves to the point. In their descriptions of gifted children, however, it seems to be evident that the intelligent children have high motivation and persistence, and a passion to learn, or at least to achieve by learning. But it is also clear that this intensity of motivation can be directed in vastly different ways. High IQ-low creativity children in these studies seem to show an intense desire to achieve, and to receive social reinforcement from other children or from adults. Their curiosity, if it may thus be described, is of a qualified kind, and is perhaps used only in academic pursuits. It may, in fact, result in a distorted fixity of intellectual goals and concrete patterns of thought (see the case history of "Jay," Wallach and Kogan, 1965b, p. 62).

III. PHYSIOLOGICAL EVIDENCES

It is of obvious interest to investigate whether or not there is a physical basis to the heightened curiosity and motivational intensity we have seen related to superior intelligence through observational and correlational evidence. There are known to be individual differences in something widely known as "arousal levels," and it is highly possible that this kind of variable could account for the behavioral phenomena of which we have made note.

Elizabeth Duffy (1957) is one of the most prominant exponents of the idea that one's average arousal level is an indicator of individual differences. Interestingly enough, she has made a clear point of the fact that intensity and direction of arousal are distinctly different, although she has also argued that these two dimensions are sufficient for classifying all human behavior. It is, of course, intensity rather than direction which her research attempts to clarify, by measuring such physical phenomena as skin conductance, muscle tension, electroencephalogram, pulse rate, and respiration.

The most reliable and impressive finding in Duffy's work is that individuals seem to have an over-all general arousal level, roughly indicated by a few physiological indicators, and that the average of this over-all arousal level is quite consistent over a time within the individual. However, individuals do vary in their arousal levels; in a stress situation, each individual will show himself to be above his own average, but in an amount roughly proportional to arousal increases among his peers in the same stress situations. Individuals may be very different from each other in their arousal levels under stress or calm, and they may vary greatly in their responsiveness to stress, but they are different from each other in clear patterns of deviation from their own average arousal levels.

Duffy warns that internal "arousal" is a concept, a hypothetical construct, while all of her measures are overt physical responses--many show responsiveness to operant conditioning, or may be controlled by internal inhibitory processes. They may not actually correspond to the internal arousal which is really the object of our interest. In spite of this, she argues that they may provide a good indicator of internal arousal. The problem is familiar (it may even be the same problem): we must estimate a biological potential (intelligence, or internal arousal) by measurement of purely overt responses.

Whether she has investigated the arousal-intelligence interaction or not, Duffy does not present any hint about the relationship. She reports no administration or scoring of intelligence tests on her subjects, although it is almost unthinkable that such tests would not have been given, given the long (if hesitant) courtship between the concepts of arousal and intelligence. However, there is evidence from other quarters that a relationship between intelligence and physiological measures of arousal does exist.

In his thorough medical investigations of the original Gifted Group, Terman (1925) obtained evidence that these highly intelligent children had higher basal metabolic rates (BMR's) than average children. The results were not reliable enough to predict IQ ranking within the Gifted Group, but they did discriminate between the gifted children and the control group. Shock and Jones (1939) obtained a positive correlation of .27 (not significant) between intelligence and

BMR. Hinton (1936) found a much clearer relationship between the Stanford-Binet and BMR ($\underline{r}$ = +.736), and the Arthur Point-Performance (intelligence) Scale ($\underline{r}$ = +.661).

Some less direct evidence is available for supporting the arousal/intelligence relationship. We know that intelligent individuals learn more efficiently (Harootunian, 1966). If we ascertain what conditions enhance learning, there is the possibility that those conditions will be the independent variables responsible for better learning in the highly intelligent brain. Several studies show that chemical, electrical and "motivational" arousal or stimulation of the brain enhances learning effects (Weiner, 1966; Gaito, 1961; and others). This may prove to be the reason that the more intelligent learn more efficiently.

From all of these different evidences, it can be surmised that intelligence increases very possible correspond with increases in body arousal or activity levels---or with some heightening of brain activity which is only imperfectly reflected in measurable physical responses. What phenomena might be expected to result from such an intelligence-arousal correspondence?

If only body arousal levels are involved, we might expect only heightened rates of activity among highly intelligent people, with concomitant increases in learning because of the greater learning opportunities inherent in exposure to many different situations. Body arousal level might correspond roughly to Hayes' EPD's. But arousal level seems not to be only a matter of gross physical activity, because high-IQ children are, for example, more able to control their physical behavior (that is, to be still and so on) in school situations

(Jaggers, 1934). Even Hayes' EPD's are probably more subtle in their influence than the simple activation of gross body movement. They include curiosity, manipulation, and exploration. Thus, we are not speaking simply of gross body activity (and if we were, we should not be able to find observational confirmation of the hypothesis). What we are dealing with is something much more like a need for stimulation than it is like a need for activity.

Hebb (1964) postulates that individuals have levels of arousal which are optimum for effective performance in a complex environment. Presumably, these optimum levels of arousal could vary from individual to individual--although a quantitative view of such individual differences is well-nigh impossible, considering the absence of any certain measure of optimum levels. Duffy may be measuring something very close to optimum levels when she measures <u>average</u> levels. But we have no proof that these two phenomena are interwoven, likely though it may be.

It is Hebb's contention, supported in spirit and fact by others (Berlyne, 1964a, 1964b, 1964c, 1965; Leuba, 1965) that each individual is motivated to maintain his optimum arousal level (except at times like sleep). If an individual's arousal level is higher than his "optimum" arousal and situational demands require, he will seek to lower it--to cut himself off from some of his environmental stimulation. If his arousal level is lower than the optimum, he may seek some environmental stimulation which will raise it.

In support of his hypothesis, Hebb suggests several typical cases of varying arousal states which can be placed on his continuum, and which appear to affect performance in the predicted ways. For example, sleep and coma are levels of arousal too low for adequate functioning. Performance during states of drowsiness or boredom is also impaired. Alert, interested states result in the highest performance competence, especially in complex tasks. At very high states of arousal, like fright and anxiety, the adverse effects of deviation from the optimum again begin to show, and performance is greatly impaired.

It is possible that instead of having an intrinsically higher average level of arousal, the highly intelligent person has an intrinsically higher <u>optimum</u> level of arousal than the average. In that case, he might actually need more-than-average stimulation from his environment in order to be at the optimum level of arousal. It is assumed that being at the optimum level is in some way reinforcing (for related evidence, see Glickman, 1960), and that aspects of the environment which helped to maintain that level would be actively sought out. Of course, we are immediately reminded of the pervasive description of the highly intelligent as strongly curious, persistent, and intense in interest. How close is this picture we have compiled of the highly intelligent to a characterization we might construct of the person who is attempting to raise his arousal level through extra stimulation?

It is actually very close indeed. Again, the exploratory, curiosity, and manipulatory drives seem intrinsically involved. Berlyne (1964a, 1964b, 1964c, 1965) and others (Hebb, 1964; Harlow, 1965; for example) believe these drives to be the result of some need for stimulation. Berlyne (1964a) argues that exploratory drives are satisfied by stimuli which have the qualities of uncertainty, novelty, complexity, and relevance, for the subject. And it is in these directions that the individual with a higher-than-average optimum level must seek for stimulation which will heighten arousal.

If people of higher intelligence have higher optimum arousal levels than the average, they would thus be expected to display a greater interest in, and persistence of approach toward, activities with elements of uncertainty, novelty, and complexity. To a great extent, these expectations are fulfilled in the accumulated results of studies of the highly intelligent. But another population, also of significant interest to us, satisfies the same expectations to a superlative degree: the highly creative.

IV. CREATIVITY AND NEED FOR STIMULATION

We have already demonstrated that creative subjects are more likely than the average to emit novel, or uncommon, responses (Mednick, 1962), and this might suggest that they prefer novel or uncommon stimuli as well. We also know that those high in creativity sample information stores in a wide-ranging way which generates greater statistical uncertainty (Karlins, 1967; Karlins, et al., 1967; Schroder, et al., 1967), and this hints at greater tolerance for uncertainty among them.

But there are some more direct evidences that high levels of creativity bring with them a preference for novelty, uncertainty, and complexity. Barron (Barron, 1957, 1958, 1968; Barron and Welsh, 1952) has discovered that one of the most reliable discriminators of creative subjects from less creative is a test of stimulus-card preferences. Figures drawn in ink on cards are categorized by the subjects into "like" or "don't like" groups. It has been demonstrated that creative subjects consistently prefer cards which are both asymmetrical and complex, and which tend toward the chaotic. If anything has been clearly shown about creative individuals, it is their preference for novelty, complexity, and uncertainty.

But, of course, creative people have a very high chance of being above average in intelligence. It could be that the preferences they display for complexity of stimulus input are merely the results of their natural need for stimulation. If this is true, we must explain why further increases in IQ do not result in greater and greater tolerance (and/or need) for complex stimulation (that is, creativity).

According to the Hebbian view, the only possible reason for an individual to avoid complex stimulation is an overheating of the arousal mechanisms--in other words, an arousal level which is elevated above the optimum. We do not see an actual avoidance of complex stimulation among the highly intelligent; they are, as a group, definitely elevated in motivation, and in curiosity about and exploration of their environments. But individuals above an IQ-level of 130 or so do not show needs for complex stimulation significantly different from people who have IQ's of 130 but are maximally creative. We can only assume that the more intelligent persons have much higher than average optimum arousal levels, that they need more than average stimulation to maintain optimum arousal, but that they are receiving stimulation from some other source which is keeping their measurable need for stimulation lower than others with lower optimum arousal levels. This extra stimulation might be in the form of social pressures falling upon the highly intelligent. Another assumption we might make about this problem is that the highly intelligent persons who show no more preference for complexity than their lower-IQ, high-creativity peers may have been punished for seeking novel and complex stimulation (and behaving creatively), and so avoid it in spite of their higher optimum arousal levels.

If our assumptions, and their logical conclusions, are accurate, the potential for creativity--for handling and seeking after complex stimulation, for deriving pleasure from uncertainty and novelty--increases constantly over the full range of intelligence. But above a certain IQ level, only a few individuals realize their full potential for creative thought and behavior. Most come under the pressures of the culture around them and are unable to reach the levels of creativity of which they are capable.

V. THE BIOLOGICAL MEDIATOR

A review is in order, to determine how well we have established our amendments to Hayes' theory. The individual variations of optimum arousal level have been proved to exist and we have suggested that they have the possibility of covarying with intelligence. Arousal is not, as were Hayes' EPD's, the sole postulated cause of manifest intelligence. Its exact influence upon one's accumulated learning and manifest intelligence as a motivator of experience and learning is unknown. However, it has been argued here that arousal level has something to do not only with the amount of searching for stimulation which is initiated by the subject, but also with his capacity for processing those stimuli which reach the cognitive "machinery." Therefore, whatever independent variable is responsible for both intelligence and arousal, it very likely has a potentially limiting, as well as facilitating, function in intelligence. It is thus argued that there could be an independent variable, with a physiological basis, which is significantly determinant of both intelligence (as an information-processing capacity) and heightened motivation (as a covariable with intelligence). Is there evidence that such a biological mediator does exist?—or evidence of its identity?

In fact, several possibilities exist for filling this role. We have seen that basal metabolic rate could be nominated—or rather the processes which underlie it (respiration, heartbeat, sugar metabolism, and so on). It is easy to see how a high metabolic rate could heighten general arousal and motivation, but its relationship with information-processing is a little less clear. Conceivably, neural cells would receive increased blood and oxygen supply and act in a more efficient manner. Whatever mechanism is finally discovered to be the mechanism, it will probably include metabolic processes in its effects. But they are unlikely as a single-factor choice because their correlation with intelligence is simply not clear and consistent enough. Hebb (1949) suggested potential neural synapses as the possible physiological mediator of intelligence differences, but this is also difficult to tie to motivational differences.

It is not likely that this writer can solve all of the problems arising from our theoretical assumptions. At this time, there simply is not enough information available in any of many fields to do so. But one psychologist has posited an interesting nominee for this particular mediation role—a variable capable of affecting both motivation and information processing—and we should like to present his suggestion.

In his book on infantile autism, Bernard Rimland (1960) makes an astounding suggestion more or less in passing: that individual differences in intelligence are the direct result of differences in the vasculature of the brain—most especially in the reticular formation of the brainstem. He points out:

> Since the human brain consumes one-fourth to one-half of the total oxygen input of the body, it would not be surprising if through sheer logistics, the efficiency of the cerebral bascular system imposed an upper limit on the functional effectiveness of the brain itself. Nor would it be surprising if the diameter and quantity of cerebral blood vessels turned out to be a heritable correlate of individual differences in intelligence (p. 130).

Rimland suggests that high-level differentiation of the vascular system of the reticular formation results in its susceptibility to oxygen damage and consequently in autism among those known to have a high probability of reaching extraordinary levels of intelligence. His tightly-woven system of support for these hypotheses cannot be considered here in full. But it is most interesting that Rimland's arguments rely only on the likelihood that a widely effective scanning mechanism like the reticular formation could play a strong role in enhancing or limiting information processing. For our purposes, it is equally significant that the reticular formation has long been known to be the center of both cerebral and bodily arousal (Hebb, 1949, 1964; Lindsley, 1964; Malmo, 1964; Milner, 1970, and others). Furthermore, attention in itself--the direct product of reticular activity--could well result in more effective informational receiving and processing. This little-understood lower brain organ may be the intersect of the two variables we have considered, and should certainly be investigated earnestly as a candidate for the "seat of intelligence" we have been seeking for so many years.

VI. A PARTIAL SYNTHESIS

It is not clear how the concept of "optimum arousal level" (the idea that one is required to find one's own optimum level by seeking or avoiding stimulation) fits together with the idea that arousal level may be innately decided (by some mechanism like the reticular formation) and at high levels would only determine a capacity for complexity, not a need for it. Perhaps the real case is somewhere in between. Unless external variables impinge upon the organism, it is very possible that processing information and organizing it and manipulating it are intrinsically reinforcing as long as they are below certain maxima of optimal reinforcement effect. They may be intrinsically reinforcing in that same way that making a kill is reinforcing to an animal, completely aside from the matter of hunger and food (Glickman and Schiff, 1967). If these assumptions are true, individuals with higher arousal levels would naturally seek complex stimulation because of their higher capacity for manipulating it and being reinforced by it.

But environmental phenomena could negate these reinforcing effects by over-stimulating the individual in certain ways or by punishing creative behavior of specific sorts. The highly intelligent person would then "need" less stimulation, or would seek to derive reinforcement and stimulation from his environment in other ways, and would not demonstrate his potential for creativity on any test of creativity.

A warning is necessary here. We cannot suggest that the highly intelligent person is normally operating at a high level of arousal without being aware that this "blessing" may be mixed. An individual attending strongly to his environment, with a strong desire to process and integrate the information he receives from it, may well be hypersensitive to that information. Indeed, it was Duffy's suggestion that those with higher average arousal levels would be intensely responsive to their environment and susceptible to its effects or its selection.

A hypersensitivity of this kind could be disastrous in certain environments, especially in those punishing creative behavior. It may be revelatory that subjects classified as high in intelligence and low in creativity often turn out to be highly sensitive to praise and criticism from adults, eager to please them, and almost insatiable in their desire to conform, to achieve in acceptable ways, and to be accepted (Getzels and Jackson, 1962; Wallach and Kogan, 1965b present the evidence from which these generalizations are derived). This complex of behaviors--which could be labeled "compulsivity"--may be the result of an aborted creative potential. Quite possibly, the highly intelligent have enormous potential not only for creativity, but also for compulsivity and conformity. But we see no reason to assume that heightened arousal levels inevitably, and by their _own nature_, are necessarily inhibitive of creativity.

CHAPTER VI

INHIBITION

Any system with arousal mechanisms would not long survive without some means of inhibiting them. Paradoxically, it is also true that increases in arousal level must be accompanied by increases in potential for inhibition, or chaos will result. Therefore, if we suggest that highly intelligent people have higher levels of average or optimum arousal, it seems necessary that we suggest they have more capacity for inhibition, as well.

It must be remembered that inhibition and excitation are perfectly harmonious and mutually dependent at an associational level. In order for an association of the facilitative type to be made, all that is necessary (in any simplified model of the real process) is excitation. But if the organism is to discriminate among stimuli, we have seen that he must inhibit all or part of his old response to the stimuli, and then attend to some unique aspect of them. Generalization involves the two processes of inhibition and arousal at more or less the same level, and in a perfectly compatible coordination.

In fact, attention itself is possible only because the organism is not only aroused, but also is free from responding to other stimuli in that moment by inhibitive mechanisms. Part of the highly intelligent person's ability to deal with and interpret and store large amounts of incoming information is probably his ability to screen out its irrelevant aspects by inhibitory means. Or, we might see very competent handling of stimulus input as the superior ability of the cognitive system to discriminate and categorize—but discrimination and categorization are also dependent upon inhibitory processes.

There is, then, no conflict inherent in proposing that intelligence covaries with arousal and with inhibitory potential, if we see inhibition as quite a specific process of the negative feedback type —a process which is quite necessary if heightened arousal is to be efficiently used.

Diamond, et al. (1963), who wrote the classic study of inhibition, call to our attention another advantage to heightened levels of both arousal and inhibition. They argue:

> Nor is it unreasonable to suppose. . . that there is an essential difference between the kind of alertness which results from a balance of strong tendencies toward both arousal and quiescence, and that which results from a balance of weak tendencies. . . One can readily imagine, for example, that the strong system could be able to maintain its balance in the face of an overload of stimulation which might overwhelm the weak system (p. 363).

What we propose is that at increasingly high levels of intellectual potential both an individual's average arousal level and his potential for inhibitory associations are greater. In fact, these together may account for his intellectual efficiency, since generalization and discrimination are not possible without the close interaction of both, and since intelligence clearly means in part a higher efficiency at processing large amounts of incoming information. Higher levels of both tendencies enable a system to stay in balance despite environmental extremes. It is completely unnecessary to think of bilateral increases in the strength of these two tendencies are redundant or useless. They may be the only way for increases in mental capacity to occur without overheating of any single system.

If our assumptions are correct, we should expect highly intelligent people to show an elevated capacity for inhibitions. And if this is true, we must ask if inhibitory mechanisms could be "inhibiting" creativity when they are highly developed.

I. INHIBITION AND INTELLIGENCE

It is probably the case that inhibition is a great deal more specific in nature than is arousal. It may be more difficult, then, to obtain a gross physiological measure of an individual's potential for inhibitory activity. That is not necessarily catastrophic. It might be easily argued that an individual's ability to learn the inhibition of an overt behavior is at least indicative of his cognitive inhibitory efficiency. Since there is little other kind of evidence available it is at such studies of overt behavior that we must look--for we must examine any possible relationship between intelligence and inhibitory processes.

At least there do turn out to be individual differences in inhibitory ability at quite a molar level of behavior. The inhibitory association may be just as universally simple to establish as the facilitory association, but measures of it so far have depended on behavior considerably more complex than the induction of rote memory items. Because of this, learning of the inhibition of a response probably always involves at least a narrow subsystem of the inhibitory processes, and perhaps even a larger piece of them. It has been understandably difficult for research to make the transition from the physiological level to the level of gross, overt behavior, but it is a step which must soon be made, if we are ever to understand the phenomena with which we deal in this paper.

Is there any indication, in the roughly-measured studies which have been made, that intelligence covaries with inhibitory capacity? There is much indication of it. First, there are all of the discrimination and categorization studies reviewed earlier (Tempone, 1965; Spotts and Mackler, 1967; Kerrick, 1956; Desai, 1960, for example). They indicate that at the simplest (presently) measurable cognitive level, highly intelligent subjects are more capable of inhibition.

Then, there is evidence dealing with the inhibition of body movements in children. Massari, Hayweiser, and Meyer (1969) made measures of children's rates of drawing and walking straight lines under different conditions. In the first condition, the subject is asked to walk a line and given no further instructions. Secondly, the subject is asked to walk or draw a line as slowly as possible. Last, the subject is asked to walk or draw a line as fast as possible. The children's scores on each task were correlated with their Stanford-Binet IQ's.

Results indicated no significant relationship between a child's ability to draw or walk a line rapidly and IQ--presumably variables such as motor development and strength were more responsible for performance under that condition than intelligence. But more intelligent children were better able to fulfill the instruction to walk slowly. And they also showed slower rates of response on the test when the instructions told them merely to avoid making mistakes (correlations were all significant and ranged from .43 to .60). Evidently, the children of higher intelligence were better able to inhibit their motor responses (or "control" them, in the authors' terminology) toward some purpose. Furthermore, they were not only better able to do this when the purpose was made explicit by request ("walk slowly"), but also when the purpose had to be inferred from the request (walk slowly, so you "don't make mistakes").

Clearly, some facility of inhibition is required for such a task as drawing a line "as slowly as you can." By habit and chained-response learning, the tendency is to draw the next section of the line immediately after the first. This response must be inhibited if the line drawing is to proceed at a slow pace. Thus, the superiority of more intelligent subjects on such a task is indicative of (if not proof of) an inherently more effective system of inhibition.

Supporting evidence is provided by Levine, Spivak, Fuschillo, and Tavernier (1959), using young mental patients and several measures of "inhibition" (they corresponded to measures of one's conception of the future, ability to estimate time, ability to overcome a word set, and motor inhibition). It was found that <u>all</u> of the inhibition measures correlated significantly with IQ scores except time estimation, which approached significance. However, only a few of these measures of inhibition correlated significantly among themselves.

The evidence quite strongly suggests that intelligence is the critical variable in an individual's development of the ability to inhibit his responses. This general potential for inhibition of responses is not in itself directional, however. Individuals evidently learn many different ways to inhibit their responses, and also have idiosyncratic discriminative stimuli for responding by means of inhibition.

II. DELAY OF GRATIFICATION AND INTELLIGENCE

The delay of gratification research is helpful in supporting these conclusions. Studies of the delay of gratification phenomenon have been done in great number and variety, and some of them have included IQ correlations in their design. For the majority of these particular studies, correlations of IQ with delay of gratification have proved to be significant and positive (for example, Mischel, 1971; Mischel and Metzner, 1962; Melikian, 1959). The exceptions which do appear tend by their very nature to clarify the relationship between intelligence and the capacity to delay gratification. Let us examine two studies. One is typical of the majority in the results it obtains, although its sample population is quite unusual. The other is atypical in every way--and an exception which clarifies our rule.

In attempting to investigate whether or not the correlation of delay of gratification with intelligence is culturally affected, Melikian (1959) used children of Arab refugees as his subjects. These children had all their lives lived in refugee camps, their food and shelter being supplied and rationed to them by charitable groups. They had attended a YMCA school, and were acquainted through the school with some of the experimenters.

The children were given the Goodenough Draw-A-Man intelligence test, and were told that for their drawings they would be paid--not according to the quality of their drawings, but according to when they wished to receive their earnings. The monetary reward offered for immediate payment was half the amount of the reward offered for payment two days later. Results showed that the children choosing delay of gratification had IQ scores significantly higher than the other group's. In other words, the more intelligent children were better able to control their immediate desires for the purpose of obtaining larger payoffs, and this was true in spite of the hand-to-mouth nature of their existence.

However, in a similar study by Bochner and David (1968) among isolated Australian aborigines, the opposite trend was apparent. In this sample, children who were the first generation of their tribe to attend school and who aside from their time in the schoolroom spent their whole lives in the traditional aboriginal manner, were administered the Porteus Maze test of intelligence and offered a choice between one candy now and two candies later. Those who chose immediate gratification were significantly brighter than those who chose delayed gratification.

What is the meaning of these results? The aboriginal culture is one of the very few cultures in the world where there is absolutely no storing, planting, or growing of food for the future, and no delay of any gratification beyond minutes--or hours, at most--except that imposed by environmental hardships. Even in a refugee camp, it may be wise to save food coupons for a later day, or to wait for eating or buying. But for the aborigines, such delays would be extremely maladaptive. Most likely, delay behaviors would be punished in children from infancy; at very least, they would receive no reinforcement for them. In all other cultures, some shaping and reinforcement of delay behaviors would be certain to occur.

We can make a generalization with these facts in mind. In almost all cultures, where delay of gratifications is of definite value, more intelligent children learn better to delay their own gratifications and to inhibit their own responses than less intelligent children. They are simply more capable of inhibiting responses. However, in a culture where delay of gratification is useless, or even harmful, it is the intelligent children, again, who best learn those responses which are adaptive to the environment. This does not prove that inhibitory processes are not highly differentiated in intelligent members of a culture oriented toward immediacy. On the contrary, "intelligence" is directly dependent upon ability for abstraction, discrimination, and so on--and these abilities cannot exist without inhibitory activity at some cognitive level, if our arguments have been correct. What we see in the aborigine study is that cognitive inhibition does not always mean generalized behavioral inhibition--it merely provides the basic potential for behavioral control.

We have seen that although a wide range of cognitive and behavioral controls are dependent upon the inhibitory processes identified with intelligence, they are independent of each other and must be specifically selected or shaped by the environment (Levine, et al., 1959). In the aborigine, basic inhibitory processes still result in "intelligence," but they may or may not result in behavioral controls, depending upon the adaptiveness of those controls. Motionlessness during hunting, a motor inhibition, is adaptive and is performed. Delay of gratification, another behavioral control, is not adaptive for the aborigine--and the more intelligent an individual is, the better he is able to understand and respond to his environment's contingencies.

Again we have seen evidence that the highly intelligent subject is more responsive, and more discriminatingly responsive, to his environment--whatever that environment contains. Such responsiveness always requires discrimination and, of necessity, cognitive inhibition. In by far the majority of cases, it also requires generalized inhibitions of responses at a behavioral level.

III. MORALITY AND INTELLIGENCE

When an individual inhibits a response which would obtain for him something reinforcing, and does so for the internalized reinforcement inherent in performing acts labeled by his culture as "good," he might be said to be engaged in moral behavior. Moral behavior, then, involves both attention to culturally recognized behavioral guidelines, and the inhibition of an otherwise reinforcing response. We might expect, from this analysis of the composition of moral behavior, that intelligent individuals would be more easily capable of it than less intelligent ones.

This proves to be the general case. In her analysis of one hundred mentally gifted children (Cox, 1926) found her subjects significantly more trustworthy and conscientious than average children. Terman (1925) observed the same phenomena in his Gifted Group (see also Tallent, 1956). In a plea for attention to the definite relationship which exists between intelligence and moral behavior, Unger (1964) suggested that the over three hundred studies showing positive correlations between the two variables cannot be disregarded. Because of the nature of the results obtained (correlations were low, but the most moral children were almost always among the brightest), Unger supports the hypothesis that "intelligence operates as a requisite precondition for these tendencies as measured (p. 300)." Intelligence is once again posited to be necessary, but not enough.

In fact, some evidence indicates that when intelligent children do not behave in "good" ways, they may be some of the most difficult children a teacher might face. Jaggers (1934) finds a strong relationship between good behavior and intelligence, but discovers several very bright children, near the top of their group in IQ scores, who were "a constant source of disturbance" in the classroom (p. 258). It appears that when they are good they are very very good, and when they are bad they are horrid.

Assuming that intelligence is the precondition--the potential--for moral behavior, what are the possible reasons this is the case? First, the intelligent person may simply be better at distinguishing "good" from "bad" behavior. One of the long-standing difficulties of the Manifest Anxiety Scale (MAS) by Taylor has been the suspicion that more intelligent subjects are able to select responses on the test which are more socially acceptable (that is, indicate low anxiety) (Taylor, 1955; Grice, 1955). In an attempt to discover the truth of this suspicion, Voas (1956) administered the MAS with the instructions to "choose the 'best' (most socially acceptable) answers (p. 87)." He found that there was a significant and negative correlation between the American Council on Education (ACE) intelligence test scores and MAS scores under these conditions. Although all subjects revealed less anxiety when instructed to respond in a socially acceptable way, more intelligent subjects were better at judging which responses were most acceptable (that is, least "anxious").

Some of the highly intelligent person's moral capacity is thus his ability to distinguish moral from immoral, acceptable from unacceptable behavior. This greater ability to distinguish good from bad is easily seen as a case of making fine discriminations, of course, and as such is very much dependent upon cognitive inhibitory mechanisms hypothetically plentiful in the highly intelligent.

The second possible explanation for moral behavior in the highly intelligent is that they are better able to concentrate on a difficult moral task, and are less likely to be distracted by other possible responses. Binet was aware of this possibility when he suggested that among other things intelligence was the "tendency to take and maintain a definite direction (as cited in Cronbach, 1970, p. 200)." Grim and Kohlberg (1968) imperfectly confirmed Binet's intuitive conviction. They found a positive relationship between conscientious behaviors and ability to concentrate. Attention is an obvious derivative of inhibitory and arousal processes, and this explanation for moral behavior among the intelligent does not require any new or unique explanatory mechanisms.

The third possible explanation is merely the simplified combination of the others: moral behavior, in the sense we have been discussing it, is an elaborate inhibitive process at a behavioral level. It requires, primarily, complex inhibitory mechanisms at the cognitive level and, secondarily, shaping by the environment. Because of their greater capacity for complex inhibition at the cognitive level, highly intelligent persons have a greater potential for moral behavior, and will emit it significantly more often, <u>if</u> their environment rewards them for it. Inhibitive processes (and so, intelligence) provide the potential, not the direction for moral behavior.

IV. INHIBITION AND CREATIVITY

What is the relation of the intelligent person's greater inhibitory capacity to his potential for creative thought and behavior? If generalized, behavioral-level inhibition, in the form of low spontaneity, "inhibitedness," and constriction, is the necessary product of cognitive inhibitory processes, then the highly intelligent person has absolutely no chance of being highly creative--because spontaneity and fluency of behavior are intrinsic to creativity (see Chapter III).

We have seen demonstrated, in a variety of ways, that intelligence and/or cognitive inhibitory processes are prerequisite to behavioral controls of many kinds. But we have also argued that when the environment reinforces not inhibition, but spontaneity, the intelligent persons are those who respond best to environmental contingencies and show least "control."

In fact, there is nothing antithetical to creativity in inhibitory processes, at least in certain of the directions they may be utilized. It was Diamond, et al. (1963) who emphasized that a capacity for inhibition of old responses is necessary for flexibility and change to be possible--and that the greater and more complex the inhibitory capacity, the greater the potential for flexibility and change. In other words, intelligence and complex cognitive inhibitory systems are necessary for the change and flexibility in the schemata group we know as creativity.

The critical variable--a variable not decided by level of intelligence or inhibitory complexity--is <u>direction</u>. The direction "preferred" by the environment will be most accurately discriminated or perceived, and most intensively responded to, by a cognitive system with a complex interaction of heightened sensitivity and heightened capacity for inhibitive controls. In essence, the highly intelligent person has a greater potential for almost any style of behavior his environment might choose for him. He can control his behavior extensively toward moral, culturally conforming, socially reinforcing ends. Or, he can establish a complex system of cognitive inhibitions and discriminations which "protect" him against the effects of external social reinforcement, and orient him toward spontaneity, originality, and creativity. Or, he can be shaped by his environment to follow a path anywhere in between.

V. SUGGESTIONS

We have argued that the highly intelligent person is more "aroused"--more able to take in large amounts of stimulation. In order to function at this level of arousal, he must be able to order incoming stimulation through discrimination and categorization, or through discrimination and inhibition of irrelevant stimulus aspects. But this limiting through inhibitory processes introduces a significant danger. The individual will have heightened responsivity to _certain_ aspects of his stimulus environment. He will have chosen, in a more finely discriminative way than his peers, to respond in a particular direction.

Diamond, et al. (1963), suggest that there is a developmental process of childhood, during which inhibitions are acquired. This process begins in infancy with the inhibition of fear responses, and continues through childhood, where the individual learns to make abstractions by inhibition of stimuli which are generically irrelevant. We suggest that the child also learns to which environmental stimuli he must be responsive, and to which he must inhibit responses. Presumably, this is accomplished through the shaping effect of his social environment. If the child is reinforced more for behaving in creative ways, he will develop inhibitions against dependent, compulsive, noncreative behavior. But he can as easily acquire other patterns of inhibition (or patterns of selective sensitivity, or schemata--for all of these are equivalent).

It is possible that there is a critical period for the development of the cognitive structures which determine an individual's selective sensitivity to the environment. The arousal correlations with IQ are highest during childhood (Terman, 1925; Hinton, 1936), and it is perhaps during this time that inhibitive controls and differential sensitivities are established. It is not argued here that such cognitive patterns are absolute, fixed, and irrevocable--merely that the general style or pattern or schemata of cognition may be fairly well stabilized at the end of childhood, and will tend not to change unless drastic environmental changes are encountered (Hebb, 1949; Piaget, 1969; Diamond, et al., 1963).

The point we wish to re-emphasize is that there is no reason to believe that inhibitory potential, any more than arousal, is intrinsically generative of compulsivity. The highly intelligent child will be more sensitive to selected stimuli, and therefore capable of great extremes of performance in the direction reinforced by the stimulus to which he is sensitive--whatever that direction may be. Thus, inhibitory process could easily enhance his creativity in a suitable environment.

But if, indeed, brighter people are more sensitive to certain aspects of their environment, and if there is a critical period in the child's growth when his schemata of the future are being developed, it must be obvious that raising a mentally gifted child is a task to which all possible resources must be brought--in order that he not respond intensively in an ill-chosen direction.

CHAPTER VII

ANXIETY

Our efforts are directed toward discovery of the crucial variables which inhibit creativity among the highly intelligent——and inhibit it to such an extent that correlations between creativity and intelligence are not significant at high IQ levels. Because this is our purpose, we must not neglect a full examination of arousal and its related phenomena. If increases in intelligence are accompanied by increases in average arousal level, then the arousal phenomenon itself may be in some way responsible for specific effects interfering with creativity.

There are two areas of concern in our examination of the literature on anxiety and its relation to intelligence and creativity. First, anxiety has often been considered a form of arousal——a state of high motivation. Duffy (1957) has suggested that heightened arousal may take the form of anxiety. It is thus appropriate to investigate any possible relationships between anxiety and intelligence, or anxiety and creativity. Second, if highly aroused, highly intelligent persons do show less curiosity or other creative behaviors than less intelligent (but highly creative) persons, and the possibility exists that they are receiving stimulation from their environment which "uses up" their stimulation needs or tolerances, we must investigate the possibility that anxiety-evoking environmental pressures fill the role of that extraneous stimulation.

I. ANXIETY, CREATIVITY, AND INTELLIGENCE

It is well established that states of high anxiety impair problem-solving ability (for a review, see Cofer and Apley, 1964). But anxiety is not an indiscriminately general agent of impairment. It impairs, to some extent, selectively. On the whole it has been found that flexibility, divergence and abstraction in thinking are the processes most adversely affected by anxiety. Behaviors emitted under conditions of chronic or acute anxiety tend to be rigid, extreme, and concrete (for example, Lewis and Taylor, 1955; Berg and Collier, 1953).

A study of the effects of anxiety on creative behavior was carried out by Krop, Alegre, and Williams (1969). The control group watched a film demonstrating the use of the chalkboard in teaching. The experimental group (the "stress" group) watched a film showing a puberty rite "in which crude surgery is performed with stone knives on the penises of adolescent boys (p. 895)." Results of the study indicated that creativity was impaired on two creativity tests (Guilford Consequences Test, and the RAT) by the induction of stress, although to a significant degree only on the more open-ended Consequences Test.[3] One difficulty with the study is that the stress induced was not in any visible way relevant to the responses required on the creativity tests--but this failing could only have decreased the magnitude of the results and makes the data obtained appear even more striking. Another study (Bruner, et al. 1964) with animals demonstrated that abstract, or

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[3] This reinforces our belief that the RAT measures creativity at a more basic, less environmentally responsive level.

"creative" response were most interfered with when responses involved were instrumental in reducing drive level, and this indicates that when a stress is relevant to the task at hand creative responses on the task are even more adversely affected.

Gelfand (1962), in yet a third study, found that when children were manipulated in such a way as to believe that they had failed on a series of tasks, they conformed or imitated more than those who believed themselves to have succeeded. Although conformity and imitation are not direct opposites of creativity, they are very nearly incompatible with it, and were here seen to be the effects of the stress we might assume resulted from recent failures.

Despite the difficulties inherent in dealing with something as nebulous as "anxiety," there do seem to be a large number of studies approaching the problem from a wide range of viewpoints which result in one fairly consistent pattern: anxiety--induced, measured, or assumed--decreases the frequency of responses identifiable as, or compatible with, creativity. Thus, if the higher arousal levels we believe to be present among the more intelligent were roughly analogous to anxiety--if arousal took that form--we would have a simple explanation for the relatively negligible increases in creativity corresponding to increases in IQ at a high level: high-IQ persons are anxiety ridden, and therefore uncreative.

Fortunately, or unfortunately, this statement does not prove to be as complex as the reality. There are other data and considerations which must be woven into the problem's solution. First of all, intelligence shows no clear, positive relationship to anxiety. In fact, the evidence more usually than not shows a negative relationship between the two variables (for example, Grice, 1955; Kerrick, 1956; Feldhusen and Klausmeier, 1962; Matarazzo, Ulett, Guze, and Saslow, 1954; Keller and Rowley, 1962).[4]

Various views have been expressed about the reasons high-IQ subjects show less anxiety on measures of general, trait anxiety. The most obvious assumption is that more intelligent subjects are simply less prone to anxiety, as the evidence indicates on the face of it. Suggest Feldhusen and Klausmeier (1962):

> Superior mental ability may make it possible for a child to assess more adequately the real and present danger in any current threatening object, situation, or person. Thus, his fears may be specific and ascertainable. . . (p. 408).

A less obvious interpretation of the anxiety-intelligence research was made by Voas (1956), whom we have already presented. He noticed that some of the studies obtaining negative correlations between intelligence and anxiety had used military personnel as subjects. After finding that intelligence made individuals better able to choose nonanxious responses on the MAS, he hypothesized that in any situation where the results of testing are of any relevance to the personal lives and future of the subjects (or even where the subjects might suspect them to be), greater intelligence will act to dampen the number of "anxious" responses. Tests administered under what appear to be less threatening circumstances show mixed results, as opposed to the customary negative correlations (see Taylor, 1955, for review; Mayzner, Sersen, and Tresselt, 1955, for counterevidence). It is not now possible to make an unqualified general statement of the

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[4] The studies cited use the MAS and its equivalents as measures of anxiety.

relationship between anxiety and intelligence--not from existing data.

II. THE ANXIETY CONCEPT

One possible reason the anxiety-intelligence data is in such a state of disharmony is the questionable validity of the concept "anxiety." Mischel (1971) concludes, in reviewing several studies, that self-reports of anxiety and other measures--for example, physiological readings or counts of avoidance behaviors--are of "low or negligible" correlational strength. Endler and Hunt (1969) devised an inventory in which they attempted to sample many different kinds of stress situations, many different possible reactions to them, and many possible evaluations of feeling-states. The variance for a wide sampling of students could not be substantially accounted for by stress situation, response tendencies (actions or feelings) or individual differences, in themselves. Only by treating the results as the product of all these variables did the analysis make sense. The individual responds in the way that he has learned (or been innately determined) to respond to a certain stimulus complex. Mischel and Endler and Hunt agree: general tests for anxiety are really much less valid than they have been assumed to be.

Sarason (1957) is one investigator who sees the need for redefining anxiety in terms more specific to situation than have been used up to now. Although he is not as anxious as Endler and Hunt or Mischel to eliminate measures like the MAS, Sarason does make this observation:

> . . . people are not anxious every minute of the day and. . . often we can specify the conditions which will lead to an increase in anxiety in the individual. Perhaps what we need are not general anxiety scales oriented towards the kind of anxiety responses (eg., sweating, awareness of an increase in tension,

> etc.) which an individual will admit to but, rather, tests designed to assess the specific conditions under which anxiety is aroused. . . (P. 485).

He chose as his own particular interest "test anxiety," and found that his test anxiety measure was negatively correlated with IQ, while the general anxiety test he devised was not.

The emphasis on situational and individual variables in anxiety research is consistent with Gaudry and Spielberger's (1970) suggestion that the MAS test measures anxiety occurring "in situations in which [subjects] experience failure or some threat to self-esteem (p. 391)." Unfortunately, their situational qualification ("threat to self-esteem") is of little help in the task of making anxiety measures as operational and specific as possible.

More specific definition of the particular "anxiety" being measured would certainly be helpful in understanding the relationships among anxiety, creativity and intelligence (if anxiety research is to be salvaged meaningfully at all). Further, some consideration must be given to individually specific as well as situationally specific factors. Until these kinds of questions are asked and answered, it will not be known for sure whether intelligence covaries positively with anxiety. Even if such a relationship were found, it would not be clear whether the anxiety was a function of innate arousal level, or of environmental factors impinging on the more intelligent.

We shall have better luck at reaching a working hypothesis for the anxiety-intelligence interaction if we investigate the differential effect of anxiety upon problem-solving performance with variation in intelligence level. What we shall be concerned with is a particular case of situation specification, and the situation specified is the setting of a problem-solving task. Immediately, the picture becomes more complex.

III. ANXIETY AND INTELLIGENCE IN PERFORMANCE

Generally speaking, anxiety has been found to have more facilitating effects on high-IQ subjects than low-IQ subjects in problem-solving tasks. It has been demonstrated that subjects who score high on the MAS make better scores on simple tasks, while subjects low on the MAS make better scores on difficult tasks (Spielberger, Goodstein, and Dahlstrom, 1958). It is assumed that anxiety facilitates the response most dominant in an individual's hierarchy of possible responses to a stimulus situation. In difficult tasks, a dominant response would have a lower probability of being the correct one, and high anxiety would decrease efficiency on the task. In simple tasks, the dominant response in one's hierarchy is most probably correct, and the task is facilitated (Duncan, 1959; Cronbach, 1970). Spielberger (1958) argued that these principles could easily be applied to individual differences in intelligence and their effect on problem solving. Among more intelligent subjects, problems are relatively easier, and across many tasks anxiety would always activate dominant responses with a high probability of being more accurate. His research, and others', tend to support his argument that highly intelligent subjects are more often facilitated by anxiety than others (Gaudry and Spielberger, 1970; Denny, 1966; Kennedy, Turner, and Lindner, 1962).

Like all good generalizations, this one has some important qualifications. First, there is evidence that anxiety is facilitory in problem solving whenever subjects are in a state of competence relative to a problem—whether by native ability, or by practice (Gaudry and Spielberger, 1970). This qualification is merely an elaboration of the generalization, of course, since correct dominant responses could not reasonably be assumed to be the result only of ability, and never of learning. Second, some research simply does not support the Spielberger argument, for reasons which are not as yet understood (Keller and Rowley, 1962; Pervin, 1967). This research is, however, very much in the minority.

For the present, at least, we must accept the strong possibility that subjects with higher intelligence are more likely to be facilitated by anxiety in their performance of tasks than are other subjects. They are better able to make constructive responses under anxiety because the dominant responses activated in them by anxiety have a higher probability of being accurate and appropriate—even when anxiety has forced their arousal levels above the optimum. The consequences of anxiety in the highly intelligent are thus not necessarily debilitating.

In fact, a distinction can be and has been made between debilitating and facilitating anxiety. Instead of tapping "general" anxiety, or attempting to define anxiety situations, the Achievement Anxiety Test (AAT) attempts to differentiate between two kinds of responses made to anxiety—responses which are detrimental to effective performance, and responses which facilitate and invigorate it. In demonstration of the idea that anxious responses are really a continuum with facilitation and debilitation at its poles, several studies have found intelligence tied to one particular mode of response in anxiety.

Pervin (1967) obtained a correlation of -.46 between the two kinds of anxious responses (facilitating and debilitating), indicating that where one mode is used, the other tends not to be. He also found that intelligence (as measured by the verbal Scholastic Aptitude Test) correlated +.23 with facilitory responses to anxiety, but -.28 with debilitating ones (all correlations were significant at the $p = .01$ level or better). Butterfield (1964) obtained results in the same direction for all of these variables, but his correlations were even higher (intelligence as measured by the WAIS correlated +.466 with facilitating anxiety and -.429 with debilitating anxiety). The evidence strongly suggests that when more intelligent subjects do have reportable anxiety, it facilitates their solving of problems---that, in fact, they are more likely than the average to have facilitory anxiety responses, but less likely than the average to have debilitating responses to anxiety.

The ability of those who are more intelligent to react in facilitory ways is probably due in part to their learning history. Anxiety has always had a better chance of activating correct responses in them, Even in anxiety, action has proved to have a high success rate; active responses are thus emitted rather than responses of self-criticism, emotionality, and so on. Another possibility is that the more highly developed inhibitory systems of the more intelligent subject enable him to inhibit more effectively those responses which he has learned are less efficient in handling a problem situation---even with the increases in arousal due to high anxiety.

We must qualify what we mean by response facilitation, however. There has been no indication in the literature reviewed by this writer that responses facilitated by anxiety in the highly intelligent subject are <u>creative</u> responses. On the contrary, there is every reason to believe that anxiety always decreases the flexibility and variability of response, because it activates dominant response tendencies. In the highly intelligent person these activated responses happen to be accurate and environmentally adaptive, but they are no less rigid and no more "remote" in the cognitive hierarchy because of their accuracy Thus, creativity could easily be impaired by anxiety in the highly intelligent, in spite of their prowess at problem solving under "anxious" conditions.

IV. SEEKING THE SUPPRESSOR VARIABLE

Is there any reason that greater anxiety should occur among highly intelligent persons? For if there were, their anxiety would easily act as a variable suppressing increases in creativity with increased intelligence.

As tested by the MAS or any other general measure of anxiety, there is no visible, stable relationship between anxiety and intelligence. Nor is there any reason to assume that the increased arousal levels we have hypothesized to exist in the more intelligent are exactly equivalent to anxiety states. It is highly unlikely that they are equivalent--at least not physiologically or necessarily--since Duffy (1957) has observed that arousal can be manifest in alertness, stability, and adaptive responses. If general arousal level--and thus, intelligence--were intrinsically identifiable with anxiety, we should be very surprised not to find clear evidence that anxiety is related to intelligence. And even more basically, we should be able to determine that a generalized trait of anxiety actually does exist. The evidence for either of these suggestions is worse than sketchy--it simply does not withstand scrutiny. Therefore, it seems safe to assume that general arousal level is not necessarily manifest as anxiety.

If this is the case, the environment (not their own cognitive penchants) must be the source of any higher anxiety among those of higher intelligence. Their tendency, like that of their peers, is to use the most available responses under conditions of stress--and to be relatively rigid in those responses, losing flexibility and creativity. They do not, however, lose accuracy and competence, because of the accurate nature of their dominant responses. This is an exact characterization of what could result from anxiety in interaction with high intelligence. And it is also exactly compatible with the suppressor variable hypothesis. Only one further link is needed for this section of the chain: it must be shown that high-IQ persons do have greater anxiety-engendering pressures exerted upon them by their environment, consistent with or covarying with their intelligence level.

In establishing groups of high and low anxiety groups, Soueif (1958) used an interesting method. Rather than measuring some response tendency which could accurately be labeled "anxiety" only for the specific situation tested, he made evaluations of the social structure of his country (Egypt) and logical hypotheses about which social groups would have such undesirable positions within the culture that they would be subject to stressful pressures. These he labeled the "high anxiety" groups. In Egypt, high anxiety groups were hypothesized to be Christians (because of their minority), adolescents (as compared to adults who were not in a transitional age), and so on. When assessed on tests of ambiguity tolerance, groups predicted to be "anxious" showed more rigidity and greater intolerance for ambiguity in their behavior. Therefore, Soueif's assumption that people under stress can be identified by examination of their cultural and social situation found significant support.

It is our intention to make that kind of socio-cultural search in regard to individuals of high intelligence, and to arrive at a logical assessment of the stress they may incur because of their intelligence level. Of course, direct measures will be used as much as possible.

In keeping with Soueif's method of looking for anxiety by logically deducing who might have cause to be anxious, we might assume that high-IQ children were under stress commensurate with their intelligence level because intelligence caused them to be alienated and unpopular in the classroom——especially if evidence could be found for such an assumption. But, on the contrary, a strong trend toward what is culturally-defined as psychological "health" and "adjustment" is readily seen in intellectually gifted subjects of many different samplings (Terman, 1925; Wechsler, 1958; Matarazzo, 1972; for comment see Tyler, 1965; and Burt, 1961). Not only are the mentally gifted well-adjusted, as rated by adults, but they have also been found to have more popularity among their peers (Terman, 1925; Gallagher, 1958).

There are, however, data qualifying our visions of the highly intelligent child as the supersocializer of the classroom. The social acceptability of a child depends not only upon how intelligent he or she is, but also upon creativity. Jacobs and Cunningham (1969) found that children in a questionnaire chose highly intelligent, highly creative peers to work with them on school-related tasks more often than anyone else. But these same children were not preferred for social purposes ——their peers rated them preferable only to children low in both intelligence and creativity. Any other combination (high in one or the other variable, but not high in both) was more acceptable). It seems that if a child who is mentally gifted is to find himself a social niche among his peers, he may very well have to forego creativity. And only when the child was noticeably brighter than others would this kind of pressure be exerted. Thus, it fits the profile of our hypothetical suppressor variable.

But it is not only for his peers that the gifted child may need to tailor his behaviors. Getzels and Jackson (1962) and Torrance (1962) found evidence that teachers prefer high-IQ students to high-creativity ones, even when both have equal levels of achievement. Wallach and Kogan (1964b) suggested that children high in both intelligence and creativity may be disruptive in a regularly structured classroom because of their expressiveness, wit, and spontaneity. Such children are often known by their peers as "naughty." It is easy to understand how teachers and parents who do not highly value spontaneity and autonomy might prefer children who did not show these qualities. Creativity may well be "disruptive" in many situations.

It is logical to assume that when both are available for selection conforming, restricted achievement will be preferred by the culture to creativity. Since more intelligent children have shown greater sensitivity to social stimuli (Rothenberg, 1970), these cultural preferences may well be sensed by them more intensely, so that they are better able to avoid creativity and the disapproval accompanying it.

These views are compatible with the position taken by Fellows (1956), who approaches the problem in a slightly different way. He maintains that although gifted people may be "adjusted"—may even be very popular—they are not necessarily "happy." Recognizing the enormous difficulties inherent in a scientific investigation of happiness, Fellows, nevertheless calls attention to his data—which show that self-ratings of happiness in college students are negatively related to scores on an intelligence measure (ACE). Although "happiness" is little more useful than "anxiety" as an explanation or prediction, these results do serve to point out the fact that "adjustment" is not necessarily a satisfactory state—that gifted people may achieve adjustment by paying a high price.

Some investigators suggest that if constricting pressures are exerted upon the highly intelligent because of their intelligence, it is only at very high levels of intelligence that this begins to occur. Gallagher (1958), for example, found what he believed to be a downward trend in popularity and social acceptability at the 165 IQ level (this IQ number is probably inflated, because of the high scholastic level of the school population he sampled). Apparently, in spite of ability to reach social acceptance by sacrifices in creativity, children may reach a maximum intelligence level, beyond which they are much less acceptable to their peers. Gallagher also found what he believed to be a tendency to sacrifice achievement for greater popularity, among children above 150 IQ who had been placed in a school with rather low norms. These subjects were still very popular, in spite of their high intelligence, and among those much less intelligent, but their achievement quotients were quite a bit lower than would have been expected. Burt's (1961) finding that at higher and higher levels of intelligence, achievement is in continuous deceleration (confirmed by Wilson, 1926), may have significance beyond his own awareness.

Gallagher's (1958) finding that children at a very high intelligence level are those who show an inability to compensate for their differences by social conformity is not without predecessor. Equivalent suggestions were made by Leta Hollingworth more than thirty years ago (Hollingworth, 1940; Hollingworth, Terman, and Oden, 1940). She made children of extremely high intelligence levels her special interest, believing them to have unique problems in finding a place among their peers. Her observation is that children of high but not extraordinarily high intelligence (130-150 IQ) adapt socially and get along better personally than any other group.

> Within this range, the person comprehends more clearly, but not too much more clearly, than the majority of his fellow men, and can thus get himself accepted as a supervisor and leader of human affairs generally, with accompanying emoluments and privileges. His vocabulary, his interests, and his hopes have, at this point, still enough in common with his contemporaries to enable and warrant cooperation (Hollingworth, 1940, p. 274).

When one's IQ level is higher than 150, problems begin to appear. His abilities begin to separate him from, rather than endear him to, his peers. Says Hollingworth: "Mutual rejection begins to appear between the deviate and nearly all his contemporaries (Hollingworth, 1940, p. 274)." She suggests that this highly gifted person will have difficulty being tolerant of others. He may seek the company of elders and of his own imagination so much of his life that unidentified problems can arise in his relationships with peers. Most of all, he may appear to be completely separated from the normal world in terms of his thoughts and values--he may lose his "common sense" and thus his commonality. Several investigators have found evidence of these hypotheses, although systematic research is difficult, given the small size and the large geographical spread of the population (Sheldon, 1959; Gallagher, 1958; Witty, 1940).

V. SUMMARY: RESTRUCTURING OUR APPROACH TO THE PROBLEM

It appears to be possible that difficulties in socialization and interpersonal relations might cause pressures to be put upon children and adults with intelligence levels which are very high (150+ IQ). Although we might call this group "anxious" because of the pressures we have hypothesized, and point to their failure to live up to their potential in creativity as evidence of their anxiety, the label "anxious" is not the most efficient label we might use for them. By hypothesizing that they are under stress or "anxiety," we have been able to make use of the anxiety literature in understanding creativity among the highly intelligent. But this label does not help us predict or modify the phenomenon we observe. And "anxiety" as a theoretical construct cannot be (or at least has not been) specified adequately to be of real usefulness. What we really need to know about are the specific pressures and manipulations exerted upon the intelligent person by his environment in shaping processes.

We have seen that preferences are expressed and shaped in its members by the culture, and that these are especially forceful in their effect upon the highly intelligent. We do not need to say, or have the basis for saying, that the emotional responses intelligent persons feel in those conditions are "anxiety." What we can say is that the pressure of their environment evidently causes a certain intensity and a certain pattern of responses in them. They respond with facilitated vigor and little-diminished accuracy.

In what directions will they respond? They will seek social reinforcements from the best available source. The best source of reinforcement is obviously not among peers. They are too likely to be to some extent alienated by differences. It is most efficient to adhere rigidly to patterns of behavior known to be accepted and reinforced by parents and teachers and others in command: to become a model student, a model child. Of course, this pattern is not always followed to its limit, in spite of the kinds of social difficulty which every particularly gifted child must feel. Some of them do manage to behave creatively (in accordance with their cognitive tendencies), to cultivate their autonomy, and to remain spontaneous and flexible. Others become rebellious under-achievers.

All of these, presumably, experience the same externally-imposed "anxiety." But it must be clear that that fact provides us with no help in predicting which of the available response modes a child may choose. We must look to the specific sources of punishment and reinforcement which enable one child to be creative and force the next to be compulsive. We must begin to understand specifically how creative schemata are formed in the developing child.

One other thing should be borne in mind: the highly intelligent child is particularly adept at responding to stress, anxiety, and demands of negative sorts effectively and efficiently (Kennedy, et al., 1962). He is particularly adept at reducing his arousal by selective sensitivity and attention to certain stimuli. He has such a high relative rate of success that he may get more information by attending to negative cues and ignoring positive ones—the loss of performance efficiency that others would show if they concentrated on criticisms and punishments prevents their being able to attend to their environment in just this way (Hurlock, 1925, presents a hypothesis of marginal relevance to this suggestion). If the highly gifted person grows up in such a pattern, with heavy emphasis upon negative cues, it is inevitable that his unusually high potential for creativity will be absorbed by the necessity for rigid, efficient responses to the environment. In order to allow him to realize his full potential, he must be saved from his own tendency toward efficient environmental sensitivity, and from a cultural environment which places little value upon creativity.

CHAPTER VIII

PERCEPTUAL AND COGNITIVE STYLES

We have hypothesized that certain patterns of physiologically based cognitive capacity and tendencies in cognitive structure (for example, fine-grained differentiation) are determined by or equivalent to intelligence. We have also investigated certain environmental sensitivities which are argued to exist in the highly intelligent, as well as some environmental effects which could have influenced them through their heightened sensitivity. We now wish to examine the interaction of cognitive tendencies with certain environmental phenomena (for example, the causal nature of environmental events) in the development of cognitive schemata.

Although there may be no physiological necessity for noncreative behavior among the very intelligent, we cannot yet eliminate the possibility that their interaction with the environment results in structures of behavioral control and selection which prevent or inhibit creativity. That is our concern in this chapter.

I. PERCEIVED LOCUS OF CONTROL

One of the major intellectual achievements of the growing child is its development of an understanding of the laws of causality. At a rudimentary level, the child understands causality as a process with himself at the center, the Cause of All Things (Piaget, 1969). Later, his parents may appear to be of over-inflated importance in causing things to occur. Real understanding is achieved when the child is able to discriminate actual causal agents among those agents which are in any other way associated to a phenomenon.

An analogous process of learning, one not so complex or abstract, is the development of a system of hierarchies of expectation---based on previous success or failure at achieving reinforcement through some instrumental response. The individual, because of his past history, has a certain expectation of reinforcement in almost any situation or stimulus complex. This expectation does not only apply to one specific situation. It generalizes to all situations which have similarity to the specific one. In studies dealing with expectations of success in more or less academic situations, Crandall and McGhee (1968) noticed that a child's past history of academic reinforcement (good grades) was significantly correlated with the expectations he expressed about future success on a wide range of tasks. Even in a perceptual task, like matching sample angle sizes to a series of model angles, the children showed expectancy levels which were correlated +.32 with school grades. But the more "academic" a task became, the closer was the correlation between grades and the expectancy of success on that task (correlations ranged to a high of +.82).

It is the conclusion of Crandall and McGhee that individuals develop generalized patterns of expectancy, based upon past reinforcement —and that these expectancies, if they are in any way contingent upon the subject's own behavior, are estimates of the adequacy and competence of his own response. They further argue that high expectancies provide greater motivation for a task, and a facilitating influence on task-relevant responses.

Children of high intelligence would of course be expected to develop high expectancy levels in intellectual tasks at quite an early age ---barring some strong environmental influence like unavoidable failures, or the inability of the child to have any significant effect in obtaining reinforcers by his own actions. But, in most cases, the intelligent child should be more effective than the average child in emitting behaviors instrumental to reinforcements. He is probably more discriminating in reading the reinforcement probabilities of his environment---knows better what are the sources of reinforcement, and upon what responses they are contingent---and should generally have much higher success at obtaining them through his own actions. Consequently, he would have higher success expectancies.

Thus, it might be predicted that the highly intelligent child would soon, with the help of his understanding of causality and of his system of reinforcement expectancies, develop an internalized representation of the controls of relevant reinforcers in his environment. It is further expected that in such an internalized representation he would see his own behavior as instrumental to many more significant reinforcers than would the average child. For the bright child, the perceived locus of control is internal.

It should be emphasized that what we mean here by perceived "control" does not necessarily mean one's perceived position in a cultural or political power structure---although these kinds of control would certainly enhance the "perceived" control which is our concern. Rather, we are discussing the individual's perception of his ability to choose and emit behaviors which will reap good consequences and avoid most bad ones in his milieu.

In the literature devoted to individual differences in perception of control, the majority of investigators designate a continuum with the axis "internal-external," any point along which an individual can select as the "locus" he perceives for control in his environment. He locates, in effect, the seat of power. Those who see themselves as very much in control of and responsible for the contingencies of their environment are labeled "internal," and those who see themselves at the mercy of a random, whimsical environment are "external."

Once an individual has experienced enough reinforcements and punishments, he can establish and internalize some expectations of reinforcement and a perception of the general locus of control in his own surroundings. His internal representation of the locus of control is quite stable, and tends to guide his continuing perceptions of the environment--it acts, in other words as a schema for organizing incoming information and executing appropriate responses.

In example of the ways in which locus of control schemata affect perception in behavior, we cite the research of Lefcourt (1966). He reported that subjects categorized as highly "external" in perceived locus of control were much more likely than "internals" to raise their reported expectancies of success after a task failure and to lower them after a success. If one has already established a world-view in which random events are conceived to be dominant--events with no relation to one's own behavior--it is as sensible to raise expectations after a failure as it is to lower them. To this "external" person, a string of successes may mean a high probability of failure--an end to a run of luck. To the "internal" subject, of course, all estimates about the future are strongly dependent upon the results of his behaviors in the recent past--he has constructed a schema, a system of information-processing which makes predictions in that way, and he continues to make them even when environmental events are really random.

Many studies have established that the more intelligent individuals are, the more likely they are to perceive their locus of control as internal (Gold, 1968; Bialer, 1961; Crandall, Katovsky and Preston, 1962; Butterfield, 1964). This is true over a wide range of subjects, from college students to retardates, and should not be surprising. An intelligent person should be better at perceiving causal relationships in his environment, at choosing and accomplishing appropriate instrumental responses, and at realization of the effectiveness of his own responses.

What is perhaps really surprising is that the correlations between intelligence and internalized locus of control are not nearer unity. The relationship is so logical and predictable, and the evidences so consistent, that as Lefcourt (1966) points out, "it might be argued that locus of control merely represents the phenomenological response to one's own intelligence (p. 217)." In actuality, one's degree of intelligence is directly related only to one's ability to perceive the locus of control accurately. Usually, intelligence is also strongly (but indirectly) related to one's ability to affect the environment. But if, for any reason, an individual with high intelligence is prevented from exerting control upon the environment which is as effective as that of which he is capable, he will then be able to perceive even his own lack of control accurately.

Let us examine a few situations in which cultural structures are selective in meting out individual control. Battle and Rotter (1963) found that among both blacks and whites, lower-class subjects showed a more external perception of control than middle-class subjects. Comparing races, they found that blacks were generally more external than whites. In fact, lower-class Negroes, even those with high IQ's were more external than low-IQ, middle-class whites. Further, there seemed to be a tendency among the lower-class blacks for the brightest among them to develop the <u>most</u> external perception of control. They were perhaps better at accurately estimating their own ineffectuality in a white culture than were their less-bright peers.[5]

Thus, the structure of the individual's environment determines to a significant extent his perception of how much control he has over environmental contingencies. It is relatively simple to choose a group one expects have little control over their own lives and thereby extract an environmental factor in the individual psychology of controls. It is considerably more difficult to predict and verify what specific environmental influences affect individuals' perception of control within a cultural group. It is possible that an autocratic parent might produce an external-control child. But such a child might perceive subtle ways of obtaining predictable reinforcements from the parent--through achievement, flattery, and so on--and be classifiable as "internal."

In a study not strictly part of the locus of control literature, effects like these were found to be important. Heilbrun and Waters (1968) discovered that college students who came from homes with strict

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[5]Graves (1961) supports the contention that repressed groups perceive themselves as little able to be in control of their environment, as has Seeman (1959) from a more sociological point of view.

"controls" were much more capable than others of the independent behaviors required for achievement during their first year away at college —but such capability was present only if their parents were seen as highly nurturant or rewarding, as well as strict. It can be guessed that the availability of rewards encouraged the students, as children, to seek ways of responding to parental "control" which would bring the rewards. Behaviors instrumental to rewards were emitted by the students, rewards were received, and for all practical purposes the students were in "control" of reinforcement contingencies. We must attend to Skinner's (1972a) warning that the pigeon controls the experimenter just as surely as the experimenter controls the pigeon.

It is probably because more intelligent persons are more capable of making out indirect as well as direct ways of controlling their reinforcements that they are, as a group, so high in perceived internal control in spite of environmental variations. It is also partly because of this capacity that they are probably more susceptible than others to any systematic patterns of environmental contingency and may be easily shaped by those patterns. Unfortunately, behaviors leading to social reinforcement may not always be creative behaviors. In fact, another definition of creativity might be that it is not only representative of an internal locus of control, but also an independence from those who administer social reinforcements—an independence that goes well beyond being able to obtain these reinforcements.

What are the results of a certain internalized perception of control? First of all, internalized control is expected to enhance one's desire and ability to achieve. We have already observed that Crandall and McGhee (1968) believe internal perception of control facilitates achievement (see also Butterfield, 1964; Lefcourt, 1966). Gold (1968) reports several small and insignificant or barely significant correlations between internal locus of control and need for achievement. This is surprisingly little support for such a logical proposition. But need for achievement measures of the McClelland type tend to depend partly on independence from parents and authority figures—a variable which may not take into consideration the desire and ability to achieve independently because of dependence on parental reinforcement, as we have just discussed. It would be enlightening to see the results of a need for achievement measure which was not tapping independence from authority figures, as compared with locus of control. Significantly enough, measures of actual achievement (grades), with intelligence held constant, results in a high partial correlation between internality of control and achievement ($\underline{r}$ = .891, in Butterfield, 1964). Therefore, we conclude that whether or not they are truly "independent," individuals high in internal control are generally high achievers.

Another predictable effect of the perceived locus of control variable is anxiety. Butterfield (1964) demonstrated relationships among measures of the Internal-External Control Scale (I-E), the Frustration-Reaction Inventory, and the Alpert-Haber Facilitating-Debilitating Test Anxiety Questionnaire. He found that both intelligence and internal locus of control were significantly correlated with constructiveness of responses to frustration (positive correlations), debilitating test anxiety reactions (negative correlations),[6] and facilitating anxiety reactions (positive correlations).

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[6]Just under significance for the I-E.

With this data we can sketch in the details of a portrait of the highly intelligent person. If he is anxious, he also is capable of analyzing and manipulating the contingencies of his environment so that his responses are most often facilitory and goal oriented. Also with this bent to successful dealing with the environment, an internally controlled and highly intelligent person may be more alert to its stimulus configuration. And that is our next topic of discussion.

II. PERCEPTUAL LEVELING AND SHARPENING

With the conviction that his responses can be instrumental in causing change within his environment, an individual will probably pay more attention to his environment, so as to learn what are its contingencies. Comments Lefcourt (1966):

> When the subject perceives that he is able through some modicum of personal activity to predict the events occurring in a given situation, he becomes more accurate in his perception of changes in that situation (p. 209).

Although it is necessary, simple predictability of events is not a guarantee that a subject will attend to and be more accurate in observing environmental changes. Reece (1954) argues that the availability of instrumental responses relevant to a stimulus is critical to the sharpening of an individual's perceptual processing of that stimulus. He showed that when subjects could avoid shock by quickly recognizing critical stimulus words: (a) they recognized all stimulus words more quickly, (b) they did not recognize more quickly words immediately preceding stimulus words in presentation, and (c) they later remembered more pairs of words (preceding word plus stimulus word) than other subjects. Reece took these last results to mean that people generalize their perceptual "vigilance" to all task-relevant stimuli, and only to task-relevant stimuli. They may attend to (and remember) stimuli associated to the critical class of stimuli by contiguity or causal relationship, but they will not necessarily "expend" their perceptual vigilance on these more or less irrelevant stimuli (see Guthrie, 1966, for supporting evidence). This research points to possible difficulties in finding stable traits among individuals for responses which evidently have such stimulus-specific characteristics.

Phares (1962) did a kind of photonegative of Reece's study. He established escape-from-shock conditions for his subjects on both "chance" and "response-contingent" bases. Since escape conditions are by definition response-contingent stimulation, Phares is really measuring something like subjects' response to manipulated success-rate for their instrumental behaviors. At any rate, his results supported Reece. When response-contingent escape is possible, subjects have shorter recognition thresholds than when escape is based upon chance.

Vigilance is the most adaptive response mode for subjects who have internal control within a test situation—that is, for those whose responses are instrumental in avoiding punishment or obtaining reward. But for subjects who have external control in the temporal context of the experiment, another kind of modification of perceptual processes is possible. It is known by the various names of leveling, repression, and defense. Both Reece (1954) and Phares (1962) found evidence that subjects who were shocked, but had no way (that is, no predictable way) of influencing the onset of the shock, recognized stimulus words more slowly than control subjects who were not shocked at all.

Clear evidence of the nature of a subject's "choice" of perceptual mode--whether vigilant or defensive--is found in a study by Guthrie (1966). He found that parachutists, prior to a critical jump, displayed significant vigilance to visual cues as compared to control subjects; while hospital patients, prior to abdominal surgery, tended to level their perceptions of visual stimuli. Obviously, parachutists have a great deal more control over their environmental contingencies than do surgical patients, and for this very reason are more alert to their surroundings.

There is no need to regard vigilance as adaptive while we see leveling as maladaptive. All behavior is produced by adaptation to the environment: it is the dominance of an old (previously adaptive), inflexible response in an inappropriate environment which is maladaptive --or the use of leveling or defensive responses when an instrumental response is actually available to the subject. As Dulany (1957) suggests:

> We might guess that one kind of subject had learned to defend against threatening stimuli because with his particular experiences and personal economy that reaction has somehow been to his advantage. Another has learned perceptual defense because that reaction has served him well (p. 333).

Although we question whether any individual does not modify his mode of perception according to the situation (Guthrie, 1966), it is undoubtedly true that any leveling or sharpening, as part of one's own schemata of perception, is adaptive to environmental events.

Dulany (1957) set out to establish this principle experimentally by training his subjects to respond to stimuli in either a leveling or sharpening (vigilant) manner. His subjects were asked to report which of the presented stimuli was clearest or most recognizable. Then one stimulus was arbitrarily selected as the critical stimulus for each subject. "Defense" subjects were reinforced (by avoiding shock) for recognizing best the non-critical stimuli, and punished (by shock) for recognizing the critical stimulus. "Vigilance" subjects were reinforced for recognizing the critical stimulus and punished for recognizing any other stimulus. When their responses were compared to their baseline patterns of response, results showed that the two groups were successfully trained in vigilance or defense.

Pustell (1957) was also able to obtain experiment effects in the direction of both vigilance and defense, using electric shock. Unlike Dulany, he was not operantly reinforcing one perceptual mode or another in each subject. He was merely classically associating shock to certain stimuli (subjects were told their responses were irrelevant to the shock, and this was the case). What he found was that there seemed to be striking sex differences in the mode of perceptual response chosen by the subjects. All of the twelve male subjects were more vigilant after training. But nine of the twelve females were more defensive.

Pustell suggests that the females were subject to more severe anxiety because of the shock than were males. This is a possibility, especially if we consider Pustell's thorough efforts at establishing the hypothesis that defense is used in situations where: (a) anxiety is intense, (b) escape is difficult or impossible, and (c) reality is unclear or ambiguous. But Pustell's reference to escape can be seen as a locus of control variable. And this variable is much more likely to be the determinant of sex differences than Pustell's suggestion that females are more frightened of pain. It is intuitively obvious that women are consistently trained to perceive their locus of control as external—to remain "helpless" while others manipulate their environments. It is only to be expected, then, that in a stress situation, like the experiment involving non-contingent shocks, they might choose "leveling" responses. Pustell makes note of the fact that when interviewed, the female subjects reported strong feelings of helplessness and resignation. This incidental evidence only supports more strongly the idea that locus of control and perceptual defense are intrinsically related.

Returning to the problem of intelligence, we shall try to relate intelligence to the variables we have just discussed. Intelligence appears to fit quite neatly at the center of the puzzle. Highly intelligent people are better able to tolerate and perform under anxiety. This may be simply because they can handle and inhibit the unpleasant stimulation more effectively, or because they are more internal in perceived control and have higher expectancies that a response can be found which will have effect. If highly intelligent people are more vigilant, it is thus because they have highly internalized loci of control, and handle "anxiety" better.

As neatly as everything may fit together around intelligence as an independent variable, there is little evidence demonstrating directly that intelligence covaries with vigilance. Intelligence covaries with internal control. Internal control covaries with vigilance. Vigilance is a determinant of a neurotic pattern, the obsessive-compulsive pattern. Obsessive-compulsives have higher average IQ's (as will later be demonstrated) than other neurotics. There is a smooth chain of relationship beginning and ending with intelligence in which vigilance is a central link—and the only link which has not as yet been clearly established as dependent upon intelligence to some degree.

It is possible, however, that our measures of perceptual vigilance are just not enough like life situations. Dulany (1957) warns:

> The forced-choice procedure simply delimits the range of competing perceptual responses to a high priority few. Until the evidence for sensitization and desensitization is less equivocal, perceptual vigilance and defense can legitimately be identified only with shifts in balance among competing perceptual responses, in the one case toward a critical percept, and in the other case against it (p. 337).

The predicted superiority of the very intelligent at attending "vigilantly" to stimuli may only be predictable if the stimuli comprise a complex input. It is in that very situation that greater information-processing skills would become invaluable, and would enable an individual to avoid negative consequences by attending. For negative consequences can be avoided not only by selecting the proper response, but also by selecting the proper stimulus to which to respond—and proper stimulus selection is possible only with broad scanning and fine-grained inhibitory capacities.

On the other hand, the more intelligent person may prove to be no more prone to vigilance than anyone else. Intelligence may determine the direction of opposing tendencies to vigilance or defense. This is not what we might guess the situation to be, but it is not a point crucial to our hypothetical structure.

All of our discussion has made, to one degree or another, the assumption that vigilance and defense are more or less unitary ways of responding to one's environment. This assumption may or may not be accurate. It is impossible to know until we have solved the problem of over-simplicity which plagues vigilance-defense research. The studies by Dulany (1957) and Guthrie (1966) indicate, however, that perceptual modes are quite situation specific in their effects.

Gardner (in Gardner, Holzman, Klein, Linton, and Spence, 1959), reviewing results of a factor analysis of the large body of data taken by himself and his colleagues, argues that leveling and sharpening describe a continuum for a kind of cognitive control which is quite generalized across tasks and situations. He notes:

> In levelers, successive perceptual impressions were assimilated to each other, so that distinctions among them were blurred. Memories of past impressions were also less available to them, presumably because of the general lack of differentiation of their memory schemata (p. 105).

The opposite trends were found in sharpeners by Gardner. From all evidence, it appears that leveling and sharpening (as well as several other cognitive controls) are fairly stable tendencies in information processing. Individuals may have quite differential response tendencies for different stimulus situations, depending upon their learning history. They may have general, overall tendencies in one direction or another, especially in response to a variable with wide-ranging effects on perceptual accuracy and information-processing, like intelligence. The evidence for this assumption, however, is not at hand.

Gardner does make note of the fact that cognitive controls hold a strong resemblance to intellectual processes in many ways. He suggests that both are essentially structures of control and delay--structures, or the capacity to construct structures, which guide and give direction to behavior. What we are really discussing are inhibitory processes--the mechanisms of direction selection, and the covariables of intelligence, to translate into terms of this paper. But if the relationship between cognitive controls (like vigilance and defense) and intelligence is so intimate, why has it been so difficult to demonstrate?

Gardner suggests that different factors in intelligence have their directional influence on different cognitive controls. From our point of view, this is merely another way of proposing that environmental differentiation of native ability (crystallization of ability, in Cattell's terms) is in turn responsible for differential directional tendencies in various cognitive controls. This suggestion may be modified to a more satisfactory hypothesis with the help of suggestions by Dulany.

Espousing a point of view more or less harmonious with our hypothetical structure, Dulany (1957) suggests that vigilance is the dominant, the preferred way of perceiving threatening stimuli. If instrumental responses are possible in an environment, the superior adaptive value of vigilance is clear. If it is most adaptive, argues Dulany, it is most basic. He cites evidence in support of his theory, and concludes by asserting that if vigilance is the "natural," dominant response of human beings to threatening stimuli, then we can assume that any tendency in any individual toward defense is the cumulative result of punishments for attending to stimuli and/or lack of reinforcement for responses to attended stimuli because of their inefficiency or inaccuracy. This pattern of spreading inhibition or defense against stimuli would be expected to some extent in everyone, but to a greater extent with each <u>de</u>crease in intelligence (if intelligence is the ability to respond efficiently to a variety of stimuli).

Of course, strong correlations between intelligence and perceptual modes would be prevented by individual differences in punishments and reinforcers resulting from responses in each of the modes. Final response dispositions might not be highly reflective of intelligence, especially considering that the tasks or stimulus situations tapped by testing leveling or sharpening may be unrealistically simple or narrow in range.

III. SUMMARY

We have seen that intelligence is equivalent to a capacity for cognitive differentiation, but that it does not determine the goals toward which the schemata made possible by differentiation guide the mental processes. By the same token, intelligence may be the critical factor in enabling an individual to attend to, and be vigilant to, large amounts of sensory information. But it also enables him to inhibit response to stimuli (here, a "response" might be awareness, for example).

As the individual develops, if we accept Dulany's (1957) hypothesis, he is (in a manner of speaking) in a process of leveling. What is leveled depends upon the contingencies attached to each stimulus class within the individual's environment. Conceivably, the highly intelligent person could--in an environment where control was almost entirely external, punishment frequent, and possibilities for escape very rare--become an extreme leveler. Of course the chances of that are slight. The probability is much greater that he will become a vigilant perceiver of the environment, since he will very likely be successful in behaviors resulting from vigilance. However, that is not to say that even the very intelligent have across-the-board tendencies at leveling or sharpening. The very intelligent person could become a leveler of, for example, socially reinforcing and/or punishing stimuli while remaining vigilant to most others--he might then be creative. Or, he could defend against any spontaneous, "creative" responses, while remaining vigilant to cues calling for compulsive, achievement-oriented responses for which he had received least punishment and most reinforcement in the past.

Again, direction is apparently environmentally determined to a great extent and tendencies in any direction might conceivably be enhanced by high intelligence. Although very intelligent people do have obvious tendencies toward an internal locus of control and vigilance to environmental stimuli, which are precursors of compulsive behavior patterns, these tendencies could also enhance creativity and have no necessarily inhibitive character to them, in relation to creative responses.

CHAPTER IX

THE ENVIRONMENTAL SPECIFICS

We have attempted to establish the foundations of the argument that highly intelligent people are more susceptible to the promptings and selections of their environment--that they are capable of responding intensively in almost any direction to its shaping. We have concentated and shall continue to concentrate on two major directions available to the very bright growing child: creativity and what we sometimes refer to as "noncreativity" or compulsivity. We have continually emphasized that the environment has its effects in ways which are situation-specific. Now, we wish to discuss some particular ways in which environments are known to affect the directions of a child's cognitive-style development.

I. THE VIGILANCE PATTERN

For this purpose, there is no research more helpful than that of Getzels and Jackson (1962). Their studies were carried out in a rather unusual private school in a cultured and well-educated population of families near a university. All students in the school were accelerated at least one year ahead of public school students by high school graduation. The mean IQ of all subjects involved in the study (including subjects not in the groups with which we shall be concerned) was 132. The top students by IQ-score who were not also among the highest in creativity were chosen as the high-IQ group, and had a mean IQ of

150. The highest scorers on a combination of creativity tests (devised by Getzels and Jackson) who were not also among the highest in intelligence were chosen as the high-creativity group, and had a mean IQ of 127.

It cannot be over-emphasized that the "high-creativity" group of Getzels and Jackson were very bright children. Essentially, the two groups were the lower and upper halves of a continuum of intelligence cut out of the superior range. The lower half were bright children reaching their potential for creativity; the upper half were brilliant children, but the only brilliant children in the sample who were low in creativity--they were not only below their own creative potential, but also below the creative performance level of their less bright peers. They were the very kind of individuals who are responsible for the ceiling effect that we have described in the intelligence-creativity interaction, because they are not behaving as creatively as we might predict from the intuitive conviction that creativity should covary with intelligence. They are then of critical significance to the suppressor-variable hypothesis. Hopefully, there should appear some critical differences in the environments of these subjects and the high-creativity subjects, which will enable us to see in specific ways how some of them found their creative potential, while others never knew of theirs.

First of all, Getzels and Jackson noticed several significant differences between the parents of the two groups. They differed not only in quantity, but also in the quality of their education. There was a trend in the high-IQ group parents toward a higher number of fathers and mothers with college degrees, but it was not highly significant. However, the high-IQ group parents had significantly more graduate training (especially the mothers) than the other parents. The authors posit that this does not so much indicate a higher level of cultivation, but rather a greater degree of "specialization of training, or, if you will, 'professionalization of education' of the IQ group (p. 63)." As for parental occupation, there was a significant tendency for fathers of high-IQ subjects to be in academic professions, consistent with their specialized and professional training. However, in spite of their high-level training, high-IQ mothers were <u>more</u> likely than high-creativity mothers to be housewives exclusively.

For Getzels and Jackson, this unusual restriction of the highly trained mother to her home and children is the first of many indications that the high-IQ group mothers "are in fact likely to be more vigilant about the 'correct' upbringing of their children than are the high-creativity mothers (p. 64)." For example, high-IQ children have access in their homes to many more magazines aimed at children than do high-creativity children. When asked about their own satisfaction with the way they have raised their children, high-IQ mothers seemed rather defensively self-satisfied, while high-creativity mothers did not. On the other hand, when asked to comment on the school attended by their children and on any unusual qualities in the children themselves, high-IQ mothers not only made more total observations, but also gave a higher percentage of <u>critical</u> comments. High-IQ mothers expressed much satisfaction with the enrichment program of the school (none of the high-creativity mothers had any comment about it), and much dissatisfaction over the inadequacies they perceived in the school's control of behavior, enforcement of rules, and lesson drill. When asked about their preferences for their children's friends, high-IQ mothers had many ideas about what qualities their children's friends should have, and emphasized family background, intelligence, manners, and study habits. High-creativity mothers, on the other hand, tended to emphasize honesty, high values, and openness as desirable qualities in their children's friends.

In summary, Getzels and Jackson report that on many different measures, mothers of high IQ-low creativity children show themselves to be more vigilant, more likely to intervene, more critical, more aware, more concerned with control, and more concerned with socially-accepted (superficial?) virtues in regard to their children. High-creativity mothers, on the other hand, were less child-centered, less critical, more tolerant of other adults and of their children, less concerned about conventional standards, and less concerned about success for their children.

Domino (1969) presented some evidence corroborative of the Getzels and Jackson results. Using groups high and low in creativity on three measures (without regard for intelligence), he found that on a personality test (California Psychological Inventory), scores of the mothers of creative subjects formed a significantly different pattern from those of the mothers of noncreative subjects. Creative mothers were more self-assured, independent, and flexible; they showed more tolerance for others, but were less concerned about social probity or favorable impressions than mothers of non-creative subjects.

How is it that mothers with these particular characteristics have the particular children they have? It can only be that behind these maternal personality "traits" are certain, specific behaviors which provide discriminative and reinforcing stimuli for their children's creativity. It will never be known how to enhance or to undo what they have accomplished in shaping their children until we understand the exact interaction between their behaviors and their children's. Even knowing quite clearly about one particular kind of behavior in parents can be of limited use until we know whether or not there is an interaction effect among kinds of behaviors, creating reinforcement schedules or deprivation conditions, and varying results in the child's behavior.

Some careful studies have been performed which attempt more precise analysis of the parent-child interaction, but which also support the more global conclusions of Getzels and Jackson. Heilbrun and Norbert (1970) discovered that college students who perceived their mothers as exerting low control but high nurturance were significantly more responsive than others to their own self-reinforcement, and significantly less responsive to the reinforcement of others.

We are not suggesting that this is the kind of maternal control and reinforcement pattern which will automatically result in creativity, as such. But there are many behaviors which are conducive to creativity in anyone, and this mothering pattern produces several of them. Above all, to be creative one must be autonomous, self-confident, and lacking in self-deprecation. That is, one must be capable of effective self-reinforcement. And Heilbrun and Norbert have shown that self-reinforcement is most frequently seen among those whose mothers are highly reinforcing, but low in strict control of their children. This is the picture painted of high-creativity mothers by Getzels and Jackson, but verified from a completely different approach.

There seem to be three other kinds of control-reinforcement patterns in mothering, as described by Heilbrun and his associates. Low nurturance subjects showed high susceptibility to external reinforcement, whether or not their mothers were very controlling. But under conditions of deprivation from maternal reinforcement, only low-control subjects showed the capacity for independent work involved in academic achievement (Heilbrun and Waters, 1968). The low nurturance-high control subjects, having very probably received many punishments, few rewards, and little chance to emit autonomous behaviors, were marked underachievers. Both of these groups, with their susceptibility to external reinforcers, could be guessed to be conformers, of some kind. Conformity is, of course, in complete disharmony with creativity, as is dependence upon external, other-initiated rewards. The subjects of both groups lack the necessary behaviors of autonomy and self-reinforcement to be highly creative.

Of most concern to us is the last group—those high in both control and nurturance presented by Heilbrun. The mothers of these subjects have been generally described by him as "overprotective" (Heilbrun and Waters, 1968). On the test of self-reinforcement effectiveness (Heilbrun and Norbert, 1970), these subjects showed themselves to be of intermediate degree of efficiency at self-reinforcement—they were neither highly susceptible to external reinforcement, nor were they highly responsive to self-reinforcement. Their idiosyncrasy shows up only in the achievement study (Heilbrun and Waters, 1968). The high control-high nurturance group was the very high group in terms of college achievement, even when ability and past achievement were taken into account. Without regard to ability, it was this combination of high levels of maternal reinforcement with strict and concerned control of the child which enabled that child to perform independent behaviors in a new environment, resulting in academic achievement.

It is our suggestion that the subjects high in both control and nurturance are not merely of "intermediate" ranking in effectiveness of external versus internal reinforcements. We suggest that they are selectively susceptible to external reinforcers, and selectively capable of internal reinforcement. They are perhaps the group of "dependent independents" we have previously discussed, who perform independent behaviors in order to obtain reinforcements from external source upon which they are in a significant way very dependent. These subjects are presumably very concerned about approval from their elders, and will perform at optimum levels in order to obtain that approval. But they cannot be said to be independent as the highly nurtured little-controlled subject is independent, or as a creative subject is independent, because their reinforcements are still very much tied to external referents--tied with long ropes, perhaps, but tied, just the same. They are not simply conforming; they are conforming to an external expectation, and they may be compulsively attempting to achieve that ideal. Comment Heilbrun and Waters (1968):

> To say that the boy behaves independently because he is dependent may reflect exactly what the HC-HN mother effects in the son by her behavior. She is in a position to use control and nurturant reward to shape those forms of instrumental independence in the son of which she approves, and the son, in turn, may learn to perform more on his own in these areas because of his wish for her approval (p. 920).

We propose that it is this pattern which is most often seen among the mothers of Getzels and Jackson's high-IQ group. It is patently obvious that these mothers would be considered high in control. "Control" was one of their major concerns in evaluating their children's school. Their attention and intervention were seen in practically every aspect of their children's lives. Their tendencies of high nurturance are less obviously demonstrated. But given the possibility that such nurturance or reinforcement can be quite selective in nature, the evidence is plentiful enough. Their frequently given permission for the child to stay home from school, their care and attention at obtaining the best advantages for their children (magazines, books, and so on)—all of these and other behaviors are the outline of a "vigilant" (Getzels and Jackson) or "possessive" (Heilbrun) mother.

And what is the response of the child to these behaviors? According to Heilbrun and his associates, it is high-level achievement and selective attention to external reinforcers. This is confirmed by evidence that bright students who are also high achievers are distinguishable by the vigilance, the involvement, and the tendency to reward academic interest found in their parents (McGillivray, 1964; Morrow and Wilson, 1961). The analogous group in the Getzels and Jackson study, high-IQ, show similar concern for the expectations and demands of the adult world. Although both highly creative and highly intelligent children show themselves to be aware of what teachers and parents value as ideals for success, the self-reported ideals of the high-creativity group are quite different from the adult protocol—while the high-IQ children report personal ideals which match those of their elders almost point by point. These children are making public announcement of their acceptance of adult standards and expectations and reinforcements as their own.

It is basic to learning theory that an internalized reinforcement system, or the ability to reinforce one's self effectively, must be the result of socialization. There must be a time when one is being externally reinforced before one is capable in internalization (Mischel, 1971). It is highly probable that the difference between high nurturance and low control and high-nurturance and high control is the degree of internalization of a reinforcement system. If a high degree of control is continually maintained over the child, then he has little opportunity to learn self-monitoring and -reinforcement. By the time most of his behavior patterns and reinforcement-seeking cognitive schemata are formed, this child still has had little reinforcement for autonomous behaviors. In the low-control child, on the other hand, autonomy has been reinforced, not punished, and self-reinforcement has been learned. The child is not continually attuned to possible external reinforcement sources with the strongest possible resemblance to his parents. He is capable of creative, independent behaviors. It is not surprising, then, that the Getzels and Jackson high-IQ group, among whose mothers this compulsivity-engendering pattern of vigilance was found, were all the brightest children among a very bright population who were not also creative. And that these same children, who did not find their way to their creative potential, showed a common tendency in the opposite direction.

Although very little further research has been done on the environmental specifics of creativity among the highly intelligent, there are groups of children finding themselves under approximately the same "vigilant" conditions as Getzels and Jackson's high-IQ group. It should be of value to examine the effects of vigilance on other children.

One of the most obviously vigilant parent groups are parents of first-born children. First-child parents are suggested by Lasko (1954) to be more anxious and protective about their child, more ready to interfere with, criticize, or attempt to accelerate him, and more controlling and demanding of him. The child produced under these circumstances of vigilance was found by Schachter (1959) to be, under the stress conditions he created, more dependent upon other people as "Sources of approval, support, help, and reference (p. 82)." He also concluded that they were more easily influenced by others in a social situation than were later-borns. Becker, Lerner, and Carroll (1964) confirmed this apparent conformity to opinions of others for social purposes, but qualified the generalization with the finding that in "informational" situations, first-borns tended to trust more to their own opinions than later-borns. This might be explained by the greater opportunity of the later-born child to ask for information from his peers, or by the first-born child's greater experience and confidence in obtaining information independently. High-IQ children would of course be expected to have the same confidence in informational tasks (Di Vesta, 1959; Lucito, 1960; Gelfand, 1962). In spite of their independence in information tasks, first-born children (and high-IQ children) still exhibit conformity in social situations.

To summarize, in first-borns we see children who have been brought up in a "vigilant," protective, demanding home, and who are as a result more dependent on the approval of others, especially adults, more likely to be conformists, and more likely to be high-achievers. They have the elements of a pattern of compulsivity. The very same home pattern is found for Getzels' and Jackson's high-IQ group, and essentially the same compulsive elements: concern for meeting the approval of adults, conformity to adult ideals, and high achievement.

II. OTHER FACTORS AND THEIR COMPLEXITY

Why do the homes of high-IQ children who are not creative follow this particular pattern? We suggest that since high-IQ children have a natural capacity for creativity, precisely these patterns are necessary to produce a non-creative child. A high nurturance level establishes the parent as a reinforcer-shaper, and prevents the child from becoming a highly creative rebel or psychopath. A high control level keeps the child from internalizing his reinforcers, and developing independent behaviors. High expectations keep him engrossed in achievement by a method of conformity and acceptability. Without control, he might exhibit autonomy, or spontaneity. It could happen, then, that most high-IQ children who do not have homes engendering creativity, have homes which produce compulsivity--because the factors which must be present to repress "natural" creativity are also those which are necessary for compulsivity.

Many children manage to survive uncreative home (and school) environments with their creative behaviors more or less intact. But we suggest that for the child of more or less average intelligence, who is not hyper-sensitized to his environment, such a task is not as difficult as it may be for the gifted child.

But this is not the only way to look at the problem. A novel approach (judged to be novel by the statistical rarity of its examples in the experimental literature) would be to examine the effect of the intelligence of a child upon his home environment. In a recent survey by this writer of the intelligence literature, <u>no</u> studies were found which took as their concern this particularly fascinating problem. Can this mean that the intelligence of a child, especially an extraordinarily high intelligence, has no effect upon the way his parents will react to him?---upon the expectations they will have, or their uncertainty at child-rearing, or their interaction with him? Would it not be exceptionally easy to expect a precocious child to be "adult" socially as well as mentally, and to demand more than he was capable of performing?

Experiments show that ambiguous situations, or situations of failure when a subject is accustomed to success, cause rigidity and conformity (Himmelstein, 1958; Gelfand, 1962). Is that, perhaps, the kind of situation in which both the parents of gifted children and the children themselves are found? Parents face situations which are unlike their expectations about children---a one-year-old capable of playing practical jokes, or a two-year-old who teaches himself to read. And children may be presented with things they understand just as badly ---reinforcement for mental prowess, but excessive physical or social demands which they do not know how to meet. Perhaps rigidity is the response of both to amgibuity.

It is well to remember that children of high intelligence appear well capable of controlling their own behaviors, of inhibiting themselves, and of exhibiting moral behaviors (see Unger, 1964). How does a parent react to a child apparently so ready and able to perform in ethical, socially acceptable ways? Does the parent learn early to appeal to the child's morality in controlling his behavior?---or is it perhaps simply too easy to expect nearly perfect behavior of children who seem to be capable of fulfilling that expectation, and to demand from them the limits of their capability?

The question of the effect of offspring intelligence on parent-child interactions is one that is in desperate need of investigation. Even attitudinal surveys would mark an improvement in the available data. What are badly needed, however, are observational studies with an approach as behaviorally specific as possible. Parental actions toward gifted children need to be compared with the behaviors exhibited by parents of average children. The difficulties in specifying differential behaviors which are the effect of a child's individual differences, and not the causes of them, are very large indeed. But they must be attacked.

Other difficulties involved in specifying the environmental causes of creativity and compulsivity among highly gifted children can be easily gleaned from existing research. Sex differences (in both children and parents) are of critical importance in investigating the effectiveness of models for conformitory or imitative behavior (Mischel, 1971; Rosenblith, 1961), or the effectiveness of parental reinforcement (Patterson, 1969; Patterson, Littman and Hinsey, 1964; Stevenson, 1961), or interactions of anxiety and achievement (Feldhusen and Klausmeier, 1962) or the frequency of creativity among the very intelligent (Barron, 1969).

Age differences interact with sex differences in some learning situations (Stevenson, 1961). In fact, age may be a highly critical variable. We have suggested that there is a time when the child's schemata for cognitive and gross behavioral tendencies are formed, more or less to stability. Thus, it is possible that children undergo a critical period for learning creative behaviors, or that there are stages involved in the learning of creativity--like the decrement of amounts of external control, or increasing reinforcement rates for independent behaviors. Hebb (1949) has made the point that the more intelligent species take longer to develop to maturity—there may be an analogous phenomenon at an intra-species level. It could be suggested that highly intelligent people are in part more socially sensitive because they require longer to develop their neurological systems to maturity—to develop their schemata—and are thus susceptible to environmental/cultural influences for a longer period of time. It is clear (in spite of the fact that not enough data exists to make more than speculation about details) that any theory of the interaction between creativity and intelligence must make due consideration for age and developmental variables.

A related area of concern is the question of reversibility of schemata. Since it has been partially demonstrated that creativity schemata are by their very nature reversible, the real question is the question of whether or not intelligent, noncreative adults can alter their schemata in the direction of creativity. It is predicted that since schemata are not simple responses, but complexes of learned responses and executive cognitive structures, they will be relatively stable and not easily reversible. It is very likely that a concerted effort at rennovating environmental contingencies, modeling, and shaping creative behaviors would be necssary to produce creativity as a set of spontaneous behaviors in adults with well-established, noncreative schemata. If, however, there is anything hopeful presented in this paper, it is the conviction that creativity, within the limits of intelligence, is dependent upon environmental contingencies, and can be encouraged and shaped.

Many suggestions have been made as to how to encourage creativity in growing children (Getzels and Jackson, 1962; Torrance, 1962, 1965). Torrance, most especially has been involved in an admirable dual attempt: to investigate empirically the effectiveness of modeling and reinforcing creativity, and to instigate immediately the techniques which are found to be effective—in as many situations as possible. This is part of the work which needs very much to be done, and a reading of Rewarding Creative Behavior (Torrance, 1965) will provide more insight into global methods for producing creativity than there is room here to explore. But these kinds of studies are necessarily molar and nonspecific.

Of course, we have seen that approaches which too heavily emphasize isolated, specific aspects of creativity are not successful. It has become clear through our review of the creativity-training literature that the training of extremely molecular "creative" behaviors is unlikely to result in spontaneous generalization or transfer. Many different creative behaviors must be reinforced at once in many different ways, so that new discriminative stimuli are provided the individual for behaving creatively at different times in a different place. Thus, it is the interaction of many reinforcement patterns which results in creativity—at least during experimental training, and we can wager that the same thing is true in nonexperimental settings.

We know, then, that the evolution of creative behavior is determined globally, by the interaction of many patterns. And this is the nature of the approach taken by Torrance and others. But at present, when we wish to make use of suggestions for enhancing creativity, we must rely on what our own culturalization allows us to derive and act out behaviorally from phrases like, "Be respectful of questions." A suggestion of this very general nature tends to result in the appropriate, intra-culturally uniform, specific behaviors. But we should not forget that "respect" is a specific behavior pattern, if it is anything, and should be specifiable; and we must specify, if we are to understand creativity and reach the limits of our ability to enhance it.

The optimal approach is a combination of specific and global approaches. Behaviors of parents which act as discriminative stimuli or reinforcers of creativity among their children must be specified as clearly as possible, as well as the elements of the creativity itself. But specification of a few creative responses and their contingencies is essentially useless--it is the total pattern of such interactions which is significant. Therefore, enough behaviors must be tapped, and fine enough distinctions made, so that these over-all patterns are not lost.

CHAPTER X

A THEORY OF THE CREATIVITY-INTELLIGENCE INTERACTION: AN ENVIRONMENTAL SUPPRESSOR VARIABLE

It has been our argument that highly intelligent children, because of the complex interaction of their high-level arousal and inhibitory systems, have a great capacity for differentiation and complexity in their cognitive systems, and that this capacity has a natural tendency to produce creative behaviors. In fact, these covariables with intelligence are necessary, although not sufficient, for creative production at any significant level of abstraction.

We have also argued that because of the very same mechanisms which predispose the gifted child toward creativity, he is extremely susceptible to environmental influence. He is capable of being more highly aroused or motivated in a given direction, and capable by his complex inhibitory systems of limiting his attention only to directions shaped and reinforced by his environment, perhaps during a critical period in his life. Not only are his response tendencies at a cognitive level (attending, and the like) environmentally selected, but also the overt responses made to the stimuli to which he does attend. All of these response tendencies might be considered part of creative or noncreative schemata which direct his behavior.

Presumably, creative schemata differ from noncreative schemata in that they allow for rapid and temporary reorganizations of associational elements into new generalizations or search categories; they must be heavily overlapping, in order to allow for such rapid recombination. Also incorporated in the schemata are hierarchies of reinforcement contingencies or expectations for certain behaviors in certain situations--these, too, will discriminate between the creative and the noncreative. It is argued that noncreative schemata will have a higher susceptibility to ongoing social reinforcement, while creative schemata may have inhibitory processes in effect for incoming social stimuli, and depend for direction mostly on already-experienced and -internalized reinforcements.

The highly intelligent child might be guessed to be more adept at analysis of the contingencies of his environment and accurate in performance of the behaviors which will meet the criteria for reinforcement in that environment--no matter what the nature of the environment, or in what direction it shapes him. Therefore, but for a few qualifying evidences, it could easily be argued that the highly intelligent person is equally sensitive to all directions in which he might be shaped, except for his tendency at a cognitive level to perform creatively. Any behavioral tendency other than that toward creativity might be assumed to be completely open to the random selection of his surroundings.

As expected from these assumptions, and as we have already seen, there is a high incidence of creativity among the highly intelligent. Torrance (1962) estimated that approximately thirty percent of those students above the eightieth percentile intellectually in the public schools are also above the eightieth percentile in creativity. This is an estimate based upon many different studies in different schools, all with about the same results. Therefore, we can safely guess that about a third of gifted school-age children have reached their potential in creativity or have come near to reaching it. This is an acceptably heavy loading, given the assumption that high intelligence leads naturally to high creativity—since we have never argued that environmental influences do not have great effect in determining the direction in which a child will invest his potential.

If, however, all the other possible behavioral tendencies have a random probability of occurrence, they should have a more or less even distribution. We shall not have to identify all the other possible tendencies. A simple evaluation of the few we can identify should be sufficient to find if there is an even or uneven distribution of occurrence among them.

For example, let us examine the general pattern of rebelliousness, intractability, and low achievement, which often occurs in children of average or below average intelligence. Of course this pattern is not unknown among children of high intelligence; and when it does appear, it may be manifest to an extreme degree, true to our expectation that any tendency will be more intensely followed by the gifted. But the overall incidence of any low achievement or antisocial behaviors among the gifted is extremely slight (Terman, 1925; Terman and Oden, 1947; Jaggers, 1934, and others). The distribution of frequency does not appear to be an even one at all.

What constructs can we invoke to provide explanation for the apparent avoidance of underachievement among the highly intelligent as an overall strategy? It is wise to think almost in terms of economics if we wish to understand what we see. It is the natural disposition of all organisms to get the most reinforcement (primary or secondary) possible from the smallest possible investment of energies. The growing human being, in the process of evolving and developing his own behavioral schemata, is no exception to this rule. The child will do what he must in order to get the most social reinforcers for the least effort invested. This means that he must use his particular abilities efficiently. Underachievement, disobedience, and rowdiness may be the most effective way of gaining attention for the average or below-average child--an investment in school work and obedience is most likely to bring only partial failure and punishment.

For the gifted child, the situation is quite different. An investment of energies in school work, obedience, and responsibility has a high probability of paying off well in social reinforcement from the teacher--and therefore, from the peers. In point of fact, there is no other means of response which has as high a probability of success and reward for him as does responsibility and scholarship--not even creativity. Any child is capable of making an original response, and evaluations and judgments of creative products are more subjective and unpredictable than those of factual products. All in all, the child has a much lower hope of success in competing with his peers creatively than he has in "academic" competition. He will very naturally play to his comparative strengths.

Of course, if the school system were structured in such a way as to demand creativity above encyclopaedic knowledge of facts, the highly intelligent child would be forced to adapt himself to the new set of contingencies. He would invest his complex cognitive abilities in creative responding, and very likely would be better at it than his peers.[7] In such "creative" schools, flexibility could be as much a part of the curriculum as reading—every subject could be taught from many different points of view, instead of one. Skinner (1972a) has pointed out:

> . . . we do not need to abandon subject matter in order to teach discovery. It is not true that if we fill the student's head with facts he will be unable to think for himself. He is not damaged by facts but only by the ways in which facts have been taught (p. 338).

As things stand in our society, however, creativity is not the preferred mode of response, and students are being harmed by the way facts are taught. The school system seems excessively guilty in this regard, with its strong emphasis on conformity and control.[8] Although it is easy to make intuitive and experiential judgment that creativity is not reinforced in the school system, we have also seen more objective data supporting our suspicions (Getzels and Jackson, 1962; Torrance, 1962). Evidently, the extra "disruptiveness" and playfulness of creative children is more bothersome to the teacher than their creativity is valuable. With such risks attached to creative behavior, it is no wonder that only the student who has a lesser certainty of strictly academic success can afford to invest his efforts in creativity and rely upon the sparse schedule of reinforcement it represents.

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[7]There is evidence that in very "creative" school systems, correlations between IQ and creativity are higher than elsewhere. Refer to data of schools "A" and "D" in Torrance (1962) although he does not draw these assumptions from the data.

[8]It is perhaps no accident that the high IQ-low creativity subjects of Getzels and Jackson (1962) were in many cases the children of teachers or professors.

If the intelligent child does not have the necessary environmental contingencies outside the typical classroom, from early in his life—contingencies specifically counteractive of school reinforcement patterns—he will almost assuredly choose to behave in a pattern of responsible, compulsive attention to scholarly and factual achievement. He will choose a pattern of "convergence," in a very real sense, because he will constantly be looking for the "right," and acceptable behavior with a high probability of being reinforced. This may well generalize into moral and social areas—he may extend the pattern, and seek in these domains as well the "right" and acceptable behavior, as reinforced by those in authority. The compulsive child of high intelligence will attend constantly to those social, verbal and nonverbal cues that tell him which behaviors to emit. His inhibitory capacities will enable him to limit his attention to these cues, and to control his other behaviors. He will become the vigilant, responsible, moral, socially-sensitive, high-achieving, gifted child we have so often been shown in research results. The more he is reinforced for social reactivity, the more this pattern will affect him.[9]

Unfortunately, susceptibility to this pattern may be heightened by higher and higher levels of intelligence. Although the gifted person's potential for creativity is undoubtedly greater with extremely

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[9] It is very significant that girls, who are known to be much more socially responsive in nonverbal ways (Exline, 1963) than boys, are also more responsible and more achievement-oriented than boys, until late adolescence. In a study of creative women, Barron (1969) found that they were very much unhappier than their noncreative peers, leading to speculation that creativity is not an acceptable pattern for female behavior in our society.

high intelligence, so, too, is his potential for compulsive attention to detail and success in meeting the expectations of others. If this is true, we should expect to find not only that the extreme case of flexibility (high creativity) has a high concentration of the highly intelligent, but also that extreme compulsivity would have a higher-than-expected frequency of occurrence among the highly intelligent.

Before we evaluate the data on this point, let us make note of the fact that extreme compulsivity is not very likely to be thought unusual in our culture. In its inflexibility, it might be expected to be an undesirable thing, its disadvantages socially recognized. But it is this end of the continuum which is preferred; given the alternatives of creativity or compulsivity, our culture will choose and reinforce compulsivity. Therefore, individuals with even a marked degree of compulsivity--well beyond any margin of adaptability--will not be readily recognized as "deviant." Rather, they will more likely be labeled "model," or "ideal." "Deviant" subjects will represent only the very most extreme cases, and their number will appear smaller than might be accurately estimated, were the facts known.

In comparing compulsivity and IQ, there is an additional problem. Highly intelligent subjects have been shown to be capable of more facilitative response patterns under conditions or states of stress. We can very easily assume that they might also be better at handling a compulsive behavior pattern or strategy, without overt emotional "problems," and would still less often than others be diagnosed as deviant or neurotic.

With this in mind, let us attempt to define what psychological pattern or "syndrome" might be representative of marked compulsivity.

The Diagnostic and Statistical Manual of Mental Disorders of the American Psychiatric Association defines the obsessive-compulsive personality disorder in this manner:

> This behavior pattern is characterized by excessive concern with conformity and adherence to standards of conscience. Consequently, individuals in this group may be rigid, over-inhibited, over-conscientious, over-dutiful, and unable to relax easily. This disorder may lead to an obsessive compulsive neurosis. . . (1968, p. 43).

With this quite extreme degree of compulsiveness as our criterion, and with the stipulations we have stated in mind, it can be shown that intelligence is related to the obsessive-compulsive personality disorder. Says Slater (1945):

> It is also generally agreed that neuroses of different types tend to occur among persons of different orders of intelligence: in particular, obsessional neuroses among highly intelligent persons (p. 40).

Although he found no evidence that other neuroses could be differentiated by any intellectual measure, the "generally agreed" upon hypothesis that obsessive-compulsives are on the average more intelligent than other neurotics was supported. Ingram (1961) obtained corroborative results.

What we apparently find here is that their environment tends to shape highly intelligent people in one particular direction: compulsivity. Although they have natural tendencies toward creativity, at a cognitive level, only a third of them approach their creative potential. The rest, with very few exceptions, choose achievement and compulsive, responsible, ongoing attention to social reinforcements (from authorities) as their life pattern. A few of these are highly responsive to an extremely demanding or restrictive environment, and they will tend in disproportionate numbers to be clinically diagnosable as obsessive-compulsives. But most of them are somewhere in between, having been reinforced for some flexibility and some creative behaviors and falling at neither extreme of the creativity-compulsivity continuum.

Although most of the gifted will fall between the extremes, the higher the individual's intelligence level the more pressure toward compulsivity is exerted upon him by his surroundings. He may lack the common viewpoint or experience of those less intelligent, he may find himself only marginally accepted because of his uniqueness, and he may find that success at concrete learning is simply too reliable to give up. Therefore, as his potential for creativity grows, so do the environmental influences against creativity. This environmental influence, stronger on him the more intelligent he is, and far stronger on him than on his peers of average intelligence, is responded to more and more intensively, with increasingly high intelligence. The higher the intelligence level, the stronger is this pressure as compared to the increases in creative potential. And this combination of factors is our suppressor variable. It is this matrix of variables which causes correlational studies to indicate a ceiling to creativity increases with IQ elevations. It is this group of variables which causes zero correlations between IQ and creativity to appear in high IQ ranges. It is this variable complex which prevents extremely high IQ individuals from reaping, as a group, the benefits of their great creative potential.

The suppressor variable does not appear to be biological in origin (although it could not accomplish what it does without certain biological factors, like inhibitory processes). It is therefore <u>environ-</u>

mentally-based, and avoidable. There is no reason to assume, as Torrance (1962) does, that intelligence in itself is inimical to creativity. Nor that highly intelligent children are hopelessly plodding and encyclopaedic.

But special attention must be given to highly intelligent children, so that they may be "vaccinated" against our culture, before it destroys or maims their creative potential. We must find out how parents may be particularly susceptible to expecting hyperresponsibility and conformity from them. And, eventually, we must change the behavioral contingencies in our school systems and in the culture, generally. If these steps are taken, we shall be able to realize far greater creative potential than we have yet gleaned from the brightest among our children.

If the steps are not taken, we must make do with the creativity we do get from the few highly intelligent children who are raised in an effectively creative environment, and from the less intelligent children—who, having less to lose, are able to take the risks involved, and behave creatively in spite of the dominant tone of their surroundings.

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APPENDIX

A theoretical system is beneficial because it introduces order where once there was chaos. That is its most obvious benefit. But the process of theory construction has other significant by-products. By assuming a theoretical position, one is led to approach the tasks of experimentation and observation in a more systematic way. In order to carry out the process of theory testing--no matter what validity the theory may have--one is led to choose certain areas of study over others, to follow a pattern, to persist until a thing is either known to be true, or known not to be true.

It is our purpose here to indicate the direction (or directions) of research suggested by the present theory of the creativity-intelligence interaction. Some of the areas suggested will be tests of the theory: others will not. Some suggestions will indicate new or presently underdeveloped research areas: others will indicate areas which have been overdeveloped or in which new methods seem to be necessary.

Although the imaginative experimentalist will be able to derive many more useful applications from the present theory than we might be able to enumerate, some of the more obvious ones can be presented here. They will be presented in terms of the specific area of investigation they suggest.

The correlation of creativity with personality "traits." Since, as we have suggested, little foundation can be found for the "trait" concept itself, the finding of relationships between traits and other variables is of extremely limited value. Furthermore, a great deal of this kind of research has already been done--it is probably the most thoroughly explored aspect of creativity--and what value it has had in the past can now easily be superseded by other areas, of critical significance, which remain untouched.

The anxiety research. Although it suffers from the same difficulties as the study of personality traits, the anxiety research has a few particular problems of its own. Anxiety must be specified in terms of situation, individual, and response tendency if research results are to be readily usable. Although much has been invested in investigating the interaction of anxiety and intelligence, little more will be gained through further investigation unless the kinds of responses made to anxiety are clearly specified, anxiety is clearly defined, and intelligence is consistently accounted for in research design.

The achievement research. It is not proposed here that we eliminate achievement as an area of significant concern--only that we limit our observations to behaviors which can be defined as achievement, and exclude concern with need for achievement until this concept has a stronger and clearer theoretical and methodological foundation. Many problems (such as the confusion of independence from parents with the "independence" corresponding to need for achievement) make the design of present achievement research almost inapplicable to investigations of intelligence. If we are accurate in our prediction that intelligent children have a kind of social sensitivity but also achieve in extraordinary measure, the field of achievement research needs drastic revision before it will be able to present an adequate test of the theory.

Creativity and generalization. It is the suggestion of this paper that creativity is representative in part of an ability at the cognitive level to generate and regenerate useful generalizations containing different populations of elements. This is closely related to Bruner's (1964) suggestion that creativity actually is a kind of generalization or abstraction. It would thus be helpful to determine whether or not creative subjects are better at forming generalizations rapidly. For example, do creative subjects more efficiently provide a common title for a group of miscellaneous objects? And are they then better able to invent another title, when some, but not all, of the objects are substituted? Or, do creative subjects construct a larger number of categories into which a group of objects can be classified, and do these show a greater overlap in category construction?

Computer simulations. Although Schroder (Karlins, 1967; Karlins et al., 1967; Schroder, et al., 1967) has initiated a great deal of research comparing creativity to certain kinds of structures and operations in computers, little has been done to extend this research to include intelligence. This may be the most promising (and perhaps the only) means for discovering the differences between creativity and intelligence at a cognitive level. An attempt should be made to design executive programs which can simulate creative or intelligent but noncreative behavior, or to tap these phenomena by observing what computer resources are used and how they are used differentially by subjects who are intelligent or creative.

Flexibility of creativity. Although the strong implication of several research results (McDonald and Martin, 1967; Wallach and Kogan, 1965a, 1965b; Duncan, 1959) is that creative subjects are able to respond well in ways that are both typical and atypical for them, while noncreative subjects cannot respond in atypical ways without great difficulty, there is little direct evidence of the idea that creativity schemata are mostly instructions of flexibility at a high "executive" level of cognition. An experimental study needs to be designed specifically to test the idea, so that we need not depend upon the by-products of research designed for completely different purposes. The McDonald and Martin (1967) method could be used—an approach in which creative subjects were taught a pattern which would not characteristically be chosen by them, and noncreative subjects taught a "creative" pattern, and both were compared for rate and efficiency of learning.

Intelligence and stimulus complexity. Although there are many, many studies in which creative subjects have been found to prefer visual stimuli which are asymmetrical and complex, (Barron, 1957, 1958), this format has not been applied to the question of intelligence. Beck (1968) has found tentative evidence of this kind, working with the Rorschach. And it might be predicted from our theory that intelligence would covary with a preference for complex, asymmetrical stimuli—although the suppressor variable which limits creativity might also have its effects on stimulus preferences. Nevertheless, research in this area would indicate how much of Barron's results is due to the effects of intelligence, and it might help to clarify how extensive are the effects of the suppressor variable.

Delay of gratification and creativity. Although correlations have been discovered between ability to delay gratification and intelligence, it has not been related in any way to creativity. It might be predicted that creativity would be enhanced by ability to delay gratification, and, although not crucial to the theory, such a result would tend to remove further any suspicion that intelligence (or covarying ability to delay gratification) is intrinsically opposed to creativity.

Vigilance and IQ. It will be impossible to investigate the true relationship (if any) between intelligence and the vigilance phenomenon, until methods are devised to present stimuli to experimental subjects which are complex enough so that information-processing ability can actually be used to advantage in a "vigilant" response mode. The use of dramatic or narrative films rather than stimulus cards as the source of stimulus input might be a beginning of the process of complication which must be carried out in the vigilance research, if intelligence is ever to be fully investigated.

Success, rigidity, and intelligence. In order to determine if a high probability of academic success can stunt creativity and result in compulsive adherence to academic pursuits among the highly intelligent, some experiments of a slightly less global nature might be of potential relevance and use. For example, do subjects who have a high probability of success at a task (that is, history of successes for the task during the experimental treatment) show a rigid adherence to that task when given the choice of other tasks? Do they manifest a tendency to remain steadfastly in one particular mode of response on a single task, when that mode has proven successful, as compared with those who have experienced a few failures? The smaller questions, if answered, would provide a good basis for conjecture about larger ones.

Self-reinforcement and creativity. Although from the Heilbrun and Norbert (1970) description of effectively self-reinforcing subjects and their mothers, we might well assume that self-reinforcers are more creative than other subjects, this has not been specifically tested.

Therefore, a design must be constructed in which creative subjects (who are known to be more independent and more self-valuing) can be determined to be more or less efficient at self-reinforcement than others. When given a task and told to reinforce themselves, will they do so more often? Will they respond more to their own reinforcement than other subjects do? Will they respond more to their own reinforcement than they respond to reinforcement from others? The answers to these questions will help us establish the kind of learning history human beings must have in order to behave creatively.

Explicit specification of creativity variables. Research must be done on the specific behaviors, and the total patterns of specific behaviors, which result in creativity. This can be done only with a combination of observational research and creativity-training experiments. Training experiments could well be elaborated to a broader scale than they have been as yet. Groups could be chosen whose parents would undergo extensive training in how to train their children to behave creatively, and observations made over a long period of time. With intensive creativity training, over long periods of time, high-IQ children would be expected (according to the present theory) to make greater gains in creativity than children with lower IQ's.

The effects of intelligence in children upon their parents. Although it is not now clear to this writer how the problems involved can be solved, research must be carried out on the effects of intelligence upon the parent-child interaction. That this kind of research could best be served by observational methods is clear--how the effects of the child's intelligence could be distinguished from other variables is not at all clear. The results of such research, however, would provide many of the fine-grained elaborations needed by the theoretical system as we have presented it.