

THE PSYCHOLOGICAL ASSESSMENT OF LANGUAGE:
A PHILOSOPHICAL CRITIQUE OF THE
ILLINOIS TEST OF PSYCHOLINGUISTIC ABILITIES

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It is argued in this paper that the Illinois Test of Psycholinguistic Abilities (ITPA), a test widely used in the assessment of certain language abilities in children, does not measure what it purports to measure; in psychometric terminology, it does not have construct validity. Specifically it is contended that the inferences concerning the processing of language which are made on the basis of children's performance on the tasks of the test are not warranted. Three competing versions of the intended inferences are characterized at the outset of the paper, with particular attention being paid to the logical relations obtaining between a given inference and the test performance on which it is based. Central among these competing interpretations is that wherein the psycholinguistic processes postulated by Charles E. Osgood in his mediational response (neo-behaviorist) theory of communication are taken to be under assessment. Considerable attention and criticism is brought to bear on Osgood's theory because of the central role it played in the development of the ITPA and because, at least on one interpretation of the test, the processes specified by him are under assessment. The latter interpretation is rejected on the grounds that Osgoodian theory is incorrect, vitiated by an equivocation on his central theoretical construct. The two other interpretations of the test, which do not depend upon the correctness of Osgood's theory for their own validity, are challenged and rejected independently. The conclusion reached is that under none of the adopted formulations, all of which are supported by the ITPA literature and have adherents among the test's many commentators, does the ITPA succeed in providing the information about children's processing of language which it purports to provide. The manifest consequences for educational decisions and programs predicated on the belief that the test does do so are noted.

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I. INTRODUCTION

"While there are many theories of psycholinguistic functioning, the schemata presented by Osgood (1957) has had the greatest impact on education." (Hammill and Larsen, 1974: 5.) I believe that most classroom teachers would find this remark quite surprising for the simple reason that they are unlikely to be familiar with Osgood, or his theory, or in what way it has affected their classrooms. Asked to name a psychological theorist whose work had affected teaching methods or materials, most teachers would almost certainly name Piaget or Bruner. Asked specifically to name a linguist who may have had some influence on their classroom practice most would, I suspect, mention Chomsky. How is it then that Larsen and Hammill attribute to Osgood "the greatest impact on education"? The answer is provided a few paragraphs later:

The educational applications of these particular psycholinguistic principles (Osgood's) have generated both assessment techniques and remedial language programs. It was this model which Kirk, McCarthy and Kirk (1968) adapted and used to construct the Illinois Test of Psycholinguistic Abilities (ITPA). This diagnostic instrument was designed to measure specific functions of psycholinguistic behavior and provides a framework for the amelioration of language disorders. The ITPA clinical model and the original Osgood schema have served as the basis for several remedial and developmental programs that are extensively used in schools. (Hammill and Larsen, 1974: 5.)

These sentiments concerning Osgood's theory and the ITPA receive even stronger expression in a recent book:

Osgood's psycholinguistic theory, however, has had an unprecedented impact on schools and clinics, undoubtedly because it is represented in the ITPA, a test which dominates the field of language measurement in the same manner that the Binet and Wechsler tests dominate the field of intellectual measurement. The vast number of research studies using the ITPA as a principal diagnostic or predictive tool attests to the interest and enthusiasm the test has evoked in education. In order to fulfill our designated task, we must necessarily concentrate on the application of Osgoodian theory through the ITPA and its related training programs. (Newcomer and Hammill, 1976: 18-19.)

Essentially, then, it is because a certain test, the ITPA, was based on Osgood's theory of how language is acquired, understood and produced, and because this test is so widely used, that Osgood's theory has so extensively influenced education. The widespread use of the

ITPA as a diagnostic instrument and the design of both teaching materials and methods in accordance with its suppositions about the "processes which underlie communication activities" is easily documented. Nancy Hanck, an advocate of the ITPA, describes the extent of its use as follows:

It is currently being administered by psychologists, speech pathologists, educational diagnosticians, guidance counselors, remedial reading specialists, and teachers of various types of handicapped children. It is being administered to children who are having difficulty learning in the mainstream of general education as well as children with specific handicapping conditions. Although it is not intended for use as the primary instrument for evaluation and placement of a child in a special class or program, it is often used as the major support for such placement. It is properly used to make note of relative strong points and weak points in a child's repertoire of learning strategies. It is regularly misused as the basis of some new curriculum that supersedes or supplants the academic program. (Hanck, 1976: 50.)

The general condition leading to administration of the ITPA is the presence of some unexplained learning or communication difficulty in children from preschool age to the age of eleven. The potential test population given this broad characterization is clearly enormous, and it has had an effect commensurate with its size. One writer simply asserts that it is "the most widely used test of language ability." (Houston, 1972: 117.) Another states, "Other than the WISC or the Stanford-Binet...it is likely that the ITPA is the most used test in learning disabilities programs throughout the United States." (Gearheart, 1973: 58.) Both judgments are very likely to be true. If one consults any library shelf containing texts on school learning problems, one will find that virtually every contemporary writer in this area at least mentions, and usually recommends, the administration of the ITPA as part of the diagnostic process. All of the major journals in special education--Exceptional Children, The Journal of Learning Disabilities, The Journal of Special Education, to mention the foremost--are laden with articles concerning the test; entries on the ITPA in educational, language, and psychological abstracts run into the thousands.

American advocates of the test are innumerable with some of the more conspicuous being Bush (1969, 1976), Bateman (1964, 1965), Gearheart (1973, 1976), Dunn and Smith (1966), Karnes (1968, 1972), and Minskoff (1976). The test is also receiving increasing attention in British publications with most of it neutral or supportive with reservations (Mittler, 1976; Wedell, 1975). Its use with English school

children has been sufficient to give rise to at least two published studies there (Mittler and Ward, 1970; Marinossion, 1974). The test is also administered widely and has generated a good deal of research in Australia. Most notable is that of Australian John McLeod, an early collaborator with Samuel Kirk, principal co-author of the ITPA, who has written a number of articles on the test and remains a staunch defender of it (See McLeod, 1976). The test has been translated into Danish (Rasmussen, 1971) and there are reports of its use with Maori, Mexican-American and Native American children. The usual host of correlation studies have been carried out--relating ITPA performance to everything from I.Q. and academic achievement to birth weight. The ITPA is also a standard member of the battery of tests administered at the well-known Marianne Frostig Center for the Study of Learning Problems, with Frostig herself an advocate of the test (Frostig and Maslow, 1973). The considerable influence of the test and of Osgood's theory in the diagnosis of learning problems is simply undeniable.

But even this extensive influence would appear to be overshadowed by the impact the test and theory have had on remediation. The Hammill and Larsen study from which I quoted in opening this paper is a review of just under forty different programs "which use the ITPA or one or more of its subtests as the criterion for the improvement of language behavior." (Hammill and Larsen, 1974: 5.) While many of these were singular programs, in the sense of being undertaken one time only, and/or in one school or school district only, what is usually referred to as the "Osgood model" or the "Osgood-Kirk Model" has served as the declared basis of at least three commercially packaged programs. These kits, involving instructional materials, workbooks, teaching manuals, etc., are the Peabody Language Development Kits (Dunn and Smith, 1966), the M.W.M. Program for Developing Language Abilities (Minskoff, Wiseman and Minskoff, 1972), and Goal Program: Language Development (Karnes, 1972). In addition to these packaged programs, a number of books and pamphlets have been published recommending teaching activities directed toward what is broadly and not very informatively characterized as "psycholinguistic training" (Bush and Giles, 1969; Kirk and Kirk, 1971). A quite comprehensive review and discussion of these ITPA-related programs may be found in Hammill and Larsen (1975) and Newcomer and Hammill (1976). No researcher can rival Hammill in the amount of research done on the

ITPA and its educational offspring. His comments on the ITPA's influence on teaching are thus those of someone very familiar with the subject:

The movement toward psycholinguistic training in the schools began, for all practical purposes, with the development of the Illinois Test of Psycholinguistic Abilities (Kirk and McCarthy, 1961). With this instrument, it became possible to profile the performance of children on different psycholinguistic abilities. As a result, it appeared that potentially useful information pertaining to communication behavior could then be used to plan individual remedial programs for the children who evidenced difficulties. The test has been so influential that the constructs measured by the ITPA have become for many school professionals the operational definition of psycholinguistics. Thus, learning centers have been set up in the classrooms and remedial programs have been developed which correspond with the ITPA constructs as manifested in the subtests. Key to this approach is the assumption that these functions are identifiable in individual children, that deficits can be remediated through a planned program, and that the constructs do in fact contribute appreciably to academic success. (Myers and Hammill, 1976: 222.)

I hope that this brief survey gives some indication of the appreciable influence of the ITPA and Osgood's theory on school practice. In my account I have underplayed that influence, if appraisals such as the following are taken as representative:

Osgood's theory has formed the basis of the most widely used test of language ability, the Illinois Test of Psycholinguistic Ability (sic) or ITPA. (Houston, 1972: 117.)

The Illinois Test of Psycholinguistic Abilities (ITPA) is simply a test, but it is a test that has gained more immediate acceptance over a short span of years than we are likely to see again in our lifetime...Other than the WISC or the Stanford-Binet, which may help to determine eligibility for learning disabilities programs on the basis of general intelligence, it is likely that the ITPA is the most used test in learning disabilities programs throughout the United States. (Gearheart, 1973: 58.)

It can be said of the ITPA what Guilford (1959) said of his Structure of Intellect, "This is only the beginning." Anders (1974) likens this appearance of the ITPA with the famous suborbital flight of John Glenn. How wonderful indeed did his brief downrange flight seem to us. And, then, within years, we were on the moon. (Bush, 1976: 87.)

The reverie exhibited in the last remark does not exaggerate by much the fulsome and largely uncritical acclaim that has been accorded to the ITPA. It has certainly been massive and in my view, and the view of some others, excessive (Ross, 1976; Rosenberg, 1970). Whatever the explanation for it the impact of the ITPA and Osgood's theory on educational practice is a fact; and I shall have nothing further to say in the way of establishing the importance of an inquiry into them both.

If the arguments to be raised in this paper are correct, then Osgood's theory and the ITPA should lose their influential status. It will be argued that the ITPA does not provide information about the processes which underly language reception and production, and that the diagnosis and placement of children in training programs and the design of teaching materials and practices that have been based on this supposition are grounded in a massive illusion. It will further be argued that an alternative interpretation of this test, in which it is not processes but "abilities" that are being assessed, is also incapable of supporting the educational decisions taken in light of it. Our first concern will be, as it must be, to carefully and correctly characterize the information which the ITPA is supposed to be providing about children, a formidable task given the vagueness which characterizes much of the ITPA literature. It will emerge that the ITPA's apparent central concern is to provide information about various postulated psychological processes which underly communication. Since, at least on one interpretation of the test, the processes purportedly under assessment are those specified by Osgood in his mediated response theory, we are led to an examination of that theory and its adequacy. Our argument will be that Osgood's theory is undoubtedly incorrect, that the processes which he specified are undoubtedly not those which underlie language reception and production, and consequently that the ITPA, if this is how we are to understand it, provides none of the information it purports to provide. The educational diagnoses and judgments of every sort based on the belief that Osgoodian processes underlie language performance and that this test assesses them, are utterly without foundation.

But not everyone agrees that the demonstrated inadequacy of Osgood's theory would have such consequences for the ITPA. Myers and Hammill, for example, have the following to say:

There are many individuals who feel that the ITPA is based upon an inadequate or incorrect conceptualization of language development. They maintain that since Osgood's theory of language is no longer considered plausible by most modern psycholinguists, Kirk's test-model must also be inadequate. Their logic implies that a test cannot be valid if the theory which underlies it is not valid. While this might appear correct philosophically, examination of psychometric devices, such as intelligence tests which relate most closely to school achievement, reveals little theoretical basis for their validity. Yet, they obviously have pragmatic value despite the fact that they purport to measure intelligence, a construct which has never been defined to everyone's satisfaction. Seemingly, if it can be demonstrated that the ITPA has educational relevance,

the test will have value regardless of the validity of its theoretical basis.

Additionally, although modern psycholinguistic theory differs greatly from the Kirk-Osgood approach, it too consists of unproven hypotheses. The phenomenal growth and continuous change within the discipline of psycholinguistics should serve as a reminder that ideas which are enthusiastically embraced on one day are often repugnantly rejected on the next. Consequently it would appear somewhat unfair to conclude that the ITPA has no value because Osgood's conceptualizations are no longer popular. (Newcomer and Hammill, 1976: 23.)

Enough straw men are provided in this passage to take a small herd of cattle through a hard winter. In the first place, the adequacy of Osgoodian or any theory is not measured by a show of hands. Only a very misguided thinker would maintain the view that it is by virtue of the fact that Osgoodian theory is out of favor--"no longer considered plausible by most modern psychologists"--(and this is a fact) and that the ITPA is based on it, that the ITPA is inadequate. I know of no one who would or does maintain this hopeless position and am in little doubt that the unidentified persons standing behind Myers' and Hammill's 'they' are made of hay. The "logic" of the case against the ITPA is rather that if it can be determined that the Osgoodian processes are not those which function during communication and if the ITPA purports to be revealing the status of those processes, then the ITPA is necessarily invalid. It cannot be providing information it purports to be providing. The "logic" here is flawless.

Nevertheless, Myers and Hammill would have us think otherwise. Their substitution of the issue of a test's pragmatic value for the issue of its validity, the latter being the issue they began with, is so conspicuous that it hardly requires comment. The argument here is that there can be and are tests whose underlying theory is questionable or even discredited, yet there remain valued inferences that can be drawn from test performance. Unfortunately, this argument is of no consequence for those maintaining that a necessary relationship between test adequacy and theory adequacy obtains when the test-theory relation is of the above specified character. Quite obviously, a test may stand in relation to a theory in ways other than that wherein the validity of the latter determines the validity of the former, as when a theory simply inspires developments in another area; and a test may support many inferences of "educational relevance," "pragmatic value," etc. which are not related to theory validity. But the test cannot be a valid measure of processes which are known not to be occurring. The

smokescreen set up by addressing pragmatic value (despite theoretical disarray) rather than validity is easily blown away, given the recognition that we can find good uses for virtually anything, even flat tires, despite the fact that such things, tests included, have failed in their central purpose.

Myers and Hammill make a final appeal to a sense of fair play when dealing with unproven hypotheses. We are cautioned that "modern psycholinguistic theory too...consists of unproven hypotheses." Though we are not told why attention to the tautologous warning "all hypotheses are unproven" is important, the authors' "logic implies" that since one unproven hypothesis is no better or worse than another, supporters of the current favorite are simply being carried along by the popular tide, which is a notoriously fickle guide to a theory's worth. If critics of Osgoodian theory acknowledge their disaffection for it as the popular prejudice that it is, they will see that it is "unfair to conclude that the ITPA has no value because Osgood's conceptualizations are no longer popular." I take all of this as showing that besides being willing to accept the popularity metric, Myers' and Hammill's straw men are impressionable enough to be guided by it. If those concerned with the logical relation between the validity of Osgood's theory and the validity of the ITPA are on the wrong track, it is certainly not revealed by arguments such as these.

At least on the central interpretation, the relationship between the ITPA and the "unproven hypotheses" of Charles Osgood's theory of how language is acquired, understood and produced, is as follows: Osgood, who remarks that the psycholinguist "by definition is concerned with discovering and employing lawful relationships between events in messages and processes transpiring in the individuals who produce and receive them," (Osgood, 1959a: 35) has postulated that when humans produce and understand language, processes of a specific sort are involved. Kirk and McCarthy have devised a test on the assumption that the processes postulated by Osgood are those by which communication is achieved--a test which hopefully will tell us about the status of those processes. That is, if the processes are what Osgood and the test authors believe them to be, then hopefully this test will provide us with information about their operation in particular children. Finally, if both of these suppositions are true, (a) that Osgoodian type processes are in fact those which underly verbal communication, and (b) that the ITPA does provide information about them, it should

be possible to use that information to guide teaching practice. Such a rationale is clearly laden with conditionals, but is certainly not for that reason objectionable. The goal is to replace the hypotheses with fact--"the language processes are such and such, and this test is a valid measure of them." But we reasonably act with that theory having the greatest claim, given critical scrutiny, to being correct. Myers' and Hammill's egalitarianism, wherein one unproven theory is as good or bad as another, thus meriting (out of fairness) equal attention, is simply wrong.

The form of the above rationale is assuredly sound. If exception is to be taken to a particular test employing it, that exception must be directed toward the truth of the assumptions (a) and (b). In the case of the ITPA, I reject the truth of both. It should be clear that the truth of (a) is a necessary condition for the truth of (b), though not vice versa. To falsify the theory is to falsify the test as well.

But this logical consequence only obtains if the relationship between the ITPA and the Osgoodian processes postulated as responsible for language comprehension and production, is as we have thus far presented it. Doubt of prodigious magnitude hangs over this belief, necessitating the discussion of other interpretations of what the ITPA claims to be assessing. A distinguishing feature of the contentious literature surrounding the ITPA is that disagreement occurs not merely over what is actually being measured by the test, but over what it was intended to measure. This, as can be imagined, makes for a very debilitating state of affairs. For the present, I wish to develop the theme of the ITPA as a test designed to assess "underlying psychological processes." A great many potential and actual confusions exist over this description of the test alone.

Psychological Processes

The procedure of speculating on what processes occur when a person listens to spoken language, reads written language or produces language in either form himself might be thought of as belonging not to the psychologist but to the neurologist. Considerable confusion arises over the mere description of such processes as "psychological," "psycholinguistic," or "mental," and of their "underlying" overt behaviors. Is it being suggested that in addition to or parallel to the neurological processes which occur when we produce or listen to

speech there is another set occurring in the mind and it is these which the psychologist is speculating about? Certainly there was a time in the not distant past of psychology where this was not merely suggested but specifically declared to be the case. As recently as 1925 we find a passage such as this:

I have definitely assumed that the body and the mind are two distinct entities, which are now in a very intimate union, which I express by saying that the former is 'animated by' the latter... I have taken the body to be very much as common sense, enlightened by physical science but not by philosophical criticism, takes it to be; I have supposed that we know pretty well what a mind is... My conclusion is that, subject to the assumption just mentioned, no argument has been produced which should make any reasonable person doubt that mind acts on body in volition and that body acts on mind in sensation... Mental qualities are what I have called 'immaterial'..." (Broad, 1925: 131-2, 598.)

However the postulated psychological functions of Kirk and McCarthy are not at all of this order. Indeed Osgood, whose processes are those which the authors accepted, was a vigorous adversary of all manifestations of such dualism in psychology. With the same enthusiasm as that of Watson and Skinner, Osgood eschewed any psychological explanation which appealed to such mental constructs as ideas, beliefs, thoughts, desires, etc., when these were regarded, in the manner of Broad, as immaterial entities capable in ways unknown of affecting the material organism. Katz has dubbed such a viewpoint "theologized mentalism" and psychological explanations in such terms have been similarly condemned by Osgood as "mystic mentalism" which renders the explanation of human behavior in terms of causal laws an impossibility.

But if we jettison the view that there is an immaterial mental domain whose constituents of ideas, beliefs, meanings, etc., are the subject of inquiry for the psychological theorist, what is left for him to study? The traditional answer has been either behavior only or physiology only. The latter position, i.e., that all explanations of how persons think, understand language, and produce language must ultimately be in terms of physiological mechanisms would certainly suggest that the study of the "processes which underly communication activities" belongs to the neurologist, the brain physiologist, etc. It would appear that the processes being described as 'psycholinguistic' are simply the physiological processes involved in language comprehension and production. This apparent elimination of the psychologists' subject matter (addressed by Fodor, 1975, 1976; Putnam, 1973) is forestalled by adopting the commonly held functionalist view of

psychological explanation. This viewpoint is well characterized in the following passage from Fodor:

The sense in which terms referring to internal states are functionally characterized in theories developed in the first phase of psychological explanation may now be made clear. Phase one psychological theories characterize the internal states of organisms **only** in respect of the way they function in the production of behavior. In effect, the organism is thought of as a device for producing certain behavior given certain sensory stimulations. A phase one psychological explanation attempts to determine the internal states through which such a device must pass if it is to produce the behavior the organism produces on the occasions when the organism produces it. Since, at this stage, the properties of these states are determined by appeal to the assumption that they have whatever features are required to account for the organism's behavioral repertoire, it follows that what a phase one theory tells us about such states is what **role** they play in the production of behavior. It follows too that the evidence to be adduced in favour of the claim that such states exist is just that assuming they do is the simplest way of accounting for the behavioral capacities the organism is known to have. (Fodor, 1964: 173.)

On this understanding, the psychology of language is neither theologized mentalism nor fanciful neurology. The psychologist does not doubt that the operations by which language is learned, understood and produced are carried out organically and that ultimately an account of the physiological mechanisms operant in human communication will be forthcoming. But the psychologist is not doing physiology or biology or neurology. Acknowledging that neurological mechanisms will be responsible, the psychologist sets out to specify what function they must fulfill. His claims must be compatible with what is known about neurological mechanisms involved in language perception and production; for example, a psycholinguistic theory of speech perception could be falsified by the demonstration that the operations it required could not be carried out by the nervous system in the time elapsed between presentation of the auditory stimulus and the subject's awareness of meaning, a matter of milliseconds. Osgood was well aware of this constraint on psychological theorizing. But the point to be grasped is that although the psycholinguist postulates operations which are to be carried out by neurological mechanisms, his hypotheses do not concern the physiology of those mechanisms. While I am not altogether happy with the following passage from Katz, especially since he ascribes the task of theorizing about the processes which underly communication to the linguist rather than the psycholinguist, nevertheless, it does succeed in conveying the distinction being discussed:

Now it is clear that the linguist, though he claims that his theory describes a neurological mechanism, cannot immediately translate the theory into neurological terms, i.e., into talk about synapses, nerve fibers, and such. But--and this is the crucial point in showing that the mentalist is not a psycho-physical dualist--this failure to have a ready neurological translation means only that he cannot yet specify what kind of physical realization of his theoretical description is inside the speaker's head. Since linguistics and neurophysiology are independent fields, it does not matter for the linguist what kind of physical realization is there. For the purpose of linguistic investigation, it is immaterial whether the mechanism inside the speaker's head is in reality a network of electronic relays, a mechanical system of cardboard flip-flops and rubber bands, or, for that matter, a group of homunculi industriously at work in a tiny office. All of these possibilities, and others, are on a par for the linguist as physical realizations of this mechanism, so long as each is isomorphic to the representation of linguistic structure given by the theory of the language. (Katz, 1964: 129.)

One way of conceiving the task of the psycholinguist is that of constructing a machine that could simulate the human behavior of producing situationally appropriate utterances and recovering the meaning from spoken or written utterances. The task of the psycholinguist in such a program is not that of specifying the requisite hardware. It is that of specifying what operations the requisite hardware will have to carry out. The central reason why the actual technology of the machine processing of language is in such an infantile state is that what computations are unconsciously and automatically carried out by the human language user in speech perception are only dimly understood. The deficiency is not in the electronics but in knowing what is to be done. In other words, psycholinguistics is in its infancy. The simple point I wish to draw from all this is that it is not the goal of psycholinguistics to describe the wiring of the human machine that is involved in language processing, and that the only alternative is not that of fantasizing about immaterial entities and states. It should be clear that the psychologists need be neither dualist nor neurologist. The position that would consign him to one state or the other is what I have been concerned to undo. (For further discussion of the functionalist account of psychological explanation, see Katz, 1964; Harman, 1973; Putnam, 1973; and Fodor, 1964, 1974.)

In accord with all of this Osgood speculated on what must be "going on in the head" when, for example, one hears an instruction and follows it. He describes psycholinguistics (the term was apparently introduced into the vocabulary by him) as follows:

The rather new discipline coming to be known as psycholinguistics

(paralleling the closely related discipline termed ethnolinguistics) is concerned in the broadest sense with the relations between messages and the characteristics of human individuals who select and interpret them. In a narrower sense, psycholinguistics studies those processes whereby the intentions of speakers are transformed into signals in the culturally accepted code and whereby these signals are transformed into the interpretations of hearers. In other words, psycholinguistics deals directly with the processes of encoding and decoding as they relate states of messages to states of communicators. (Osgood and Sebeok, 1965: 4.)

In earlier papers Osgood not only speculated about what functions were necessary for such encoding and decoding to occur, but also about the neurological mechanism by which these functions were achieved. Thus in his earliest formulations he believed that the crucial process in his theory, i.e., the elicitation of the r_m , occurred somewhere in the peripheral nervous system. However he maintained from the beginning that it could well be a brain event, correctly noting that the locus of the process, i.e., the ultimate discovery of the neurological mechanism which carried out the postulated process, was immaterial to its theorizing. In keeping with this, Fodor's remark (1965) that "the psychologist needn't demonstrate where the processes occur" was accepted without question by Osgood. Given this fact, Roger Brown's rejection of the r_m postulate on the grounds that we wouldn't know where to "hook up our electrodes to find it" is quite beside the point. That shortcoming, given the current state of our knowledge of the brain, is common to all psychological speculation about the cognitive processes. To put this another way, what we presently know about the brain mechanisms involved in communication is at such a basic level as to put none but the most broad constraints on psychological theories of verbal communication. In any case, the point being made here is that Osgood's theorizing about "psycholinguistic processes" is not to be regarded as a neurological theory, nor is it about processes of an ethereal sort occurring in a pseudo-object called the mind or psyche. Rather, it consists of postulations concerning what functions must be occurring when language is understood and produced based on inferences derived from an analysis of the input and output of the human organism. In accord with all the foregoing, Kirk and McCarthy say that "The Illinois Test of Psycholinguistic Abilities does not make any assumptions with respect to neurological or neurophysiological correlates of behavior. Its emphasis is on assessing behavior manifestations in the psycholinguistic field, in relating the assets and deficits to a behavioral (not a neurological)

model, and in extending this type of behavior diagnosis to a remedial teaching situation." (Kirk and McCarthy, 1961: 411-12.)

A great number of characterizations have been offered of the processes believed to underly language acquisition and use. The theories are broadly divided into associationist and cognitivist categories, with Osgood belonging in the former group. Central to the associationist theory is the dictum that if the learning theoretic principles accounted for animal and non-verbal human learning and behavior, they must also explain language acquisition and use. Osgood explicitly took the position that children learn the meaning of words via conditioning, that learning a language was a matter of having responses to things become conditioned to the words that signify them. All of this is wholeheartedly accepted by the ITPA authors. (See McCarthy, 1974.) Miller has commented on this widely shared conception of language learning in terms of its being a "root metaphor":

Until about 1950 the metaphor accepted by most American students of the psychology of language was "association." For those who accept the association metaphor, the psychology of language is a special chapter in the psychology of learning. The nervous system is man's great connecting machine; learning is the process of establishing new connections; to learn a language is to learn connections between words and things. (Miller, 1974: 401.)

While on most issues of psychology of language I would think twice before questioning Miller, his dating of the reign of associationism "up to" rather than through the 1950's, would seem to me to be incorrect. The foundational papers of all the neo-behaviorists mentioned above occurred in the 1950's and '60's. Miller's own classic paper directed toward weaning the associationists away from some of their follies appeared in 1962 (*American Psychologist*) and would hardly have been required had the paradigm faded away twelve years earlier. No great weight attaches to this point. I have mentioned it only to avoid misportraying Kirk and McCarthy's adoption of Osgoodian theory, circa 1957, as the adoption of an outdated theory. The neobehaviorist paradigm was at that time current and widely held, with Osgood being a foremost member of what was then the mainstream. The ink was not yet dry on Chomsky's Syntactic Structures.

But such historical data aside, it is surely within the framework of associationist psychology of language that the ITPA must be understood. Associationist and cognitive psychologists alike have sought to determine what processes take place during human communication. Kirk and McCarthy sought to devise a series of different tasks

from which discrete inferences could be made about the status of these underlying processes:

These discrete tests have been constructed to differentiate defects in (a) the three processes of communication, (b) the levels of language organization, and/or (c) the channels of language input and output. Poor performance on specific subtests of this battery should therefore indicate the existence of psycholinguistic defects. (Kirk and McCarthy, 1971: 405.)

Before any such differential diagnosis could be made, it was, of course, necessary to have an understanding of what the language processes were. Kirk and McCarthy pinned their hopes on Osgood in this endeavor and, as revealed by a paper presented by McCarthy almost twenty years after work on the test began, their hopes remain pinned there. (McCarthy, 1974)

II. THE ILLINOIS TEST OF PSYCHOLINGUISTIC ABILITIES

Samuel Kirk, the principal co-author of the ITPA, spent a considerable amount of his time in the 1930's and '40's working with mentally retarded children. His experience with and research into the teaching of such children (Kirk established in 1949 what has been described as "the first experimental nursery school" (Hallahan and Cruickshank, 1973: 107) for mentally retarded children) led to the publication of a number of articles and books on the subject. More importantly from the point of view of this paper, dissatisfaction with the educational evaluation of mentally retarded children, as Kirk himself relates, led to his decision to devise a diagnostic test of children's communication abilities:

A quarter of a century ago, the senior author of the Illinois Test of Psycholinguistic Abilities was attempting to increase the rate of mental development of young disadvantaged mentally retarded children. In this work it was noticed that many children who were labeled mentally retarded displayed wide discrepancies among their abilities. Although some of these discrepant abilities were spotted by informal methods (and often the disabilities were amenable to remedial procedures), the need was felt for a systematic, diagnostic device which would tap and differentiate various facets of cognitive ability. The theoretical basis for such a measure grew out of Osgood's (1957A, 1957B) principles concerning the communication process. In 1961 the early form of the ITPA was developed and published in an experimental edition as a diagnostic test of communication abilities. (Kirk, McCarthy, Kirk, 1968: 5)

As the passage reveals, it was Kirk's belief that many children diagnosed as mentally retarded did not possess the largely irreversible, global deficits in intellectual and social functioning which are definitive of a mentally retarded child; rather, he believed that the children may have had specific and remediable difficulties in understanding, expressing, or thinking in language. The hope was that if these difficulties could be formally diagnosed, specific remedial teaching could be undertaken (Kirk and Kirk, 1971: 15-16). Kirk cites a number of examples of children believed to be mentally retarded who upon closer diagnosis using the ITPA were putatively diagnosed as having a specific problem in the processing of language. It is claimed that in some cases remedial practices based on the ITPA diagnosis led to improvements resulting in the reversal of the original

"mentally retarded" classification (Kirk and McCarthy, 1961: 406-7; Paraskevopoulos and Kirk, 1969: 7-8, Case C).

But while the test origins are significantly related to Kirk's work with mentally retarded children, the ITPA was certainly not intended to be used, nor is it used most commonly with children presumed to be mentally retarded. On the contrary, the test is administered primarily to school children whose academic performance or communicative skills (the latter especially in the case of children below school age) deviated from what might be reasonably expected, given their normal levels of intelligence and freedom from sensory and neurological impairment. Kirk discusses a child fitting this description:

Case 2, M.W., was referred for examination because of the inability of the school to understand his lack of progress. M.W. entered school at the age of six. He made no progress in school because of his apparent inability to understand the teacher. It was believed that he had a severe hearing loss and he was placed in a class for hard-of-hearing children. After a year in this class, it was discovered through his speech and audiometric tests that he did not have a hearing loss. He was returned to the regular grades and remained in the second grade until the age of nine. At this time, the teacher reported that he was unable to learn and that he seemed unable to understand directions.

Intelligence tests resulted in an IQ of 66 on the WISC Verbal and 73 on the Binet, although he was within the normal range on performance tests. On the basis of his lack of academic progress in class and the psychometric tests, he was placed in a class for the educable mentally retarded.

At age 10-1, the boy was again examined with various psychometric tests including the test battery of the ITPA....

On the profile of Psycholinguistic Abilities, the assets and deficits of this boy appear in a clearer focus. He scored above the norms on visual decoding at the representational level and was relatively superior in both vocal and motor encoding....

The profile, however, shows the various deficits in the boy and helps to explain why he was unable to respond to the instructions in the classroom....

The assets of this boy, together with the deficits shown in the profile, now give us clues to a training program which was not forthcoming from the series of verbal and performance psychometric tests given previously. Programmed instruction for this boy can follow a pattern of instruction which will utilize the assets to develop the deficits. (Kirk and McCarthy, 1961: 407-8)

M.W. certainly typifies the majority of children who are referred for testing on the ITPA. As already noted, these are generally children

of normal intelligence with no known sensory or neurological deficits who are encountering difficulties in such academic activities as reading, spelling, writing or arithmetic, or in such communication activities as following directions or expressing ideas. The central concern of the ITPA is to determine whether these difficulties are related to some deficit in the psychological processes believed to underly language comprehension and production. It could be, for example, that the child who fails to follow spoken directions correctly does so because of inattention or poor hearing or even deliberate resistance. On the other hand, the child may have perfect hearing and yet fail to comprehend spoken language (the condition known as developmental receptive aphasia) or he may have some deficit in short-term auditory memory which prevents him from storing the information long enough to act upon it. It is such conditions as the last two which the ITPA seeks to diagnose. Kirk writes:

When a child can hear but cannot understand the meaning of the spoken word, or is delayed or retarded in understanding the spoken word, he is known to have an auditory receptive (symbolic or representational) disability. This has been called sensory or receptive aphasia. Likewise, when the child is unable to attach meaning to what he sees, or what has been presented to him visually, it is said that he has a visual receptive disorder. In the ITPA, to be described later, these have been referred to as a deficit in auditory or visual decoding, or the ability or inability to understand what the child hears or sees.

When a child has difficulty in expressing ideas vocally or manually, the child is known to have a symbolic or representational expressive disability. This disability has been termed in adults, expressive or motor aphasia. (Kirk, 1968: 401; see also Kirk and Kirk, 1971: 9)

It is critically important to understand the type of diagnosis which the ITPA purports to make. Not all learning or communication problems can be traced to deficits in psychological processes related to the comprehension and production of language. On the contrary, most would not be so traced, but would find as their source such factors as emotion, attention, motivation, states of knowledge and belief, sensory impairments, etc. The purpose of the ITPA is specifically to determine whether the observed shortcoming can be traced to what the ITPA co-authors variously describe as "the habits necessary for language usage," "the psycholinguistic processes," (Kirk, 1968:407) "psycholinguistic functions," (Paraskevopoulos and Kirk, 1969: 23) "psychological functions which operate in communication activities," (McCarthy and Olson, 1964: 23, 36) and "language processes (which) constitute learned abilities necessary for language usage." (Kirk and

McCarthy, 1961 : 403). Thus, the claim above that an auditory decoding deficit as diagnosed by the ITPA is the same condition as that referred to as receptive aphasia must not be underestimated. For excluded (as the source of the observed shortcoming) by such a diagnosis are such factors as poor attention, motivation, lack of necessary knowledge, etc. To diagnose a child like M.W., for example, who was failing to follow instructions, as having an auditory decoding deficit is to exclude his not knowing the words of the instructions as the cause of this shortcoming. To learn that M.W. fails to follow directions given in words which he does not know is to acquire a most unexceptional piece of information. Having a limited vocabulary is not to be confused with having receptive aphasia. The diagnosis of M.W. as having an auditory decoding deficit is a diagnosis not that he fails to understand words that he doesn't know, but that he fails to understand spoken utterances of words that he does know, this failure being due to a deficit in one of the psycholinguistic processes. The crucial point, i.e., that the observed shortcomings are to be traced to malfunctions in psychological processes, is brought out in this passage from Kirk concerning a reading problem:

Reading disability. This condition is not uncommon in school children but is also confused with other forms of reading failure. Some children, because of environmental or instructional factors, are retarded in reading but show nothing abnormal within themselves. Such retardation is most often amenable to corrective reading, since the child is developing normally in psychological abilities but requires developmental and correctional forms of instruction in a classroom. The child with a true reading disability is one who is diagnosed as having a deficit in the development of psychological characteristics basic to the acquisition of academic skills. A vast number of labels have been used to describe such conditions, including such terms as word blindness, strephosymbolia, congenital alexia, dyslexia, congenital symbolamblyopia, bradylexia, specific reading disability, amnesia visualis, and other terms. In describing children with such deficits, Marion Monroe (1932) used a behavioral or educational term as the title of her book, Children Who Cannot Read. The present writers prefer the latter term with the addition, "because of psychological developmental deficits." (Kirk and Kirk, 1971: 8)

One of the most persistently advanced notions in the ITPA literature is that of the "diagnostic test." Kirk repeatedly contrasts the ITPA with tests which are used primarily to classify children. Such tests as the Stanford-Binet, Wechsler, and achievement tests in reading, arithmetic, spelling, etc., yield scores which indicate the level of a child's ability relative to other children on some common dimension. But while such tests will reveal whether or not a child is below average in reading, spelling, etc., and thus give a general

indication that special attention is required, they do not provide information pertaining to the source of the observed deficit and thus cannot be used to aid in the determination of specific remedial programs. In accepting the authors' definition of a diagnostic test as one which "assesses specific abilities, disabilities, and achievements of a child in such a way that remediation of deficits can logically follow," (Kirk and Kirk, 1971: 10-11) we must also agree that I.Q. tests, reading achievement tests, etc., are not diagnostic tests in this sense. Quoting Kirk:

The purpose of the present report is to submit a procedure for diagnosis--a scheme which extends beyond classification of the Binet or Wechsler type test into an assessment which will suggest the areas needing remediation. This area is not new, since clinical workers have attempted to appraise acquired or developmental defects such as the aphasia, apraxias, agnosias, agraphias, and dyslexias. These appraisals have usually been made by informal diagnostic methods with some assistance from psychometric tests. In the field of reading disabilities, diagnostic instruments leading to remediation have been developed. In the intellectual field, evaluating the primary mental abilities can be considered an attempt at differential diagnosis. The present approach is an attempt at diagnosis in the psycholinguistic field. (Kirk and McCarthy: 1961: 399.)

The Revised Edition, as well as the original ITPA, was conceived as a diagnostic rather than a classificatory tool. Its object is to delineate specific abilities and disabilities in children in order that remediation may be undertaken when needed....The ITPA bears the same relation to the field of communication and learning disorders that diagnostic reading tests bear to the field of reading. A diagnostic reading test differs from a general reading test insofar as it delineates areas of difficulty in reading rather than merely determining the level of overall reading ability. Similarly, the ITPA is used to delineate areas of difficulty in communication more than to determine overall ability. (Kirk, McCarthy, Kirk, 1968: 5)

The authors' emphatic portrayal of the ITPA as a diagnostic instrument appears to run contrary to another declared feature of the test which the authors stress, i.e., that it does not provide information about the etiology of observed deficiencies. The apparent contradiction is clearly brought out by contrasting the statements of A and B with those of C and D:

- A) Diagnostic tests attempt to identify specifically the various disabilities or faulty habits used in the acquisition of academic skills of reading, writing, spelling, and arithmetic and in the various psychological functions involved in the processes of thinking, listening, talking, and perceiving.... The dissatisfaction with classification instruments has led to the recent development of tests for specific functions that give clues to remediation. The Illinois Test of Psycholinguistic Abilities (ITPA), among others, represents an effort along these lines. (Kirk and Kirk, 1971: 11)

- B) Such an assessment (as the ITPA) is diagnostic rather than classificatory, since it pinpoints underlying areas of deficiency basic to the observable problem. (Kirk and Kirk, 1971: 12)
- C) In education a child who has the basic potential to learn, but does not learn after adequate instruction, is probably a child with a learning disability. The knowledge of the etiology of the disability in most instances is not helpful to the organization of remedial procedures. (Kirk and Kirk, 1971: 15)
- D) Success in educational diagnosis cannot depend upon the determination of etiology, because in children with learning disabilities etiology is usually presumed. While presumption of etiology does not prevent an adequate diagnosis of educational problems, it complicates matters by requiring the treatment of symptoms. In stuttering, for example, where etiology is usually presumed, the treatment of symptoms is routinely undertaken. Thus, though the theoretical causes of stuttering differ widely, the course of treatment is largely the same regardless of theoretical orientation. (McCarthy and McCarthy, 1969: 17)

These remarks certainly appear contradictory, and I would maintain that McCarthy's comments (D) are, in addition, obscure. The considerable puzzlement generated by the apparent incompatibility of the claim that the test is diagnostic yet does not provide information about the causes of the observed learning or communication difficulties is only resolved when one comes to understand that the authors use the term 'etiology' only in reference to physical causation. Given this restricted sense, they would not, for example, regard the discovery that a child of six who was well behind in language development lived in a home where both parents were mute as information concerning the etiology of her condition. For what has been discovered here is an environmental, not a physical cause of her deficiency. Given this restricted sense of 'etiology,' it is clear that the ITPA does not reveal the etiology of observed deficiencies in understanding language, speaking, reading, etc. For the ITPA does not, and Kirk and McCarthy stress that it does not, provide information concerning sensory or neurological deficits. Nevertheless, the test remains a diagnostic test since, in the sense the authors operate with, that description is applicable to any test which provides specific information leading to specific remedial practices. But certainly in the customary sense of 'etiology,' in which the term is not restricted to physical causation in its application, the ITPA in providing information about short-term memory, the psychological processes involved in the comprehension of spoken language, etc., is most certainly etiological (Mann, 1971). Awareness that this customary use is not that employed by the authors, but that the restricted sense is operant, also enables the reader to

cope with Kirk's upending claim that awareness of etiology is in general of little help in remediation. 'Awareness of etiology,' of course, is read 'awareness of physical causation.' Quoting Kirk:

While the medical specialist is concerned primarily with etiology and with the relationship between communication disorders and the location of a possible cerebral dysfunction in children, the special educator is concerned primarily with the assessment of the behavioral symptoms and with special methods of ameliorating the disability. The etiology of the disability, in most instances, is not helpful to the organization of remedial procedures. Whether a child is labeled as brain-injured or not (usually inferred from behavior) does not, with the exception of rare instances, alter the remedial procedure. (Kirk, 1968: 402)

As indicated before, the term "brain injury" has little meaning to me from a management of training point of view. It does not tell me whether the child is smart or dull, hyperactive or under-active. It does not give me any clues to management of training. The terms cerebral palsy, brain injured, mentally retarded, aphasic, etc., are actually classification terms. In a sense they are not diagnostic, if by diagnostic we mean an assessment of a child in such a way that leads to some form of treatment, management, or remediation. (Kirk, 1974: 77)

The ITPA purportedly functions as a diagnostic instrument, therefore, not by revealing the physiological causes of observed shortcomings, inferences to which are often made by analyzing the character of overt behavior; but rather, by analyzing the structured samplings of behavior required on the test and making inferences about psychological processes believed to be manifested by them. In somewhat the same sense that a golfer's poor abilities to hit iron shots can be diagnosed by analyzing the golf swing into such components as grip, stance, backswing, wrist action, etc., thus identifying particular weaknesses and leading to particular remedies, the ITPA is designed to analyze discrete components of what McCarthy calls "the complete language act." By this is meant that a separate assessment is made of whether a child has difficulty in simply comprehending language, or in thinking in language, or in expressing himself--whether any of these are specific to just spoken or written language, whether there is a problem in auditory short-term memory or visual short-term memory, etc. Importantly unlike the diagnosis of the golf swing, however, the behavior required on the ITPA is not a sample of the very behavior (performance) about which inferences are going to be made. On the contrary, the overt behavior produced by subjects on the ITPA is taken as a sign of non-overt processes believed responsible for it, and it is these processes about which inferences are to be made. On the Auditory-Vocal Association subtest, for a straightforward example, the behavior

observed is speech, but the inference does not concern the child's speech, but the child's thought processes. The behaviors required of the subject on the test are taken as indicators of "the psychological functions which operate in communication activities." (Kirk and Kirk, 1971: 21) Hence, these behaviors are in some places described as "behavioral symptoms," or "behavioral manifestations": "While the medical specialist is concerned primarily with etiology and with the relationship between communication disorders and the location of a possible cerebral dysfunction in children, the special educator is concerned primarily with the assessment of the behavioral symptoms and with special methods of ameliorating the disability." (Kirk, 1968: 402; see also Kirk and McCarthy, 1961: 411; Kirk, 1974: 77.)

I would suggest that since the behaviors elicited on the ITPA are taken as symptomatic of unobserved processes, and not as representative samples of some domain of overt behavior about which inferences are to be made, that the comparison of the ITPA with diagnostic reading tests may be very misleading. For many diagnostic reading tests, like the golf swing diagnosis, are analyses of performance in which the child's reading aloud is scrutinized for such component errors as reversals, substitutions, consonant errors, etc.--and the ITPA is not like this. Such tests are frequently and revealingly described as inventories. Ruth Strang, a reading specialist, supports this understanding of the situation:

Diagnosis of reading disabilities may be made on different levels of comprehensiveness, psychological depth, and competence (Strang, 1964a, pp. 3-23). On the surface level, the effort is made to describe reading performance--strengths and weaknesses in vocabulary, word recognition, sentence and paragraph comprehension, and related abilities....A third level of diagnosis attempts to analyze the student's reading process rather than merely to describe his reading performance. This may be done systematically in the Illinois Test of Psycholinguistic Abilities (ITPA). (Strang, 1968: 4-5.)

The same appraisal is implicit in Alex Bannatyne's division of the diagnosis of reading problems into diagnosis "on the surface and manifest levels" and diagnosis on any of his proposed "supporting levels which are less obvious." It is at Bannatyne's first underlying level, which he describes as the "cognitive and sensori-motor ability level" that the ITPA is placed (Bannatyne, 1972: 133-4). In a similar vein, Wedell, in an article on diagnosing learning disabilities, cites the ITPA as fitting in at the level concerned with the "analysis of component skills and processes." (Wedell, 1972: 204; see also Frostig and

Maslow, 1973: 125-32.) I regard all of these writers as observing a distinction which is certainly worthy of attention, that between using overt performance to make further inferences about that same type of overt performance in situations beyond the test situation, and using the overt performance to make inferences about processes presumed to underlie it. I believe that it is this distinction or something similar that Kirk is discussing in this passage:

It should also be pointed out that functional analysis of behavior plays a unique role in evaluating and modifying aberrations in social behavior while discrete diagnostic tests are used primarily in the analysis of linguistic, cognitive, and perceptual abilities. If a child bites his nails or sucks his thumb, cognitive tests are of little value. On the other hand, if a child is unable to learn to read, it is necessary to find the correlates of his inability to learn which may be deficiencies in auditory closure, sound blending, visual and auditory short-term memory, or other functions. These deficits are not readily observed, since the observation relates to the end result--that is, he cannot read. The most effective approach to diagnostic analysis is through tests to pinpoint the specific areas which can then be subjected to functional analysis if needed. (Kirk and Kirk, 1971: 62)

Another distinction which Kirk develops in providing a general characterization of the ITPA is that between interindividual and intra-individual differences, the latter referring to "differences of ability within a single child." The determination of interindividual differences, according to Kirk,

...has been found administratively helpful but not educationally productive. A statement that a child has a low I.Q. or is at the 25th percentile in his reading class does not necessarily lead to educationally relevant hypotheses for remediation....(But) the concept of intraindividual differences led logically to psychometric tests that could measure a number of specific and discrete areas of psychoeducational development. These areas could then be compared to determine discrepancies in growth and developmental imbalances in the child himself. (Paraskevopoulos and Kirk, 1969: 6)

The sense in which the ITPA may be regarded as diagnostic should, I hope, be achieving some clarity. Confronted with many children who are encountering difficulty in learning to read, write, and spell, or who have apparent difficulties in comprehending speech or expressing their ideas vocally, the educator naturally sets out to determine the source of the difficulty. There may well be physical factors, there may well be environmental factors; but it may also be that in conjunction with or in the absence of such factors there is some malfunctioning in one or more of the psychological processes relating to language use. It is factors of this latter variety, which Kirk calls the "psychological correlates" of learning problems (as opposed to the environmental and physical), that the ITPA purports to make manifest.

Children whose difficulties are primarily due to deficits in such psychological functions are generally described as children with "learning disabilities" (or "specific learning disabilities"); and indeed it was Kirk himself who introduced this term and its corresponding concept to education. The introduction of this new criterion for the differentiation of children with learning problems has been of enormous influence in special education and, for better or worse, will surely be recorded as one of the most significant events in the history of this field of endeavor. The following commentary by Gearhart is not atypical:

During the 1950's a special educator of international renown started investigative efforts that played a major role in the recognition of learning disabilities as a subarea of special education. Dr. Samuel Kirk, known for his work with the mentally retarded and for a variety of efforts on behalf of all handicapped children, became involved in the development of a new type of diagnostic tool (the ITPA)....Kirk's prominence as a special educator plus his interest in the ITPA undoubtedly played a role in his involvement in a conference convened by the Fund for Perceptually Handicapped Children, Inc., on April 6, 1963. In a speech at this conference... Kirk noted that he had recently been using the term "learning disabilities" to describe children who had disorders in language, speech, or reading or associated communication problems but two (sic) did not have sensory handicaps such as blindness or deafness. He also indicated that he did not include within this group those children who exhibited generalized mental retardation (Wiederholt, 1974)....(T)he next day they (the conferees) organized the Association for Children with Learning Disabilities (ACLD)....The field of learning disabilities may thus be considered to have been officially born on April 7, 1963. (Gearheart, 1976: 3-4.)

Kirk was also a member of the National Advisory Council on Handicapped Children, which in 1968 proposed what has become the most widely accepted definition of a child with a learning disability:

Children with special learning disabilities exhibit a disorder in one or more of the basic psychological processes involved in understanding or in using spoken or written languages. These may be manifested in disorders of listening, thinking, talking, reading, writing, spelling, or arithmetic. They include conditions which have been referred to as perceptual handicaps, brain injury, minimal brain dysfunction, dyslexia, developmental aphasia, etc. They do not include learning problems which are due primarily to visual, hearing or motor handicaps, to mental retardation, emotional disturbance or to environmental disadvantage. (Myers and Hammill, 1976: 3-4)

The element of this definition to be singled out for attention is that it is children whose learning problems are not due primarily to physical handicap, mental retardation, etc., but to disorders in "one or more of the basic psychological processes" that have a learning disability. It is possible for a child to have a learning disability and have any

of the other handicaps, provided that these are not the primary cause of the problem in learning or communication. Accordingly, the ITPA is not only used with children who are having learning problems in the regular classroom, but also with children of every handicap. Indeed, when it was first introduced, the authors described it as "a diagnostic instrument which leads to clues for remediation of deficits in various psycholinguistic functions found particularly among cerebral-palsied, brain-injured, and some emotionally disturbed children." (Kirk and McCarthy, 1961: 411; consult also Kirk and Kirk, 1971: 5 and Myers and Hammill, 1976: 8-9, for discussion of learning disabilities in relation to other handicaps.) But given that it is some malfunction in one of the psychological processes, usually described as 'underlying' such activities as speech, reading or writing that is partially definitive of a learning disability (a necessary condition) then it is readily apparent that the ITPA is designed for and is, in fact, extensively used for the diagnosis of learning disabilities. Quoting Kirk:

The concept of learning disability as used in education does not deny or reject a neurological deficit (acquired, genetic, or otherwise) but neither does it depend on a neurological determination. The major emphasis is on the use of psychological tests and/or observation for the purpose of organizing a remedial educational program. Such a program is rarely dependent upon a neurological or biological diagnosis but is very dependent upon the determination of psychological abilities and disabilities....

The major purpose of developing the ITPA was to be able to discover psychological correlates of different learning disabilities.... A child who is unable to learn words may have a sound blending disability. This is considered a correlate of inability to learn to read, and leads to a remedial program which will include training the child's sound blending in relation to teaching him to read. (Kirk and Kirk, 1971: 12-13, 57)

The general character of the ITPA as a diagnostic test has, I hope, emerged clearly from this discussion. It is quite a complex test, and difficult to characterize, if for no other reason than that such a test "had little specific precedent in the psychometric literature." (McCarthy and Olson, 1964: v) If uncertainties remain, they may be alleviated when the individual subtests are discussed. The understanding at this point is that the ITPA purports to provide information concerning the status of psycholinguistic processes believed to underly language production and reception. At least on the central interpretation of the test, these processes under assessment are taken to be those postulated by Charles Osgood in his neobehaviorist theory of communication. As the authors themselves claim:

The development of a comprehensive test had to wait upon the development of a comprehensive psychological theory of language acquisition and use. In 1952, Professor C. E. Osgood of the University of Illinois, produced such a theory. With his assistance, a listing and definition of all essential psycholinguistic abilities was made and tests were constructed to assess them. (McCarthy and Kirk, 1961: vi)

In an attempt to formulate a hypothetical construct which would give us a systematic approach to the behavioral study of mental retardation, heavy reliance was placed on the theoretical formulations of Charles Osgood (1963), who has developed models of the communication process through the extension of Hull's learning theory and his hypothesized mediation process. (Kirk, 1967: 188)

In other passages and more frequently the authors cite not Osgood's theory as the basis of the ITPA, but Osgood's 'model' (Kirk, McCarthy, Kirk, 1968), the latter term occasionally and even more obscurely being replaced by "the hypothetical construct" (Kirk and Kirk, 1971: 19-20). This vagueness, it will be seen shortly, has been far from harmless. But for the moment, we take the message as a straightforward one. Osgood has postulated "psychological functions of the individual which operate in communication activities" (Paraskevopoulos and Kirk, 1969: 11; Kirk, McCarthy, Kirk, 1968: 6) and "the Illinois Test of Psycholinguistic Abilities was originally conceived as a diagnostic intraindividual test of psychological and linguistic functions." (Paraskevopoulos and Kirk, 1969: 6)

There remains only the task of presenting the test itself--and this will be done in the authors' own words. For while I am willing to accept the complexity and novel character of the ITPA as partial determinants of the difficulty involved in understanding it, they are not to my mind the most important factors. Principally responsible for this difficulty, I would argue, is the amorphous character of the authors' description of the test. I maintain that vagueness, not complexity, presents the greatest obstacle to understanding the ITPA, and that it is by virtue of this same indefinite and metaphorical description that the test has been protected from effective criticism. It is genuinely difficult to identify just what is under assessment on the ITPA and it is to that extent difficult to assess the test itself.

A concern both for showing fairness to the authors and for giving credence to the foregoing appraisal obliges me to let the authors speak for themselves. Hence, the quite lengthy quoted passage which follows. It is taken from the 1968 Test Manual, the one piece of ITPA literature which is certain to have been read by all involved in administering, interpreting or evaluating the test. The passage occurs verbatim or is closely paraphrased in most other publications by the test authors.

There is no question of its being the major and most influential statement produced by the authors to characterize their test:

The psycholinguistic model on which the ITPA is based attempts to relate those functions whereby the intentions of one individual are transmitted (verbally or nonverbally) to another individual, and, reciprocally, functions whereby the environment or the intentions of another individual are received and interpreted. It attempts to interrelate the processes which take place, for example, when one person receives a message, interprets it, or becomes the source of a new signal to be transmitted. It deals with the psychological functions of the individual which operate in communication activities.

The adoption of this theoretical model for the battery served two purposes: (a) it was a parsimonious device by which the essential features of communication were delineated so that their relationships were specified; (b) it provided a framework within which to observe and evaluate a child, making it possible to verify and elaborate on test results and to suggest remedial measures.

As indicated in an earlier publication (Kirk and McCarthy, 1961), the clinical model of the ITPA is an adaptation of the communications model of Osgood (1957A, 1957B). Clinical observation and the practical problems of test construction necessitated some alterations in the theoretical model to give greater applicability to the field of education and particularly remedial education.

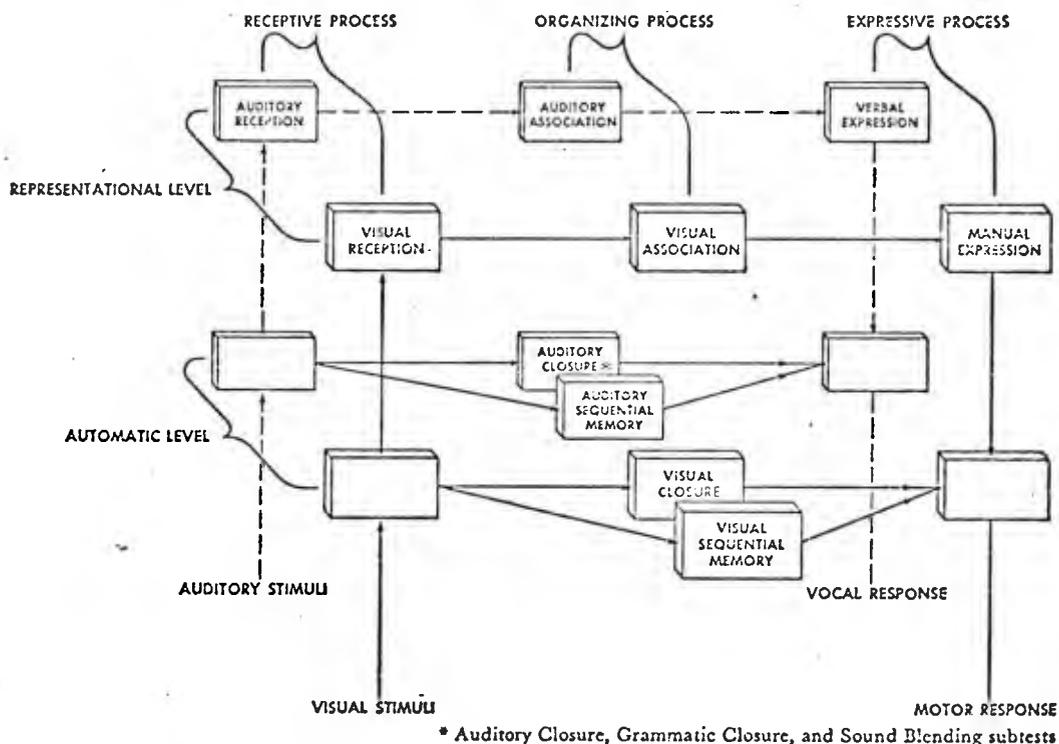
The present model, which is diagrammed in Figure 1, postulates three dimensions of cognitive abilities:

1. Channels of Communication. These are the routes through which the content of communication flows. Included here are the modalities through which sense impressions are received and the forms of expression through which a response is made. The channels may include various combinations of sensory input and response output. The major modes of input are auditory and visual; those of output are vocal and motor. Complete channels involving these modes of input and output would be auditory-vocal, auditory-motor, visual-motor, and visual-vocal. Theoretically, many channels are possible. Helen Keller, for example, used tactile-motor and tactile-verbal channels. Due to practical limitations, the ITPA incorporates only the auditory-vocal and the visual-motor channels. These channels were selected as being most relevant for the developmental level of subjects in the test's age range.
2. Psycholinguistic Processes. In analyzing behavior which occurs in the acquisition and use of language, three main processes are considered: (a) the receptive process, that is, that ability necessary to recognize and/or understand what is seen or heard; (b) the expressive process, that is, those skills necessary to express ideas or to respond either vocally or by gesture or movement; (c) an organizing process which involves the internal manipulation of percepts, concepts, and linguistic symbols. It is a central mediating process elicited by the receptive process and preceding the expressive process.
3. Levels of Organization. The degree to which habits of communication are organized within the individual determines the level of functioning. Two levels are postulated in the clinical model of the ITPA: (a) the representational level, which requires the more complex mediating process of utilizing symbols which carry the meaning of an

object; (b) the automatic level, in which the individual's habits of functioning are less voluntary but highly organized and integrated. The automatic chain of responses of the latter level is involved in such activities as visual and auditory closure, speed of perception, ability to reproduce a sequence seen or heard, rote learning, synthesizing isolated sounds into a word, and utilizing the redundancies of experience.

[These three dimensions -- process, level, and channel -- serve to define the psycholinguistic abilities tapped by the ITPA. A psycholinguistic ability is defined as a specific process at a specific level via a specific channel.] (These bracketed sentences not in 1968 Test Manual. Added in Kirk, 1969: 14)

Figure 1. Three-Dimensional Model of the Illinois Test of Psycholinguistic Abilities



The model described above and presented graphically in Figure 1 has been used to generate ten discrete tests and two supplementary tests for the purpose of assessing specific abilities and disabilities in young children. Wide discrepancies among these abilities and disabilities help to identify the child with a learning disability and help to delineate the areas requiring remediation. (Kirk, McCarthy, Kirk, 1968: 6-9)

It would be unwieldy to include the lengthy description of the individual subtests which follows at this point in the Test Manual. It is also unnecessary since most of them are treated in detail later. The summary description which follows is from another source:

The subtests of the ITPA which were generated from this model tap discrete functions which are incorporated in the three dimensions just discussed. Each utilizes a channel, a level, and a process. It will be noted from the model that the following functions, each of which is tested by a separate subtest of the ITPA, occur at discrete intersections of the three dimensions described above.

1. Auditory Reception (the ability to understand auditory symbols such as verbal discourse) is represented at the intersection of the receptive process, the auditory-vocal channel, and the representational level.
2. Visual Reception (the ability to gain meaning from visual symbols) is represented at the intersection of the receptive process, the visual-motor channel, and the representational level.
3. Auditory Association (the ability to relate concepts presented orally) is represented at the intersection of the organizing process, the auditory-vocal channel, and the representational level.
4. Visual Association (the ability to relate concepts presented visually) is represented at the intersection of the organizing process, the visual-motor channel, and the representational level.
5. Verbal Expression (the ability to express concepts verbally, i.e., vocally) is represented at the intersection of the expressive process, the auditory-vocal channel, and the representational level.
6. Manual Expression (the ability to express ideas manually) is represented at the intersection of the expressive process, the visual-motor channel, and the representational level.
7. Grammatical Closure (the ability to make use of the redundancies of oral language in acquiring automatic habits for handling syntax and grammatical inflections) is represented at the intersection of the organizing process, the auditory-vocal channel, and the automatic level. The two supplementary tests of Auditory Closure and Sound Blending also fall at this intersection.
8. Visual Closure (the ability to identify a common object from an incomplete visual presentation) is represented at the intersection of the organizing process, the visual-motor channel, and the automatic level.
9. Auditory Sequential Memory (the ability to reproduce from memory sequences of digits of increasing length) is represented at the intersection of the organizing process, the auditory-vocal channel, and the automatic level.
10. Visual Sequential Memory (the ability to reproduce sequences of nonmeaningful figures from memory) is represented at the intersection of the organizing process, the visual-motor channel, and the automatic level.

(Kirk and Kirk, 1971: 23-4)

III. INTERPRETATIONS OF THE ITPA

Starting from the firm ground that on all tests inferences are made about the subject on the basis of behavior produced by him in response to the test items, it is possible to distinguish with appreciable clarity various inferences which users of the ITPA take to be the intended inferences. What is at issue at present, I must emphasize, is not what inferences can be made on the basis of the subject's test performance on the ITPA--the important issue of whether the intended inferences are warranted--but the prior determination of what inferences were indeed intended by the test constructors. What is the test supposed to be measuring? The original ITPA literature gives a very mixed reply to this question.

Interpretation A

From the subject's test performance, inferences are to be made concerning the status of the processes postulated by Osgood as underlying language production and reception. This interpretation, addressed in the introductory section, would appear to be the primary interpretation of the test. It is fostered by such facts as that (a) 'decoding', 'encoding', and 'association' are technical terms in Osgoodian theory which label different associative processes involving Osgood's central construct, the representational mediation process (r_m), which he proposed to explain language comprehension, reception and thinking; (b) the ITPA subtests were originally called the Decoding, Encoding and Association subtests; and (c) the oft-cited basis of the ITPA upon Osgood's theory. But the belief that the ITPA was designed to assess the Osgoodian processes, which I will from now on refer to as OPI (Osgoodian Process Interpretation) of the ITPA is not dependent upon the assemblage of such circumstantial evidence. It receives explicit grounding in passages such as these:

The development of a diagnostic test had to wait upon the development of a psychological theory of language acquisition and use. In 1952, Professor C. E. Osgood, of the University of Illinois, produced a model for the communication process based on an extension of Hull's learning theory. With his assistance, a listing and definition of essential psycholinguistic abilities was made and tests were constructed to assess them. (McCarthy and Kirk, 1963: v.)

Within recent years there has evolved an increased interest among psycholinguists in linguistics and communication theory. By extending and elaborating Hull's (1943) formulations, Osgood (1957A, 1957B) has furnished a plausible psycholinguistic model from which a diagnostic test could be constructed as a necessary prelude to the designing of remedial programs. (McCarthy and Kirk, 1961: 2)

Further grounds for the OPI are provided by the authors' rare but revealing discussions of language processing. Osgoodian theory, as noted, is neobehaviorist wherein s-r associations (habits) involving stimuli and implicit responses (r_m 's) are postulated as the mechanism by which language is understood and produced (see Osgood, 1957B: 356, and elsewhere). Consistent with the Osgoodian account, McCarthy discusses the psycholinguistic processes of Decoding, Encoding and Association as follows:

The second major dimension of language is "processes." In the parlance of the behaviorist, a process is a habit, something that is learned. We can identify three families of processes that are associated with language: decoding or reception, encoding or expression, and association or inner language....Thus, decoding is that collection of habits required to ultimately obtain meaning from linguistic stimuli; encoding is that collection of habits required to ultimately express oneself in common words or gestures....

So, in sum, through appropriate conditioning practices, we acquire habits that are modified by operations like generalization and inhibition. When these habits are associated with language, they are called processes and are further specified as decoding or receptive processes, encoding or expressive processes, and association or inner language or organizational processes....

Using observations on classical conditioning as a point of departure, we have attempted to recapitulate some of the thinking Osgood went through in developing a model of behavior upon which the ITPA, a language evaluation device, was based. The importance of relating the ITPA to its underlying (Osgood) model lies in the use the clinician can make of this relationship in interpreting test outcomes. (McCarthy, 1974: 60, 62, 64.)

The behaviorist commitments of the authors, their particular reliance upon Osgood's theory, and the relation between the test and theory are made explicit in the above passage. (Similar behavioristic discussions of language processing are to be found in Kirk and McCarthy, 1961: 403; Kirk, 1968: 407; Kirk, McCarthy, Kirk, 1968: 7, 11.) The authors' conviction that language comprehension and production were achieved by the operation of associative mechanisms is revealed in other places. The statement: "Processes encompass the acquisition and use of the habits required for normal language use. Their acquisition is dependent on learning theory for a complete and adequate explanation." (McCarthy and Kirk, 1961: 3) would seem quite direct in this respect.

There would seem to be no question but that the Osgoodian processes are under assessment on the ITPA. Osgood himself, in one of the two references to the test to be found anywhere in his writings (a somewhat intriguing paucity relative to the frequently addressed association with Osgood occurring in the ITPA literature) leaves little doubt about the understanding which he had concerning the test and the postulated processes of his theory. It was Osgood's understanding that the test was attempting to "sample them":

An example of the use of a theoretical model in test construction, discussed by the seminar, is the Illinois Test of Psycholinguistic Abilities...on which James McCarthy is presently working. Osgood's general behavioral model...envisages three levels of organization (projection, integration, and representation), three stages or processes (decoding, association, and encoding), and several channels (only two of which, perceptual-motor and audio-vocal, are involved here). McCarthy and his associates have been standardizing a battery of tests which are intended to sample performances from each of the levels, processes, and channels as purely as possible." (Osgood and Murray, 1963: 13-14.)

A final source of support for the OPI is this consideration. If the OPI is the intended interpretation, the correctness of Osgood's theory is a necessary condition for the validity of the ITPA--as indicated in the earlier discussion. A way of putting the same thing more specifically and in psychometric terminology is that if the constructs under assessment in the ITPA are those of decoding, encoding and association as specified in Osgoodian theory, then those processes must in fact be the processes which underly language comprehension and production if the ITPA is to have construct validity. For if OPI is the operant interpretation, one could show that the ITPA does not have construct validity, i.e., is not measuring what it purports to measure, by showing that Osgood's theory is incorrect. This being so, one would expect serious concern over the correctness of Osgoodian theory to be evidenced by the ITPA authors. While generally this is not what one finds--a fact which runs contrary to OPI--nevertheless the concern is there. For McCarthy (1974) published a paper (quoted from earlier) thirteen years after the test's introduction, whose declared purpose was that of aiding users in their interpretation of the ITPA, and this paper is devoted to an exposition of Osgoodian theory. While the reader does not emerge from this paper in possession of an explicit declaration that the OPI is the interpretation intended by the test's authors, the fact that such an exposition of Osgoodian theory makes little sense otherwise is most persuasive. Also in support of this understanding of the test-theory

relation are the following two comments--the only ones in the ITPA literature which allude to it:

It is most important that the construct validity of the ITPA be demonstrated since the ITPA is based on theoretical constructs. From such constructs predictions about linguistic behavior can be made. These predictions can then be tested empirically to demonstrate construct validity. To date, no such studies are available for the ITPA although evidence in support of the basic theory is extensive (See Osgood, 1953). (McCarthy and Kirk, 1963: 38.)

The ITPA battery represents a collection of subtests, each of which purports to assess a linguistic ability (or abilities) prescribed by a theoretical model (Osgood, 1957a, 1957b)...(O)ne might examine the completeness of the model upon which the test is based and the thoroughness of the theory upon which the model is based in judging the content validity of the ITPA battery. (McCarthy and Olson, 1964: 26, 28.)

On the issue of there being "extensive support" for Osgood's theory, I will only note that in the 1957 paper which the authors cite as the source of their test conception and in which Osgood publicly presented his mediation theory for the first time, Osgood describes it as "a highly speculative conception of behavior, which at least pretends to be a complete theory, in scope although certainly not in detail." (Osgood, 1957a: 76.) The "extensive support" of Osgood (1953) consists entirely in interpretations of animal experimentation and human communication episodes in a manner conducive to these speculations. On the issue of there being "no serious objection" to the theory at the time of the test publication, one could comment both that there had hardly been time and that the validity of the theory whose truth they were assuming should have been a central concern of the authors themselves. The situation regarding Osgood's theory had certainly reversed by the end of the 1960's, with few seriously accepting it. Yet none of the relevant contrary data and argumentation which arose between the test's introduction and McCarthy's 1974 presentation, not even that directly involving Osgood (Osgood, 1963c; Osgood, 1966; Osgood, 1971b), is even mentioned by him. The inescapable impression which one receives from reading the entirety of the ITPA literature is that the authors have been equivocal with respect to the test-theory relation from the beginning to the present--claiming an important relation, hinting that the validity of the theory is critical, while for the most part ignoring it. Certainly any straightforward discussion of the issue or its consequences is not to be found. In its place one finds extended variations on the notion of "model" and a confusing lineage from theory to test which has Osgood's theory giving rise to "Osgood's model" which yields the ITPA model which yields the ITPA. So numerous are the models and

their various descriptions--"Osgood's theoretical model," "Osgood's communication model," "Osgood's general behavioral model," "clinical model of the ITPA," "psycholinguistic model," and the imaginatively fence-riding "clinical-theoretical model," among them--that McCarthy (1974) and a number of commentators have had to give written attention to the matter of sorting things out. A great deal of ink has been consumed in relating these entities to each other despite the fact that nothing of importance appears to rest on them. The point I am distilling from all of this is simply that if one looks at some of what the authors say while ignoring the contrary indications, OPI looks like the intended interpretation.

There is certainly justification for the judgment that OPI, regardless of what other interpretations are made of the ITPA's purpose, is one of the intended (if not the principal intended) interpretations of this test. One need not read into, but merely read from the literature in order to obtain it. Furthermore, as will be seen later, certain design features of specific subtests make sense only if this interpretation of the test is operant. Not surprisingly, then, OPI appears to be the interpretation of the test taken up by a number of the ITPA's commentators. (Rosenberg, 1970: 208, 212; Chase, 1972; Carroll, 1972; Myers and Hammill, 1969: 223-4; Spradlin, 1963: 522.) Quoting Spradlin: "The ITPA and the PLS [Parsons Language Sample] are both based on learning models; however, there are considerable differences between the rationales for the two tests. The rationale for the ITPA assumes that the test items are measuring implicit processes within the person and that the language responses are merely effects of those processes." (Spradlin, 1963: 522.)

Interpretation B

The second interpretation of the ITPA is a simple variant of the first. This interpretation is one wherein the intended inference is again taken to be from overt performance to psycholinguistic processes believed to underly it, but with no restriction on those processes being Osgoodian. Quite simply, the diagnosis is that the subject has a malfunction in one or more of the processes which are responsible for language comprehension and production, whatever the actual character of those processes might prove to be. Since the authors have arbitrarily equated language processes with habits in the behaviorist sense, I am somewhat hesitant in proposing an interpretation of the ITPA consistent with any type of psychological

process. It could be that this variant is one wherein the inference is from overt performance to whatever associative mechanisms are presumed responsible for language comprehension and production. Be that as it may, the central notion is that the commitment to Osgoodian processes being under assessment is dropped. The diagnosis might be, for example, that some deficit in short-term memory is responsible for a child's failure to follow spoken directives. The diagnosis is that the storage of auditorially received information for short periods of time is not being achieved, the specific character of the processes responsible for this function being of no concern. It perhaps goes without saying that such a diagnosis is nevertheless quite informative, since a failure to follow spoken directives, given neurological and sensory integrity, could yet be due to some malfunction in the processes which underly speech comprehension or in executive processes of vocal or motor expression and not in short term memory.

The test-theory relation on this interpretation of the test, which I will henceforward call simply PI (Process Interpretation) is radically different from that on OPI. For on PI, since the test authors have no commitment to any particular theory of language comprehension and production, Osgoodian or otherwise, the correctness or incorrectness of Osgood's formulation is of no consequence to the test. Support for PI comes from many quarters. The simple fact that there is no explicit statement of OPI, despite the ample support for it, may be regarded as weak evidence that PI might be the operant interpretation. It is also a matter of record, as already mentioned, that Kirk and McCarthy have shown comparatively little concern for the validity of Osgood's theory--a condition that is understandable given PI, unthinkable given OPI. It also appears to be the case, if one surveys the original ITPA literature, that in the years following the test's introduction (1961), the connection between the ITPA and Osgood's theory becomes increasingly attenuated. The last reference to the test being based on Osgood's theory is in 1963. Thereafter, the declared basis is Osgood's model, by which the authors seem to mean Osgood's schema or rationale for categorizing human communication behavior (e.g., "Osgood's model provided a framework from which a series of tests could be generated which collectively would be comprehensive." (McCarthy and Kirk, 1963: 37; see also McCarthy, 1974.)). Certainly many commentators treat it as no more than this. If the relation of Osgood's work to the ITPA is simply that of providing a

classificatory scheme, "a framework" with which to consider the language processes, primarily being that of distinguishing language reception from expression and verbal thinking, distinguishing processes involving consciousness of meaning from those that do not, and distinguishing all of these operations according to the character of input stimuli and mode of expression, then the validity of his explanatory theory is of no consequence to the ITPA. In a sense, one could say that what has been taken from Osgood is the language of his theory, but not the propositions or claims that he makes using that language. Again, this is consistent with PI, but not OPI.

Further evidence for it simply being Osgood's classificatory scheme (model), and not his theoretical claims, that was adopted and adapted by the test authors is provided by the adaptations themselves. The Osgood model is repeatedly characterized in terms of its expedience and usefulness for test construction rather than in terms of correctness or validity, and there are many references to it being modified: "Clinical observations and the practical problems of test construction required several alterations of Osgood's original model." (McCarthy and Kirk, 1961: 2; see also Gearheart, 1973: 58 and elsewhere.) Not only is it certain that no addition or subtraction has been made to Osgood's theory of communication by Kirk and McCarthy, but the very notion that an explanatory theory could be modified to suit practical demands is unconscionable. It is no more susceptible to such modification than is germ theory to the demands of medical treatment. But while one is not at liberty to alter explanatory theories in this way, one may rearrange and alter a classificatory scheme (model) in order that it make whatever distinctions one is interested in (See Gearheart, 1973: 58). (Characteristically, however, this line of thought like so many with respect to the ITPA cannot proceed unhampered. For the term 'model' is used ambiguously by the authors; sometimes as a synonym for "frame of reference", sometimes for "diagram," and at others as a synonym for "theory". (See Paraskevopoulos and Kirk, 1969: 9; McCarthy and Kirk, 1961: 2; McCarthy, 1974: 47; and elsewhere.))

What all this points to is that either there has been a reordering of the conception of the ITPA-Osgood relation following the test's introduction such that it is only his mode of categorizing language behavior and not his postulated processes that are accepted by the ITPA; or this was the understanding from the start. There are more than a few passages which read as though it was simply Osgood's diagram of his theoretical conceptions (also referred to as "the Osgood model")

that caught the attention of the ITPA authors, providing a source of ideas or "frame of reference" for the construction of the test.

The whole view, the closure or generalization, is Osgood's mediation hypothesis. This hypothesis is graphically characterized in his behavior model, which became the basis for the construction of the ITPA, both the 1961 and 1968 versions. Osgood's views derive often from real data and are eclectic in the range of theoretical views they consider. (McCarthy, 1974: 47.)

This again lends support to the process interpretation over the Osgoodian Process Interpretation of the test's purpose.

As a final indicator of this interpretation of the ITPA, there is this consideration: as far as I can determine, none of the many researchers studying the construct validity of the ITPA (See reviews of the validity research: Newcomer, et al, 1974; Newcomer, et al, 1975; Weener, et al, 1967; Sedlak and Weener, 1973; Proger, et al, 1973; Kirk and Elkins, 1974.) has paid the slightest attention to the processes of decoding, encoding and association as specified in Osgood's theory. While most make some vague declaration that the test is based on Osgood's model or schema or construct, and that this must be validated, none take the view that the truth or falsity of Osgood's postulates concerning how language is comprehended or produced is of any consequence to the test validity. What they do consider to be "Osgood's construct" and the validation of a construct is extremely unclear, but that is of no concern at present. The universal disregard of the Osgoodian processes certainly indicates that these researchers are not operating with the belief that OPI obtains. We may infer from this that such an interpretation was perhaps not intended. But against this must be weighed the considerable amount of documentation presented earlier which leads persuasively to that very interpretation. It seems to me, having read virtually everything there is to read on this test, that the only certainty is the uncertainty. As with the OPI, many commentators on the ITPA can be placed in the PI camp (Gearheart, Lerner, Myers and Hammill). Quoting Lerner, "The ITPA (Kirk, McCarthy and Kirk, 1968) was designed to diagnose problems of learning by assessing specific and discrete underlying psychological functions of young children." (Lerner, 1976: 92.) There is no suggestion here that the processes are Osgoodian or even associative.

Interpretation C

On this interpretation, from the subject's test performance inferences are to be made concerning his "psycholinguistic abilities"--the psycholinguistic abilities being such as "the ability of the child to express his own concepts vocally," "the ability of a child to derive meaning from verbally presented material," "the ability to express ideas manually," etc., as listed earlier (Kirk, McCarthy, Kirk, 1968). If we return to the bedrock position of tests being devices for obtaining behavior from a subject on the basis of which inferences are made about the subject, the marked difference between this interpretation of the ITPA and those just developed can be clearly established. In the case of OPI and PI, inferences were made from the subject's performance to the processes believed to underlie them. In the present case, there is no inference to underlying processes. Rather, the inference goes from the subject's performance to his possession of the trait supposedly manifested by it. To be more precise, the test performance is used to warrant attribution to the subject of that characteristic (trait) which is taken to be exemplified (manifested, reflected in) the performance. As one writer in his discussion of testing put it, "Such things, then as mechanical ability, musical talent, clerical aptitude, and even intelligence are to be considered as postulated attributes of people--attributes which are assumed to be reflected in observable behavior." (Helmstadter, 1964: 17.) As for mechanical ability, so also for auditory decoding ability, visual motor association ability, vocal encoding ability, and the other nine abilities presumably manifested on the remaining subtests of the ITPA. These psycholinguistic abilities are postulated attributes of the subject which are ascribed on the basis of his performance. In making this ascription, whatever processes are responsible for the subject's performance are of no concern; only the overt behavior itself is considered.

While on OPI and PI, the behavior was taken as a sign or indicator of unobserved processing, on this trait interpretation (TI) behavior displayed is treated as a representative sample of the type of behavior regarded as indicative of the attribute. In fact, ascribing an ability to someone--"X has Y-ing ability"--is warranted only because X manifests behavior of some characteristic type. A critical difference between this interpretation (TI) and the two process interpretations is that on TI the existence, let alone the validity, of Osgood's theory is of no concern. The fact that Osgoodian

theory on thinking inspired some of the conceptualizations employed is purely incidental.

The belief that the ITPA subtests are to be understood in these terms, i.e., the belief that TI is an intended interpretation of the ITPA, is like the other alternatives fostered by the original ITPA literature. In the passage from the 1968 Test Manual which was quoted in presenting the test, it is stated that the Auditory Reception subtest "is a test to assess the ability of a child to derive meaning from verbally presented material," that the Manual Expression subtest measures "the child's ability to express ideas manually," etc. On any impartial reading of this discussion, and by impartial I mean one wherein the reader is not convinced from his reading of other ITPA literature that the process interpretation obtains, it appears indisputable that the authors have devised various constructs, "the psycholinguistic abilities"; and in each case, have provided a characterization or definition of the respective construct (i.e., psycholinguistic ability) in everyday language. The situation here is precisely the same as that which would obtain were the trait under assessment "mechanical ability." A characterization would have to be provided of what is meant by "mechanical ability" as a precondition of a test for that trait being designed and evaluated. This characterization, it would seem, is just what the authors offer in the presentation of their test. The absence in these statements of any reference to the test's assessing psychological processes believed to underly these various abilities, the lack of any indication that a process interpretation was to be given to test performance, sets these descriptions of the test's purpose in direct conflict with the earlier understanding. For these descriptions of the test manifestly portray it as a test designed to measure abilities, i.e., traits, and not psycholinguistic processes. Statements that it is psycholinguistic abilities which are under assessment are to be found everywhere, beginning with the very title of the test. Such then is the trait interpretation of the ITPA.

The trait interpretation is assuredly the one taken up by the many commentators who emphasize that a "task analysis" must be made of the subject's performance on the various subtests. The notion here is that from the test one only learns that the child has difficulty in a certain area, and not the source of those difficulties. Thus on the Manual Expression subtest it is purportedly learned that the child has "difficulty in expressing ideas manually," a determination that is made on the basis of some pantomimes required of him. Further analysis of

what is required of the child in completing the task must be undertaken to determine the source of the problem. His low score may be due to unfamiliarity with the objects, unfamiliarity with the customary use of the object, an incorrect understanding of their use, emotional inhibition, lack of recall, etc. Thus further observation and assessment outside the test situation is required to identify the cause of the difficulty. The revealing feature of this approach to the ITPA is that the test's relations with Osgood's work, whatever they might be, are of no import whatsoever, and of no interest other than historical. The test is seen simply as providing a structured situation for the same sort of analysis of overt behavior that is presumably undertaken in countless informal situations; and ironically, the sort of analysis which may well have led to a teacher's decision to have a child tested on the ITPA in order to find out whether some deficit in psychological processing was the source of the problem. Nancy Hanck, an advocate of the task analysis approach to the ITPA, specifically complains that too much attention has been given to relating test scores to the Osgood model and not enough to task analysis (See Hanck, 1976). Myers and Hammill also take the position that the test simply reveals "areas of weakness" which must be further analyzed if the source is to be determined. In discussing the profile of a child obtained after testing on the ITPA, the authors have this to say:

The test authors make it quite clear that the results alone should not be used to make overall conclusions about any individual. They recommend that it be used to demonstrate areas of weaknesses which should then be investigated through further formalized testing, informal or criterion-referenced testing, and general observation. (Myers and Hammill, 1976: 32.)

The reference to Kirk and McCarthy taking the position that the test simply revealed "areas of weakness" whose source must then be determined by other means, is indeed supported in the ITPA literature (See Kirk and Kirk, 1971). But so also are the other interpretations. So it must be understood that this position is maintained simultaneously with that (which we have documented thoroughly) wherein the subtests are to be regarded specifically as revealing deficits in psycholinguistic processing. One must bear in mind, for example, that the authors explicitly describe the identification of an "auditory decoding deficit" by virtue of the auditory decoding subtest as akin to a diagnosis of aphasia (Kirk and McCarthy, 1961) when one reads elsewhere that poor performance on the auditory decoding subtest may be due to anything from poor hearing or inattentiveness to a limited

vocabulary (See Kirk and Kirk, 1971: 103-4, 137-9). In one place we are given to believe that a defect in the psychological processing of language has been revealed by the child's test performance, while in another, the message is that the test performance does not isolate any of the possible contributive factors to the observed shortcoming. What the juxtaposition of claims such as these (and many more could be provided) tellingly portray is the fundamental equivocation over the intended interpretation of this test which characterizes the ITPA literature. Are psycholinguistic processes being diagnosed? Or are we simply determining areas of strength and weakness--areas labeled by the the terms "auditory reception disability," "grammatic closure disability," etc.?

Summarizing this section on the trait interpretation of the ITPA: The ITPA is taken as a measure of so-called psycholinguistic abilities. The ascription of such abilities, which are simply attributes or traits to be predicated of the person, depend upon the subject's performance on the test. The required test behaviors are treated as instances of the sorts of behavior warranting ascription of the trait--e.g., pantomime as an instance of expressing ideas manually, other instances being gestures, sign language, finger spelling, and even hand writing. The inference from test performance, given TI, is not to the operation of processes presumed to underly it, but to the trait presumably manifested by it. As with the process interpretation, the trait interpretation has its adherents among the ITPA commentators. I include all those who describe the ITPA as a measure of abilities (or even skills) in the TI camp. Some members on this criterion would be Gearheart (1973), Hanck (1976), Bush (1969, 1976), Minskoff, Wiseman and Minskoff (1972), McLeod (1976) and many others.

Summary

Relative to the serious effects of the equivocation in the ITPA literature over what the test purports to assess, the reaction amongst its hosts of commentators has been slight. I believe that this is most probably due to the fact that in the literature the various interpretations are hopelessly intertwined and thus difficult to identify. They could only be given distinct characterization following a close examination of the ITPA literature in its entirety. The first researcher to be genuinely disturbed by the situation was Lester Mann. In a 1971 article, Mann took the position that the ITPA authors and its many supporters were treating abilities as if they were actual

processes responsible for the production of the behavior upon which the ascription of ability was based. On this understanding one might say in answer to the question 'Why is Joe proficient at repairing cars?', --'Because he has mechanical ability.' In so doing, one has reversed the proper rendering, which is that it is because Joe is proficient at repairing cars that we say that he has mechanical ability. Mann's concern was related specifically to the test's declared differential diagnosis of children and the attendant claims that the abilities diagnosed could be learned and/or remediated: "Such 'learning abilities' are hypothesized to be basic to academic and other types of achievement or malachievement and are presumed to be finitely separable, diagnosable, and (by many) trainable." (Mann, 1971: 5.) Mann continued:

The "abilities" or "skills" to which the differential assessment and training programs address themselves, however, are abstractions fleshed out and reified in psychometric garb as real "processes." ... (I am) vehemently opposed to present conceptualizations of abilities as processes that can be identified precisely by existing tests for training purposes. (Mann, 1971: 6-7, 2f.)

It is clear to me that Mann is reacting to the pervasive equivocation on the ITPA concerning its function as a measure of psycholinguistic processes or of certain abilities. Such an equivocation is disturbing to a mind which, as Mann's apparently does, sees important differences between processes and abilities. At any rate, it is certain that at least one other commentator on this test has been troubled by its vague and equivocal characterization.

Two other researchers have recently thrown down the gauntlet. Newcomer and Hammill (1976), both of whom have conducted considerable research on the ITPA raised what they described as "concerns which are of a philosophical nature" with respect to the test. I quote their discussion at length:

Specifically, we are confused as to what the proponents of the Kirk-Osgood model mean when they refer to "auditory reception," "visual memory," "verbal expression," "representational level," etc. Seemingly they differ markedly among themselves in the manner in which they interpret these terms.

On the one hand, certain of their number appear to accept Osgood's and Kirk's theoretical postulations quite literally. Osgood, of course, clearly delineates his constructs as designators of actual mental functions within the brain. As a behavioral psychologist applying a stimulus-response paradigm to explain learning, he hypothesizes the existence of fractional mediating responses within the central nervous system which permits the human organism to process and store language information, i.e., to acquire language. Viewed in this light, "auditory reception," for example, refers to the functioning of those particular neural processes which are

responsible for a person's ability to take in and to understand all types of verbal information, both meaningful and nonmeaningful.

Kirk, McCarthy, and Kirk seemingly adopt Osgood's philosophical position as well as his rationale for their test....To Kirk and his colleagues the terms "auditory reception," "verbal expression," "visual sequential memory," etc., very definitely refer to inferred psychoneurological processes which operate within the brain. Although Kirk is perhaps more psychological and less neurological in his focus and choice of terminology than is Osgood, he essentially seems convinced that Osgood's conceptualizations represent actual mental operations or functions.

An alternate position regarding the nature of these psycholinguistic processes is that they simply represent hypothesized constructs, rather than actual neural functions; and, as such, they may be conveniently employed to label and group various behaviors....From this point of view, terms such as "auditory reception" are generic labels representing superordinate categories under which countless subordinate behaviors can be assembled. These subordinate behaviors may be grouped together because they all satisfy a common definition, rather than because they have a similar neurological foundation. For example, such apparently different tasks as discrimination among pure tone sounds, comprehending a conversation, and recognizing verbally presented absurdities are readily identified as belonging to the "auditory reception" category because they all involve, in some manner, the ability to understand what is heard. Those who regard the model as a "frame of reference" apparently chose to ignore the theoretical implications expressed in the work of Osgood or Kirk, and make few statements about training underlying psychoneurological processes. (Newcomer and Hammill, 1976: 156-7.)

While I certainly do not regard the remarks here as especially lucid themselves, the resemblance between the process and trait interpretations of the ITPA which I have developed, and Newcomer and Hammill's discussion of "auditory reception," "verbal expression," etc. taken either as labels for "inferred psychoneurological processes" or as "labels representing superordinate categories under which countless subordinate behaviors can be assembled" is to my mind most striking. The congruence of our independently reached conclusions concerning the test's ambiguous character supports the belief that the confusion is the creation of the test authors, not the test critics.

This rather tedious effort toward characterizing what the ITPA intends to measure has been a necessary preliminary to advancing an effective critique. Unless one knows what information the ITPA is claiming to provide about the children to whom it is administered, one cannot determine whether or to what extent it succeeds. The fact that the intended inference is very unclear leaves the critic in a jeopardous position. The jeopardy resides in his seeking to evaluate the test against a given interpretation, only to find that the interpretation he selected was not operant. In such a case his arguments

become neither true nor false, but neither here nor there. They are quite literally beside the point. However, were the authors to avoid an effective critique of one interpretation of the test by taking sanctuary in another, the researcher would at least have achieved the desirable result of depriving the ITPA of one of its currently available refuges. Naturally, were the equivocation more apparent, the ITPA authors would have been pressed some time ago for an unambiguous statement of just what is to be made of this test. The fact that so many researchers have gone untroubled by the existing state of affairs is somewhat of a mystery, though the high tolerance of many educationalists for vagueness is a commonplace. As I have stressed, the situation in the ITPA literature is that of thoroughgoing vagueness from which the various interpretations must be extracted. It is not that of clear-cut ambiguity which, relative to the actual state of affairs, could be somewhat redeeming. Given this vagueness, the vast majority of construct validity studies have been conducted in the most dreamlike fashion (See Sedlak and Weener, 1973; Proger et al., 1973; Newcomer et al., 1975) with one researcher after another declaring his intention to evaluate the ITPA against the "Osgood model" or the "Osgood construct," etc., without in any case any effort being made to specify what those constructs are taken to be or how they relate to test performance. When this unforgivable lack of specificity is combined with the indirect and malleable character of factor analysis, which has been the principal method employed in the construct validity research, the result is one of guaranteed confusion and inconsequence. Be that as it may, even if the many construct validity researchers had taken seriously the issue of what was to be evaluated, they would have come up with not a single, but a collection of proposed interpretations; or perhaps, like Mann, would have taken the position that the ITPA claims were uninterpretable and thus incapable of validation. I have elected to follow the safer, though more complicated course of confronting the beast in all its forms. There can be no escape by metamorphosis if the ITPA is shown to be inadequate given any of the interpretations which are competing for the place of "what this test purportedly measures." It is just this inadequacy which I hope to demonstrate.

IV. OSGOOD'S THEORY OF COMMUNICATION

Charles Osgood first presented his mediational response theory in 1952 in a paper titled "The Nature and Measurement of Meaning." Since that time he has developed and forthrightly defended his position in numerous articles and books. His effort has justifiably been described as "the most detailed and sophisticated attempt to explain language behavior in a stimulus-response framework." (DeVito, 1970: 69) Few commentators would disagree with this judgment. Osgood is not alone among the mediational response theorists; like-minded researchers include O. H. Mowrer (1960), Jenkins and Palermo (1964), Staats and Staats (1964, 1971) and M.D.S. Braine (1963a, 1963b, 1965). However, Osgood's formulation is arguably the strongest of this group and has certainly had the greatest influence on education via the development of the ITPA, the Peabody Language Development Kits (PLDK), and (tangentially) the Semantic Differential, all of which are based upon it. In this paper I concentrate on Osgood's particular version of neo-behaviorist theory, since it is this particular theory upon which the ITPA is based, though many of the descriptions and criticisms raised apply to mediated response theories generally.

The full crop of mediated response theorists are planted firmly in the tradition of American behaviorist psychology. Osgood is explicit in tracing the source of many of his formulations to seedbeds prepared by Clark Hull and D.O. Hebb; and as will emerge later, his explanation of word learning is simply and explicitly a modification of Pavlovian conditioning. Osgood's behaviorist moorings are also manifested in his appraisal of the book on language behavior produced by the arch-behaviorist, Skinner. In contrast to the classic, merciless review of Skinner's Verbal Behavior, produced by Chomsky in 1959, Osgood's 1958 review of the same text found it "one of the two or three most significant contributions to this field in our time, and for anyone interested in language behavior...it is a must." (Osgood, 1958: 212) And while Osgood had many criticisms of his radical behaviorist colleague, he emphasized in the review and again ten years later that he was only challenging "the sufficiency of Skinner's conception, not its correctness as far as it goes." (Osgood, 1958: 210) Such facts and fragments as these, even apart from the substance of Osgood's theorizing,

are indicative of Osgood's location in the American behaviorist tradition. Indeed, the mediational response theorists are often grouped under the heading, "neobehaviorists" with Osgood being a self-described member:

Representational mediation theory is the only learning approach that has seriously attempted to incorporate the symbolic processes in general and meaning in particular within an S-R associationistic model. My own version of neobehaviorism, as it has come to be called, has made explicit the origins of mediating (symbolic) processes in nonlinguistic, perceptuo-motor behaviors... (Osgood, 1971a: 11)

Many writers (Katz, 1966; Black, 1972; Fodor, Bever, Garrett, 1974; Harrison, 1972) have traced the heritage of the mediational response theorists in their reflections about meaning beyond the psychological predecessors to their philosophical sources. The geneology is usually traced to Locke and Hume, but Slobin, quoted below, brings in some others:

The approach has its roots in the pragmatism of Peirce and James and Dewey. Peirce, for example, suggested that the sentence, "This is hard" means something like, "If you try to scratch this, you will fail." The underlying notion was that meaning is tied to the performance of certain operations; that symbols have consequences in human action.

This pragmatic philosophy is congenial to psychologists who wish to emphasize the active, operational, behavioral aspects of meaning, as opposed to its passive, introspective, subjective aspects. Since the First World War, behavioristic theories of meaning have developed in America in consonance with these notions of pragmatism, and in consonance with Pavlovian conditioning theory. John B. Watson's behaviorism, which he proposed in 1913, developed in close connection with the work of Pavlov and his school. The behavioristic theories of meaning have looked for a response to mediate between symbol and referent. Response theories of meaning, however, have had a peculiar history. At first, Watson proposed a "substitution theory." To him, words had meanings because they were responded to in the same way as one would respond to their referents. The responses involved were gross, observable, and peripheral in the early psychological studies--responses like movements of the hands, mouth, throat, and other parts of the body. Osgood (1952) reviews this work of the twenties and thirties, summarizing its ambiguity and general failure. It became clear that people do not respond to a word in the same way as they respond to its referent. That is, when you hear the word apple you do not begin to make apple-eating responses as you would in response to a real apple.

Accordingly, the responses which are considered to be the meanings of words have become smaller and smaller and have retreated into the brain. That is, perhaps you only make minimal apple-eating responses when you hear the word apple; or perhaps you only think of making those responses. And so the response theory of meaning progressed from a theory of overt responses to a theory of implicit muscular responses, then to fractional responses, and finally, in Osgood's mediation theory, the meaning of a word can be a tiny segment of a response which occurs totally within the central nervous system. (Slobin, 1971: 90-91)

Continuing where Slobin leaves off, the central claim of Osgood's theory is that we understand words because utterances of words elicit implicit, i.e., unobserved but observable in principle, responses as conditioned responses. These responses are portions of the total behavioral response to the thing named by the word. They are termed by Osgood, representational mediation processes (r_m 's); representational because they are reduced portions of the behavior originally elicited by the object, and thus representative of it, and mediational because they intervene between the overt stimulus and the overt response. The r_m 's are regarded as the nonverbal counterparts to words which are, according to Osgood, literally the words' meanings. It was "self-evident" to Osgood that in verbal communication, people respond not to the sounds of words, but to their meanings, and if he was to speak about responses to meanings, it required that meanings, like the word utterances which elicit them, be physical events. Hence, the identification of word meanings with representational mediation processes: "This representational mediation process is the meaning of the sign." (Osgood, 1953: 697) Given this understanding that a critical feature of human communication was that utterances of words elicited their meanings as a conditioned response, it was incumbent upon Osgood to explain how they first became conditioned. The explanation of how r_m 's became conditioned to words was for Osgood the explanation of how language is learned. Once these utterance- r_m associations are established, the explanation of language comprehension and language production follows quite naturally. We understand the word because the utterance of it, spoken or written, elicits the r_m which has become conditioned to it, the r_m giving rise to consciousness of meaning. We are able to utter a particular word because the r_m which has been established for that word functioning now as a stimulus in its own right (thus the r_m is frequently noted as r_m-s_m to emphasize its stimulus properties) elicits the proper vocalization. While this is a very condensed statement of Osgood's theory, it is faithful to its central tenets.

It is interesting to speculate about what led Osgood to treat meanings as physical events. The following comment is very revealing in this respect:

Whatever the representing symbolic process (meaning) may be, it cannot be directly dependent upon the physical characteristics of the sign (word) itself. This is because the relation of signs (words) to their significations or meanings is essentially arbitrary --one man's meat is another man's poison; what we call "horse"

another man calls "cheval" and yet another man calls "Pferd."
(Osgood, 1957b: 354)

Thus, when mothers in England, France and Germany produce the sounds which mean "please close the door, son," the sounds are all different, but the response, let us suppose, is the same. Each son closes the door. This indicated clearly to Osgood that the different boys hearing these different sounds were responding not to the different sounds, but to their common meaning. Meanings, whatever they are, were eliciting responses and the distinctive feature of language behavior was that people responded to the meanings of words. But if we are to talk of responses to meanings--of meanings eliciting responses in the behaviorist sense, then, as noted, meanings must be physical stimuli. But patently, meanings are not part of the environmental stimulus display of the organism making a response, just the word utterances are. Hence Osgood postulated that meanings are internal events which are elicited by the utterance of words, and which in turn are responsible for eliciting overt behaviors. The response we see to an utterance such as "please close the door" has been mediated by this internal event. These unobserved physical events are seemingly required if we are to explain the overt responses to linguistic utterances. Furthermore, their postulation avoids a number of pitfalls of theories which make the subject's response dependent upon local environmental stimuli, as Osgood himself notes (Osgood, 1953; Osgood, Suci, Tannenbaum, 1957), and is not a mere flight of fancy. As Osgood put it, the word meaning certainly "refers to some implicit process or state which must be inferred from observables." (Osgood, Suci, Tannenbaum, 1957: 1) The meaning of a word must be (strange as this may sound) this "distinctive mediational process or state which occurs in the organism whenever a sign (word) is received (decoded) or produced (encoded)." (Osgood, Suci, Tannenbaum, 1957: 3)

The burden of Osgood's effort in psychology has been to characterize, measure and describe the function of this process. Osgood reminds us that "psychological theories of meaning differ among themselves as to the nature of this distinctive process or state"; his is but one proposal, but one that has had great influence. He states: "(We) have identified this cognitive state, meaning, with a representational mediation process (r_m) and have tried to specify the objective stimulus-response conditions under which such a process develops." (Osgood, Suci, Tannenbaum, 1957: 9) And "...This representational mediation process is the meaning of the sign (word)." (Osgood, 1953: 697.)

Osgood's claim that the meaning of a word is an implicit process should now be understood. We can translate what is being said as follows: every word has a nonverbal counterpart which is its meaning. It is this nonverbal counterpart which is the subject of Osgood's research. He has theorized concerning how the nonverbal counterpart becomes linked with a word, and how this nonverbal counterpart then enables us to both understand and produce language. Osgood sees his research as an inquiry into the phenomenon of meaning, and that, he claims, is no easy task:

Of all the imps that inhabit the nervous system--that "little black box" in psychological theorizing--the one we call "meaning," is held by common consent to be the most elusive. Yet, again by common consent among social scientists, this variable is one of the most important determinants of human behavior. It therefore behooves us to try, at least, to find some kind of objective index. (Osgood, Suci, Tannenbaum, 1957: 10)

The Character of the Representational Mediation Process (r_m)

Osgood sees the problem for the psychologist interested in discovering the nature of meaning, the nonverbal counterpart of words, in the following way. Words, it is assumed, stand for or refer to things. In Osgood's terminology, "signs" signify "significates." The word 'apple' refers to the thing, apple; the word 'hammer' refers to the thing, hammer, etc. Furthermore, it is quite clear, indeed Osgood says it is a "self-evident fact (that) the pattern of stimulation which is a sign (word) is never identical with the pattern of stimulation which is the significate (object)." (Osgood, Suci, Tannenbaum, 1957: 3) The word 'hammer' does not have the same stimulus characteristics as the object it signifies. "The former is a pattern of sound waves; the latter, depending upon its mode of contact with the organism, is some complex of visual, tactual, proprioceptive, and other stimulations." (Osgood, Suci, Tannenbaum, 1957: 4) This surely is true. The sound waves produced when someone utters the word 'fire' or the light waves produced by the visual pattern of the written word 'fire' are not the same as the stimuli produced by fire itself.

Nevertheless, says Osgood, despite this radical dissimilarity in stimulus characteristics between words and the things they signify, words, as stimuli, do elicit from humans behavior which "is relevant to" or "is appropriate to" the objects they signify. This is not at all to say that the behavior elicited by the word is the same behavior as that elicited by the object referred to. Manifestly, it is not. As Osgood says, "The word 'fire' has meaning for the reader without

sending him into wild flight; the word 'apple' has meaning without eliciting chewing movements." (Osgood, 1953: 692) What Osgood has in mind when he says that the behavior elicited by the word is somehow "appropriate to" or "relevant to" the behavior elicited by the object is nowhere made explicit. It is alluded to by means of examples.

- (1) The little child whose language development we have been studying may say 'kitty' when stimulated by that furry, four-legged object, but this is no guarantee that this noise represents anything to her. Now suppose the child's mother asks, 'Where is Kitty?' and she immediately begins searching--in the sunny corner of the porch, by the cat's dinner plate. Does 'kitty' now have meaning? Is it functioning as a sign? It would seem that such is the case: the child is responding to a stimulus that is not the object (to the word 'kitty') in a manner that is relevant to the object signified; the child's behavior is apparently organized and directed by some implicit process initiated by the word. (Osgood, 1953: 690.)
- (2) A man reacts to the auditory noise, "apple", e.g., in the utterance, "do you like that apple?" in ways appropriate to the object signified, not to the noise per se." (Osgood, 1957b: 354.)

The notion apparently is that the child in the first example, in looking for the cat, was reacting in a way which was in keeping with the stimulus characteristics of the cat object itself, not the stimulus characteristics of the noise 'cat', and not to the stimulus characteristics of any other object. Cats are of a certain size and they are locomotive. The cat could be almost anywhere in the house. And the child did look in other rooms. If the child had looked under a book or under her shoe or under a lamp, this would not be indicative of behavior elicited by the cat object. It would not be "appropriate" to such behavior. It would have been appropriate had mother asked, "Where is the pin?" But the child did not do this. Somehow the word 'kitty' elicited the very behavior that the kitty itself would supposedly have elicited. This is the sense to be given to the claim that words elicit behavior "appropriate to" the object they signify.

This, then, is the problem for the psycholinguist. How is it that the word, which as a stimulus is never the same as the object it signifies, comes to elicit from the organism behavior appropriate to the object? The problem of learning how words stand for or refer to things is the problem of finding out how they come to elicit behavior originally elicited by the things themselves. Quoting Osgood:

(T)he sign ("hammer") does come to elicit behaviors which are in some manner relevant to the significate (HAMMER), a capacity not shared by an infinite number of other stimulus patterns that are not signs of this object. In simplest terms, therefore, the problem for the psychologist interested in meaning is this: Under what conditions does a stimulus which is not the significate become a

sign of that significance? (Osgood, Suci, Tannenbaum, 1957: 4)

It is in response to this question that Osgood proposes the representational mediation process, r_m . Something similar to the r_m had been proposed in the behavioral theory of Clark Hull, one of Osgood's predecessors in psychology, and to whom Osgood gives a great deal of credit. But the specific characterization of the r_m and its integration into a comprehensive theory of behavior is Osgood's own. The idea essentially is this.

Out of all the behavior the organism engages in, with or toward a certain object (let us stay with the hammer, Osgood's own example) a certain portion is internal to the organism, nerve impulses, muscular changes, etc. In the case of the hammer, behavior would include things like muscular changes involved in grasping it, sensations from the centrifugal pull as it is swung, as well as whatever neurological events are involved in seeing it from various distances. Out of the behavior from the many encounters or experiences with the hammer which Osgood calls the "total behavioral response (R_T)," a certain portion of the response becomes conditioned to the stimulus object involved. According to conditioning principles, this will be "those reactions involving the least energy expenditure (and) the least interference with ongoing behavior, etc." (Osgood, Suci, Tannenbaum, 1957: 6.) While Osgood originally believed that the occurrence of the r_m would be identified as a glandular or muscular event, or one somewhere in the peripheral nervous system, he later adopted the view that it would be a brain event. It is, of course, safe to assume that distinctive brain events must be taking place when overt behavior is produced. However, treating these events themselves as behavior involves considerable stretching of that notion. Be that as it may, Osgood writes, "Certainly in the adult human language user, these mediating symbolic events have become purely cortical processes--processes whose neurological nature and locus will not be known for a long time. (Osgood, 1971b: 523.) This portion of the total behavioral response (R_T) is what Osgood called the representational mediating process (r_m). The establishment of such r_m 's is in Osgood's view a crucial element in pre-linguistic behavior. It is not merely possible, but the rule, that r_m 's become established for the various objects that a child plays with before he learns the word for the object. ("representational processes first become associated with the nonlinguistic visual, auditory, and other cues from objects.") (Osgood, 1963d:285) The r_m can very correctly be thought of as meaning waiting for a word to go with it. Quoting

Osgood:

(T)he acquisition of perceptual significances is prior to that of linguistic significances, i.e., representational processes first become associated with the nonlinguistic visual, auditory, and other cues from objects. In everyday language, young children tend to learn the significances of what they see and hear about them (the food significance of the bottle, the security significance of mother's voice, the danger significance of a scowl, and so forth) long before they begin to learn the meaning of words. (Osgood, 1963d: 285.)

How r_m 's Become Conditioned to Word Utterances or How Language is Learned

In Osgoodian theory the basic process by means of which the r_m 's become associated with words is quite simple. The words which refer to the objects are temporally paired with the objects themselves a sufficient number of times so that utterances of the word come to elicit the r_m originally elicited by the objects. The explanation is a simple modification of Pavlovian conditioning. In one of Osgood's descriptions, he says that the word 'ball' is likely to be heard by a young child in conjunction with the thing, ball. Parents, older siblings, etc., engaging in play with the child are likely to be saying such things as "Throw the ball," "Catch the ball," etc. Eventually the r_m which has been established for the thing, ball, as a result of the young child's actual behavior with balls, becomes conditioned to the word 'ball'. Now this sound, which was initially meaningless, elicits the r_m which had been established for the ball object and is understood. It is no longer just a sound but a word with meaning. Henceforth it will elicit behavior which "is appropriate to" the ball object and not to any other, this being the reason why the r_m is termed 'representational'. It is representative of the behavior actually elicited by the object. Most often this elicited behavior will be the simple activation of the appropriate r_m , i.e., an internal unobserved response will take place. No overt behavior need ensue. The mere elicitation of the r_m is necessary and sufficient for understanding a word. If the situation calls for overt behavior, e.g., if the child hears "throw me the ball," the r_m acting as a stimulus in conjunction with other stimuli, ensures that it is the ball and not some other object that is thrown. Hearing the word 'ball' has elicited the r_m for that object. The r_m mediates between the word and overt behavior, and this is why it is called the representational mediation process. The following passage, from Osgood, is a clear statement of his account of how the meaning of words is learned:

One of the necessary conditions for the formation of representational processes is association of sign stimuli with referent stimuli. We may assume that referent stimuli are capable of eliciting certain reaction patterns in the language-learning individual, either innately or in terms of previous learning. APPLE objects elicit certain eye and hand movements as well as salivary and other physiological reactions; aggression situations may elicit anxiety or anger reactions depending on the role of the individual; printed signs like dangerous and bad already have meaning for the young reader when he encounters alien. We are therefore dealing with three types of events: significates (physical objects and events, e.g., the juicy APPLE itself); signs (other physical stimuli associated with significates, e.g., the word "apple"); symbolic processes (events within sign-using organisms which develop from the association of signs with significates, e.g., that process set in motion by hearing the word "apple").

The basic assumption I make about the behavioral nature of sign processes or meanings is this: those stimulus patterns we call signs (be they perceptual or linguistic) acquire their representing character by coming to elicit some minimally effortful but distinctive portion of the total behavior produced by the things signified. This reduced portion of the total behavior toward things is a symbolic process, which I call a representational mediation process. It is representational by virtue of the fact that it is part of the very same behavior that is produced by the significate--thus the sign "apple" represents that juicy, edible thing rather than any of a million of other possible things because it calls forth in the language user some distinctive part of the total behavior to APPLE objects. It is mediational because the self-stimulation set up in the language user by making this representational reaction can come, through ordinary instrumental learning, to evoke a variety of overt reactions appropriate to the thing signified. (Osgood, 1959a: 38-9.)

Having told us in one place that word-referent pairings are "necessary conditions for the formation of representational processes," Osgood reneges elsewhere. Indeed, we are told this condition is satisfied in the case of relatively few words and that most words are learned without pairing the "sign stimuli" with the "reference stimuli." Quoting Osgood: "The vast majority of signs used in ordinary communication are what we may term assigns--their meanings are literally "assigned" to them via association with other signs rather than via direct association with the objects signified." (Osgood, Suci, Tannenbaum, 1957: 8.) The learning of assigns is described as follows:

A very large proportion of the verbal signs used in communication are what we have termed assigns--their meaning is literally 'assigned' to them via association, not with the objects represented but with other signs. Consider for example the word ZEBRA: This word is probably understood by most 5-year-olds, yet few of them have ever reacted in any way to the object itself. They have been told that zebras have stripes (signs), run like horses (sign), and are usually found wild (sign). These previously established signs (or assigns) elicit certain meaningful reactions, and since the new

assign is temporally with these reactions, it also tends to become conditioned to them. To the extent that an assign is associated with many and varied signs (e.g. the class term, ANIMAL), its ultimate mediation activity would presumably be rather hybrid in nature--we know very little about this aspect of concept formation. (Osgood, 1953: 698.)

It is important to understand what specifically is being said here. Quite obviously there are many words whose referent will not have been part of the immediate experience of the language user. Few people have directly experienced Antarctica, and no one, presumably, has met an elf. How, then, can we learn the meaning of the words 'Antarctica' and 'elf' since there has been no behavior elicited by the things they refer to from which the r_m could be derived? According to Osgood, we are able to learn these words because we frequently hear them in conjunction with related other words for which we do have r_m 's established. Often these other words are "primary signs," i.e., words whose r_m was established by actual pairing of the word and object in experience. Thus, in the case of 'Antarctica', the utterance of this new word is likely to occur in conjunction with words such as 'snow', 'ice', 'cold' (primary signs, let's assume), and 'iceberg', 'glacier', 'South Pole' (previously established assigns, let's assume). Since the r_m 's for these words are being elicited in temporal conjunction with the new word, the assign-to-be, 'Antarctica', they are as implicit response events part of the total behavioral response (R_T) to the novel stimulus. The new r_m can thus, it is theorized, be derived from them; and according to Osgood, it is. The meaning of--the r_m --for the word 'Antarctica', is thus dependent upon the "prefabricated" r_m 's of the words actually uttered in conjunction with it while it is being learned. Someone who first hears 'Antarctica' with an entirely different set of words with previously established r_m 's, e.g., 'sled dog', 'frozen-to-death', and 'tundra', will necessarily, according to this thesis, acquire a different meaning for the word 'Antarctica'. Thus Osgood is compelled to note that "variations in meaning should be particularly characteristic of 'assigns' since their representational processes (r_m 's) depend entirely upon the samples of other signs with which they occur." (Osgood, Suci, Tannenbaum, 1957: 9.)

And what of elves, unicorns, and fire-breathing dragons, which are definitely nonexistent and therefore incapable of being experienced by anyone? They are not a problem; there is no requisite that they be experienced by anyone for the words labelling them to acquire meaning. As indicated above, all that is required is that their utterance occur in conjunction with words for which r_m 's have already been established.

Once again, according to Osgood, this is what happens:

Thus in the reading of young children non-sense forms (prior to gaining meaning from context) like thief appear with meaningful forms like gun, bad, man, night, dangerous, and against-good-policeman while nonsense forms like elves co-occur with meaningful forms like tiny, quick, magical, fairy-story, and so on. Obviously a very large proportion of one's vocabulary is acquired by this route..... (Osgood, 1966: 408.)

It should be clear from this that direct experience with the referent of a word is not necessary in order to acquire an r_m for it. Presumably words which have no referent, words which label imaginary things, and words which label things not experienced by the learner will be learned as assigns.

In concluding this discussion of Osgood's treatment of word-learning, it is necessary to point out that in many passages from Osgood, one understands his theory to be concerned with how emotive meanings for words are learned, not their literal meanings. This fact is brought tellingly home by such passages as the following where Osgood is discussing how the meaning of the word 'spider' is learned. It is introduced as:

...an abbreviated symbolic account of the development of a sign, according to the mediation hypothesis. Take for illustration the connotative meaning of the word SPIDER. The stimulus-object (S), the visual pattern of hairy-legged insect body often encountered in a threat context provided by other humans, elicits a complex pattern of behavior (R_r), which in this case includes a heavy loading of autonomic "fear" activity. Portions of this total behavior to the spider-object become conditioned to the heard word, SPIDER. With repetitions of the sign sequence, the mediation process becomes reduced to some minimally effortful and minimally interfering replica--but still includes those autonomic reactions which confer a threatening significance upon this sign. This mediating reaction (r_m) produces a distinctive pattern of self-stimulation (s_m) which may elicit a variety of overt behaviors (R_x)--shivering and saying "ugh," running out of a room where a spider is said to be lurking, and even refusing a job in the South, which is said to abound in spiders. (Osgood, 1952: 204-5.)

The above passage is riddled with problems. But our only concern for the moment is that this account of how the meaning of a primary sign is learned is clearly an account of acquiring an emotive meaning (connotation) for a term, which is a far cry from learning the term's sense and/or reference; the latter being what theories of language acquisition are presumably concerned with explaining. The same difficulty is revealed in this discussion of learning the word 'alien' as an assign:

Suppose now an individual is exposed repeatedly, and more or less exclusively, to the Chicago Tribune: He experiences the initially meaningless stimulus ALIEN in such contexts as "Aliens are not to be

trusted," "Our national life is being poisoned by alien ideologies," and "We should deport these dangerous aliens." The resulting connotation of ALIEN is predictable from the meaningful contexts in which it appears. The important thing to consider is that most of the linguistic signs with which we deal in the mass media--Eisenhower, Fixed Farm Supports, the U. N., desegregation, and so on ad infinitum--acquire meanings as assigns rather than through direct behavioral experience. (Osgood, 1956: 182.)

It is tellingly clear that this reader of the Chicago Tribune is in prior possession of the meaning of the word 'alien'. How else is he to know what all these bigoted assertions about aliens are about? If he is acquiring anything, he is acquiring a slanted view of aliens, and perhaps negative connotations for the word 'alien'. He is certainly not learning the meaning of a new word, as the passage implies.

The equivocation on the term 'meaning' is a hallmark of Osgood's theory and one which makes for a very confusing and debilitating state of affairs. Chomsky recognized this at an early date (Chomsky, 1959: 49f). Having originally identified the r_m vaguely with a word's 'meaning', leaving it ambiguous as to whether this was its denotation or connotation (emotive meaning) or both, Osgood later explicitly qualified his claim as follows:

I define connotative meaning as that habitual symbolic process (r_m-s_m) that occurs in the sign user when a particular sign (perceptual or linguistic) is received or produced....Here we are concerned with the "interpretative" process as Charles Morris has termed it, with the states or processes in organisms which become associated with sign stimuli through experience, hence, with "psychological" meaning. Linguists usually call this "affective meaning," which I think is too narrow a term. (Osgood, 1961: 103.)

(Osgood later adopted the term 'affective meaning'.)

The same claim was made in expanded form, attributed specifically to word meanings, in Osgood's reply to Weinrich's review of his book, The Measurement of Meaning:

The connotative meaning of a linguistic sign I define as that habitual symbolic process, x , [later r_m] which occurs in a sign-user when: (1) a linguistic sign is produced (with reference to speaker); or (2) a linguistic sign is received (with reference to hearer). It is such symbolic, representational processes (x 's) that are presumably indexed by the semantic differential. The conditions for learning denotative meanings have been well described by Skinner in his Verbal Behavior (1957), and I have tried to describe the conditions for learning connotative meanings in my Method and Theory in Experimental Psychology (1953) and elsewhere. (Osgood, 1959b: 194.)

One can object to Osgood's claims both that he tried to describe the conditions for learning connotative meaning and that the conditions for denotative meanings were well described by Skinner; for Osgood nowhere specifies in his Method and Theory in Experimental Psychology that his theory was a theory of connotative meaning. Indeed, in this

book, he describes the process involved in learning the meaning of "largely denotative" as well as "largely connotative" signs. The remark about Skinner is simply false. There is perhaps nothing more deplorable in Skinner's Verbal Behavior than his total failure to provide an intelligible description of language; and from our point of view, neither does Osgood. But the point which this latter discussion should have established beyond any question is that there is equivocation on the concept of meaning in Osgood's theoretical literature which is as perverse as it is pervasive. One is left genuinely uncertain about whether the theory was ever intended to account for the acquisition and use of natural language. Manifestly, if the theory is only concerned with explaining how people who know the literal meaning of words acquire connotations (emotive meanings) for them or the things they refer to, it is of secondary interest to those concerned with how language is first learned and how humans are able to produce and comprehend verbal messages. Of more direct import is the irrelevance of such a theory to a test such as the ITPA which is purportedly assessing the processes involved in a child's production and recovery of literal meaning from spoken and written language. The ITPA is most certainly not concerned with a child's understanding of connotations. In this respect, McCarthy's discussion of Osgood's account of how words are learned is very revealing. For he maintains that Osgood's r_m represents connotative meaning, yet sees no trouble whatsoever for the test-theory relation in this fact. But clarity with respect to Osgoodian theory is not generally in evidence in McCarthy's discussion of it, which at other points contains error and at most points is vague:

And so, using a classic conditioning explanation of early language learning (as modified by Osgood to a two-stage model), it is suggested that it is indeed possible to use words as stimuli to elicit ideas. And in classical conditioning, generally when the CS is a word, the learning is a linguistic event (decoding). Note that r_m is, for Osgood, the meaning of meaning, the connotative meaning (basis of semantic differential). If the S is an apple symbol, its associated r_m is the meaning of apple, i.e., the meaning of apple is some detachable portion (internal and/or external) of the total unconditional response to apple. This is how one learns the meaning of all kinds of symbols--not just language symbols. (McCarthy, 1974: 55.)

I will simply venture here that the status of Osgood's theory was far more ambiguous than the authors of the ITPA ever acknowledged, with McCarthy's inattention to its equivocal character and its consequences representative of an attitude present from the beginning. Every reference to Osgoodian theory to be found in the ITPA literature

bespeaks the fact that its details had been very lightly considered indeed. McCarthy's 1974 discussion of the theory, for example, is extremely general, draws on about a fifth of the available sources, and makes no mention of amplifications of his position offered by Osgood in 1963 and 1968. The test authors' regular parading of the claim that the ITPA is "theoretically based" stands in revealing contrast to their very evident disconcert for the theory being evoked. But as noted earlier, this carefree attitude toward Osgood's theory may be related to the fact that on certain interpretations of the test, the details and correctness of Osgood's position really are of no importance. The fact that we are uncertain of this is simply further evidence of the test authors' vagueness which I have just addressed.

With the character of the r_m and its acquisition thus explained, its role in language reception, production, and thought must be discussed. For it is Osgood's decoding, encoding and association processes which underly these activities that are on one interpretation, the Osgoodian Process Interpretation (OPI), under assessment on the ITPA.

The Decoding Process

Decoding is the specific process postulated by Osgood whereby utterance of words results in the occurrence of an event in the listener, which event is equated with "the awareness of meaning." Quoting Osgood: "In human communication decoding refers to the process whereby certain patterns of stimulation (usually auditory and visual) elicit certain representational mechanisms (ideas or meanings) via the operations of a complicated central nervous system." (Osgood and Sebeok, 1965: 126-7.) The process is simply this: when a listener hears a word that has been conditioned to an r_m , as previously described, the word elicits that r_m which is, according to Osgood, the meaning of the word for that listener. As we have learned, an implicit portion of a person's total behavioral response to stimulus objects (the r_m) becomes conditioned to another stimulus, the written or spoken word which names that object. Once this has been done, the utterance of the word is now capable itself of eliciting the r_m which originally was (and still can be) elicited by the object itself, and that is what Osgood calls (semantic or linguistic) decoding.

By the term semantic decoding I refer to the selective association of signs with representational mediation processes...in semantic decoding by the receiver the occurrence of specific lexical items

in messages are predictive of the occurrence in his nervous system of those representational mediation processes which he has developed in association with these signs. (Osgood, 1959a: 39.)

Careful reading of Osgood reveals that the r_m is the central mechanism in his explanation of how we learn the reference of terms while also serving as the central mechanism for the acquisition of what Osgood calls "psychological meaning" or what we would commonly call connotations. This is the source of the equivocal character of Osgood's theory mentioned earlier. Aces, places and faces are all taken as capable of eliciting r_m 's, making the subject aware of their "psychological meaning," i.e., some significance which they hold for the subject given his particular behavioral history with them. The dual function of the r_m with respect to word meaning--its being the mechanism for both reference and emotive meaning--has generated what Osgood himself rightly described as "no end of confusion." As recently as 1975 Osgood again made the attempt (after three earlier tries in print) to clarify what he was concerned with in his research into meaning (Osgood, 1975). He had commented fourteen years before, "Bousfield feels that "meaning" is a concept bound to lead to confusion. With this I most heartily agree...I know that I have contributed my own full share." (Osgood, 1961: 92.)

Certainly the most baffling feature of Osgood's theory of meaning is that the r_m is identified with what would commonly be regarded as connotative meaning, yet is the central element in persons acquiring agreement in the reference of terms. But the problems do not end there. For as the following very puzzling passage indicates, Osgood does not see his theory as explaining how humans learn the sense or significance of the words in their language and does not regard this as any matter with which the psychologist need concern himself! I honestly confess that I do not understand.

There is a third usage of the term "meaning" that has legitimacy in philosophical and linguistic circles, but need not concern psychologists. This is what may be called the signification of a term. This is the "semantic rule" for its usage, as distinct from a mere cataloguing of its uses (i.e., its denotation). The terms father and me mentioned above actually provide illustrations. The denotation of my me and father is obviously different from the denotation of your me and father, but the semantic rule or signification is the same: me refers to the speaker, whoever he may be, and father refers to the speaker's male parent (in this language-culture group, but not necessarily for others). The reason I say signification is not particularly important to psychologists is that it is part of the metalanguage about language used by third-person observers, not part of the behavior of primary sign users. (Osgood, 1961: 104.)

We can summarize the account of decoding as follows. Decoding is the process in which the utterance, written or spoken, of a word, elicits in the subject the r_m that has been conditioned to it. The r_m is an implicit response which has been derived from the subject's total behavioral response to the thing signified by the word that is uttered. This implicit response, the r_m , represents the psychological meaning of the object signified, i.e., the personal significance of the object for the subject as that significance has been determined by his particular behavioral history. All instances of word learning consist of the association of this psychological meaning to a given word; and, assuming that the subject is exposed to language correctly used, the pairing of word and object will result in his learning what the word refers to. In decoding, the elicitation of the r_m by the utterance of the term to which it has been conditioned results in (a) the speaker correctly identifying what is being talked about and (b) becoming aware of the psychological meaning of the term. For many terms it is assumed that the psychological meaning will be quite common among individuals, but for many others it may vary considerably. Variation in psychological meaning does not, however, adversely affect the conduct of communication since this is dependent upon speaker-agreement on the reference of terms, not their connotations. As Osgood puts it, "(T)he ideal case for effective communication...is simultaneous denotative and connotative agreement between persons A and B..." (Osgood, 1961: 103.) "... (B)ut connotative agreement is not necessary for denotative agreement to occur--indeed it is entirely irrelevant to it." (Osgood, 1959b: 194.)

The Encoding Process

"By the term 'semantic encoding' I refer to the selective association of representational mediation processes (r_m 's) with spoken or written linguistic responses." (Osgood, 1959a: 39.)

The encoding process, as can be recognized from the above description, consists in a dependency relation between an external observable event and an internal unobservable event. The same is true, as we have already seen, of decoding. In fact, encoding is readily understood as the converse of decoding. In the latter, the utterance serves as stimulus, with the occurrence of the internal r_m being its conditioned response. Now, in encoding, we have the r_m serving as internal stimulus with the utterance being the conditioned response.

Quoting Osgood:

(B)y the term semantic encoding I refer to the selective association of representational mediation processes with spoken or written linguistic responses....In semantic encoding by the source the occurrence of specific lexical items in his messages is indicative of the immediate prior occurrence in his nervous system of the corresponding representational mediation processes... (Osgood, 1959a: 39.)

(T)he process of intentional encoding of speech as it occurs in spontaneous talking in contrast to reading aloud, say, is one in which the self-stimulations from representational (symbolic) reactions elicit those motor integrations with which they have been associated and hence result in lexical items in spoken messages. The representational self-stimulation, s_m , may be termed an intention for convenience in discussion. Thus, what we are talking about here is the "expression of ideas" or encoding of intentions. (Osgood, 1957b: 400-02.)

The distinction which Osgood makes between "spontaneous talking in contrast to reading aloud" is significant. The utterance of the word 'fish' by someone who has read the word on the printed page would be explained by Osgood in terms of that utterance being under the control of the printed word as external physical stimulus. But how then do we explain the utterance of 'fish' by a man sitting on an empty train platform or staring at a self portrait? As with most verbal behavior, there would appear to be no possibility of explaining the utterances such as these as being under the control of specifiable local stimuli. As Osgood puts it in one place, "One can also conceive of a person spontaneously emitting (or encoding) words that are meaningfully relevant to his external and internal situation without requiring the immediate presence of external S's that have been conditioned to these words as responses." (Osgood and Murray, 1963: 97-8.) One certainly can so conceive, since this is manifestly an accurate description of the majority of human verbalization! A major advance which Osgood claims for his theory is that of freeing utterances from the stimulus control of the object they refer to, as is the case with Skinner's tacts. Skinner's account, according to Osgood, "can handle only sheer labeling in the actual presence of object or situational cues..." (Osgood, 1963d: 285), whereas the mediational response theory can "account for the abstract use of 'thing' language when the 'thing' is not present...because anything which sets the mediation process in action, with or without the physical presence of the object is now capable of producing the label..." (Osgood, 1963d: 285.)

The nagging question is, having conditioned the vocalization of words to the occurrence of a private stimulus event (r_m), how does Osgood get the r_m itself elicited in these cases of spontaneous speech?

Suffice it to say that Osgood interprets his remark about "anything which sets the mediation process in action" being capable of doing this job very broadly. The prime movers enlisted for this crucial role are the members of "one important class of manipulatable antecedents... which we call motives." (Osgood, 1957b: 414.) In this class we find everything from a keep-things-to-oneself motive (Osgood, 1957b: 409) which accounts for what we don't say, to an all-purpose motive which is invoked to account for all that we do say: "With regard to the control of spontaneous conversation, I think we may safely postulate a very generalized motive 'to please one's correspondent.'" (Osgood, 1957b: 377.) Between these two motives, serving literally and figuratively as the extremes, Osgood includes a host of other particular wishes, interests, desires, etc., all under the heading of intentions. Any premonition that Osgood, the anti-dualist, has now landed us with a hoard of immaterial entities mystically capable of physically eliciting the r_m 's which result in utterances is dispelled by the fact that intentions are given an adapted Hullean characterization by Osgood in which they are all treated as secondary drive states (r_g 's). We won't ask what elicits the r_g 's. For our purposes none of the motivational devices or their integration with Osgood's semantic encoding process are of concern. Only the character of the latter process and not its ignition system must be understood. Whether or not the utterance of a given word is regarded as the end product of a process set in motion by an external or mysterious internal stimulus is beside the point. What is crucial is simply the understanding that linguistic encoding as Osgood characterizes it, takes place when r_m 's as antecedent events elicit utterances of single words as their responses. The occurrence of the r_m for a given word is a necessary condition for that word's utterance. Conversely, the utterance of a given word is sufficient evidence for attributing to the speaker the prior occurrence of the r_m for that word. To conclude in Osgood's own words:

In human communication encoding is the process whereby a speaker's intentions become coded in those vocal reactions which produce intelligible sounds in a given language. This is commonly called the 'expression of ideas.' It involves both the formation of complex motor skills and their association with representational mechanisms of the sort discussed above. (Osgood and Sebeok, 1965: 128)

The encoding process only explains single-word utterances. Osgood's ponderings on the role of the r_m in sentence production are to be found in Osgood, 1963c, 1971. A refutation is provided by Fodor, Bever and Garrett, 1974.

The Association Process

An understanding of Osgood's association process is assisted by turning to Hebb, whose hypotheses in this area significantly influenced Osgood. Quoting Hebb:

To "mediate" means to form a connecting link, and the simplest function of the mediating process is to connect S with R. Theoretically, however, a mediating process can be excited by other mediating processes instead of by its own sensory event, and when a number of mediating processes interact in this way--being excited by each other as well as by sensory events--the result is thinking; so, theoretically, a mediating process might also be defined as the unit or elementary component of thought, replacing the term "idea." (Hebb, 1966: 90-1.)

The crucial notion here is that internal events could become associatively linked to one another. These associative connections (habits, associations) obey the same laws traditionally invoked to describe connections between external stimuli and responses. Hence the laws of contiguity, frequency, similarity, inhibition, etc., should apply to internal events just as to external events. Since the r_m 's or word meanings as internal events could become associated in such a way, for example, that the occurrence of r_m A was a sufficient condition for the occurrence of r_m B, and this in turn for r_m C, we have the possibility of internal sequences of meanings, i.e., we have the mechanism for thought as postulated by traditional learning theory. Again, as Hebb put it, "When a number of mediation processes interact in this way...the result is thinking." (Hebb, 1966: 91.)

Osgood, in fact, says very little about the Association Process. One of his few references corresponds to that of Hebb:

An inference about the "association structure" of a source--what leads to what in his thinking--may be made from the contingencies (or co-occurrences of symbols) in the content of a message. This inference...is anchored to the principles of association which were noted by Aristotle, elaborated by the British Empiricists, and made an integral part of most modern learning theories. On such grounds it seems reasonable to assume that greater-than-chance contingencies of items in messages would be indicative of associations in the thinking of the source. If, in the past experience of the source, events A and B (e.g., references to FOOD SUPPLY and to OCCUPIED COUNTRIES in the experience of Joseph Goebbels) have often occurred together, the subsequent occurrence of one of them should be a condition facilitating the occurrence of the other: the writing or speaking of one should tend to call forth thinking about and hence producing the other. (Osgood, 1959a: 54-5.)

The idea, as I understand it, is that the co-occurrence of certain events or objects in a person's experience leads to the co-occurrence of their r_m 's which then form an associative chain. If any

member of this chain is elicited, it is probable that other members will be elicited, and we thus have the occurrence of a series of meanings or a thought. Via the encoding process, given the additional ingredient of a motive to speak, these thoughts become public. But the central notion seems to be that thinking is a matter of r_m 's eliciting one another and the r_m 's that will elicit one another are those which, by virtue of contingencies in the subject's experience, have frequently occurred together. Quoting Osgood:

Our general assumption is that (1) contingencies in experience come to be represented in (2) an individual's association structure by patterns of association and dissociation of varying strengths, which help determine (3) the contingencies in messages produced by this individual. (Osgood, 1959a: 56.)

Not surprisingly, a person's behavior in free recall or free association tests is taken as prime evidence of the association process at work. Osgood described such behavior as a result of the operation of a "semantic association hierarchy," i.e., the words uttered by the subject in response to the stimulus word to correspond to the frequency with which their r_m 's have been paired with occurrences with the stimulus word's r_m in the experience of the subject. Quoting Osgood:

(B)y virtue of the innumerable redundant sequences in which signs and the events they signify have been transitionally related, this mediation process, as an antecedent stimulus event, s_m , has become associated with a hierarchy of subsequent mediation processes, as dependent response events. This, of course, is what we talk about as "the association of ideas." The strengths or probabilities of these alternative associations will vary with the frequency of event or sign pairing in experience. (Osgood, 1957b: 411-12.)

Apparently Osgood would agree with the remark of Berlyne, another mediational theorist, "We shall refer to a string of cue-producing symbolic experiences as a train of thought." (Berlyne, 1965: 27.)

The association process defined by Osgood as "a dependency relation between any antecedent and any subsequent neural event, this relation being variable in strength and acquired through experience...." (Osgood, 1957b: 358-9), is dependent upon the past experience of the learner to such an extent that any original thinking would appear to be entirely beyond the capacity of the association process to explain. Linked as it is to past experience in such a way that thought mirrors reality, Osgood's association process would appear inadequate as the mechanism underlying the most commonplace sorts of problem-solving, analysis, weighing of alternatives, forecasting, deductive reasoning, etc. Quoting Judith Greene:

Concerning the relation between past experience and current thinking ...S-R theory certainly has to go into a lot of contortions to explain the production of the less-reinforced or completely novel responses which are the hallmark of solving a problem....The other main difficulty is that S-R associations allow only one kind of relation between a stimulus and the response it elicits. This is true not only of overt Ss and Rs but also of all the little r-s mediating links. It is difficult to see how even the most complex interactions between associations of this kind could account for behavior that is clearly influenced by other relations, such as those holding between numbers, for instance. Another case would be responses governed by other sorts of relationships such as answers to analogies problems, e.g. 'As foot is to leg, hand is to ----', and 'As foot is to shoe, hand is to ----'. The question of whether 'arm' or 'glove' are more or less likely responses to 'hand' is quite irrelevant to our knowledge of the relationships required by the task. (Greene, 1975: 32-3.)

The point here is simply that Osgood's association process would appear an inadequate mechanism for the explanation of quite common sorts of thinking, and it is not certain from the little that he says on the subject that it is even being proposed as such. This process which receives limited development by Osgood would appear to be similarly limited in its explanatory capacity. The sort of problem-solving tasks which are presented on the ITPA as measures of the association process are assuredly beyond the capacity of such a mechanism. Indeed, they are the very sorts of analogy problems which Greene cites as counter-example to mediational accounts of thinking. It is again, therefore, very revealing both of the understanding of association as a process and of the uncertain relation between the ITPA and Osgood's theory that McCarthy in his summary of Osgood's theory does not describe association as a habit mechanism, as he does both encoding and decoding. Rather, he lets the term stand, in so many words, for verbal reasoning:

Thus, decoding is that collection of habits required to ultimately obtain meaning from linguistic stimuli; encoding is that collection of habits required to ultimately express oneself in common words or gestures....The process of association is entirely internal and largely inferred. We've defined it as the sum of those activities required to manipulate linguistic symbols internally. (McCarthy, 1974: 60.) (Italics mine.)

This completes the presentation of Osgood's theory with its three processes that underly language behavior. It is their functioning which, on Osgoodian Process Interpretation, is purportedly being assessed by the ITPA.

V. CRITIQUE OF OSGOODIAN THEORY

As explained, Osgood maintains that learning the meaning of a word consists in having part of the response, originally elicited by the thing signified by the word, become conditioned to utterances of the word as a conditioned response. Given this understanding, only words that signified some spacio-temporal object, capable itself of eliciting a response, could acquire meaning. Or, to put this another way, an assumption about words which is implicit in Osgood's account of word learning is that they are names for things. This is not an assumption which Osgood can avoid. His central postulate binds him to it.

It is difficult to see how a satisfactory account of language acquisition and use could be constructed on such a narrow conception of words. It is also difficult to conceive of Osgood, whose theory is quite complex, consciously binding himself to such a simplistic position, which is so open to criticism. Manifestly, many words do not name things--'if', 'but', 'when', 'perhaps', 'yesterday', etc. However, Osgood's frequent assertions about words standing for things are never qualified by the word 'some.'

Words represent things because they produce some replica of the actual behavior toward these things. This is the crucial identification, the mechanism that ties signs to particular stimulus-objects and not to others. (Osgood, 1953: 695-6)

The mediation process must include some part of the same behavior made to the object if the sign (word) is to have its particularistic representing property. (Osgood, 1952: 204)

(T)he major difficulty with most attempts to deal with 'the meaning of meaning' has been their failure to offer any convincing explanation of why a particular sign refers to a particular object and not to others. The mediation hypothesis offers an excellent and very convincing reason: the sign 'means' or 'refers to' a particular object because it elicits in the organism employing it part of the same behavior which the object itself elicits. (Osgood, 1953: 412)

This conception of words would appear to be inevitable (compulsory) given the intention to account for word learning in terms of Pavlovian conditioning. For the minimal elements in such an account are a stimulus which naturally elicits a response from the subject and another neutral stimulus to which the unconditioned response is to transfer. Hence, there is little wonder that Osgood's examples of word learning are almost exclusively concrete nouns--'apple', 'hammer', 'ball'--since it is only words such as these which have physical

referents that are susceptible to description within the Pavlovian scheme (See Greene, 1975: 87). But regardless of what motivated it, the conception of words as names for things is severely impoverished. No verb, adverb, preposition, conjunction, article, adjective, auxiliary, or abstract noun can intelligibly be regarded as naming or standing for an environmental object. What is the thing signified by 'violates', 'shrewdly', 'trite', 'of', 'but', 'could', 'possibility', etc., which could originally elicit the behavior of which a portion (r_m) is to become conditioned to utterances of them? It appears unarguable that Osgood simply has no position on how words without physical referents are to be learned; and since such words constitute a major portion of the natural languages whose acquisition is being explained, Osgood's theory manifestly fails in this task.

What I have indicated is that Osgood's account of language acquisition rests on an assumption about language as a system that is altogether inadequate. A related challenge which could be developed is that for the broad category of words known as indexicals (or deictic words) such as 'I', 'it', 'then', 'here', and 'now' and all demonstrative pronouns, the referent is determined by and varies with the context of their utterance. In the sentence, "I will show it to them here," the referent of the terms 'I', 'it', 'them', and 'here', will depend on the conditions of who is uttering the sentence about what, to whom and where. But if, as Osgood maintains, words have meaning by virtue of eliciting behavior appropriate to their referents, words such as these must change their meaning every time they are uttered in a different context. And this is nonsensical. The phrase, "he is not here," means the same in every context, though the person and place talked about need never be the same. The essence of this criticism is that the indexicals, like the broad categories of words discussed earlier, do not conform to the characteristics of words assumed by Osgood, and necessary for his explanation of how they are learned. Both of these are quite characteristic challenges to associationist theories of language acquisition. Their force derives from showing the theories in question to be dependent upon assumptions about words which are inconsistent with what is known about natural languages. A great many words cannot be characterized in the way that is required by the theorists. Since this is so, such theories will at best be of severely limited application.

Because of criticism which has been advanced against Osgood, with which I do not want this criticism confused, I must say that the

challenge is not that Osgood requires co-occurrence of word and object in the experience of the subject in order for words to be learned. He does not require this--words may be learned as assigns. The criticism is that there are many classes of words which do not label things. Learning the meaning of words such as these cannot even be intelligibly described as having a portion of the behavior originally elicited by the thing signified become conditioned to word utterances, and to that extent, are altogether unaccounted for in Osgood's theory. I do not believe that Osgood can escape this criticism, though the attempt has been made, as will emerge later, via the proposal of assign learning.

One escape route which might suggest itself is to considerably enrich the category of "thing signified" such that the thing signified by adjectives will be properties; by verbs--processes; by prepositions and conjunctions--relations; etc. This gambit proves to be empty, however, as is argued by Fodor with customary verve:

It seems clear that the homogeneity of the naming relation can be maintained only at the expense of postulating metaphysical objects to stand in the sort of relation to common nouns, verbs, adjectives, prepositions, etc., that physical objects have to the proper nouns that name them. That is, the simplicity that is claimed for semantic theories based on naming is characteristically gained at the price of an extremely complicated (not to say extremely dubious) ontology. It is clear, for example, that adjectives cannot name the sorts of things that proper nouns do. For while the referent of a proper noun has a location, a date, an individual history, etc., not one of these things can be said about the referents of adjectives. If we nevertheless assume that adjectives are names we are ipso facto committed to the existence of a special kind of thing--a property or universal--that is tailor-made to be what adjectives are the names of. Similarly, "activity" becomes a technical word when it begins to be used as a cover term for the sort of thing a verb "names"; and one psychologist (Skinner, 1957, p. 121) has supposed that "pastness" must be "a subtle property of events" in order that there should be something named by the ed in "violated" and the t in "lost." One wonders, indeed, whether this project is not doomed to circularity. It is uninformative to say that all nouns name objects if it turns out that all such objects have in common is that they are named by nouns; nor is it easy to see what else of interest is common to, say, short naps and tall stories. (Fodor, Bever, Garrett, 1974: 145-6)

It is this sort of "complication of the ontology" that must be going on when Osgood says that the word 'good', like the word 'ball' is learned by being paired in the experience of the learner with the object it signifies: "Primary signs (e.g., the adjective GOOD) acquire meaning through direct association with significates (e.g., gratifying situations), a representational portion of the total behavior to the significate becoming associated with the sign as its mediation process."

(Osgood, Suci, Tannenbaum, 1957: 286) But in what sense is a gratifying situation or any situation a stimulus object? When the pile of blocks doesn't fall over? When it does fall over? When it's time for dinner? When it's time for bed? Since Osgood's theory relies upon differences in stimulus properties among physical objects to differentiate the meanings of the words which stand for them ("the sign means or refers to a particular object because it elicits in the organism employing it part of the same behavior which the object itself elicits" (Osgood, 1953: 412)), he must specify what the regularities are, i.e., regularities in sensory properties which are common to gratifying situations. This enterprise is assuredly doomed to failure. Not only is the situation an abstraction, but its goodness is not a sensory quality, but an appraisal imposed upon it. The aroma of charcoal-broiled steak is loathsome to a vegetarian, while delicious to the steak-lover, yet the stimulus properties are the same. John B. Carroll echoes Osgood's claim that 'good' refers to gratifying situations in asserting that "some signs, like 'hi' and 'thanks' bear referential relationship to certain kinds of social situation." (Carroll, 1964: 6.) Fodor's brisk treatment of Carroll's position takes on Osgood as well:

Though desperation might suggest that "hello" is the name of a situation in which persons are greeting one another, this is a case in which the counsels of desperation ought to be resisted. "What is the name of this situation?" is a bizarre question (compare "What is the name of this dog?") and "This situation is named 'hello'" is barely English (compare "This dog is named 'Posh'"). If it is still insisted that all words are kinds of names, then it must be replied that there must be as many kinds of names as there are kinds of words and that there is no reason for supposing that the relation between names like "Posh" and their bearers provides a model for the relation between names like "hello" and their bearers. (Fodor, Bever, Garrett, 1974: 144-5)

I believe enough has been said to show that a vague notion of "thing signified" goes no distance toward solving Osgood's problem of how the meanings of words which do not label physical things are learned. The original criticism thus remains a significant challenge to his position. In the opening pages of his Philosophical Investigations, Wittgenstein comments on the characterization or picture of language upon which St. Augustine based his speculations about language. The characterizations of Augustine and Osgood, despite the vast separation of these men in both time and the nature of their interest in language, are remarkably similar. The only essential difference is that Augustine equates the meaning of a word with the thing it refers to, whereas Osgood equates it with a portion of the behavior elicited

by the thing it refers to. What Wittgenstein said of Augustine may also be addressed to Osgood:

Augustine does not speak of there being any difference between kinds of words. If you describe the learning of language in this way, you are, I believe, thinking primarily of nouns like "table," "chair," "bread," and of people's names, and only secondarily of the names of certain actions and properties; and of the remaining kinds of word as something that will take care of itself.... Augustine, we might say, does describe a system of communication; only not everything that we call language is this system. And one has to say this in many cases where the question arises, "Is this an appropriate description or not?" The answer is: "Yes, it is appropriate, but only for this narrowly circumscribed region, not for the whole of what you were claiming to describe." (Wittgenstein, 1968: 2.)

But does Osgood succeed in explaining how children learn even "nouns like 'table'"? As explained, we are offered two versions of how this is achieved: one in which the word and its object are actually paired in the experience of the learner--primary sign learning, and the other in which actual experience of the object signified does not occur --assign learning. These will be discussed in turn.

Primary Sign Learning

Osgood's own example of primary sign learning merits a closer look. It is presented as a simple retelling of the classical conditioning story where the hungry dog's unconditioned response (salivation) to the unconditioned stimulus (food) is transferred to the conditioned stimulus (sound of a buzzer) by means of consistently pairing the sound of the buzzer with the presentation of the food. Eventually the sound of the buzzer alone elicits the response of salivation. In the retelling, a child is substituted for the dog, a ball for the food, and the sound of the utterance of the word 'ball' for the sound of the buzzer:

It is characteristic of human societies that adults, when interacting with children, often vocalize those lexical items in their language code which refer to the objects being used and the activities underway. Thus Johnny is likely to hear the noise "ball," a linguistic sign (S), in frequent and close continuity with the visual sign of this object....(T)he linguistic sign must acquire, as its own mediation process (r_m s_m), some part of the total behavior to the perceptual sign and/or object--presumably the mediation process already established in perceptual learning... should tend to be transferred to the linguistic sign. Thus a socially arbitrary noise becomes associated with a representational process and acquires meaning, e.g., a unit in linguistic decoding. (Osgood, 1957a: 94-5.)

In describing the conditioning of the dog, we customarily say that the buzzer has become a sign of food to the dog. Quoting Max Black:

Faced with the facts that have been reported, it is very natural for the layman (and the scientist too) to say that T has become a sign of S for the dog: or, in anthropomorphic language, that the sound of the buzzer 'means' to the dog something like 'Food coming!' ...The next step is to identify the meaning of the 'sign' (the buzzer, the warning cry) with some aspect of the reaction of the responding animal. I use 'reaction' here, more broadly and more loosely than 'response', to stand for whatever it is about the animal at the instant of receiving the sign that makes that sign bear its definite signification or meaning. We must not assume that the 'reaction' will be identical with the 'conditioned response.' (Black, 1972: 216-17)

Osgood would have us believe, then, that in precisely the same way that the buzzer becomes a sign for the dog, the word becomes a sign for the child. Indeed, Osgood stresses in many places that the mediational account was not specially constructed to account for verbal learning and behavior, but was merely an extension of learning theoretic principles already believed successful in the explanation of infra-human behavior (Osgood, 1963c; Osgood, 1971b). If we examine the supposed parallel at all seriously, we see that the learning which takes place in the case of the child is not language learning. The child may have acquired a meaning for certain word utterances, but she has certainly not learned the meaning of the word.

In the first place, it must be realized that as far as learning the meaning of words is concerned, the presence of the object is not necessary. Osgood himself concedes, as he must, that the vast majority of words could not be learned by pairing them with their object if for no other reason than that adherence to this position would lead to the unacceptable consequence that people could not learn the meaning of words signifying anything that they had not actually encountered in their own experience. And there is the well-rehearsed litany of types of words which have no physical referent. Obviously, the meaning of these words and of words which do have physical referents are regularly learned without regular pairings of word and referent. Recognition of this feature of word learning alone reveals an extreme lack of comparability between it and the conditioning of the dog. The thought of a dog learning that a buzzer means "food coming" without the food ever being paired with the buzzer in the experience of the dog is patently absurd.

But the suggested parallel between the conditioning of the dog and the child learning the meaning of a word, which McCarthy, it must be noted, accepts unquestioningly (See McCarthy, 1974), is unacceptable for deeper reasons than this. Upon examination, one finds that in a

description of the two cases, the central terms 'stimulus' and the 'response' are being employed in different senses and that the conditions which obtain during the word learning situation share none of the important features of Pavlovian conditioning. The putative resemblance of the two cases is thus achieved via the combined influence of equivocation and vagueness in the description of the word learning situation which, when disclosed, renders the primary sign account vacuous.

Skinner's equivocation on his central theoretical terms with respect to language learning and use was exposed by Chomsky in his incisive review of Verbal Behavior. Encapsulating this portion of his critique in a later essay, Chomsky remarked: "The notion that linguistic behavior consists of 'responses' to 'stimuli' is as much a myth as the idea that it is a matter of habit and generalization. To maintain such assumptions in the face of actual facts, we must deprive the terms 'stimulus' and 'response' (similarly 'habit' and 'generalization') of any technical or precise meaning." (Chomsky, 1973: 237) This point is expanded upon by Fodor:

(W)hile there can be no objection to considering the verbalizations of fluent speakers to be "linguistic responses," one must not suppose that, in this context, "response" means what it usually means: "A stimulus-occasioned act. An (act) correlated with stimuli, whether the correlation is untrained or the result of training." On the contrary, a striking feature of linguistic behavior is its freedom from the control of specifiable local stimuli or independently identifiable drive states. In typical situations, what is said may have no obvious relation whatever to stimulus conditions in the immediate locality of the speaker or to his recent history of deprivation or reward. Conversely, the situation in which such correlations do obtain (the man dying of thirst who predictably gasps "water!") are intuitively highly atypical. (Fodor, Bever, Garrett, 1974: 163-4)

The nontechnical and therefore noninformative use by Osgood of such terms as 'stimulus', 'response', 'conditioning', etc., in his descriptions of language acquisition and use, is not a point which I intend to belabor by the gathering of texts. Many have already appeared earlier in this paper. The existence of such equivocation, the breezy alternation between a strict and loose employment of theoretical terms, is a commonplace of the associationist literature on language, and Osgood is no exception. Thus, for example, when Osgood says that environmental objects such as tables, hammers, spiders, etc., are like food, unconditioned stimuli, we are at a loss to know what their unconditioned response equivalent to salivation must be. When we are told that new words are learned by having utterances of them paired with the

objects, we are at a loss to know why any of the hundreds of words which are as certainly co-occurring in these situations do not become conditioned to the object. We must be aware that the necessity of frequency has been declared, not shown, and that the learning of words from single occasions of their utterance is a commonplace. Conversely, we must be aware that in the face of a long history of redundancies, habitual incorrect "responses," e.g., mispronunciations or erroneous uses of terms, are capable of instant "extinction" simply upon being called to the attention of the offender--again in violation of behaviorist principles. Finally, we must be cognizant of the fact that when such things as the thoughts of a future event are treated as stimuli, and refraining from some action a response--the terms 'stimulus' and 'response' have lost all defining characteristics, labelling neither physical nor observable events (see Osgood, 1953). In short, the language learning situation is at variance with the Pavlovian conditioning paradigm in so many theoretically important respects that its comparable description in stimulus-response terms amounts to a serious deception. Ruth Clark, whose discussion will serve as a summary, addresses this issue in specific regard to mediational theories.

In laboratory studies of learning by contiguity, on which mediation theory leans for its scientific support, several constraints on such learning have been discovered, which do not seem to apply to the language situation. Pavlov himself was well aware of the fact that language functioned differently to salivation in dogs, and subsequent Russian work has taken this into account, though American work by stimulus response theorists has not. Among the constraints are the following: if contiguity learning is to be successful the stimuli need to be presented together very frequently, the time interval between them has to be very short, the new stimulus must always occur first, and the connection has to be revived periodically or extinction will occur....The mediation theorists are asking us to believe in a stimulus response connection theory which is not tied to any of the constraints of traditional contiguity theory, and to accept its validity as an explanatory device. The trouble with mediation theory is that it is so free from constraints that it explains too much. It can explain practically anything after someone else has discovered it, but it can make few clear-cut predictions. (Clark, 1975: 306)

If we look specifically at Osgood's account of primary sign learning in light of the previous discussion, its vacuity becomes apparent. Strictly speaking it should be the case, in accordance with this account, that the meaning of a word could be learned by hearing the word alone paired with one of its referents. This would be the precise parallel to the buzzer-food pairing in the case of the dog, the case upon which Osgood's primary sign learning is based. The

utterance of a single word, not widely varying samples of correct discourse containing the word, should be paired with the object. But it is utterly inconceivable that a word could be learned in this way. Merely imagining competent language users ritually uttering "grass" in the presence of grass for the benefit of the new learner is enough to convince us that this view is at extreme variance with the character of the speech to which children are typically exposed. And the difference between this imagined scene and the reality of what occurs in the nursery, etc., should not be ignored. So extreme is this case for word learning, i.e., hearing solely the word paired with one of its referents, that not unsurprisingly Osgood avoids it. In none of his examples of primary sign learning do we have simple word-object pairings and this is no doubt due to the fact that it is inconceivable that the meaning of words be learned in this way. I quite agree with Harrison's analysis of what such pairings should result in:

There is, for example, a gap, which the theory fails to fill, between, on the one hand, attending to an event and drawing inferences from the fact of its occurrence, and on the other, interpreting it as an utterance of a name. If it in fact happens that a child hears the word 'bottle' uttered at all and only those times when a bottle is present or when one is just about to appear, one can see why the child should come to take the noise 'bottle' as a sign that it is about to be fed. One would expect, that is, some sort of reflex response connected with the expectation of food to become conditioned to the noise "bottle" as stimulus. But why should the child take the noise "bottle" as a word meaning, "bottle"? (Harrison, 1972: 54)

The primary sign account, on the basis of all the foregoing, would appear to be demonstrably inadequate.

Assign Learning

According to Osgood, assign learning is the way in which the vast majority of words are learned. This alone establishes the account as worthy of attention. Further importance attaches to such a consideration since it is seen by some as enabling Osgoodian theory to surmount difficulties which would otherwise be very damaging (see Terwilliger, 1968; Houston, 1972). Quoting DeVito:

One of the theory's assumptions is that the learning of words takes place through association with the actual object...yet it is obvious that the meanings of many words are learned and understood without their being associated with the actual objects...This objection is at least not beyond the theory's capacity to handle (cf. Osgood, 1953). There is nothing inherent in the model which demands that the actual object be paired with the word. The original stimulus (S) can be one which has acquired a certain meaning and which can then function in much the same way that other stimuli (that is,

actual objects) do. Thus, for example, the word mermaid can be learned from being associated with various other stimuli, such as pictures, verbal descriptions of tails and torsos, etc. and not necessarily from association with actual mermaids. (DeVito, 1970: 76-77)

Yet further motivation for examining assign learning derives from the fact that McCarthy would have users of the ITPA believe that the assign account is a satisfactory account for a broad range of human learning:

In this operation, several signs (written or spoken words) are presented simultaneously, and the representational mediators they elicit join (via conditioning) to form a unique meaning (r_{ma}) associated with the assign /S/. This is how, for example, we can learn about things, places, people, and so on, that we have never experienced firsthand. (McCarthy, 1974: 60-1)

How assign learning takes place has already been described. It is summarized briefly as follows:

(T)he assign is consistently associated with a certain sample of primary signs and gradually acquires as its mediation process the most common elements of mediators for the signs with which it appears. In other words, the meanings of assigns develop out of the context of primary signs with which they occur. (Osgood, Suci, Tannenbaum, 1957: 287)

This account of word learning is susceptible to decisive refutation. The requirements for assign learning are (a) the new word is uttered, (b) previously learned words (words which elicit their r_m 's) are uttered in conjunction with it, and (c) this occurs with some frequency. The consequences of this position make clear its inadequacy. It is consistent with the account, for example, that a child will learn any new word by hearing it paired with any known word. Thus the utterance of 'fanciful' with 'doggie', 'truck', 'bye-bye', etc., is sufficient, in theory, for the child to learn its meaning. Furthermore, there is no restriction on the use or character of the previously learned words, so the same set may be used to teach all other new words. This may be done by ritual chanting or left to the workings of unmonitored ordinary discourse--in either case the conditions for assign learning are satisfied, since previously acquired r_m 's will be elicited and thus available for the "distillation of the new meaning." There is no reason why different children should not be exposed to different sets of previously learned words, since the choice of known words is subject relative. And there is no reason in principle why the entire lexicon could not be acquired via the frequent pairing of the first two words a child learns with all others. Readers unfamiliar with the word 'febrifuge' should acquire its meaning by frequently reciting this sentence to themselves, assuming that at least some of the words in it

are known! A further consequence of the assign account is that explicit definitions will be no more effective than any other sentence in teaching the meaning of new words, and of equal efficacy, whether delivered properly or in scrambled word order. All that is necessary is that previously learned words be uttered in conjunction with the new one. Many other arguments of a reductio ad absurdum type could be raised against the assign account, but enough has been said to show that it is in trouble.

One wonders how such a woefully inadequate thesis could have been advanced. Certainly one of the principal factors responsible for its uncriticized existence has been the vague way in which it is presented. Osgood claims that the "analysis of assign learning appears in many places in my writings," but when one confers with the relevant passages, one finds the notion conspicuously unanalyzed. We are told that the meanings of assigns are "literally assigned" to them via association with other signs, that assign meanings are "distilled" from the r_m 's of other signs, etc. Metaphor is the characteristic explanatory device. The reader is left in bewilderment, unsure of whether any challenges are on the mark because the mark has not been made. The character of the explanation, I suggest, is traceable to Osgood's primary paradigm wherein the meanings of words are portions of behavior toward the thing signified. In the cases where Osgood could not provide the actual thing, he attempted to provide a surrogate in the form of words describing or labelling other things associated with the thing signified by the new word. Thus, in all of Osgood's examples, the words that are supposed to occur frequently with the new word are not a random set, but a set of associated words. A good deal of imagination is called for in accepting these stories:

Thus in the reading of young children non-sense forms (prior to gaining meaning from context) like thief appear with meaningful forms like gun, bad, man, night, dangerous, and against-good-policeman while nonsense forms like elves co-occur with meaningful forms like tiny, quick, magical, fairy-story, and so on. (Fodor, 1965b: 406)

In other words, the meanings of assigns develop out of the context of primary signs with which they occur. As the child who has learned to read with some facility moves through a story, the matrix of familiar signs limits the possible meanings which the new and unfamiliar words can have. And since the adult story writers are reasonably consistent in the signs they put together (PRIESTS are kind and calm, LIBERTY is good and free, VICIOUS is something characteristic of wild animals, bad men, and so on), a reasonably stable assign-meaning develops. Certainly, the vast majority of lexical items employed and understood by adult humans are assigns in this sense. (Osgood, Suci, Tannenbaum, 1957: 287)

What Osgood claims here is that new words ("nonsense forms") will, as a matter of fact, be paired with what might be called related words. Thus, 'thief' with 'gun', 'bad', etc. The appeal to this empirical condition is required for there to be some limits on the previously learned words from which assign meanings are to be distilled. Thus, Osgood relies on story writers and speakers being "reasonably consistent in the signs they put together" in the sense specified above. This is all very fanciful and presented without any evidence. But even if every new word did occur in conjunction with tailor-made sets of associated terms, Osgood's account of assign learning would remain in its hopeless state. For if this new condition is to serve its purpose of placing some constraint on the sorts of utterly random word pairings that are possible, it involves the entire exercise in circularity. This becomes apparent if one asks oneself how it is that for the one hearing the supposedly meaningless assign-to-be for the first time, the r_m 's for the associated words will form the pool from which its r_m is to be derived. According to theory, the r_m for every previously learned sign uttered in conjunction with the assign-to-be is being elicited. Only someone who already knew the meaning of the new word could recognize certain of the words uttered in conjunction with it as being related to its meaning, while others are not. The utterer has this knowledge, but the new learner does not. Yet in Osgood's examples the learner, or more correctly his distillation mechanism, makes this discrimination. It seems that far from hearing it as a meaningless noise Osgood's naive learner must already have the concept to be acquired. A further problem for this program is that there appears to be no way of distinguishing when the learning of a word comes to an end and it is available for use. Will the meaning of every word be in a constant state of flux, since words constantly occur in different linguistic contexts? What principled explanation is there for any word's verbal context conferring or altering its meaning in some cases, but leaving it unmodified in others? When does learning the word's meaning stop and knowing its meaning begin?

The central flaw of the assign account can be stated as follows: It provides not at all for the constructive role of the learner. There is no room for the learner evaluating what he hears, relating what he presently hears to what he has heard in the past, understanding that what is being uttered is a definition of a new word, an example of its use, a list of the things it refers to, what it is the opposite of, etc.

In Osgood's theory the learner is totally passive, the content of the meaning of a new word being entirely dependent upon the circumstances in which it is learned. Yet these need have nothing to do with its meaning. Quoting Findlay:

I should therefore like to frame the two following counter-assertions: (a) that it is never right to argue that because some observable circumstance mediates the communication of a meaning, that it necessarily plays an important role in the communication, or that it does more than touch it off; (b) that it is even more wrong to argue that because some circumstances cannot be observed when an expression is taught it is not playing a vital role in the teaching ...A man might learn what it is for something to be so and so, or for such and such to be the case, by being shown something that illustrated the exact opposite of the sense we desire to impart, or by being shown something that vaguely approximated to it or pictured it, or by being shown something of which it was in some sense a natural complement, or even by wild words and ritual gestures that somehow 'get it across.' (Findlay, 1962: 171)

Thus, though frequency obviously plays no critical role in the learning of new words--people regularly learn words from a single definition--it could be that the words frequently co-occurring with the assign are part of a description of what the new word does not mean, or of what it is the opposite of, or are being used metaphorically, etc. Distillation of a new meaning from the co-occurring words in cases like these would surely result in the wrong meaning being learned. The cases bring out the important point that beyond knowledge of the literal meanings of the words which occur in conjunction with a new one, how they are being used must be appreciated. Suppose a child hears the word 'riddle' for the first time in conjunction with frequent utterances of the word 'elephant' (all the riddles are about elephants). According to the assign account, the meaning of 'riddle' must therefore, for this child, have something to do with elephants. For other children, it may have something to do with pancakes or rabbits, etc. Yet again, a child may well have a certain concept, though not the word for it, and first hear that word uttered in frequent conjunction with words unrelated to its meaning. Thus, a child may have the concept 'body of land surrounded by water' but not know the word 'island.' He first hears the word 'island' used in a story of a plane crash in Hawaii. What sense can be attributed to the claim that the meaning of island will be distilled from 'plane', 'survivor', 'victim', 'dead', etc.? A common case like this cannot be accommodated by the assign account. What it shows, in accordance with the earlier remarks of Findlay, is that factors which are not part of the stimulus situation in which the new word is uttered may well be the most critical in

learning its meaning.

It is clear to me that in attempting to explain the uniquely human achievement of learning language as he would the learning of nonlinguistic behavior in animals, Osgood has failed dismally. What must not be ignored is the fact that the imposition of such constraints on his theorizing is required neither by the phenomena nor by the canons of science. It was only taken to be so by Osgood and the other mediational theorists. Rejecting as mentalistic and unscientific such notions as beliefs, expectations, etc., Osgood insists on the "anchoring of meanings to subsequent as well as antecedent observables." (Osgood, 1971b: 526) We can't observe people's attitudes, beliefs, feelings for things, etc., but we can observe their behavior toward those things. So the task of explaining how people's meanings for things become conditioned to words is reduced to this: explain how words can come to elicit the response originally elicited by the things they stand for. Explain that, and one has supposedly explained how the words have become a sign of the things or, to put it another way, how people have come to have meanings for the words. The elaborate process of the r_m is Osgood's mechanism and the dominant reason for its postulation was Osgood's dogma that meanings must be physical entities of some sort. But as Max Black once commented: "Metaphysical prejudices about what the world must be like invariably lead to the invention of useless fictions." (Black, 1972: 214) This surely has been the fate of Osgood's theory.

I take it that the explanation of language acquisition as the conditioning of implicit responses to utterances of words has been shown inadequate by the arguments presented here. I have focused my attention narrowly on Osgood's treatment of language acquisition, not his account of language comprehension (decoding), production (encoding), and thought (association). These accounts are open to fatal attack on many fronts, some of which were noted in the course of their exposition. However, in denying that Osgood has given a satisfactory account of how r_m 's become conditioned to words, I have rendered the r_m unavailable for elicitation as required by his account of decoding, encoding, and association. In a word, Osgood's explanations of language reception, production, and thought have been simultaneously undermined. They are all dependent upon the operation of s-r association and I have argued that such s-r associations are never established, are not the mechanism by which language is learned. There appears

to be no reason to accept that the processes underlying language use are those specified by Osgood and hence no justification for accepting the ITPA as a measure of them. The Osgoodian process interpretation (OPI) of this test is certainly invalid.

In the section which follows, an original discussion of Osgood's theory is presented which attempts to show that he in fact has no position regarding human communication, only the appearance of one sustained by a heretofore unrecognized ambiguity in his central construct, the r_m . This section should be of greater interest to those following the career of Osgood's theory than to those interested in the ITPA. The latter group may therefore wish to skip it and pick up the discussion of the test in the section following where attention is turned to those interpretations of the test not linked to the validity of Osgood's theory.

VI. OSGOOD'S EQUIVOCATION ON THE r_m

It has been pointed out by Max Black that all behaviorist theories of verbal communication, those wherein the utterance of a word elicits a response from the organism which is theorized to be the meaning of a word "agree in defining a sign as some kind of causal substitute or surrogate for its referent." (Black, 1969: 121) Osgood's theory, it should be clear by now, is certainly consistent with this picture. His own definition of a sign begins: "a stimulus pattern which is not the same physical event as the thing signified will become a sign of that significate when it becomes conditioned to a representational mediation process..." (Osgood, 1971b : 523) As we have shown, implicit in such a claim is an assumption about the nature of words, i.e., they are taken to be names for things. There is similarly a characteristic challenge made to this distinctive assumption of behaviorist theories; a challenge to the effect that the assumption is either false, since many words do not conform to it, or that it is vacuous since it employs the term 'stand for' in no single sense. Osgood's theory has been challenged on the ground that it requires all words to have a physical referent in order for their meaning to be learned, since his theory claims that the meaning of a word is a portion of the behavior toward the thing signified. Osgood, however, has countered this challenge on the grounds that his theory does not require that words have referents, i.e., environmental objects which they signify, in order to acquire meaning. For he specifies that the r_m is a portion of the behavior toward the significate, and significate is a technical term defined in such a way that it includes not only environmental objects but utterances of previously learned words: "Significates (referents or things signified) are simply those patterns of stimulation, including previously learned signs, which regularly and reliably produce distinctive patterns of behavior." (Osgood, 1971b : 523) Any word whose r_m is not derived from actual behavior toward the thing signified will, therefore, have its r_m derived from the utterance of previously learned words in conjunction with it. In short, all words that are not primary signs are assigns, and Osgood's theory does not succumb to the criticism outlined above.

It is my intention to show that Osgood's theory does not avoid

this critique but only appears to do so. The appearance is achieved by means of a serious equivocation in the use of the term 'significate'. This equivocation in the use of a crucial term, an equivocation which has gone equally unnoticed, I believe, by both Osgood and his commentators, places his theory in the position of being either false or vacuous.

Osgood gives what we shall henceforward call his technical definition of significate in the following (or equivalent) words in many places: "We may define a significate, then, as any stimulus which, in a given situation, regularly and reliably produces a predictable pattern of behavior." (Osgood, Suci, Tannenbaum, 1957: 6) The critical element of this definition is, as specified earlier, that previously learned words can be significates, since they regularly and reliably, according to the theory, elicit the r_m which has become conditioned to them. Specifically, it is the utterance of these words which may serve as significates in a technical sense; words are not physical events, utterances of them are. Osgood nowhere makes this clear. This technical definition of 'significate' should be kept in mind while considering the following characterizations of the representational mediation process:

- (1) (The r_m) is part of the very behavior produced by the significate...." (Osgood, 1957b: 356.)
- (2) A minimal but distinctive portion of the total behavior (R_t) originally elicited by an object (s) comes to be elicited by another pattern of stimulation (sign) as a representational mediation process. (Osgood, 1953: 697)
- (3) Thus, according to this view, words represent things because they produce in human organisms some replica of the actual behavior toward these things as a mediation process. (Osgood, Suci, Tannenbaum, 1957: 8)
- (4) With regard to the source of representational mediation processes, it is true that the theory postulates...derivations of r_m 's (mediating reactions to signs) from R_t 's (overt reactions to the things signified). (Osgood, 1971b : 523)
- (5) The basic assumption I make about the behavioral nature of sign processes or meanings is this: those stimulus patterns we call signs (be they perceptual or linguistic) acquire their representing character by coming to elicit some minimally effortful but distinctive portion of the total behavior produced by the things specified. This reduced portion of the total behavior toward things is a symbolic process, which I call a representational mediation process. (Osgood, 1959a: 38)

These passages relate what Osgood stresses is a critical feature of his theory. It is evident that in characterizing the r_m 's as representational, Osgood means that they are representative of the environ-

mental objects which they stand for. Not only is this made clear in his formal definitions of the representational property of r_m 's, but it is supported universally by the way in which he uses the term; it is indirectly supported by the tradition in which his theory is placed and by his description of the problem which language presents for the psychologist as that of explaining how words come to elicit behavior appropriate to the things they signify. (Recall the 'Kitty' example and Osgood's remark that "words represent things because they produce some replica of the actual behavior toward these things.") Thus, the reason why the word 'hammer' refers to (represents) the hammer object is that the r_m for hammer was derived from behavior with that object. 'Knife' means knife, and 'fork' means fork, etc., for the same reason. These objects possess different stimulus characteristics, we therefore have had different total behavioral responses to them (Osgood requires this in one of his postulates), and that is why we have different meanings for these words. Were this not so, there would be no reason why different words could not elicit the same r_m (i.e., have the same meaning) or why a single word could not elicit different r_m 's, thus producing an endemic ambiguity which would render language impossible. It is for this reason that the characteristic of representationality is theoretically essential for Osgood. Indeed, he remarks that it is in virtue of it that his theory is enabled to overcome a stumbling block to earlier behavioral theories of meaning: "The major difficulty with most attempts to deal with 'the meaning of meaning' has been their failure to offer any convincing explanation of why a particular sign refers to a particular object and not to others. The mediation hypothesis offers an excellent and very convincing reason: the sign 'means' or 'refers to' a particular object because it elicits in the organism employing it part of the same behavior which the object itself elicits." (Osgood, 1953: 412)

Thus, r_m 's are representative of nonlinguistic reality, the environmental objects which elicit the behavior from which they are derived. It is for this reason only that a word, when it becomes conditioned to an r_m , means what it does. Osgood relies on this relationship between words and the world, established via the r_m which links the two, to explain how particular words get their distinctive meanings and how also people come to share a common meaning for the same word. Put bluntly by Osgood: "The mediation process must include part of the same behavior made to the object if it is to have its representing property." (Osgood, 1953: 696)

The second defining characteristic of r_m 's is that they are mediational. What do they mediate? Behavior "appropriate to" the thing signified. How do they achieve this distinctive mediation? By virtue of being representational. Quoting Osgood:

(The r_m) is mediational because the self stimulation (s_m) produced by making this short-circuited reaction can now become associated with a variety of instrumental acts (R_x) which 'take account of' the significante.... (Osgood, Suci, Tannenbaum, 1957: 6)

(The r_m is) mediating because this process, as a kind of self stimulation, serves to elicit overt behaviors, both linguistic and non-linguistic, that are appropriate to the things signified. (Osgood, Suci, Tannenbaum, 1957: 318-19)

(The r_m) is mediational because the self stimulation produced by this representing reaction can become associated, through ordinary instrumental learning, with various overt responses appropriate to the object signified.... (Osgood, 1957b: 356)

(The r_m) is mediational because the distinctive self stimulation (s_m) can become associated selectively with various instrumental acts which are appropriate to, or take account of, the thing signified. (Osgood, 1963c: 740)

It should be quite apparent that the characteristic of being mediational depends on that of being representational. It is because the r_m for 'hammer' is representative of the behavior elicited by the object that (supposedly) when I hear someone say, "go get the hammer," I get the hammer and not the tomato juice. The r_m for 'hammer' "organizes and directs" my appropriate response just as the r_m for 'kitty' supposedly mediated the behavior of the child, as discussed earlier in the Kitty example. In short, the reason why the r_m is capable of mediating behavior appropriate to the thing signified is that it is, in the first place, a portion of the behavior toward that object.

Given that the r_m 's essential properties of representationality and mediationality are acquired by virtue of the r_m being a portion of the overt behavior to the thing signified, which has actually taken place in the experience of the subject, it follows necessarily that no assign r_m can have these properties. For assign r_m 's are by definition those very r_m 's which are not portions of the behavior toward the thing signified. And this is so even for those assigns which can have environmental objects for their referents, such as "cavern." If the person who learns this word has never been in a cavern, his r_m cannot, by definition, be representational. The property of representationality, as technically defined, only applies in the case of primary signs. For as Osgood himself writes, "The representing relation is that between the mediation process and the object represented."

(Osgood, 1953: 698) But assign "meanings are literally 'assigned' to them via association with other signs rather than via direct association with the objects signified." (Osgood, Suci, Tannenbaum, 1957: 8) Assign r_m 's cannot, therefore, have the property of being representational. Whatever, if anything, the r_m for the necessary assign 'mermaid' is representative of, it is not the thing signified, mermaid. Imaginary things do not have stimulus properties capable of eliciting behavior.

Readers of Osgood's theory should not be misled by the fact that some sense can perhaps be given to assign meanings being "representational." One might be inclined to think, for example, that since assign r_m 's are supposedly derived from r_m 's of other associated signs, they must be in some sense "representative" of those signs, e.g., that the r_m for 'mermaid' is in some sense representative of 'fish', 'woman', 'ocean', etc. Perhaps they are, but this is entirely irrelevant and misleading. To believe that anything turns on assign r_m 's being representational in some sense other than that technically defined is to become involved in the very problem so damaging to this theory, i.e., equivocation in the use of a technical term. Assign r_m 's cannot have the theoretically essential property of being representational. (In one place Osgood cautions that we should keep "in mind that the 'objects' for assigns are other signs." Osgood's own use of inverted commas reveals, presumably, his awareness that the 'objects' of assigns are quite unlike the 'objects represented' of primary signs.)

The same is true for the property of mediationality. Very little need be said to indicate that this property, supposedly shared by all r_m 's, cannot in principle be characteristic of assign r_m 's. For r_m 's are in theory capable of mediating behavior appropriate to the thing signified only because the r_m is representational, i.e., a portion of the behavior toward that object. Since no assign r_m can be representational, neither can it be mediational. If one attends to any list of words which would have to be learned with assigns since they have no physical referent--try 'possibility', 'was', 'if', and 'also'--it is immediately apparent that talk of the r_m being representative of or mediating behavior appropriate to the thing signified by these words is plain gibberish.

What this amounts to is that there is no unitary construct of the r_m present in Osgood's theory of communication. While Osgood treats the r_m as a single type of construct in the case of both primary signs and assigns, suggesting that the difference is only one of how they

originate, this cannot be so. For the theoretically essential properties which Osgood ascribes to the r_m , the properties which enable his theory to explain how humans learn the reference of terms, how they achieve agreement on the meaning of terms, how ambiguity is avoided, etc., are defined in terms of behavior toward things signified; and as we have seen, these properties can only apply to words which have environmental objects, things in the world, as their referent. It is obvious and unavoidable that assign r_m 's deprived by definition of being "part of the behavior toward the thing signified," are deprived also of the natural endowments of representationality and mediationality which that lineage provides. The consequence is that Osgood is without any account of how the vast majority of words in natural language are learned and used. His theory is vacuous.

I opened this section saying that an equivocation led to the state of affairs just portrayed, an equivocation on the term 'significate.' I will endeavor to show why this is so. While in its technical sense a significate is any stimulus "which regularly and reliably produces distinctive patterns of behavior" and thus may include utterances of previously learned words; in the sense in which the term 'significate' is used in characterizing the r_m , it simply means the thing signified by a word. This can be seen if one examines the two sets of quotations with which I introduced the discussion of the properties of representationality and mediationality. In each of these sets, the term 'significate' itself occurs in the first quotation, and on the basis of Osgood's formal definition the reader will presume that it is being used in its technical sense. But one sees from reading the remaining quotations in each set that the term is simply the apposite of "the thing signified by a word" or "the object signified." Let us call this sense of the term 'significate,' where it simply means 'the thing signified,' the referential sense of significate. In the following very revealing statement, Osgood, in the course of giving the technical definition of 'significate,' gives the referential definition in parentheses: "Significates (referents or things signified) are simply those patterns of stimulation, including previously learned signs, which regularly and reliably produce distinctive patterns of behavior." (Osgood, 1971b : 523)

The short statement of the problem which this ambiguous use of 'significate' causes for Osgood is this. While according to the theory the establishment of an r_m requires only that words have significates in a technical sense, and thus permits Osgood to escape any objections

that his theory requires actual experience with environmental objects as the source of meaning since 'significates', in the technical sense, include not only environmental objects, but also utterances of previously learned words; nevertheless, the theoretically essential properties of the r_m require that the significate in the technical sense which gives rise to the r_m also be the thing signified, i.e., the significate in the referential sense of the word being learned. To put this another way, the environmental event or object that gives rise to the r_m must also be the environmental event or object that the word comes to stand for, if this theory is to explain what it claims to explain. But these two conditions are not satisfied by any assign.

Utterances of previously learned words may be the significate in the technical sense of an assign, the stimulus events which give rise to its r_m , but they are not the thing it signifies. (The only "word" for which utterances of previously learned words are the thing signified is the phrase 'utterances of previously learned words' or some equivalent.) Many assigns, as Osgood himself notes, do not have "any referent in the behavioral sense (e.g., the assign FASCISM)." (Osgood, Suci, Tannenbaum, 1957: 286) But more importantly, even those assigns which do signify "referents in the behavioral sense," e.g., the word 'cave' if learned as an assign, do not signify the previously learned words which gave rise to their r_m . In talking about caverns, one is talking about naturally occurring underground spaces--one is not talking about 'safety lamp', 'rope', 'weekends', 'the Appalachian spelunking club', etc. or whatever set of previously learned words co-occurred with 'cavern' while it was being learned and gave rise to its r_m . Again, the utterances of the words 'fish', 'woman', 'scales', 'ocean', etc., may be the significates in the technical sense which elicit the r_m 's from which, it is theorized, the r_m for 'mermaid' can derive, but they are not the thing signified, the significate in the referential sense of the word 'mermaid'. When one talks about mermaids, one is hardly talking about utterances of 'fish', 'woman', etc. When one says "mermaids are glamorous," one is not saying that utterances of the words 'fish' 'woman' etc., are glamorous. The thing signified by the word 'mermaid' is in fact the imaginary thing, a mermaid, and cannot therefore be the significate of 'mermaid' in the technical sense. It is not a stimulus object. Indeed, all words other than nouns which name things cannot in principle have the same object be their significate in both senses.

This amounts to saying that the only r_m 's that could play the role which Osgood gives them in decoding and encoding are those that have been derived from environmental objects. His theory says nothing concerning how the vast majority of words in natural languages are learned or function in communication. A theory of nouns which name things is not a theory of language. Ours is not a language composed solely of names; no language could be. Thus, Osgood's equivocation not only has a deleterious effect on the intelligibility of his theory, but has a deleterious effect on the theory itself. Osgood simply has no position on how the vast majority of words are learned, understood and produced, and the claim that the r_m is the mechanism by which language behavior is explained is empty. Hopefully, this discussion has been enlightening with respect to how the opposite impression was conveyed.

VII. CRITIQUE OF THE PROCESS INTERPRETATION

To this point only the Osgoodian Process Interpretation (OPI) of the ITPA has been addressed and shown invalid. There remain the other two interpretations of the test--the non-Osgoodian Process Interpretation (PI) and the Trait Interpretation (TI) as characterized in the opening section of this paper. These remain unaffected by the previous critique. The first of these, PI, is taken up in the following discussion of individual subtests, with the treatment of TI reserved for the end.

The critique of PI has the following character. Considering the subtests individually, as is demanded by the fact that each subtest bases a different inference on different overt performances required of the subject, I question whether the desired inference is warranted. These inferences are, of course, inductive--they are supported by the evidence to a greater or lesser extent, but not guaranteed by it. I seek to show that in one subtest after another, the desired inference is so weakly supported as to be unjustified. I argue in the case of each subtest considered that there exist factors relevant to the explanation of test item failures which have not been attended to by the authors, but which have a far greater claim to being responsible for such failures than does the accepted one, a language processing (psycholinguistic) deficit. In these discussions I rely heavily upon a distinction drawn between knowledge and process (competence/performance) variables involved in the performance of the tasks of the various subtests. A pattern emerges wherein it becomes clear that this distinction, which is of critical importance both in making the diagnosis of children taking the test and in determining a remedial program for them, has not been observed by the test authors. Inattention to the distinction, I argue, is the major and fatal error of the ITPA. The consequences of its being ignored are damaging for the test, for its users, and most importantly, for the children whose educational fates are affected by the uncritical acceptance of their test performance as a legitimate measure of their capacity to process language.

The ITPA, as noted, is composed of twelve subtests. I discuss eight of these individually. These eight include both Reception

(Decoding) subtests, both Expression (Encoding) subtests, and the Auditory-Vocal Association subtest--all of which are at the representational level; and the Grammatical Closure, Auditory Closure, and Sound Blending subtests at the automatic level. Not discussed is the Visual-Motor Association subtest (the remaining subtest at the representational level), and the remaining automatic level subtests, Visual-Sequential Memory, Auditory-Sequential Memory, and Visual Closure. The role of language in the latter group is so peripheral that their description as psycholinguistic abilities appears at best uninformative, at worst, misleading. One needn't, for this reason, feel guilty over not giving them individual attention. It is this state of affairs which led Carroll, in his review of the test for the Mental Measurements Yearbook, to remark:

It requires some stretching of meaning to call the ITPA a measure of "psycholinguistic abilities"...only about half of the subtests in the ITPA clearly involve a natural language system, i.e., English; the remainder of the tests are essentially non-language tests that could be performed, conceivably, by individuals who had never acquired any language system at all....(A) "psycholinguistic ability" was apparently viewed as any ability that reflects or involves some kind of "communicative" transaction between the individual and his environment...But by this definition, almost any testable cognitive ability could be regarded as "psycholinguistic." The title of the ITPA is a misnomer, and users should be cautioned to look carefully at the true nature of the test which might less misleadingly have been named something like the "Illinois Diagnostic Test of Cognitive Functioning." (Carroll, 1972: 819.)

While I have billed this section as a critique of PI, the arguments, if correct, are fatal for OPI as well. For the contention is that the inferences to the processing of language are not warranted by the child's test performance and that lack of warrant obtains regardless of the processes' specific character--Osgoodian or non-Osgoodian.

A. THE AUDITORY RECEPTION (AUDITORY DECODING) SUBTEST

What the Auditory Reception (Decoding) subtest is measuring is described as follows:

Auditory reception (decoding) involves the ability to gain meaning from auditorily received stimuli. Although communication may be achieved when such stimuli are nonverbal (e.g., musical tones, a whine, a growl), by far the most educationally relevant stimuli are of a verbal nature. For present purposes, therefore, the definition of auditory receptive ability is limited to understanding the spoken word. (Paraskevopoulos and Kirk, 1969: 29.)

The test consists of asking the child 50 questions of the following sort, to which the child is to respond "yes" or "no" or indicate the same with a nod of the head:

Do trees fly? Do ants crawl? Do pincushions cheer? Do zebras burrow? Do wingless birds soar? Do mute musicians vocalize? (Kirk, McCarthy, Kirk, 1968: 23.)

This subtest, despite the considerable confusion generated by its being described as assessing an ability, actually seeks to assess the status of one of the psycholinguistic processes underlying communication behavior. Given PI, what is under assessment on the Auditory Reception (Decoding) subtest is not to what extent the child has the ability to understand the spoken word, but one underlying psycholinguistic process considered responsible for that competence. As Kirk himself remarks, "It cannot and should not be immediately assumed that a child's score on the Auditory Reception Subtest is entirely descriptive of the full range of auditory receptive ability. The score must be utilized as an index of one facet of auditory receptive ability without which auditory comprehension of spoken language could not develop." (Paraskevopoulos and Kirk, 1969: 31.)

The "one facet" being "indexed" is the postulated process whereby token utterances of single words result in the subject's comprehension of their meaning. The inspiration is straight from Osgoodian theory, such that under OPI the test would be regarded as assessing whether individual words are eliciting the r_m 's which have been conditioned to them. On PI the test is taken as an indicator of the condition of whatever processes are responsible for single word comprehension. In either case it is the status of the unobserved processes whereby utterances of single words result in the subject's consciousness of their meaning that is under assessment on this test.

The authors make a point of their interest being in the subject's ability to comprehend words, not sentences, as will be addressed later.

That comprehension and not the ability to think in words or the ability to express oneself in words is what is being assessed is clearly revealed in the test's design. As has been seen, the test consists of 50 so-called verbal absurdities. There is good reason for such questions being used. They were chosen on the grounds that they virtually reveal their answer merely in being understood. The expectation is that the items do not require reflection on the part of the subject to any significant extent, since responses to them would then be regarded as involving association as well as decoding, and the test would lose its desired "single process" purity. That merely understanding the words of the items should enable the subject to answer them is thus very important. It makes the items significantly unlike such questions as "Who was the first man to sail around the world?", wherein the mere understanding of the words obviously does not "give the answer away," and where failure is most likely due to not having the relevant information. In the terminology of the ITPA, it was hoped to assess "decoding ability" (word comprehension) uncontaminated by "association" (verbal thinking). It was also important that the task not require significant amounts of verbalization on the part of the subject since it would then increase the chance of incorrect replies being due, not to the child's failure to understand the words (decoding), but to his failure to express what he understood (encoding). The inference to the functioning of the specific underlying process whereby single words are understood would then be very insecure. All of this is summarized by Kirk and McCarthy:

Test 1. Auditory Decoding is the ability to understand the spoken word. While the standard vocabulary test is perhaps the best possible way to assess this ability, it is unsuitable for our needs because (a) very young children cannot define words in a formal manner, and (b) such a test requires excessive vocal encoding (i.e., talking). If a subject must talk much, failure to define words might be attributable not only to inability to comprehend the word, but also the inability to express his ideas vocally, or for both reasons. To overcome these objections, a "controlled vocabulary" test was developed in which the subject is presented with a simple question, the answer to which depends upon his knowledge of the words involved more than upon the content (e.g., Do females slumber?). Subjects answer all questions with a simple "yes" or "no" response. It is assumed, therefore, that failure is due to an inability to decode. (McCarthy and Kirk, 1963: 7.)

While I find this objective of assessing the functioning of the decoding process separately from association and encoding entirely

reasonable, I certainly do not believe that the Auditory Reception subtest achieves it. In fact, one could concede that encoding or association difficulties as characterized by Kirk and McCarthy are not likely to be responsible for test item failure, without conceding that it is decoding that is being assessed. It doesn't follow at all from the fact that the other two processes have been excluded that the authors have succeeded in getting the one they want. One critic (Carroll, 1972) has not been willing to concede that association, if by this is meant appreciable reflection, has been reasonably excluded as a factor affecting performance. Carroll noted, and I fully agree, that a number of questions hardly reveal their answers simply in being understood--Do zebras burrow? Do scouts signal? Do beverages quench? Do magicians entertain?. These are certainly not straightforward "yes" or "no" questions. They are certainly not questions, "the answer to which depends upon...knowledge of the words involved more than upon the content." Very few of the questions when cast in their corresponding statement form prove to be true or false by definition. The vast majority are empirical claims. Appreciable reflection on the content and evidence for these claims is surely required in many cases--contrary to both the authors' intention and interpretation. The answers are certainly not immediate, as in "Are you cold?".

There seem to be much more basic and pervasive difficulties facing this test. The first is both produced and revealed by the increasing complexity of the test questions. Why, in a test with the objectives given above, should there be degrees of difficulty at all? If the objective is to determine the status or functioning of the processes whereby spoken words are understood, then questions such as 1 through 5, wherein there is a prima facie certainty that the words are familiar to (known by) all subjects, whatever their age, are the only appropriate sort. For the point of the task is to reveal whether the subject understands on a given occasion spoken utterances of words whose meaning is presumably known. Failure to comprehend on such occasions can at least putatively be attributed to some malfunction in the auditory decoding system, i.e., in the mechanism responsible for recovering the meaning from the acoustic signal, and not to ignorance, i.e., to not knowing the meaning of the words in the test item. But if the items are "made more difficult" by making the vocabulary less familiar to the subject, as they are

on this subtest, failures to answer them are most plausibly attributed to not knowing the words, as opposed to failing to decode known words. In other words, given the stated objective of the test, there seems to be no point to "couching things in increasingly less familiar terms." Indeed, doing so runs directly contrary to the test's purpose. A decoding failure is to be clearly distinguished from failure to comprehend when one or more of the constitutive words in the question is not known.

I am making the point that there is a world of difference between (a) determining whether a subject understands the words he knows when he hears them spoken and (b) determining what words the subject knows, and that this distinction is ignored on the Auditory Reception (Decoding) subtest of the ITPA. Indeed the distinction is ignored in the passage quoted above where Kirk and McCarthy declare that standard vocabulary tests assess the child's ability to understand the spoken word. This is blatantly incorrect. Standard vocabulary tests where the test items are spoken assume that the child can understand the spoken word and exploit this ability in order to determine what words the child knows. Similarly, written vocabulary tests do not assess the ability to understand the written word. They assume this ability and make use of it in order to determine what words the child knows. Vocabulary tests assess the child's word knowledge, not his capacity to process auditory or visual verbal stimuli. Failing to make this distinction, this subtest claims to be providing a discrete assessment of the latter; whereas, it is most plausibly regarded as measuring the former. Under the guise of telling us something about the subject's capacity to process auditory verbal stimuli, the test rather tells us something about the size of his receptive vocabulary, the state of his word knowledge. At least there exist no grounds for believing otherwise. For the word processing, as opposed to the state of word knowledge interpretation of test performance, is not simply confounded, but diminished, by the very design of the test.

That this situation obtains is painfully obvious to anyone taking a serious look at the test's design and scoring. Suppose that one five-year-old (A) answers all 50 questions correctly, while another (B) answers only eight. The scores would put A well above, and B significantly below, the norm for the standardization group "of approximately 1,000 average children between the ages of two

and ten....selected as being of average performance on traditional measures of intelligence, school achievement, and socioeconomic status and of intact motor and sensory development." (Kirk, McCarthy, Kirk, 1968: 93.) The supposed inference is that A has "superior ability to process the spoken word" or "superior auditory decoding ability," or some such formulation; while B very definitely has an "auditory decoding deficit," "deficiency in processing the spoken word" or some similar formulation. But such an inference is clearly unwarranted. We have no reason whatever to believe that B's processes responsible for understanding spoken language are in any way inferior to A's, despite the vast discrepancy in scores. In other words, we have no reason to believe that the difference between A and B's performance is attributable to defective language processing on B's part. For it is perfectly obvious that B, like A, understood some spoken utterances. Since this is so, his performances must be regarded as evidence that he is capable of processing spoken language. Given this, it becomes bizarre to regard B's failure to answer more than eight items as indicative of a deficit in the decoding process already shown to be functional by his first responses. The most plausible and justifiable interpretation is that, while being perfectly capable of understanding spoken utterances of words he knows, B simply doesn't know many words. His deficiency resides in vocabulary development, and not in speech processing. If the latter interpretation is adopted, the authors must explain the strange phenomenon of the speech processing mechanism functioning only on certain utterances. How, for example, would Kirk and McCarthy interpret an erratic scoring pattern wherein the subject, while failing many items, never missed three items in a consecutive block of seven (the stipulated standard for stopping test administration)? Does this patchy performance indicate sporadic malfunctioning in the decoding process? Such an interpretation would be ridiculous.

Clearly item failures on this subtest are most plausibly attributed to the subject not knowing certain words or to other factors such as not being able to recall them, and are least plausibly attributed to a defect in the processing of auditory verbal stimuli. The latter interpretation is groundless. Not only do failures in performance on this test not legitimize the inferences of a decoding deficit but such an inference is most unlikely. Failures on this subtest may be due to any of the factors which contribute to the

execution of the required task--memory, recall, attention, vocabulary, knowledge of the social and physical environment. I am not singling vocabulary out as the operant factor, though it is heavily implicated. In plain language, the test is plausibly regarded as showing only how the receptive vocabulary of particular subjects compares with that of the children in the standardization group. Naturally I maintain that what was established in the standardizing of the subtest was the word knowledge of that group of children, and not degrees of decoding ability in any procedural sense of that expression. It should be emphasized that the challenge to this subtest does not depend upon showing what factor or set of factors actually account for test item failures. It need only be shown that test item failures do not warrant the unambiguous inference to a deficit in the processing of spoken language.

Yet consistent with the assumption that this test tells us not, for example, about children's word knowledge but about the underlying process responsible for the comprehension of auditory verbal stimuli, Kirk suggests a host of auditory activities to help children like B, such as:

Identifying everyday sounds in blindfold guessing games or on tape....Developing auditory figure-ground discrimination. Ask the child to respond when he hears a specific sound imbedded in background noise....Creating exciting programs so that the child will want to listen....Conditioning the child to make meaningful responses by reinforcing with tangible rewards and social approval. (Kirk and Kirk, 1971: 139-40.)

Yet all that B may need is a bit of vocabulary enrichment. Not surprisingly, Kirk recommends activities directed to this end as well, e.g.,

Teach words within categories: family members, toys, foods, colorsUsing synonyms to expand verbal concepts....Labeling and describing such abstract concepts as emotions, feelings, and intangible qualities. (Kirk and Kirk, 1971: 140-41.)

I take this as an acknowledgement that this subtest cannot distinguish between defective language processing and limited vocabulary. Nor does it exclude recall, short term memory, etc., as explanations of test item failures. To that extent, it fails completely in its purpose. The diagnosis it makes of children is completely unjustified, and the remedial practices based upon the diagnosis are at best no more determinate than those that would be initiated without the benefit of the test score. At worst, they are misguided.

(Winifred Kirk's advice to examiners administering the ITPA pro-

vides unwitting support that it is not the subject's decoding process that is being assessed by this test:

Q: Some children are afraid to make a mistake and therefore answer "I don't know" to many questions. Are these responses scored as failures?

A: It is quite permissible to encourage a child with "Try it," or "What do you think, do ants crawl?" or (if necessary) "Make a guess." With such a child you may want to say, "You won't get them all right, but try them anyway." Do try to get a response from him. If you cannot entice him to answer, score the item as a failure." (Kirk, W., 1973: 74.)

Note: (a) that the "I don't know" answer indicates that the child "decoded" (comprehended) the question perfectly, but in fact didn't know the answer; (b) that the child is encouraged to reflect about the supposedly self-evident question, i.e., encouraged to "associate" (manipulate concepts mentally), thus contaminating this "single process" test; and (c) that any child heeding the examiner's spoken encouragement is, in so doing, demonstrating the decoding ability so desperately awaiting a response in order to be assessed!)

An entirely different line of criticism of this subtest arises if we ask what the point of determining the child's ability to comprehend isolated words is. The importance of such a determination is certainly not apparent--there appears to be no justification for such an assessment in terms of single word comprehension being an established component or stage in language understanding. Such a notion finds no theoretical support from contemporary psycholinguistic research, which unlike Osgoodian and all behaviorist and neobehaviorist theorizing, takes the sentence and not the single word as the unit of communication.

Clearly the comprehension of sentences requires the understanding of the individual words that make it up. But most words have more than one meaning. This is easily seen by noting some of the words occurring in the Auditory Reception subtest itself--'fly', 'bark', 'paint', 'drink', 'drill', etc. Quoting Danks and Glucksberg:

It could be argued that it is impossible to decide unequivocally what the meaning of a word is unless we have that word in context. Does the word pen refer to a writing instrument, a place to keep pigs, a prison, the action of writing, or the action of trapping animals in an enclosure? The word pen is by no means unusual in "having" so many meanings. Linguists differ in their estimates of the ambiguity of single isolated words, but all agree that ambiguity is the rule rather than the exception...Most of the words we use can be interpreted in more than one way, and if we include metaphorical usage, then virtually all words can be interpreted in more than one way. Any given word, in principle, can be assigned

more than one meaning. (Glucksberg and Oanks, 1975: 49-50.) Since this is so, the process by which the listener comprehends any sentence containing words with more than one meaning (most sentences) must include some mechanism by which he selects from among possible meanings.

What an example such as this reveals is that in customary human communication, which consists of sentences, not single words, the meaning assigned to individual words is partially determined by the linguistic context, i.e., by the grammatical role they play and by the meaning of other words. As Miller remarked, "The interpretation of each word is affected by the company it keeps; a central problem is to systematize the interactions of words and phrases with their linguistic contexts." (Miller, 1967: 73; see also Massaro, 1975: 19.) This being so, the attempt to assess single word comprehension independently of such contextual contributions would appear to be without theoretical motivation. A sentence is not a list of words, but a structured string of words. It is not interpreted by concatenating the dictionary meanings of its individual words determined independently of context. Quoting Miller, "The meaning of a sentence is not the linear sum of the meanings of the words it contains. If it were, then "Brutus killed Caesar" and "Caesar killed Brutus" would be synonymous; all blind Venetians would be Venetian blinds; all ambiguous sentences would be puns." (Weimer and Palermo, 1974: 402.) Why then is it of value to find out whether the subject is capable of assigning meanings to context-free utterances of single words?

It must be understood that the latter, i.e., the process of isolated word comprehension, is what the Auditory Reception subtest seeks to assess. This is related directly to the fact that in Osgoodian theory there is an r_m postulated for each word. The authors' supposition was that the r_m 's elicitation was responsible for word utterances giving rise to consciousness of meaning, and that this was being assessed. Practical considerations forced Kirk and McCarthy to use sentences rather than single word utterances in order to determine this ability and they were concerned that the subject's performance might therefore be based on his comprehending the full utterance as opposed to the single target words. Thus McCarthy writes: "Auditory Decoding appeared to assess the ability to comprehend related word sequences; the original intent was the comprehension of single wordsit appears that while the subtest is not contaminated with visual

decoding or vocal and motor encoding, it does include a small but undesirable dependence on the comprehension of related words rather than single words, as intended. In short, it appears to include some auditory-vocal association." (McCarthy and Olson, 1964: 21, 30.) Elsewhere Kirk seeks to make clear that the test is so designed that the subject in comprehending the target word (we are never told whether it is the noun or verb or both) in each item receives little help from the linguistic context: "The function of determining meaning from syntax has been minimized by retaining only one sentence form." (Paraskevopoulos and Kirk, 1969: 17, 30.) This obscure and unexpanded remark can receive no plausible interpretation. Since each target word(s) only occurs once in the test, whether the other target words occur in sentences of the same or different grammatical form is an irrelevance. Each target word occurs in only one sentence and that sentence contributes significantly to its interpretation. This "one sentence form" is all that is necessary for the subject to select 'fly'--'travel through air' rather than 'fly'--'insect' in item 2; so also for 'bark'--'emit sharp sounds' rather than 'bark'--'outer covering of tree'. Similarly, it determines the noun rather than verb assignment to 'trees', 'clowns', 'leaves', and 'weasels' in their respective items. It is the linguistic context that determines the interpretation of the homophones 'marry' rather than 'merry' or 'Mary' in item 11, 'soar' rather than 'sore' in item 48, 'burrow' rather than 'burro' in item 33, and 'do' rather than 'due' or 'dew' in all test items. In short, the contribution from linguistic context rather than syntax to the understanding of the test items is pervasive and inescapable. Awareness of this fact exposes Kirk and McCarthy's timid detection "of a small but undesirable dependence on the comprehension of related words rather than single words, as intended" (McCarthy and Olson, 1964: 30) as one of the many gratuitous displays of scientism that it is. It would be interesting to learn just how the context-independent as opposed to the context-supported comprehension of single words occurring in sentences was differentiated, let alone measured as "small."

My criticism of this subtest from the standpoint of the psychology of language thus reduces to two points. In the first place there is no justification given, nor does there appear to be any available, for making the determination of isolated word comprehension ability. Human communication is customarily achieved via sentences and when words occur in sentences the meanings of the other words and the

syntax of the sentence inevitably affect the interpretation of the individual constituent words. This being so, there seems to be no point to determining the subject's ability to assign meanings to words in a context-free situation. Meaning assignment of this sort would not appear to be a component of normal language comprehension. (See Fodor, Bever, Garrett, 1974: 14.) But while lacking justification, the inspiration for such an assessment is readily traced to the behaviorist underpinnings of the test. Quoting Bransford and McCarrell:

Brown (1958) notes that psychologists' search for the "click of comprehension" led them to ask how linguistic symbols give rise to meanings. Classical accounts generally dealt with individual words and their referents. Words were assumed to acquire meaning by association with their referents. The click of comprehension was assumed to result from arousal of an image of the word's referent or from an implicit response to the word that was similar to one's response to the object in the real world (e.g., Osgood, 1953; Watson, 1924). Many problems with referent approaches have been noted, but we believe the most pervasive ones to be that words were considered the basic units of linguistic analysis (cf. Lyons, 1968, p. 403) and that isolated objects were the units of analysis of "the world." Linguistic communication generally does not involve isolated words, but rather sentences, and a sentence's meaning is not equivalent to the summed meanings of its component words (cf. Miller, 1965; Neisser, 1967). (Weimer and Palermo, 1974: 189-90.)

The second point made is that even if such decodings played a significant role in sentence comprehension, understanding the items of the Auditory Reception subtest so obviously and inevitably involves additional semantic and syntactic processing that the purported unambiguous inference to the occurrence of such a process is not justified. It would appear that from the standpoint of psycholinguistics alone this subtest is to be rejected.

B. THE VISUAL RECEPTION (VISUAL DECODING) SUBTEST

This test is the counterpart to the Auditory Reception subtest. Kirk tells us that it is "a comparable test in a different sense modality; it is an effort to measure the child's ability to gain meaning from visually presented material." (Kirk, 1968: 408.) The test is described by Kirk and Paraskevopoulos as follows:

The term visual reception denotes the ability to gain meaning from visually received stimuli. Such stimuli run the gamut of a multi-dimensional and complex continuum. Infinite variations and combinations of color, form, intensity, number of elements, and so on are feasible. For present purposes, the term visual reception is limited to the ability to understand the significance of pictures.

The construction and subsequent evaluation of a test of visual receptive ability necessitated consideration of numerous factors. First, such a test must require of the subject minimal association or encoding ability, and preferably no auditory or tactual decoding; that is, for results to be readily interpretable the test must measure a unidimensional ability area....

The Visual Reception Subtest is comprised of 40 picture items, each consisting of a stimulus picture on one page and four option response pictures on a second page. The subject is shown the stimulus picture which is subsequently removed; he is then shown the response picture, from which he must select, by pointing, the option which is conceptually most similar to the stimulus. Alternatives denied credit include pictures of objects with varying degrees of superficial or structural (rather than functional) similarity, or pictures which are merely associated with the stimulus or with the acceptable choice. Item difficulty level is increased by making the option pictures physically but not conceptually similar to each other or to the stimulus picture, and by requiring the choice of an item which is widely different in superficial appearance but serving the same function as the stimulus picture. (Paraskevopoulos and Kirk, 1969: 32-3.)

The first point to be made with respect to this subtest is that it does not assess the processing of language. It emerged that the Auditory Reception subtest sought to measure the processes taking place when the child understood spoken utterances of words that he knew. Since the stimuli in the Visual Decoding subtest are, however, nonlinguistic--they are not written words--it is impossible to treat the processes under assessment in the analogous sense of being those responsible for understanding written utterances of words known to the subject. (This was at one time considered by the test authors.) Here, despite Kirk's claim, the test is patently not comparable to the Auditory Decoding subtest in any direct way and language is not

being processed visually. This must be emphasized. For in a number of places and without any justification, the authors claim that test performance is indicative of the process whereby written words are understood:

Visual Decoding is the ability to comprehend pictures and written words. Clearly, written words could not be used if the test was to be appropriate for preschool children; consequently, a picture test was employed. This test is designed to be the visual counterpart of the Auditory Decoding test. By a simple pointing response, the subject must indicate that he comprehends or gets meaning from the pictures. (McCarthy and Kirk, 1963: 7; see also McCarthy and Olson, 1964: 31; McCarthy and Kirk, 1961: 4.)

It should be clear, however, that since the test requires no reading--since in fact subjects with no ability to read whatsoever can complete this subtest--the inference from test performance to the ability to comprehend written words is utterly without foundation. This perhaps explains why, without comment, such claims are judiciously omitted from the more recent descriptions of the subtest (as in Paraskevopoulos and Kirk, 1969, above). The important but unaddressed difference between this subtest and its supposed auditory counterpart is thus not that the test items are presented visually rather than auditorally, but that the symbols to be "decoded" are nonverbal rather than verbal. Spoken language is only involved incidently in the presentation of the essentially nonverbal task. The reason given in the last quotation for written words not being used, i.e., that the test could not then be given to pre-schoolers, is interesting. Rather than acknowledge that there was no point to assessing the ability of 2- or 3-year-olds to "comprehend the written word," a task of so-called "picture comprehension" was set instead, with it still being maintained that test performance was indicative of visual language processing. The fact remains, however, that a non-reader is perfectly capable of doing well on this test, and a good reader may do poorly, resulting in the bizarre inference that the former has greater ability to understand the written word than the latter. What this consideration shows, on the contrary, is that the task set on this test is unrelated to the comprehension of written language, and this may stand as our first and considerable objection to this subtest. On what grounds, it must be asked, is this test to be regarded as comparable to the Auditory Decoding subtest when the key element of its assessing a psycholinguistic process has been dropped? There is no decoding of written language.

The comparability which remains supposedly resides in the fact

that the tasks of this subtest, like the tasks on the Auditory Decoding subtest, involve the simple recognition of the test stimuli, not recognition followed by their employment in thought; i.e., the first stage of the information processing is being assessed. The similarity between the two subtests can be claimed since the same phase of processing, that leading from the stimulus to its interpretation, is under assessment. Association and encoding have presumably been excluded. Thus Kirk and McCarthy stress that the difference between visual association and visual decoding is that the latter does not involve relating visual symbols, merely recognizing them (See McCarthy and Olson, 1964: 34; Paraskevopoulos and Kirk, 1969: 36). On this understanding, I would have thought that simply identifying what objects were pictured would be a plausible indicator of visual decoding. It could be assessed perhaps by showing the subject pictures of familiar objects, e.g., a rope, a pole, and asking him to name them. We would not ask, however, how many times the subject thought the rope could wrap around the pole, since that task clearly demands that the subject employ the visual symbols in thought (visual association) and obviously involves much more than the simple recognition of the objects. (Simple recognition is not so simple. See Gregory, 1974: 197-9; Dransford, 1974: 191; Clark, H.H., 1973: 313.) Such a test design would have been consistent with Osgood's view that objects and pictures of them as well as words elicit their r_m 's (and thus, their significance is recognized--they are "comprehended") and would be apposite to the Auditory Reception subtest in that it would parallel the simple recognition of known objects with the simple recognition of known words in the former. I bring this matter up simply to provide a consideration worth bearing in mind while examining the test actually designed by Kirk and McCarthy. Their interpretation of "gaining meaning from visual stimuli" goes well beyond the simple recognition of known objects.

Indeed, they have gone beyond this to such an extent that the notion of decoding as a discrete process or stage in information processing would appear to have been abandoned. Since so much more than simple comprehension (recognition) of the pictured items is required, since the items do not require verbal comprehension, I am at a loss to see what this subtest and the Auditory Reception subtest have in common. Of one thing I am certain, however. It is that in the same way in which failures on the Auditory Reception subtest did not justify inferences to the faulty processing of auditory stimuli, item failures

on this subtest do not serve as unambiguous indicators of the faulty processing of visual stimuli. One thing that the two subtests have in common, then, is that they fail for the same reasons.

All these claims are substantiated by analyzing what is demanded on the test. Children are required to categorize visually presented objects on the basis of abstract (nonphysical) properties, e.g., on the basis of the pictured items having similar uses or functions or sharing membership in a superordinate class that is not defined over shared physical properties. Since this is so, it is clear that execution of the task is heavily dependent on the subject's conceptual repertoire and his knowledge of the physical and social environment. Consider the first demonstration item of the subtest. The child is shown a picture of a collie, while the examiner says "see this." The page is turned. Before the child are four pictures: a man, a girl, a boy, and a short-haired dog (pointer, perhaps?). While viewing these, the child is given the directive, "find one here." The point we wish to make is that this choice depends entirely on how the child has perceived the stimulus picture and how he perceives the response pictures. Perception involves determining whether the stimulus object falls under some concept. And any given stimulus may be categorized in a great variety of ways. Thus, if the stimulus picture was perceived as a collie, there is not one to be found on the second page. Nor is there one to be found if the child has perceived the stimulus picture as Lassie. On the other hand, if the category imposed was that of 'dog', then the selection of another member of this category from the response pictures should be easy. The point being made is the general one that what is perceived is not singularly determined by the sensory properties of the pictures. What is perceived will depend largely on the knowledge, beliefs, attitudes, motivations, etc., which the subject brings to the situation. Indefinitely many categories may be imposed--quadruped, mammal, carnivore, domestic animal, etc., upon this stimulus item as upon all the pictured items.

This point receives emphasis if we consider item 24 in which the stimulus picture is again a dog, a wet one standing in a tub. Turning the page, the subject finds no dog at all among the response pictures. Rather, there is a girl reading a book, a boy (apparently crying) in a raincoat, a woman (airline stewardess?), and a pair of women dancing. The correct response is the boy in the raincoat, with the relevant category being "wet living things" or "things miserable

when wet," or some similar formulation. That the child must impose the category of comparison upon the pictured items is very clear here. Similarly, we may consider item 34. Stimulus picture: a teddy bear reflected in a mirror. Response pictures: a comb and brush, eye-glasses, two figures standing in a rowboat, a girl looking at a picture. The correct choice is the third, the category being "reflected objects" (the men are reflected in the water). The choice of the girl, under the category "looking at things" or of the eyeglasses under the category "things in which reflections may be seen" are incorrect. To reiterate, the point we are making which applies to these and all items on the test is that their perception requires the imposing of categories throughout. This construal of the pictured items may be relatively automatic, i.e., nondeliberative, involving conscious effort, as in the search for such categories as "leverage" (item 40) or "musing" (item 19), wherein a very conscious process of elimination takes place. But regardless of the degrees of concentration or awareness, it is undeniable that in both the recognition of the stimulus picture and the determination of the response, the subject's contribution is considerable. Quoting H. H. Clark:

In the present paper we take the view that perceptual events, like linguistic events, are interpreted when they are processed. That is, when we perceive objects and events, we do not merely store them as visual or auditory entities, but rather we ultimately interpret them semantically and store these interpretations....

Most, and perhaps even all, perceptual events can be coded, or interpreted, in many different ways. There are a number of obvious examples. The best known, perhaps, is the Necker cube, which is seen sometimes with one vertex nearest the onlooker, sometimes with another. This ambiguity occurs despite the fact that the same pattern of contours, lines, and angles strikes the eye under both interpretations. That is, although the stimulus itself does not change, the interpretation given that pattern does. Other examples include Wittgenstein's "rabbit-duck" drawing, which is seen either as a duck going in one direction or as a rabbit going in the other direction....In all these instances, the picture is the same for two very different interpretations. These examples are striking but hardly atypical. It would seem impossible to find a perceptual experience that could not be interpreted in alternative ways. (Clark, 1973: 311-13.)

Performance on this subtest can thus be summarized as follows: the subject construes the stimulus picture as an instance of some concept. He then construes each of the response pictures as instances of some concept. If any of the latter construals match the designation assigned to the stimulus picture, which designation is being held in the memory, it is chosen as the response. If no match occurs, the

subject reconstructs the stimulus picture, the response picture, or both, attempting to find some category under which both the stimulus picture and one response picture can be subsumed. Far from the objects eliciting or giving rise to their significance, as the Osgood model proposes, treating their significance as a conditioned response elicited by the object, the subject is in fact imposing alternative interpretations upon the sensory data.

With this appreciation of the complexity of the task, we may ask what test item failure might indicate. The answers are legion. In agreement with the authors, we may exclude encoding defects as plausible factors and would further regard motivational and attentional factors, which are possible determinants of failure, as nevertheless unlikely. It still remains, however, that failures can be attributed to the subject not knowing what an object is or what it is used for, or to his being misinformed as to what an object is or is used for, to his having but not finding and imposing the relevant description, or to his assigning a plausible but noncredited description, etc. This being so, the attribution of test item failure to a visual decoding deficit, i.e., to a malfunctioning in the mechanism whereby visual stimuli give rise to consciousness of their significance, is totally unwarranted. For there is no way of telling whether a test item failure is due, not to a failure in recognition (consciousness of significance), but to recognition being impossible, as when the object or its function is not known, or (more commonly, perhaps) to recognition having occurred, but because of the many possible categories of recognition, the categorization made does not coincide with that desired by the test. In such cases as the latter, we manifestly have an instance of visual decoding, but the performance is treated as evidence of the opposite. In cases such as the former, the deficit is in knowledge and not decoding. Neither instance, it should be clear, can be correctly regarded as evidence of a decoding deficit. Rather, there may be a deficit in knowledge or memory or beliefs or no deficit at all, simply a difference in interpretation. My second major criticism of this subtest is thus that test item failures do not legitimize the inference to a decoding deficit that is made on the basis of them. What may well be a simple difference in what children know is misleadingly and with great potential harm presented as a difference in the functioning of those mechanisms involved in gaining information from visually presented material.

The third criticism of this subtest, already alluded to, is that the distinction between decoding and association has been blurred beyond recognition. Via the theory and the example set by the supposed counterpart of this subtest, we are led to believe that what is under assessment is the first stage in information processing which purportedly does not require "the manipulation of concepts internally," i.e., association as defined by the authors. Yet as we have seen, the tasks on this test require a great deal of active reflection for their solution. If this is not "relating concepts," then what is it that differentiates the association process from decoding? With the Visual Decoding subtest so obviously demanding a considerable amount of reasoning (every adult colleague to whom I have "administered" both the Visual Decoding and Visual Association subtests finds the Visual Decoding test harder), on what grounds can it be claimed that it does not involve reflection (association) to any significant extent?

The requisite insight at this point is that in Kirk and McCarthy's view, the tasks set by the Visual Decoding subtest are not at all of the complexity we have revealed. In their view what is taking place on this test is the simple elicitation of a conditioned response, i.e., the r_m that has been conditioned to the various objects pictured is being elicited, whereas on the association subtest, concepts must be related, i.e., we have complex chains of r_m 's eliciting other r_m 's. On this understanding each object not only will, but must have its uniquely determined r_m , i.e., significance, which it gives rise to in the passive subject. There is no place in such a conception for the subject as active interpreter of his experience. If the naivete of their view of perception is recognized, the authors' inattention to the considerable cognitive requirements of the decoding subtest's tasks will be appreciated. Our argument can be taken as claiming that what Kirk and McCarthy describe as association aptly describes what the subject is doing on the decoding subtest; i.e., the subject is not "simply recognizing" relations, he is imposing these relations--constructing the pictured objects in different ways according to the concepts at his disposal which he recruits for this purpose. This is manifestly a case of verbal reasoning. Hence the distinction between decoding and association is lost and the differential diagnosis of the processes is not achieved because no difference exists. That spells failure for this subtest.

My final criticism of this subtest is that, as with the Auditory

Reception subtest, the notion of making items increasingly difficult contravenes the declared purpose. It would seem that, given that the subject is familiar with the various items on the test and their uses, etc., each item places an equivalent demand in terms of decoding. What then would make one object harder to visually decode than another? Or, to put this another way, how do we establish degrees of difficulty in visual decoding? One very misleading answer would be that objects which are less familiar to the subject are harder to decode. But if we equate "objects harder to visually decode" with "objects unfamiliar to the subject" as McCarthy does, then the pretense wherein differences in states of knowledge are cast as differences in processing ability, is laid bare; and this is exactly what has been done. McCarthy states explicitly that item difficulty "was increased not only by using increasingly less familiar stimulus pictures, but also by making the comparison pictures physically similar to each other, or by making an incorrect comparison picture (physically) similar to the stimulus picture." What further comment is needed to indicate that a most critical factor affecting test performance will be the extent of the subject's conceptual categories and his knowledge of the physical and social world? Indeed it would seem that it is this knowledge and not the ability to process visual stimuli that is under assessment. For according to this standard of difficulty, ascribing a deficit in the visual decoding process is simply a convoluted and misleading way of saying that the subject's familiarity with (knowledge of) physical objects deviates from the norm set in the test. The masquerade wherein this test of general information is cast as a test diagnosing the subject's "ability to gain meaning from visual stimuli", however we construe that vague description, is thus exposed.

I will not elaborate on the remedial procedures which Kirk recommends for children whose test score reveals a so-called visual decoding deficit. Needless to say, most of these activities concern training in visual perception under the illusion that this is the child's problem; e.g., "teach the child to recognize shapes when imbedded in other visual material (figure-ground perception)." (Kirk and Kirk, 1971: 162.) In fairness, though, Kirk also recommends "experiences in shopping, travelling, visiting places and people of interest, and organized field trips... Allow the child active participation with such things in his environment as household objects, manipulative toys, school materials, common foods, colors, letters."

(Kirk and Kirk, 1971: 163.) Why so? Because "the child may lack knowledge and experience." (Kirk and Kirk, 1971: 163.) The truth is out. For an even quicker way of remediating a so-called visual decoding deficit, I recommend giving the child a department store catalog, a very rich book of pictured objects, then retesting after two weeks.

C. AUDITORY (AUDITORY-VOCAL) ASSOCIATION SUBTEST

According to the 1968 Test Manual:

This test taps the child's ability to relate concepts presented orally. In this test the requirements of the auditory receptive process and the vocal expressive process are minimal, while the organizing process of manipulating linguistic symbols in a meaningful way is tested by verbal analogies of increasing difficulty. A sentence completion technique is used, presenting one statement followed by an incomplete analogous statement, and allowing the child to complete the second statement appropriately. There are 42 orally presented analogies, such as, "I cut with a saw; I pound with a _____." "A dog has hair; a fish has _____." (Kirk, McCarthy, Kirk, 1968: 10.)

Presented below in order to give a fuller appreciation of their character are items 20-29 of the subtest itself:

<i>Item</i>	<i>Correct</i>	<i>Incorrect</i>
20. Mountains are high; valleys are _____	low, deep	small, little, short, long, dark, down, green
21. A pickle is fat; a pencil is _____	skinny, thin, slim	small, short, round, tiny, straight, hard, big, flat, sharp
22. Holsters have guns; envelopes have _____	letters, notes, mail, cards	tops, writing, stamps, papers
23. Coffee is bitter; sugar is _____	sweet, sweeter	spice, white, good, candy, strong, sour
24. A jail has criminals; a hospital has _____	patients, sick, sick people, injured ones	doctors, people, medicine
25. Iron is heavy; feathers are _____	light	feathery, soft, wings, fly with, easy
26. A bee has a hive; a man has a _____	house, home	leg, hat, job, body
27. Trees have bark; people have _____	skin, flesh	clothes, feet, hands, blood, fingers, mouths
28. Churches have aisles; cities have _____	streets, roads, sidewalks, lanes, alleys	buildings, stores, cities, cars, roofs, people
29. Desks have drawers; pants have _____	pockets	legs, straps, buttons, belt, knees, zipper

Inasmuch as what is under assessment is "the central process of making associations," the familiar effort to ensure that the flanking processes of decoding and encoding were not simultaneously being assessed was made. On the decoding side it was necessary to ensure that all words in the incomplete analogy would be understood by the subject. Were this not assured, the subject's failure to determine the analogous relationship could not be unambiguously attributed to a failure in the process of "manipulating concepts internally"; he may simply not have had the concepts to manipulate. On the encoding side, precautions had to be taken such that test item failure not be due to an inability to express a relationship that in fact had been determined in thought. The design of the test with these necessary precautions in mind is explained by McCarthy:

One can observe that children, familiar with every word in an analogy statement, and having the correct response in their speaking vocabulary, still may not correctly complete the analogy. The decoding and encoding functions, then, may be adequate, but the association function may not be. We attempted to construct each item in the test so that decoding and encoding requirements were at least two years below the level for which a given analogy was designed, so that failure on this test is probably due to a defect in association ability, rather than in either decoding or encoding...

A rather regular and substantial degree of relationship is found between auditory decoding and auditory-vocal association. Auditory decoding cannot be eliminated from this task, but we hope it will be shown that its difficulty can be reduced substantially below the difficulty of the analogy component so that interpretation of failure may be relatively non-ambiguous. (McCarthy and Kirk, 1963: 8-9.)

The auditory presentation was to differentiate the channel implicated in this process from that employed in associating visual stimuli. In the authors' terms, this subtest, like all ITPA subtests at the representational level, sought to assess a single process at a single level in a single channel (auditory). It was the authors' belief that by virtue of the design features noted above the test succeeds in its ends. These ends are to be distinguished from those sought in the similar verbal analogies components of many mental abilities tests (e.g., Otis-Lennon) or intelligence tests (e.g., Stanford-Binet) which use the subject's performance to make trait interpretations. Such tests do not use the subject's performances to make inferences concerning the mechanisms responsible for them. They are indifferent to this. This subtest, on the other hand, is specifically concerned with the processes responsible for the outcome:

The functions of association, both visual and auditory, cover a wide

field and probably encompass much of what we refer to as "reasoning," "critical thinking," and "problem-solving." The processes of both divergent and convergent thinking are probably incorporated in the ITPA process of association. Much of what Piaget calls cognitive thinking involves association (concrete operations, the ability to perceive and evaluate two dimensions, classification, evaluating sets and subsets). Many of the common activities in workbooks also require the function of association as it is hypothesized in the model of the ITPA. (Kirk and Kirk, 1971: 143.)

If we are to take this characterization at face value, then our understanding is that the association subtest assesses a process believed to underlie thinking of virtually any sort. How else could one take this inclusive description? That this is a fair reading of the notion is reinforced by considering the following discussion by the authors of what is to be expected of a child who does poorly on the test:

Children who function inadequately in this area often have difficulty categorizing objects verbally, as in the game of Beast, Bird, or Fish. They seldom use similes and metaphors. It is difficult for them to grasp the idea of sets and subsets or outlining material. They are slow to respond to tasks requiring generalizations. They have difficulty relating the moral of a story because it is difficult for them to see the correspondence between the abstract situation and the tangible example given in the story. They may not detect incongruities in absurd statements. They may have trouble solving riddles or understanding puns, proverbs, and parables. They may also fail to understand a joke or see what is funny in verbal absurdities. They do not see relationships like whole-part, tool-user, opposites, size, temperature, or texture. Frequently they have difficulty saying in what way things are alike or different.

Children with this difficulty often do foolish things because they do not see the present situation in relationship to past experience or future consequences. They do not see the two situations in juxtaposition. It is often difficult for them to generalize from one situation to another. (Kirk and Kirk, 1971: 106-7.)

What then is the character of this process which is claimed to underlie such diverse instances of human thought? The authors present a characterization of excruciating vagueness and brevity, which never emerges from the metaphorical. According to McCarthy:

The process of association is entirely internal and largely inferred. We've defined it as the sum of those activities required to manipulate linguistic symbols internally....In brief, there are many kinds of internal operations that are said to occur as association processes at the representational level. The general process involved here is an internal manipulation of symbols; when those symbols are linguistic, we have called the process association. The use of linguistic analogies, similarities, and differences, and tasks of this sort are used to test for the presence and development of association processes at the meaningful level. (McCarthy, 1974: 60-1)

To my knowledge this is the most detailed discussion of association to be

found in the ITPA literature--a clear echo of the few murmurs on association to be found in the Osgoodian literature. From snatches that are available elsewhere, however, one gets the unmistakable impression that concepts are regarded as images and that association consists in a conscious scanning of them. The impression given is that each test item delivers two images to the subject verbally which he must then compare as if he had been handed two objects and asked, "What do these have in common?" The suggestion is "that the subject manipulate concepts internally in such a way as to find meaningful relationships" (Kirk and Kirk, 1971: 106), as if the resultant classifications will be products of some internal analogue of getting the objects in the hand turned the right way. The following discussion of a child with an association deficit typifies the treatment:

The child may have difficulty holding two or more concepts in mind and considering them in relation to each other...When the child seems to be focusing only on one of the two concepts being related, ask him leading questions which will help him attend to one concept and then the other until he becomes aware of a relationship between them. Example: If the child is asked "How are a spoon and a fork alike?" he should be helped to find some parallel attributes by such suggestions as "Think of a spoon. Now think of a fork. What do you do with a spoon? What do you do with a fork? Then how are a spoon and a fork alike?" (Kirk and Kirk, 1971: 144).

If the authors' understanding is that concepts are images then their association process is susceptible to immediate criticisms. For we readily acknowledge that people both have and employ in thought concepts of "things" for which logically there can be no image--such concepts as 'justice', 'jealousy', 'cause', or 'factor'. We are also aware that people are capable of having concepts of things for which having images, while not being logically impossible, would appear to be an empirical impossibility, e.g., one can have the concept, though not an image of, a thousand faceted diamonds. Furthermore, we would certainly not deny the possession of concepts to someone who sincerely insists that he is incapable of imagining things. But whether or not the authors take the conscious state involved in the solution of the test items as characterized by the presence of images (and this is surely uncertain--not enough is said for any interpretation to be secure) or of verbal categories, the test appears to be susceptible to decisive criticisms.

The first of these is that the tasks are falsely represented as being self-contained in the sense that, given the child's familiarity with the words of the test items, the input to the association process

is regarded as complete and any failure to produce the required response is treated as indicative of a breakdown or malfunction in this process of "relating concepts" that had all the raw data it required for functioning. This is simply not so. Completion of these items demands that the subject know and call into thought both the relational concepts of which the states of affairs described by the task statements are instances, and the specific information required for completing the second statement once the relevant category has been identified. Understanding the words (having the concepts) of the test items despite the considerable emphasis given to ensuring it (See McCarthy and Olson, 1964: 33), is no more than a necessary condition of carrying out the task. The impression given is that it is quite sufficient, that if there is assurance that the subject knows the words then test item failures are unambiguous evidence of association deficits. But surely the problem may equally well be one of an information deficit. If we move away from the uninformative description of the task as a matter of relating concepts to an appreciation of the fact that the subject is being required to identify the described state of affairs as an instance of some sort of relation, the constructive or contributive role of the subject becomes clear. We realize that "manipulating" the presented concepts, however substantively we construe that description, fails to do justice to the essential and considerable marshalling of other existent knowledge and beliefs that the task involves. Not only must the child determine the relevant category of which the item is an instance, e.g., part-whole, tool-user, but also upon such knowledge of both language and of the social and physical world as that old men frequently limp, that a female monarch is called a queen, that ponds are shallow relative to oceans, that the coin representing a fourth of a dollar is called a quarter, etc. The point to be taken from all this is that test item failures may be due to the subject's non-possession or non-recall of any of this requisite information. Such failures may be due to deficits in information, in memory, or in recall. Such factors would seem to be different in relevant respects from what one might call a deficit in thinking. If all of them are being included under the heading of "the association process," we must be aware of just how broad and uninformative that description is.

The character of this criticism is brought out more clearly by example. It appears that the mean number of items correctly answered by the 5-year-olds in the standardization group was 16. Suppose we

encounter a 5-year-old who answers all 42. Is the inference here that whatever mechanism is responsible for thought (manipulating concepts internally) is in some sense superfunctional in this child? Or is it that he simply knows more than the average 5-year-old, and knowing more, is able to complete more items? I should think the inference is clear. The child has simply shown us that he is in possession of more concepts and more information than the average 5-year-old, and is thus able to complete many more items than most of his peers. But being able to complete more items is not the same as being better able to complete items, as if what we are observing here is some superfunctioning concept-relating process. It is interesting to note that Kirk and McCarthy never discuss the interpretation of scores well above the norm. I submit that this is because the notion of a process overfunctioning is at worst unintelligible, at best far-fetched, and would therefore be most difficult to sustain. The notion of a process malfunctioning, on the other hand, is readily understood and amenable to acceptance. But if we are confronted with a 5-year-old who answers only four items, we are no more justified in inferring that the processes responsible for thinking are defective or malfunctioning in his case than we were in thinking that those same processes were overfunctioning in the child just discussed. Our options are wide open as far as the explanation of the low performance of this child is concerned. Without independent evidence that language and experiential deficits amongst other factors have been reasonably excluded, we have no justification for ascribing this low performance to faulty association, i.e., reasoning. It may well be that this child simply has fewer concepts and less information than the average 5-year-old. If this is so, his deficiency rests in his state of knowledge, not in the mechanism responsible for thought.

The second criticism of this subtest is closely related to the first. It is that thinking is no more properly regarded as a single type of mental activity than is gardening regarded as a single type of physical activity. The authors' conception of thinking is clearly that of an internal operation which remains quite invariant regardless of whether the input to it is a joke, a proposal, an argument, a puzzle, etc. At a general and uninformative level, it is certainly true that all cases of thinking could be described as "relating concepts," but the character of the thinking in different instances differs in such significant respects as to render this characterization trivial.

Specifically, the sort of analogical reasoning involved on the test is so distinctive as to make the inferences from failures in it to the host of expected shortcomings rehearsed by the authors most tenuous and unwarranted. Once one rejects the simplistic conception of the child internally perusing two concepts till he "sees their relationship", the specific character of the task emerges. A rough characterization of the analogical reasoning required on the test would go something like this: In general the subject must identify the property being asserted of the subject of the first statement as a token of some type --as a predicate of place, size, shape, color, taste, etc. He must then supply the appropriate token of the same identified property type as the predicate of the second statement. This is a very specific and quite complex sort of reasoning. The reasoning involved in "understanding puns and proverbs," using "similes and metaphors," or generalizing "from one situation to another" share no significant features with it as is made apparent if one analyzes the reasoning involved in those cases in a similar fashion. There are simply no grounds for treating such different cases of thinking as these as the same sort of mental operation such that shortcomings in one would be predictive of shortcomings in the other. The authors' uninhibited speculations in this respect (no evidence is provided for their validity) are vacuous generalizations derived from their trivial characterization of all thinking as "manipulating concepts internally." Their speculations are made possible by the fact that their description of thinking is so general and uninformative. Similar logic would treat a failure at playing a sonata on the piano as evidence that the subject should have difficulty doing anything with his hands.

A third and important criticism of this subtest is that if it is to be genuinely taken as an assessment of the operation of thought processes, as distinct from an assessment of the quality of thought, i.e., a test of right reasoning or intelligence, then it must be acknowledged that incorrect answers may be every bit as indicative of thinking as are the correct answers. Consider Item 34 ("A letter has a stamp; a passenger has a _____.") as faced by an imaginary 8-year-old as an example. We assume that our 8-year-old knows all the words in the test item and that he has the correct response word in his vocabulary. This is accepted by the authors as ensuring that if he fails the item, the failure must be due to an association deficit. Now suppose that our 8-year-old notes that stamps are placed on letters

(note he applies some social information) and answers, "Clothes," since clothes are what is on the passenger. This response is not credited. The relationship of one thing being on another is not what was being sought. Perhaps our 8-year-old notes that the stamp goes with the letter, so he offers, "Parcel," "bag," or "suitcase" as his reply. No credit is given. This is not the distinction being sought. Our 8-year-old fails this item. He need only miss a few more to be diagnosed as having a deficit in the auditory association process and be referred for remediation. As the authors remark, "We attempted to construct each item in the test so that decoding and encoding requirements were at least two years below the level for which a given analogy was designed, so that failure on this test is probably due to a defect in association ability, rather than in either decoding or encoding." (McCarthy and Olson, 1964: 31.) But does our 8-year-old have a defect in association ability? Didn't his "clothes" and "suitcase" answers demonstrate his "ability to draw relationships from what is heard," to "manipulate linguistic symbols internally"? They most assuredly did. That is the ability purportedly under assessment, and our 8-year-old certainly has it. What he didn't have, or if he had it didn't employ it, was the concept of "certificate of payment for transport" of which both stamps and tickets are instances. What his performance revealed is that he didn't have, or having it didn't employ this particular item of knowledge. He has certainly not shown that he has some difficulty in finding relationships, in reasoning, critical thinking, etc., all of which are encompassed by the term 'association', but simply that he did not find the particular relationship sought after in the test.

This same point can be made with respect to many if not all of the test items. Incorrect responses (see samples) virtually always display, if not some definite rationale, at least some associative connection that is indicative of "concepts being manipulated internally." Indeed, it is difficult to think of a response that would not serve as evidence of that broadly construed process. The association process, it must be remembered, is presented as underlying virtually all thinking. Poor thinking, I am pointing out, is thinking nevertheless. What we have in this subtest is an intelligence test masquerading as something else. Verbal analogies are, it should be remembered, a standard ingredient of most tests of that description. McCarthy himself remarks, without being overly troubled, that--

...this subtest appears to be a general test of intellectual and linguistic ability with a greater emphasis on the latter than previously thought.... (McCarthy and Olson, 1964: 18.)

...Auditory-Vocal Association appears to be a more general test (i.e., assesses a number of abilities) than intended...However its emphasis on linguistic (versus intellectual) skill emerged from the analysis as a positive trait. (McCarthy and Olson, 1964: 21.)

It is my view that this test is primarily a test of intellectual ability and that its being "more general than intended" is a euphemism for its utter failure to assess what it purports to assess. The gratuitous scientism characterizing the remark, "linguistic skill emerged...as a positive trait" should be recognized for what it is. How, on a test of completing verbal analogies, could linguistic skill not "emerge" as a factor?

I have argued so far that we have no grounds for inferring from subjects' performances on this subtest to the status of a specific psycholinguistic process. Either this inference is unwarranted, or the "process" is not specific since "association" is being used to cover any factor other than incomprehension of the test item or malfunctioning in the encoding process. Under this umbrella an association deficit may be anything from a lack of knowledge or poor recall to poor memory. The process of diagnosis is thus a shambles. I would now challenge the purported channel diagnosis, i.e., the claim that performance is significantly affected by the fact that the items are presented orally and that poor performance is therefore specifically indicative of deficiencies in the child's ability to cope with spoken language.

The acid test for determining the influence of the stimulus mode of item presentation upon the ensuing cognitive performances readily suggests itself. Present the same 42 items in written form to subjects who can also read. The reasonable expectation, I submit, would be that the subject's scores should be approximately the same, with the same patterns of hits and misses resulting under both formats of administration. This is the reasonable expectation because we regard the subject as being faced with the same problems in each case. But if this is so, our implicit judgment must be that the stimulus mode in which the problem is presented is a peripheral factor with respect to performance on this subtest. In other words, we are taking a position that the same demands upon the subject's knowledge and reasoning are being made under both formats, that the same type of verbal reasoning is being required and assessed by means of the same problems

with the form of their presentation regarded as inconsequential. If this is so, the position which Kirk and McCarthy maintain, that in one case the reasoning involved is essentially auditory while in the other essentially visual is without foundation. The stimulus characteristics of the signal by which the questions to be answered are conveyed (the relationships to be identified are conveyed), contrary to the test authors' position, would appear to have no bearing on the character of the thought processes. This is the case we must make.

Quoting Glucksberg and Danks: "Once a sentence has been perceived and interpreted, we may do various things with the resultant product. In general, we consider that a sentence has been understood or comprehended when we are able to use the information derived from the sentence in some appropriate way." (Glucksberg and Danks, 1975: 96) The straightforward and important point made in this short remark is that it is only after a sentence token has been perceived and interpreted that we can employ its information in thought. The Auditory Association subtest is intent upon assessing the employment of language in thought and is thus designed to ensure that the sentences are uttered are indeed perceived and understood; i.e., that the subject gets the information he is to work with. The input to the thought processes is not the external stimuli but the perceptions (understanding) they have given rise to. To put this another way, it is assumed that the speech sounds have been correctly interpreted and what is being assessed is the ability of the subject to carry out a mental operation using the information provided by that interpretation. The interpretation is the product of the processes which recovered it from the speech signal. In the terminology of Kirk and McCarthy, the input to the association process is the output of the decoding process; and that output, we are everywhere told, is the awareness of meanings, ideas, concepts. In a word, the decoding process is responsible for the subject getting the message; in the association process he does something with it.

With this clear, it is not difficult to establish the irrelevance of the stimulus mode of the speech signal to the association process. For in association, the subject is operating upon the received message, i.e., the interpretation, and not the signal which conveyed it. The crucial fact about this message is that it remains invariant regardless of the stimulus mode by which it was transmitted.

The message "there is a hurricane approaching" is the same regardless of whether it is communicated by a shout, a whisper, in print, over the radio, or by hand signals. While the signal has a concrete reality--light waves, sound waves--the message does not. The message is an abstraction. Signals can be visual, auditory or manual; messages admit no similar characterization according to sensory modality. This matter is put very well by Miller:

The sort of thing I have in mind when I talk about levels has already been incorporated into the theory of communication in terms of a critically important distinction between the signal that is transmitted and the message that the signal conveys. The need for this distinction becomes obvious as soon as one recognizes that the same message can be encoded by many different signals. Indeed, in the course of a single transmission from source to destination a message may be recoded several times into acoustic, electrical, or printed forms; the nature of the signal will change with each recoding, but the message should remain invariant throughout. Without some concept of the message as different from the signal, we would have no way to talk about what should remain invariant under transformations of the signal.... (Miller, 1974: 5.)

Since the message to be operated upon or with remains the same regardless of the character of the stimulus mode by which it is conveyed, it should be apparent that this operation, association, cannot correctly be distinguished according to the stimulus character of the signal initiating it. Knowing that the questions were spoken rather than written is as inconsequential as knowing the pitch of the examiner's voice or (in the case of written questions) the size of script on the question sheet. To put this another way, one could say that the character of the stimulus mode is removed in decoding so that the resultant interpretation is nondistinctive relative to the stimulus character of the signal which gave rise to it. The association process operates upon this nondistinctive output of decoding, upon the interpretation or message, which is neither auditory nor visual. It cannot intelligibly be characterized according to stimulus mode. And it is more than incorrect to do so; it is seriously misleading. For what in fact is required on this test is verbal reasoning, the use of language in thought, and such an ability does not depend on, nor is it reflective of, the subject's auditory capacity. Such reasoning requires that the subject have language but not hearing. To describe it as an auditory process, as the authors everywhere do (See Kirk, McCarthy, Kirk, 1968: 10 and elsewhere.), is serious, though characteristic misrepresentation.

Our argument can be recapitulated as follows: In the case of

linguistic stimuli, the stimulus mode of their presentation does not alter the character of the interpretation they give rise to. "Sam likes cigars" means the same whether written, spoken, or manually signed. The consequence of this observation for the Auditory Association subtest is this: Since in the association process the subject is operating with the output of the decoding process, the interpretation or message, and since that output is not auditory or visual or in any way determined by the stimulus character of the input to the process recovering it, to regard linguistic stimuli of differing stimulus modes as giving rise to different types of reasoning process is incorrect. Whether on a particular occasion the specific concepts to be related have been conveyed to the subject vocally rather than visually is irrelevant, provided we assume (as Kirk and McCarthy do) that there is no defect in the mechanism making these concepts available for reflection, i.e., that the subject's decoding process is sound. This is to deny that the test assesses what it purports to assess--a single process in a single channel--on the grounds that the channel distinction is meaningless. We are not getting a diagnosis of an auditory process--the child's test performance does not warrant any such inference. What is being assessed, as noted earlier, is a specific type of verbal reasoning. Kirk says as much himself: "The Auditory Association subtest has been described as assessing 'the ability to draw relationships from what is heard' or as 'the ability to manipulate linguistic symbols internally' (Kirk and McCarthy, 1961, p. 403)." (Kirk and Kirk, 1971: 106.) If one is attentive, one notices that in this remark, verbal reasoning is illegitimately equated with drawing relations from what is heard. Once this merger between verbal and auditory is made it is easy to go on to treat "the ability to see logical relationships" (Kirk and Kirk, 1971: 106) as an essentially auditory function, and then treat shortcomings in the former as evidence of some peculiarly auditory deficit. But as we have seen, this does not at all follow.

In bidding farewell to this subtest, I shall explain away a paradox facing the authors. It appears that a good number of children score poorly on the decoding subtests yet do well on the association subtest in the same channel. Given that the association process supposedly begins where the decoding process leaves off, this is a confounding result. How can the child operate well with spoken messages when the process whereby such messages are recovered from the

speech signal is supposedly malfunctioning? The authors are left shaking their heads, convinced that there must have been a slip-up somewhere. Quoting Winifred Kirk, "The test is not infallible, however, and once in a while such vagaries do occur. If it were not so, the statistical reliabilities would be higher. When a marked deviation occurs, the scoring and administration should be very carefully checked." (Kirk, W., 1973: 75.) Yet a very reasonable account of such "vagaries" is at hand. According to this account, the Auditory Reception subtest is just a well-disguised vocabulary test masquerading as one which tells us something about the subject's ability to process speech signals. But in fact, it doesn't do so. The Auditory Association subtest, on the other hand, is a test of verbal reasoning, general information, etc., in which the vocabulary demands are kept low and which has nothing of any significance to do with audition. On this understanding, there is simply no reason to regard a low Auditory Decoding score paired with a high Auditory Reception score as problematic. The tests are not measuring interdependent functions. The paradox thus vanishes, along with the test claims.

D. VERBAL EXPRESSION (VOCAL ENCODING) SUBTEST

There is a customary and important distinction drawn between speech and language which is well expressed by Slobin:

It is important to grasp the distinction between overt behavior and underlying structure. In English, and other languages, the distinction is expressed in the concepts of language and speech: speech has a corresponding verb form, whereas language does not. We say: "He speaks the English language." To speak is to produce meaningful sounds. These sounds have meaning because they are systematically related to something called "the English language." Speech is behavior. You can listen to it; you can record it on magnetic tape. You cannot tape record the English language. You can only record English speech. Because we know the English language, we can understand each other's speech. Language is thus something we know. The English language is a body of knowledge represented in the brains of speakers of English. The description of such bodies of knowledge has been traditionally the province of linguistics, while psychology has traditionally defined itself as "the science of human behavior." (Slobin, 1971: Introduction.)

It is clear that humans are able to produce words and sentences without uttering them. We think silently, we formulate thoughts but don't express them, we keep to ourselves what is "on our mind," etc. In such cases we have certainly produced language, but not speech. We've generated sentences but not utterances. It is also clear that we commonly do utter the sentences we have generated. We speak. Our messages, formulated internally and which could have been left unspoken, are encoded into speech sounds so that they can be conveyed to others. It is possible, given the distinction between language and speech, to characterize the stages in speech production in the following way:

Very little is known about the production of language; but we are beginning to understand a considerable amount about the production of speech; and most of the physiological mechanisms involved in producing individual speech sounds are now fairly well described. In order to make clear the limits of our knowledge, we may consider a speech act to consist of four stages. The first stage consists of the formation of an idea, the production of a thought that has to be expressed. Second, this thought has to be arranged in terms of an appropriate phrase or sentence. This stage includes determining which lexical items should be used, and arranging these items within a suitable semantic and syntactic framework. The third stage involves devising a program of skilled motor movements so as to produce the speech sounds corresponding to this sentence. Finally, there is the execution of this program and the production of speech.

The first two stages constitute the production of language. They have to be achieved irrespective of whether the thought is finally expressed in terms of speech or writing. The sentence we have just

written has, as far as we know, never been spoken by anybody. The third and fourth stages outlined above have never occurred. But there is no doubt that the words form a sentence in the English language. Thus we can distinguish between language and the medium of expression of that language. Speech is the medium of expression of spoken language (Abercrombie, 1967). (MacNeilage and Ladefogod, 1976: 75-6.)

This subtest of the ITPA is ostensibly concerned with assessing the latter three stages in sentence production as distinguished by Ladefoged. In other words, it is concerned with determining the status of the processes leading from formulated message to vocalization. It is not concerned with whatever processes take place in formulating the message; for those are the processes of "manipulating concepts internally" which the authors distinguish as association. The ability to formulate ideas is not under assessment here; rather, it is the ability to express them. The authors maintain that it is not decoding or association, but encoding that is being assessed. The following characterizations of the Verbal Expression (Vocal Encoding) subtest make this clear:

Vocal Encoding is the ability to express ideas in spoken words. In this test, the subject is asked to describe a simple object such as a block or ball. His score depends on the number of unique and meaningful ways in which he characterizes a given test object.

The basic strategy is to present the subject with an object which he cannot fail to recognize. Thus, if he fails the task, it would not be due to a lack of recognition (decoding), but to an inability to encode. (McCarthy and Kirk, 1963: 9.)

Verbal Expression (Vocal Encoding). The purpose of this test is to assess the ability of the child to express his own concepts vocally. The child is shown four familiar objects one at a time (a ball, a block, an envelope, and a button) and is asked, "Tell me all about this." The score is the number of discrete, relevant, and approximately factual concepts expressed. (Kirk, McCarthy, Kirk, 1968: 11.)

While in the first citation only decoding, and not association, has been specifically mentioned as having been excluded by the design of the test items, the latter must assuredly be regarded as having been similarly excluded. For the purpose of each subtest, as we have frequently noted, is to make a discrete diagnosis of a single process. I find it significant, however, that association is not mentioned, since it emerges quite clearly upon examination of the test that this process is heavily implicated in test performance. Encoding, it will be recalled, refers to a specific stage in the processing of language, which is to be distinguished from the reception and organization stages, this distinction again being motivated by Osgoodian theory,

Quoting the authors:

Processes encompass the acquisition and use of the habits required for normal language usage. Their acquisition is dependent on learning theory for a complete and adequate explanation. There are three main sets of habits to be considered: (a) decoding or the sum total of those habits required to ultimately obtain meaning from either auditory or visual linguistic stimuli, (b) encoding or the sum total of those habits required to ultimately express oneself in words or gestures, and (c) association, or the sum total of those habits required to manipulate linguistic symbols. (McCarthy and Kirk, 1963: 2.)

A psycholinguistic ability has been defined as a unique combination of one level, one process and one channel. To construct a test for such an ability is not possible in the literal sense, since the minimal requirements of a test situation demand that a subject be stimulated in a standard manner and that he respond. Thus, any formal testing requires two processes, decoding and encoding. Practically, however, useful interpretations of results can be made if the requirements for a given process are regularly increased in difficulty while the requirements for other processes are kept minimal and constant. (McCarthy and Kirk, 1963: 6.)

The belief is that the tasks on this subtest have been so designed as to legitimize an inference to the last of the processes--"encoding or the sum total of those habits required to express oneself in words..." I take the position that its design does not permit this.

Suppose that a child is given the spoken directive, "Tell me the names of your brothers and sisters," and the child remains silent. Even if we assume that all sensory, motivational, and attitudinal factors are up to par, and that the words of the directive are in the vocabulary of the subject, there remain a number of factors which might explain the child's non-performance. It could be that the child has a genuine auditory decoding deficit, i.e., that he replies immediately when given the same directive in written form. It could be, on the other hand, that the child comprehends the task but cannot formulate the answer--perhaps he has forgotten the names of his brothers and sisters, temporary lapse of memory. Failure due to a factor such as this would be regarded as an association deficit in Kirk and McCarthy's schema. Yet again, it may be that the child understands the task, formulates the requisite answer (the idea to be expressed), but for some reason, can't express it vocally. A failure attributable to the latter condition is a genuine encoding failure. Thus Kirk and McCarthy link what they describe as an encoding deficit in children to the condition known as "expressive aphasia." "There may be some similarity between our 'visual decoding' and 'visual agnosia' or some forms of dyslexia. Likewise, there may be a suggested relationship between what Wepman calls 'aphasia' and our concepts of auditory-vocal

association, and between 'vocal encoding' and expressive aphasia--or in Wepman's term 'apraxia.'" (Kirk and McCarthy, 1961: 405.)

Given these possibilities for the explanation of an unlikely reticence as in the case exemplified above, a genuine gain in understanding is achieved if we can determine what stage in the processing of information is responsible for the observed failure "to express ideas in spoken words." As noted, Kirk and McCarthy maintain that the test does just this. But it does not. To see why we must consider the required behavior (already described) and the method of scoring. The scoring is described in the 1968 Test Manual as follows:

This test assesses S's ability to put ideas into words by asking him to describe verbally four simple objects. The scoring does not reflect elegance of expression or grammatical propriety, but focuses on quantity of concepts expressed. A concept is any relevant, discrete, and approximately factual term which expresses a characteristic, function, or relationship of the object. To be relevant, a concept must be specifically appropriate for that object. To be discrete, a concept must express a single idea that is not redundant to the expression of that same idea in another form. To be approximately factual, a concept must provide attention to reality within certain rather broad limits. (Kirk, McCarthy, Kirk, 1968: 51.)

When we consider the character of the tasks and the method of scoring, it becomes impossible to take seriously the claim that this test provides a discrete assessment of the encoding process. For such a claim to stand, all factors other than the encoding process which contribute to the subject's performance must have been reasonably excluded as plausible explanations of test item failure. The authors believe that decoding has been so excluded; i.e., that we can reasonably assume that all children understand the directive "tell me all about this." I agree. The authors also, however, make the claim that "there is no evidence to indicate that this subtest draws on association..." (McCarthy and Olson, 1964: 34.). It must be realized that, translated out of jargon, this is the equivalent of saying that in complying with the directive "tell me all about this," the child's thinking of what to say is not to be regarded as a significant factor. To put it another way, the claim is that there is "no evidence" to indicate that test performance failures are due to the subject's not formulating thoughts for expression. Rather, it is being claimed, they are indicative of formulated thoughts not being encoded into speech. There could hardly be a less justified claim.

Suppose that the ideas required by the test items for vocalization were in some sense immediately available to the subject; perhaps,

as in response to such questions as, "What is your name," "Can you swim?", "Are you cold?", "Does your head ache?", etc. On items such as these, we would at least have some reason to believe that inability to formulate the idea to encode would not be a significant factor affecting performance, since the information to be encoded is presumably of immediate familiarity to the subject. The so-called verbal absurdities of the Auditory Reception (Decoding) subtest could also perhaps be regarded as questions of this sort. We have reason to believe that such tasks require a minimum of reflection, a minimal amount of searching for the reply, of formulating an idea to be expressed. Lack of a reply to such tasks would not plausibly be ascribed either to the subject not being in possession of the relevant information (lack of knowledge), or for some reason, not being able to formulate the idea (association deficit); and we would at least have some cause to think that there might be a malfunction in encoding, i.e., in those processes leading from formulated ideas to their expression. But is there any resemblance between items of this character and the items of this subtest? Here the subject is shown an object and given the bald directive, "Tell me all about this!" The child must call to mind such properties as color, size, shape, composition, function, numerosity, and to think of uses and of comparisons. How can it possibly be inferred in such cases that failure to produce spoken responses is due to a malfunctioning in the process whereby formulated ideas are vocalized, rather than in the process whereby the ideas are formulated for expression (association, in the authors' sense)?

The answer is that it is not possible. The test warrants no such inference. It is by no means surprising, therefore, that Kirk, in his recommendations for remediation following the diagnosis of a so-called encoding deficit, proposes that virtually anything and everything be tried. Poor performance on a test may have been due to anything under the sun; there is no means of telling from this supposedly discrete diagnosis just what. Vocabulary, personality, family background, emotional inhibition, speech defects, lack of ideas, are all specifically cited as possible sources of test item failure. What, we must ask, has become of encoding?

(A)bility here should, of course, be viewed in relation to mental age, personality, and family background. The examiner should be familiar with levels of normal language development.

Emotional inhibition and family habits of reticence may also affect responses on this test, but the diagnostician should sparingly discount a low score on the basis of personality unless there are

definite indications that the child can and does express himself in other situations....Severe articulation disorders and/or other speech problems can be underlying factors of poor or limited verbal expression ability....It is important therefore that the examiner evaluate other correlates of poor verbal expression. Does the child lack the basic vocal skills which make speech flow freely? Does he lack the content of ideas to express? Does he have an adequate receptive and/or expressive vocabulary? Is he able to organize his ideas and delimit the relevant from the irrelevant? (Kirk and Kirk, 1971; 108-9.)

I fully agree with Kirk that any of these factors may be responsible singly or collectively for poor test performance on the encoding subtest. But I regard it as an awesome equivocation on the term 'encoding' to treat all such conditions as these as instances of an encoding deficit. To say that a child has an encoding deficit is, given this extended sense of the term, an obscurantist way of saying that the child for some reason doesn't talk much. Patently, on the basis of his performance, we have no more reason to believe that the child has a deficit in the processing of language than that he has a deficit in his vocabulary or in his experience, etc. Indeed, we have far less reason to believe so. This being so, the claim that a disorder in language processing is detected by this subtest, a disorder supposedly akin to expressive aphasia, is a serious distortion.

Further and telling evidence against this interpretation of the test is revealed by examining the scoring standards. Just what determines whether a child has an encoding deficit? Quite simply, this is established by his production of fewer "discrete, relevant, and factual ideas" than were produced by the children of the same age in the standardization group. According to this standard, and referring to scoring samples provided by the authors, we learn that responses to the ball object such as (1) "It has a mark on it," (2) "My sister doesn't like them," (3) "My brother has one like it," (3) "It could get lost under the sofa in the living room," (4) "It's made in a factory," (4) "You can hold it in your hand," are all non-creditable responses (Kirk, McCarthy, Kirk, 1968: 55-6.). Utterances of type (1) are excluded because they express an "accidental detail" of the particular ball; those of type (2) are excluded on the grounds of being emotional reactions; responses of type (3) are excluded for making "reference to extraneous objects"; and those of type (4) go uncredited because they have "universal" meaning, i.e., the remark could "apply to a large number of objects." The grounds for exclusion are arbitrary and enigmatic. But what is truly incomprehensible is that there should be "quality control" of any such rigid kind on a test of this

purpose. One of the more convoluted inconsistencies is that "responses (such as (3)) are considered irrelevant because they do not refer directly to the object at hand;" yet it is simultaneously maintained that responses such as (1) be excluded because they do refer directly to the object at hand, as do the replies "red," "round," etc., which are credited! Vocal encoding, among other things, thus measures the child's ability to intuitively determine which immediate stimulus properties of the object are to be described, i.e., which ideas are relevant. "Red," according to the Test Manual, is demonstrative of the subject's ability to put ideas into words; "Scratched," on the other hand, is not. The beautifully insightful remark about the block, "Can turn it over and it will stay in place," receives no credit (Kirk, W.D., 1973: 47). I will not attempt to describe the rationale. The remaining categories of non-creditable and conditionally-creditable responses are similarly arbitrary, enigmatic, and entertaining. But enough has been said to make the bankruptcy of this subtest clear. Manifestly, many responses which receive no credit on this subtest are indicative of encoding ability; for such responses as those noted above, and many others, including such unclassified non-creditable as "This is silly," and "I find this boring," are indicative of the ability to express ideas in spoken words. What is actually being assessed here is the child's ability to produce a privileged set of ideas, and he is performing under the handicap of not having been informed of the characteristics of that set. It is not the simple expression of ideas, but the character of ideas expressed that determines the child's evaluation. Thus we learn that not only are we unable to determine whether failure to produce responses of the specified type is due to not formulating the relevant ideas, or, having formulated them, not being able to express them; but we also find that many expressions of ideas in spoken words are treated as evidence that the child does not have the ability to do just that! The test and its interpretation are without a defense.

To summarize our critique, the interpretation to be made and which is made on the basis of performance on this subtest is clear. One is to infer from the child's performance the status of whatever process is responsible for formulated ideas being vocalized. Low scores on the test, we are advised (as noted above), may be taken as possible indicators of expressive aphasia. They are at least to be treated as an indication that the subject did "not develop normally in his ability to talk." (Kirk and Kirk, 1971: 151.) Yet the briefest

examination of the test items reveals the glaring injustice of such an inference. Given the stultifying and unqualified directive, "Tell me all about this," one child comes up with a host of responses that demonstrate his knowledge of the object, its uses, etc., while a second produces a host of responses of the non-creditable sort indicated earlier. Despite his manifest verbosity, the latter child is diagnosed as having a verbal expression (encoding) deficit. Yet it is obvious that he is able to "express ideas in spoken words" and that whatever mechanisms are responsible for such a competence are as assuredly intact and operant in him as they are in the first child. So not only does the design of this test--which makes considerable demands upon the subject's knowledge of the world, vocabulary, and imagination in the formulation of ideas--render its unambiguous interpretation as a measure of encoding an impossibility, but the scoring standards are such that no matter how many ideas are expressed, only those of specified content are taken as indicative of the ability. It is not the mere expression of ideas, but the expression of choice ideas that is being assessed, and there is a world of difference.

In apparent ignorance of the test's inconsistency, wherein an assessment of the character of children's thoughts is portrayed as an assessment of their simple ability to express thoughts, Kirk says the following:

Most teachers have had a few children who appear dull but who, on closer acquaintance, exhibit knowledge and acumen which ranks them above the average of the class. Often these children have difficulty expressing themselves. At the other extreme are those children who talk a lot but have little to say. They may give off a lot of static, a lot of irrelevant chatter, and much repetition, but few relevant concepts. Both of these kinds of children would probably score low on the Verbal Expression test. (Kirk and Kirk, 1971: 110.)

In other words, the substance of one's thoughts is being used as a measure of one's ability to speak. What we were given to believe was an assessment of the ability to talk is in fact a measure of the ability to "say something" in the very loaded sense of "say something of significance." It is indeed a test of "knowledge and acumen," not the ability to express ideas in spoken words. Nevertheless, the diagnosis of the low scorer is, "(did) not develop normally in his ability to talk." (Kirk and Kirk, 1971: 151.)

E. MANUAL EXPRESSION (MOTOR ENCODING) SUBTEST

The ability under assessment is defined as follows:

Motor encoding is the ability to express one's ideas in gestures. The manual language of the deaf is an example of motor encoding. This ability is tested by showing the subject an object and asking him to supply the motion appropriate for manipulating it (e.g., drinking from a cup or strumming a guitar). (McCarthy and Kirk, 1961: 6.)

The test designed to assess this ability is described by Kirk as follows:

Manual expression is the ability to express ideas by gestures. It is representative of a larger function involving the use of body and facial expression to transmit ideas. Other tests of motor expression such as dramatizing stories or pantomiming activities or conversing in gestures were deemed to be impractical because of difficulties in scoring and in eliminating contamination by other functions.

To make it suitable to the purposes of the ITPA, the task was limited to purely manual gestures and confined to the expression of how to manipulate specified objects. The child is shown a picture of a common object and is asked, "Show me what we do with a _____." Stimulus input involves both auditory and visual channels simultaneously to minimize decoding requirements. Difficulty level is increased not through the degree of familiarity of the objects pictured, but primarily through increased complexity and/or precision required for adequate communication of object manipulation. (Paraskevopoulos and Kirk, 1969: 39-40.)

The familiar attempt to hold the demands on the other two processes (decoding and association) at a minimum was made. It is important to note, however, that in the passage describing how this was achieved, the association process is left conspicuously unmentioned--we are not informed of any measures to control or diminish its involvement. It was the same, it should be recalled, in the case of Verbal Expression (Encoding):

So that decoding could not be the possible cause of failure in this task, actual photographs of familiar objects with no distracting background were employed. The earliest versions of this test presented the stimulus picture with three alternative pictures. Before demonstrating the motion association with that picture, the subject was first asked to show the examiner the picture in question (THE ONE YOU MAKE MUSIC ON, THE ONE YOU POUND WITH, and so forth). In this manner, we assured ourselves that even the youngest subjects could identify the picture by use (decoding) although they often failed to supply the appropriate motion (encoding). This, in turn, precluded the use of increasingly less familiar stimuli as a mode of increasing the difficulty of the test. (McCarthy and Kirk, 1963: 10.)

It must be clearly recognized that what is being required of the subject is that he pantomime customary behavior with supposedly familiar objects. Not just any behavior will do; e.g., pantomiming a toss of the hammer into the air, etc., receives no credit. This is presumably why in the 1961 edition of the test the directive given to the subject was, "Show me what you should do with this." The test thus assumed that the child is not only familiar with the objects, but with the way they are customarily (or appropriately) used. The test consists of fifteen pictured items in addition to the demonstration items of an actual toy hammer and a pictured coffee pot and cup. The fifteen test items include such things as a guitar, telephone, binoculars, camera, suitcase, and clarinet.

The pantomimes are scored according to the subject's display of arbitrarily chosen elements of detail, with a maximum number of credited elements being indicated for each item. Thus, the guitar pantomime is scored as follows:

1. GUITAR (3)

- a. Plucking or strumming.
- b. Additional point is given if S holds other hand away from body at or above the level of the plucking hand. (Only credited if it occurs with Point a.)
- c. One point is also given if hand "holding" neck of guitar makes fingering movement. (This may be credited without Points a or b.)

(Kirk, McCarthy, Kirk, 1968: 78.)

That higher scores are achieved by production of selected elements of detail in pantomime is not the same thing as achieving them on the basis of the faithfulness of their imitation of reality. The credited elements are faithful, but not just any faithful element is credited. Motions of carrying the suitcase, or focusing the camera, for example, while faithful to reality, are not among the selected elements, and are thus not taken as indicative of being able to express one's ideas in gestures. This obvious contradiction points to one problem which this test faces. Manifestly such actions are indicative of the process purportedly under assessment; that is, such performances as carrying the suitcase are prima facie evidence that the child has (a) called to mind an idea about how suitcases are used and (b) that the processes wherein ideas are encoded into gestures are operant. If we are to infer from performance to the status of (b), i.e., to the condition of the encoding process, such performances as focusing the camera, etc., are surely positive indicators. Yet they are disregarded. On what grounds? In a remark which I am still not sure I fully compre-

hend, Winifred Kirk explains:

Q: In the Manual Expression subtest many children demonstrate carrying the suitcase. Why is this not given credit?

A. Because this response was found to lower the age discrimination of the subtest. Young children tended to make this response, whereas older children did not. Therefore it would have been misleading to give credit for this response.

Hence, the grounds for excluding some clear instances of expressing ideas manually are, apparently, that crediting such responses might mislead one into thinking that the average four-year-old has greater encoding ability than the average nine-year-old; this because on the basis of a large test sample it was found that younger children regularly thought of carrying a suitcase while older children did not. If one has a preconceived and unargued notion that encoding ability must increase with age, as the test authors do (Paraskevopoulos and Kirk, 1969: 160), and if one has decided that increased ability is to be determined by a greater number of certain elements being displayed, one can ensure that the test "confirms" this preconception by incorporating as credited elements only those actions which on the basis of experience have been shown to increase in likelihood of occurrence as age increases. The fact that this preservation of the concept of age-related ability is achieved by disregarding patent demonstrations of the ability under assessment is not permitted to trouble. Similarly ignored in the concern that the test's capacity for making age discriminations be preserved is the oft-repeated refrain that the test is designed to discriminate processes within, not levels of ability among children. This is not simply ignored, it is overridden. Finally, and most importantly, it is not noticed that the decision to score only certain gestures as manifestations of the encoding process is inconsistent with the stated purpose of the test and vitiates the interpretation that can be made of it; for now it is not the subject's ability to express ideas by means of gesture that is being assessed, but what ideas he so conveys. More specifically, it would be inferred from the performance of a child whose pantomimes lack many of the selected elements but contain such non-credited gestures appropriate to the pictured objects as tightening the strings on the guitar, blowing out the match, focusing the camera, moistening the reed on the clarinet, etc., that whatever mechanisms are responsible for the encoding of ideas into gestures were deficient. Yet the subject's test performance clearly demonstrates the exact opposite. What the subject has failed to do is to pantomime certain preselected actions, to encode

certain ideas, but she has certainly encoded ideas manually--she is manifestly capable of expressing ideas in gestures. The diagnosis of a deficit in such cases is clearly unjustified and incorrect.

But what if a child produces few gestures or even none at all? To what do we attribute such poor performance, such a low score? The purported inference is not (a) that the subject didn't know what the object's were or (b) that she didn't know how they were customarily used, or (c) that she didn't recognize them (decoding) or (d) that she failed to think of displaying known functions (association). Rather, the supposedly unambiguous inference is that despite the presence of ideas, she (e) couldn't express them in gestures. The inference to (e) attributes the observed failure to the malfunctioning of a specific process, encoding, in a specific channel, motor. While it is possible in light of this test's design to reasonably but provisionally exclude (a), (b), and (c) as likely determinants of item failures, factor (d), far from being justifiably discounted, looms large as a most plausible explanatory factor. There is simply no justification for taking the child's non-production of the required pantomime elements as evidence of an encoding deficit, rather than of a failure to think of (call to mind) the required elements. There is certainly an important difference between not being able to execute desired gestures and not thinking of gestures to display. This type of refrain must be becoming quite familiar by now. With yet another ITPA subtest, we find little reason to accept the preferred inference. It is at least certain that the observed performance does not in any way endorse an inference to (e) over an inference to (d). And if (d) is the sort of thing the authors would classify as association, as it appears they would, then one can say that the test makes no distinction between encoding and association. It is important to note further that other factors such as memory, recall, motivation, and emotional inhibitions, whether regarded as part of or distinguished from the association process, are also viable factors in the explanation of the test item failure. Acknowledgement of such factors establishes beyond question that the inference to an encoding as distinct from an association deficit, or some combination of both, or some other factor is totally unwarranted. The diagnosis of a malfunction in a specific process and channel is blatantly, in this test as with all the ITPA subtests, underdetermined by the evidence.

Kirk and McCarthy's manner of coping with this objection is ingenious. It is to include what they elsewhere distinguish as the

separate process of association under the heading of encoding. This perhaps explains why we are nowhere given, for either of the encoding subtests, an account of how the association process was excluded. It wasn't excluded. It was simply absorbed into the process of encoding, included under the banner 'encoding'. Thus Kirk asserts, "In manual/motor expression, for example, three types of difficulty have been noted: the child may lack basic motor skills; he may lack ideas leading to motor expression; or he may not make his ideas operational. The teacher, then, is asked to make further diagnosis of the child's functioning to find out why the tasks presented are difficult for him..." (Kirk and Kirk, 1971: 134) (my italics). Yet we thought that diagnosing why the child had difficulty was the purpose, the achievement, of this subtest and that the diagnosis revealed it to be the third type of difficulty. The recommended remediation for a child diagnosed as having a motor expression deficit includes the following:

Demonstrate the kinds of ideas that can be expressed motorically: actions...emotions...occupations and personalities...physical qualities...directions...(Demonstrate) common gestures: "Come here," "shh," "Go away"....

Develop imagination or make-believe by: dramatizing stories... acting out songs...helping the child identify with some character or animal in a story...imitating inanimate objects... (Kirk and Kirk, 1971: 176.)

I can read these remarks as nothing other than admissions that the subtest makes no distinction whatsoever between encoding and association in the authors' terminology--between not being able to communicate in gestures and not thinking of things to communicate. The remarks indicate Kirk's acknowledgement that the test performance provides no discrete diagnosis of the former since teachers are advised to determine whether such factors as emotional inhibition, limited imagination, lack of motivation, etc., underly the observed deficiency (See Kirk and Kirk, 1971: 175). The only factors reasonably excluded by the subtest are, as we have seen, lack of knowledge of the object and its customary use, and inability to recognize it on a given occasion. But even here one could question in particular cases the familiarity of the objects and their customary employment. If a child is not familiar with binoculars, stethoscopes, combination locks and clarinets, his failure to display the required pantomime reveals a deficit in knowledge, not in manual encoding. The mere fact that all children get to see pictures of the objects in no way guarantees that they know what they are and how to use them.

One may well ask why an assessment of a child's ability to pantomime is of any importance. We have seen that this is not a psycholinguistic activity and would further note that most children taking the test are capable of speech. Why assess the ability of speakers to communicate in gestures? The answer is found in the following passage:

Remediation in this area (manual expression) aims to increase a child's ability to express his ideas in nonverbal, motor terms. Some deaf children learn finger spelling or the manual language and are able to express their ideas by use of their fingers or hands. This is one form of manual expression. There are various reasons that a child fails to develop this ability. Sometimes he lacks the basic body orientation to make such activity meaningful. Sometimes he lacks the ability to express an idea in nonverbal terms. Sometimes his cultural and experiential background has not stimulated this kind of communication. He therefore is apt to find it difficult to draw, write, gesticulate, or demonstrate manual operations. This often hinders academic progress. (Kirk and Kirk, 1971: 175.)

It is clear in the first place that the authors confuse the notions of nonverbal and nonvocal (as they do the notions of verbal and vocal elsewhere). All gestural communication is nonvocal but not all of it is nonverbal as is claimed here. It is also clear that the authors make no distinction between iconic gestural communication such as that required on this test and noniconic gestural communication systems such as finger spelling, the sign language of the deaf, and (taken loosely) handwriting. Finally, as we have already noted, the verbal-nonverbal distinction is glossed over as well. Finger spelling, signing, and handwriting are all verbal activities; pantomime is not. The former are all noniconic gestural communication systems. Pantomime is iconic. Cognizance of the radical differences here serves to dissolve the breezy predictions of difficulty in such noniconic verbal activities as handwriting which the authors make on the basis of the children's pantomimes. The assumption made in the above passage that these gestural activities are all the same sort of thing is certainly incorrect. On the contrary, the test behaviors and the nontest behaviors about which predictions are made differ in every relevant respect. The test can claim to be no more than a measure of the child's "ability to pantomime," arbitrarily defined, and the import of obtaining knowledge of this capacity is not at all apparent.

F. A NOTE ON LEVELS AND MODELS

Before examining three of the subtests at the automatic level, some effort at clarifying what the "level" distinction involves is advisable. The levels notion is addressed by the authors in their presentation of the test quoted in Section II of this paper. As presented, those processes of language perception and production which involve the subject becoming conscious of meaning and which may be brought largely under voluntary control, e.g., association (thinking), are described as representational. The term is taken from the fact that these processes are regarded as involving or, at least, as being modelled after the Osgoodian processes involving the representational mediation process (r_m). Those processes involved in the perception and production of language "in which the individual's habits of functioning are less voluntary but highly organized and integrated," and which do not involve consciousness of meaning are called "automatic". The phonetic processing of the speech resulting in the continuous physical signal being perceived as separate words is taken as an example of automatic level processing. Similarly, on the production side, the automatic, i.e., the essentially non-reflective, habitual use of correct word order, agreement between subject and verb, etc., are treated as the product of automatic level processes.

The crucial notion here is that the authors consider habit mechanisms in the behaviorist sense responsible for these phenomena. Hence the listener's ability to hear a string of sounds as the word 'cat' is explained by the fact that these sounds have been paired frequently in his experience and have set up a corresponding chain of response in his nervous system. Similarly, a child produces words in grammatical order because the redundancies in his experience of hearing words in correct order have resulted in the establishment of a corresponding internal response chain. Thus McCarthy asserts that the roof of a house and its base are two stimuli which always occur together in the external world, and this invariant contiguity will be reflected "through ordinary principles of conditioning, contiguity and repetition" in the nervous system of the learner such that perception of part of a house will enable the subject "to produce a

neural image of the whole house." (McCarthy, 1974: 56.) The same stimulus contiguities are presumed to be operant between the letters of a word and between one word and another, giving rise to the speaker's ability to identify words or sentences upon hearing only parts of them --the so-called phenomenon of closure. When the external stimuli are only paired with some frequency, rather than invariably, the resultant neural patterning is called "predictive" since the occurrence of one stimulus may or may not fire the remainder of the chain; when the external stimulus pairing is invariant "the resulting neural patterning is called evocative since the activation (evocation) of any part of the neural assembly is sufficient to fire the entire assembly." (McCarthy, 1974: 56-7.) All of this is taken directly from Osgood (1957a) and is reiterated by McCarthy in 1974 despite the fact that in the intervening years such associative theories of speech perception and production were shown to be inadequate (See Fodor, Bever, Garrett, 1974; Miller and Chomsky, 1963). My only concern here is that the reader appreciate that the automatic processes are regarded by the authors as consisting in the operation of stimulus-response chains in the traditional behaviorist sense. The mediated response is not involved. Closing with a comment from McCarthy:

These configurations are learned (acquired) through conditioning mechanisms (previously described) and are named after their manner of stimulus patterning. The entire level, because it seems automatic and typically handles sequences of s's and r's is called the automatic-sequential level. Because of the absence of mediational representations, meaning is not involved at this level; it is sometimes called the nonmeaningful level. (McCarthy, 1974: 59.)

The tests of processes at the automatic level are not separated into those involved in encoding, decoding and association, but rather are what the authors describe as "whole level" tests.

A look (below) at the Osgoodian model in relation to the ITPA model may be helpful as a final aid in understanding the relation between the subtests and the various processes generally and the levels distinction in particular. With a little effort Diagram A, Osgood's Theoretical Model, can be related to Diagram B, Clinical Model of the ITPA (I have included the model for the original ITPA simply because of its visual similarity to the Osgood model). It should be apparent from this how the tests discussed so far--numbers 1, 2, 3, 5, and 6--relate to the Osgoodian processes of decoding, association and encoding involving the r_m ; while the tests at the

automatic level, which have been added to and renamed, do not involve it. Those we will be considering are the Auditory Closure subtest, the Grammatical Closure subtest, and the Sound Blending subtest which would correspond to number 7 on the ITPA model and with predictive and evocative s-r neural response chains on the Osgood model.

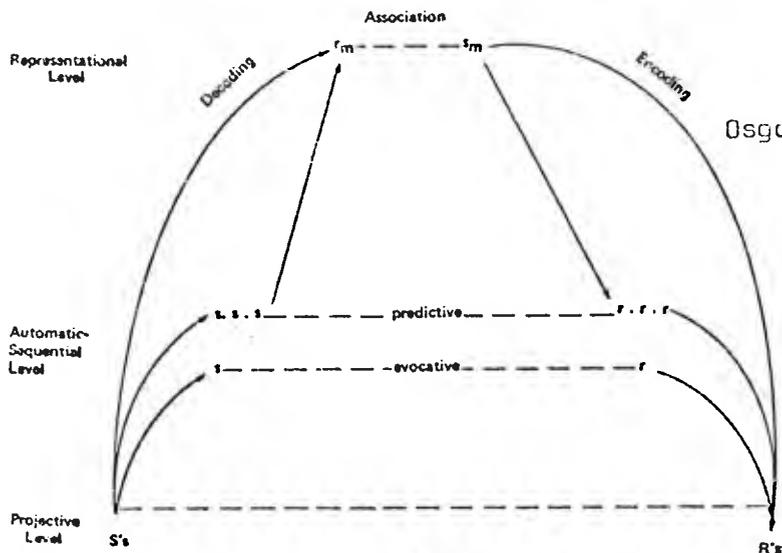


Diagram A.
Osgood's Theoretical Model

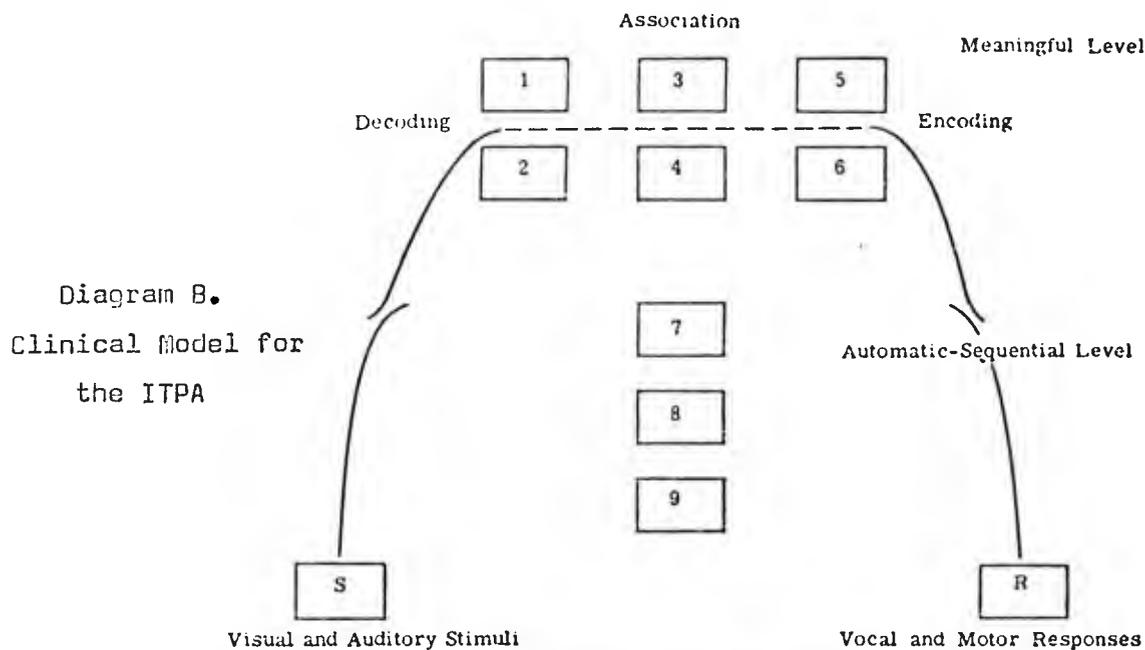


Diagram B.
Clinical Model for
the ITPA

- | <u>Meaningful Level</u> | <u>Automatic-Sequential Level</u> |
|-------------------------------|-----------------------------------|
| 1. Auditory Decoding | 7. Auditory-Vocal Automatic |
| 2. Visual Decoding | 8. Auditory-Vocal Sequential |
| 3. Auditory-Vocal Association | 9. Visual-Motor Sequential |
| 4. Visual-Motor Association | |
| 5. Vocal Encoding | |
| 6. Motor Encoding | |

G. THE GRAMMATIC CLOSURE SUBTEST

Of all the subtest descriptions, this one is the most explicit with respect to the authors' behaviorist commitments. We are left in no doubt that a child's customary production of appropriate grammatical forms is regarded as the result of habit formation in the behaviorist sense. On this understanding the response utterance that is actually produced by the subject on the test is taken as an indication that the presumed S-R mechanism is functional; for the stimulus utterance is regarded as eliciting the response utterance as a conditioned response. What we have on this test, then, is an explicit statement of the behaviorist account of language processing which is implicit in all of the subtests. Thus, McCarthy writes:

Though limited in flexibility, association at the automatic-sequential level is a process critical to proper language acquisition and usage. We have tried to assess the function of automatic-sequential associational processing by tests requiring the recall of auditory and/or visual sequences and by the demonstration of the acquisition of those "automisms" involved in grammatical rules. It should be noted that we regard the use (and maybe development) of grammatical mechanisms as occurring at the automatic-sequential level, predictive sublevel. Imitation, generalization, and inhibition probably play large roles in the acquisition of grammatical mechanisms. When we test a child's grammar, we are interested not in knowledge of grammar per se, but in the development of those predictive automatic-sequential mechanisms that make grammar (as well as corresponding nonlinguistic skills) possible. (McCarthy, 1974:62)

Kirk writes in a similar vein:

In the Grammatical Closure test an attempt has been made to test the degree to which a child has acquired automatic habits for handling syntax and grammatical inflections....The verbalized rules of grammar are not important. The grammar itself is not important. The meaning is not important, for the function we are dealing with here is at the automatic level and deals with a highly integrated automatic response rather than a meaningful interpretation of symbols. The grammar is merely a medium through which to observe the ease with which a child utilizes the redundancies of his experience to learn to predict and use common verbal expressions. (Kirk and Kirk, 1971: 112)

The suggestion is, as best I can determine, that the frequent occurrence of correct word order in speech has resulted in a dependency relation being established between adjacent words. Thus, the child says "they are," "they went," "they came," and not "they is," "they goes," and "they comed," since the former pairings have occurred frequently and resulted in the formation of habit mechanisms while the latter

pairings have rarely been heard if at all. "Grammar," Kirk writes elsewhere, "is a habit acquired automatically." (Kirk, 1966: 26) The child's production of the correct forms is "a highly integrated automatic response," the product of an established "automism." This view is typical of the behaviorist school in the psychology of language. (See Ruth Clark, 1975)

Fortunately, from the standpoint of the criticisms which we will advance, a clear understanding of the character of the underlying mechanism being proposed here is not required. There exists ample evidence and argumentation to discredit behaviorist accounts of children's acquisition of syntax. (See Chomsky, 1959; Fodor, Bever, Garrett, 1974; Clark, 1975) But since what is at issue is whether the child's test performance can support inferences concerning any speech processing operations, the specific character of the proposed processes need not be known. The test is described as follows:

This test assesses the child's ability to make use of the redundancies of oral language in acquiring automatic habits for handling syntax and grammatic inflections. In this test the conceptual difficulty is low, but the task elicits the child's ability to respond automatically to often repeated verbal expressions of standard American speech. The child comes to expect or predict the grammatic form so that when part of an expression is presented he closes the gap by supplying the missing part. The test measures the form rather than the content of the missing word, since the content is provided by the examiner.

There are 33 orally presented items accompanied by pictures which portray the content of the verbal expressions. The pictures are included to avoid contaminating the test with difficulty in the receptive process. Each verbal item consists of a complete statement followed by an incomplete statement to be finished by the child. The examiner points to the appropriate picture as he reads the given statements, for example: "Here is a dog; here are two ____." "This dog likes to bark; here he is ____." (Paraskevopoulos and Kirk, 1969: 20)

Other items on the test, which I include to give a fuller impression of it are such as these: "This boy is writing something. This is what he ____." "This child has lots of blocks. This child has even ____." "This man is painting. He is a ____." "Here is a mouse. Here are two ____." And in concluding this presentation of the test, it must be understood that according to Kirk, "the test increases in difficulty by requiring the correct use of increasingly less familiar English inflections of nouns, verbs, and adjectives." (Kirk, 1966: 27)

It is evident that success on these items involves both knowledge of standard English and the satisfactory operation of whatever processes are involved in calling that information into play on the test occasion. In a word, there are both knowledge and performance

variables. In general, psycholinguistic researchers are careful to distinguish between these two and a third set of so-called "pragmatic" factors when using the child's spontaneous utterances as evidence of mechanisms underlying them (See Limber, 1975). For our purposes, attention to the knowledge and performance factors is sufficient. Specifically, there exist two possibilities for the nonproduction of the required correct form by the test subject: either the child doesn't know the correct form (hasn't acquired it as a conditioned response in the authors' view); or knowing it, fails to utter it. Any explanation or diagnosis of the failure to produce the required form must at least take into account these two possibilities. The response may not be available, or it may be available but not produced.

The information/performance distinction is readily brought out by example. Suppose that a child with only a moderate knowledge of English, e.g., a recent immigrant, were administered the Grammatic Closure Subtest and missed many items. The interpretation of these errors as indicative of a language processing deficit would be conspicuously unwarranted. Presumably the child is fluent in his native language and would be expected to do well on the same test given in that language. Clearly, what is revealed by his performance is that his knowledge of English is limited and not that he has some deficit in his ability to process speech. His deficit is in his knowledge of English, not in "the degree to which... (he) has acquired automatic habits for handling syntax and grammatic inflections." (Kirk and Kirk, 1971: 112) This difference receives support from the fact that precise replicas of the Grammatic Closure subtest items are to be found on virtually any test of English as a second language--where clearly what is under assessment is the subject's knowledge of English, not her capacity to process speech (See Corder, 1974). The distinction between information and performance variables in accomplishing such tasks with the corresponding difference in the interpretation of task failures should be quite clear. It should also be clear that the inference to a knowledge of English (information) as opposed to a language processing deficit is to be favored in cases other than that of a foreigner. The child who is in the earlier stages of language development is similarly a 'foreigner' to some of the standard forms required, e.g., correct use of the third person plural reflexive pronouns. So also is the child who has been exposed to and has acquired a non-standard English dialect. In a word, in any case where there is good reason to

believe that the target forms have not been learned by the subject, the inference to an information deficit must take precedence over a defective processing interpretation of test performance. Kirk comes close to admitting this:

The ITPA does not circumvent the effects of cultural factors. Every examiner must be cognizant of these effects on test results. A child from a language-deprived home, for example, could understandably be deficient on the Grammatical Closure subtest of the ITPA, since this subtest involves grammatical usage to which he may not be accustomed. Although an effort has been made to eliminate from this test items frequently misused by adults using non-standard English, some of these do remain and should be noted in evaluating a child's score. Analysis of the items failed in some cases may mitigate a low score as indicating a disability in auditory closure or the ability to acquire automatic responses. It would indicate, instead, that certain auditory experiences would pass this child by because of his deficient background, and this omission could affect the development of other functions. (Kirk and Kirk, 1971: 98)

Kirk fades into mystery at just the place where we expect the truth to pass his lips. The truth is that evidence that the subject has acquired nonstandard English forms renders the inference of a deficit in "the ability to acquire automatic responses" groundless. To substantiate such a speech processing deficit interpretation of test performance in the light of patent evidence of an information deficit requires justification for discounting the latter. This is not forthcoming. Clearly the child who produces such nonstandard forms as "hisself," "mines," "hisn' s," or "mans" may plausibly (I would say most plausibly) be regarded as having well and truly acquired grammatical habits in the conventional sense, though the grammar differs from that of Standard English. (See Glucksberg and Danks, 1975: 158) There is no deficit here in acquiring "automisms"--there is simply a difference in the automisms acquired. In light of such considerations Kirk's claim that children who have acquired nonstandard dialects should have difficulty making use of "auditory experience " (presumably experience of spoken language) which "could affect the development of other functions" (presumably reading and writing) is, to the extent that it is interpretable, utterly without foundation. What we have here is a strained attempt to construe data in accordance with a preferred interpretation, despite the conspicuous presence of countermanding evidence. The same sort of effort was made in the case of the Vocal Encoding subtest, where the acknowledged talkativeness of a child in nontest situations was to weigh but lightly and hesitantly against the diagnosis via the test that the child had a "disability in expressing ideas in spoken words." (Kirk and Kirk, 1971: 108-9) There, as

in the present case, it is the preferred test interpretation and not, as the authors would have it, the external data that is to be regarded as very weak.

It has not been my intention in all of this to argue the restricted case that the ITPA may well discriminate against certain categories of children. I certainly believe that this is a likelihood, given the non-discriminative character of the test items and the inattention in the test literature to the role that knowledge of Standard English plays in test performance. My central interest, however, has been simply to establish that the test items are indeed non-discriminative, i.e., that they give no advantage to a processing deficit over an information deficit explanation of test item failures. Low test scores do not for this reason unequivocally support the diagnosis of a malfunction in language processing in the case of any child. The test is at least as much a test of a child's knowledge of Standard English as it is of the processes which are responsible for bringing that knowledge into play in completing the test items. To read Kirk and McCarthy on this matter, however, one would gather that the knowledge of grammar factor could be eliminated by decree:

The verbalized rules of grammar are not important. The grammar itself is not important....(Kirk and Kirk, 1971: 112)

When we test the child's grammar, we are interested not in the knowledge of grammar per se, but in the development of those predictive automatic-sequential mechanisms that make grammar... possible. (McCarthy, 1974: 62)

Let us then consider the interpretation of the test performance of children who are known to have had considerable exposure to the target forms. In the case of high test scores we can surely infer that the child both has the requisite information and has operant those processes responsible for making use of it. Can we not with justification also regard a low score as evidence of a processing deficit, given the fact that the child is known to have had considerable exposure to the target forms--thus diminishing the grounds for treating nonproduction as evidence of an information deficit? The answer, I should think, is 'yes,' as long as one is aware of the generality of this processing inference. One theorist may hold that the process involved is one of information retrieval and that nonproduction may indicate faulty storage, faulty retrieval, or both. Another may hold, as the test authors do, that what we have here is the non-appearance of a conditioned response despite enough exposure for an S-R dependency relation to have been established. Either there is some sort of

interference with the habit mechanism or the organism has failed to acquire the habit (S-R dependency relation). It is the latter which the ITPA authors specifically infer (see quotation opening this section). But such a specific process inference as this is not in the least justified by nonproduction of the required form. The authors have again greatly overstepped the evidence. For the preferred inference can only be correct if the authors are in fact right about learning grammatical forms being a matter of acquiring interverbal S-R dependencies, i.e., of habit formation. And even then we would need to be told why the nonproduction of the target forms is taken as indication that there is a deficiency in the mechanism for acquiring the necessary S-R dependencies and not some sort of interference in the functioning of some that had been acquired. Hence the preferred inference is so clearly unwarranted as to merit no serious attention.

Even on the factual issue of how grammatical forms are acquired, the test can be shown false. Quite briefly, the authors have assumed that frequent exposure to the correct forms, while not a sufficient condition for their acquisition, is a necessary condition. They not only do, but must hold to this, given that they treat the learning of such grammatical forms as a case of habit formation in the behaviorist sense. The assumption is that the correct grammatical forms are acquired via frequency of exposure so that the child who produces them is ipso facto demonstrating his "ability to make use of the redundancies of experience in acquiring automatic habits for handling syntax and grammatic inflection." (Kirk, McCarthy, Kirk, 1968: 11) Conversely, it is inferred that the child who fails to produce the forms despite a history of frequent exposure is indicating that the mechanism responsible for internalizing "what is heard over and over again" (i.e., the habit mechanism) is malfunctioning. It is upon this belief about the necessary role of repetition in acquiring certain verbal constructions that both the diagnosis and the predictions concerning other areas of expected difficulty are based (See Kirk and Kirk, 1971: 112-13).

But manifestly, it is not necessary to make use of redundancies, "to internalize what is heard over and over again" (Kirk and Kirk, 1971: 112) in order to learn grammatical forms. The plural of 'child', the past tense of 'bring', the comparative and superlative forms of 'good' may be learned on single occasions--by being told, reading a grammar book, hearing them used, etc. Indeed it is characteristic of language users that they are capable of producing correct inflected forms and grammatical constructions which they have neither heard nor

uttered before. And in a well-known study by Jean Berko (1958) children produced the "correct" inflectional endings for words that they had never heard before--the plurals of such nonsense words as 'blik,' and 'wug,' the possessive of 'bik,' etc. Quite ironically, the authors considered using facsimiles of the Berko test items for the Grammatical Closure subtest; unaware, it seems, that children's performance on such posed a serious challenge to their account which linked the acquisition of inflected forms to frequency of exposure. (See Berko, 1958; McCarthy and Kirk, 1963.) Kirk himself notes, "The young child may fail to use verb tenses or plurals or idioms in an acceptable manner. He may speak of a 'mop' as a 'mopper' or an 'iron' as an 'ironer,' both of which have a logical basis but do not follow the customary form. At this automatic level of functioning, the child learns what is habitual or customary rather than what is logical." (Kirk and Kirk, 1971: 112) On the contrary, the children are clearly following some rule and are "producing what is logical" and not habitual. (See Slobin, 1971; Clark and Clark, 1977) Presumably 'mopper' and 'ironer' are words that have rarely if ever occurred in the child's verbal experience, yet as Kirk himself has correctly observed, they do occur in his speech. Finally, the empirical studies done on the relation between frequency and the learning of grammatical forms give no support to the authors' crucial assumption. Roger Brown, perhaps the foremost contemporary researcher into child language, after reviewing the research on frequency, had this to say about it:

Frequency and perceptual salience will be minor determinants of order of acquisition. The possibility that the frequencies with which either specific utterances or construction types are modelled for small children affects order of acquisition has been exhaustively probed in Stage II. The upshot of the several kinds of test made is that, for the fourteen English grammatical morphemes, there is no evidence whatever that frequency of any sort is a significant determinant of order of acquisition. (Brown, 1973: 462; see also Clark and Clark, 1975: 346)

What such studies indicate is that the behaviorist assumption that frequency is a critical factor in this aspect of language learning, which is implicit in the proposed interpretation of test performance and upon which the interpretation is dependent for its truth, is false on the available evidence. The interpretation of test item failures and the remedial recommendations calling for exposing the child to frequent repetition of the forms that one wishes him to learn, would appear to be without foundation (See Kirk and Kirk, 1971: 154-8).

In concluding this discussion of the Grammatical Closure subtest

I shall mention three other nagging issues. The first is that if the authors were consistent in their behaviorist assumptions, the later-occurring test items ought to be those to which children in general are less frequently exposed. It is known both that irregular forms such as 'went', 'came', 'did', etc. occur very frequently in the adult speech to which children are exposed and that they are among the first to be learned. Yet on the Grammatical Closure subtest, all irregular forms occur in the latter half of the test, suggesting that the metric of difficulty was not frequency, but an unfounded assumption that "irregulars are harder." Second, test users need to be aware of the well-documented and apparently universal feature of language development known as over-regularization (Slobin, 1971; Clark and Clark, 1975). If it is a fact that in normal language development children go through a stage of producing such forms as 'goed', 'comed', 'catched', etc., following a period where the correct forms have been used, and preceding a final return to the correct form; then children who are in the over-regularization stage in their language development are not revealing any kind of deficit in their production of such forms on the test. They are, of course, scored as incorrect. Finally, there is the curious character of Item 19, "Here is a soap. Here are two _____." 'Soap', like 'water', 'butter', and 'snow', is a mass noun, which means that its referent is not countable. There may be one or two bars of soap, like one or two glasses of water; but there are not two soaps, and the appropriate "automatic response" to this test item would appear to be silence.

In light of the manifold difficulties that have been raised, the inference to any deficit in the child's processing of language on the basis of his performance on this test would appear to be without warrant. What we have here is an assessment of children's knowledge of an arbitrary selection of standard English locutions masquerading as quite something else.

H. THE SOUND BLENDING SUBTEST

This subtest is one of two introduced in the revised edition of the ITPA to supplement the Grammatical Closure subtest in the purported assessment of closure ability. The subtest is presented as follows:

This test provides another means of assessing the organizing process at the automatic level in the auditory-vocal channel. The sounds of a word are spoken singly at half-second intervals, and the child is asked to tell what the word is. Thus he has to synthesize the separate parts of the word and produce an integrated whole. (Paraskevopoulos and Kirk, 1969: 21)

Again it is claimed with no evidence to support it that the task draws on habit mechanisms which by definition have been established via redundancies in the experience of the learner (see McCarthy, 1974: 59-62; Kirk and Kirk, 1971: 24, 154). Since it is impossible to believe that children have had anything more than negligible experience (if any) with the sounds of words "spoken singly at half-second intervals" followed by utterances of the word, this claim cannot be taken seriously. There is simply no justification whatsoever for treating the child's test response as a conditioned response to the stimulus utterance. It is certainly not habitual in the behaviorist sense. Nor is it habitual in the conventional sense if by this we mean it has the features of being immediate, regular and non-deliberative. Anyone doubting that the synthesis of separated speech sounds into words is not habitual in this sense need only play the test record on which the separated sounds are presented to be assured of the conscious effort and guesswork that goes into supplying the supposedly automatic response.

The sound blending subtest seems to be based on this conception of ordinary speech perception: When we hear the word 'cat' we hear in rapid succession three sounds that are represented by the three letters of the orthographic representation of the word. C-A-T are the letters and K-A-T are the sounds. Similarly, in hearing the word 'dinner' we are hearing four sounds: D-I-N-N-E-R rapidly blended; 'telephone' has seven, T-E-L-E-F-O-N. On the sound blending test, these sounds are uttered separately, thus presenting the child with the ordinary speech perception task in, one might say, slow motion. The task is regarded by the authors as "automatic" because the child is believed to have frequently heard such sound sequences, presumably every time the whole

words have been heard. As the authors put it, "some children learn these redundant units of experience more readily than others. These redundancies include common sequences of phonemes in words..." (Kirk and Kirk, 1971: 154). Thus, the test items are taken to be presenting the child with the discrete links of a chain of sound events which have occurred often in the experience of a child in their interlocked form. The child presumably has established an internal habit chain on the basis of his experience of these sound sequences and it is this habit mechanism that the test item activates. This may give some sense to the characterization of the test as a matter of automatic habits. It is certainly the basis of the authors' belief that this subtest "taps" a habit chain, thus permitting inferences concerning its operation in particular children.

This entire program is undermined when one recognizes that the sounds presented on the subtest not only are not, but cannot be those which are heard when the word is ordinarily produced. The sound sequence presented on the test is simply not a slow-motion version of the sounds of the word that the child hears in ordinary discourse. This is due to the fact that it is physically impossible to utter the constituent sounds of a given word below the syllable level of segmentation. To put this another way, the syllable is the minimal pronounceable unit of speech. Constraints imposed by the human articulatory apparatus are such that it is impossible to utter any consonant sounds without appending some vowel sound, usually "uh." The point is put well by Gleitman and Rozin:

Every reading teacher has experienced the difficulty of explaining to a child which features of the spoken language are represented by such letters as P. It is impossible even to pronounce this entity without adding a vowel (thus, "puh"). The child must somehow discern that in the instance "puh" the "uh" is an artifact, and only the "p" was intended. To get this obscure point across, we sometimes try such tricks as saying "puh-ah-tuh, say-it-very-fast, pat." Yet we know that "puh-ah-tuh," regardless of speed, never will sound like pat. When the teacher asks the child to blend such units (to pronounce the sounds in such a way as to obliterate the demarcation line between them), this is in some ways tantamount to asking the child to "know how to read." This is so because the inability to say or hear most consonants without adding a vowel is grounded in the nature of human speech perception and production. In short, blending of alphabetic units can be accomplished conceptually (and this is a fundamental component of reading skill), but it cannot be accomplished physically. (Gleitman and Rozin, 1973: 457-8)

The "puh-ah-tuh" phenomenon alone serves to falsify the authors' interpretation of test performance. Not only is the child not being

presented with the phoneme sequence he has heard in his perception of ordinary speech, but he could not be. In being presented with "kuh-ah-tuh" the child is not hearing the true phonemes he customarily hears in "cat." On the contrary, he is hearing the five phonemes of "kuh-ah-tuh," two of which, "uh" and "uh" never occur in the word "cat." Similar criticisms apply to all items of the test. The Sound Blending subtest is not presenting the child with the links of the phonetic chain that he hears in ordinary speech and there is no warrant for any inferences from the child's performance on this task to the operation of his speech process mechanism. The sound sequences confronting the child on the test are utterly novel.

Yet another way of making the point that the sound blending task is radically different from that facing the child in ordinary speech perception is to point out the phenomenon of "shingling"--a term used to refer to the fact that in the flow of speech, the actual physical sound of any given phoneme is affected by the phonemes uttered in conjunction with it. Roughly this means that the "ah" in "bag" will produce a different pattern on a sound spectrograph than will the "ah" in "cat" because of the different position of the mouth, tongue, teeth, etc., taken before and after production of the "ah" sound. This being so, the isolated vowel sounds separated by half-second intervals of silence as presented on the sound-blending task will not be the same as those occurring in utterances of the word in ordinary speech. Hence the similarity of the two situations is again called into question, though it may be that they are not different in relevant respects. The subtle differences raised in this argument are in any event insignificant relative to those addressed earlier. That the authors were not aware of the radical difference between the test task and the ordinary speech situation is evidenced by the fact that they recommend examiners to consult speech specialists in order that the sound blending be done properly. The view is that one can utter at the phoneme level "the sounds of a word...at half-second intervals":

Adequate sound blending is a technical skill that is difficult for examiners unless they have had training in sound blending. Anyone who administers this test with less than adequate ability to present the materials smoothly is giving an invalid test....Distortions occur, for example, when the examiner sounds f as "fuh," c as "cuh." Those examiners who are not trained in sound blending should be checked by someone who is familiar with the system. (Kirk, McCarthy, Kirk, 1968: 85)

But as we have learned, these are not distortions but inevitabilities that neither practice nor professionalism can overcome. Quoting

Gleitman and Rozin, "some teaching manuals and testing devices caution users not to introduce the vowel sounds into their renditions...the tester cannot obey such advice, since the articulatory apparatus does not allow production of the intended speech sound in very many instances. (Gleitman and Rozin, 1973: 458)

With the conditioned response or automatic habit view of the child's test performance discredited, the sound blending tasks merit the characterization of requiring the subject to do with deliberation and awareness what he ordinarily does automatically and unconsciously. This raises serious doubts as to whether the level of performance on the former warrants any inferences about the latter. The implicit and automatic phonemic analysis which is part of speech processing is supposedly being made available for assessment by artificially separating the speech sounds of single words and having the subject consciously synthesize them. The task confronting the child is one of considerable conceptual sophistication for which his day-to-day perception of speech has provided no preparation. Sound blending is quite literally an extraordinary task. Just how complex and cognitive, as opposed to perceptual, is suggested by Rozin and Gleitman's comment, "The child is asked to hear, identify, and later blend three items that the teacher instances as three syllables...the child's real task (approximately) is to identify three phones that appear within the teacher's three spoken syllables, and then to pronounce the three-phone monosyllabic outcome." (Gleitman and Rozin, 1977: 43) My point here is simply that the sound blending task requires reflection. It is not automatic and it would appear as good a characterization as any that in being presented with anything from two to seven syllables from which the relevant phoneme must be extracted, the child may be engaging in a routine of spelling the word out in her head. The issue is that performances on this task would seem to warrant no inferences regarding ordinary speech processing since the connections appear so remote. Could anyone "synthesize" the word 'refrigerator' from a (necessarily syllabic) presentation of its constituent phonemes, all twelve of them, at half-second intervals? Yet young children readily identify this word in the flow of conversation. I fully agree with the judgment "that the child who finds it difficult to make explicit the phonetic segmentation of his speech need not have any problems at all in the regular course of speaking and listening." (Lieberman, Shankweiler, et al, 1977: 210) There is indeed mounting evidence that the conscious identification of phonemes is quite generally difficult for

young children. What evidence there is reveals what one would expect, i.e., that there is no connection between the child's ability to consciously identify phonemes and his ability to perceive speech (in which the unconscious identification of such phonemes does take place). (See Savin, 1972)

In conclusion, I would add that the authors take for granted a connection between sound blending and reading success and sound blending difficulties and reading difficulty that is never shown. There is/was substantial popular belief but scant support for the ability to blend sounds being a skill necessary for reading. Savin, Gleitman, Rozin, Liberman, Gibson and others have all provided strong challenges to this view. The evidence runs contrary to what is no more than speculation on the part of the test authors. (See Gibson and Levin, 1975; Gleitman and Rozin, 1973, 1977; Savin, 1972; Liberman, Shankweiler, et al, 1977.)

I. THE AUDITORY CLOSURE SUBTEST

On the Grammatical Closure subtest the subject was required to produce the missing part of a sentence; in the Auditory Closure subtest he is required to identify a word, having heard only part of it. The test is described in this way:

This is basically a test of the organizing process at the automatic level. It assesses the child's ability to fill in missing parts which were deleted in auditory presentation and to produce a complete word. Auditory closure is an automatic function which occurs in everyday life in situations such as understanding foreign accents, speech defects, or poor telephone connections. In this test the child is asked, "What am I talking about--- bo / le? tale / one?" There are 30 items ranging in difficulty from easy words such as "airpla / " to more difficult ones such as "ta / le / oon" and "type / iter." (Paraskevopoulos and Kirk, 1969: 21.)

Once again, what is purportedly under assessment is the operation of "automisms," or habit chains (See McCarthy, 1974: 62). Again, no empirical evidence is provided in support of treating the acknowledged ability of competent language users to correctly recognize spoken words that have been mutilated by mispronunciation, foreign accents, distortion, noise, interference, etc., as the product of conditioning. What we have here is the all too familiar sleight-of-hand wherein what is habitual in the conventional sense is treated as habitual in the behaviorist sense, the response made by the subject treated as the conditioned response elicited by the task utterance, etc. I take it that the character and the futility of this equivocation have received sufficient attention earlier so as not to require further discussion here. The claim that in this test we are observing the operation of stimulus-response dependencies is simply without substance. The inference that a child who does poorly on the test is manifesting some deficiency "in the development of those predictive automatic-sequential mechanisms that make grammar (as well as corresponding nonlinguistic skills) possible" is equally vacuous. Far from revealing some deficit in the presumed habit mechanisms underlying speech perception at the phonological level, this test would appear to tell little or nothing about a child's speech perception. For the task confronting a child is that of solving a contrived linguistic puzzle wherein she is required to identify words in the absence of most of the information which in ordinary conversation she implicitly

brings to bear on this task. On what grounds is a child's performance on this task to be taken as an indication of her ordinary speech processes? The answer, in my opinion, is on no grounds.

It must be recognized that in ordinary language processing phonological, semantic, syntactic, prosodic, and nonlinguistic contextual clues all contribute to the recognition of individual words. As early as 1951 (Miller et al., 1951; Miller and Isard, 1963) it was shown that words in sentences are recognized more readily than words uttered in isolation when both are presented in conjunction with white noise. In a later study (Miller et al, 1963) it was found that syntax and semantics make a further and independent contribution to the identification of words in speech. One again against a background of white noise it was found that words in grammatical, non-anomalous sentences were recognized most easily, words in grammatical but anomalous sentences came next, and most difficult to identify were words in ungrammatical strings. The role of prosody, i.e., rhythm and stress, in speech perception, is also important, as are facial expression, gestures, etc. (See Darwin, 1976). Perhaps the most revealing study with respect to word perception in continuous speech is that of Pollack and Pickett (1964). Their research is summarized by Clark and Clark:

Although normal conversational speech seems lucid and unexceptionable, it is in actuality quite unintelligible when taken word by word. This has been demonstrated by Pollack and Pickett (1964). They surreptitiously recorded several people in a spontaneous conversation and then played single words excised from these tape recordings to other people for identification. Single words like this were correctly identified only 47 percent of the time--a surprisingly low percentage. To show that this wasn't peculiar to spontaneous conversations, Pollack and Pickett had other people read passages at a normal rate. Single words excised from this speech were correctly identified only 55 percent of the time. When the passages were read quickly, this percentage fell to 41 percent. To the casual listener, however, all of this speech, when heard intact, sounds quite intelligible. People don't have the impression they are guessing at words, filling in for the sloppy speech where intelligibility is nil. (Clark and Clark, 1977: 211-12.)

Interesting also in this respect is the work of Warren (1970) and Warren and Obusek (1971) in which coughs were substituted for entire words or parts of words in tape recorded sentences. Despite the substitution, the missing items were perceived by the listener. There is also a growing amount of literature concerning the role of non-linguistic context in speech perception with the Clarks themselves being among the major contributors. The straightforward point being made is that

in the everyday situation the speaker-hearer has at his disposal a considerable amount of data beyond the phonological which is brought to bear on the identification of a word that receives partial or distorted presentation. Research has progressed well beyond the stage of gathering evidence that phonological, syntactic, semantic, prosodic and contextual factors contribute to the identification of individual words in the flow of ordinary speech to the stage of establishing the relative weight of these factors. Most importantly, there is no doubt that it is the combined contribution of these elements that makes the perception of distorted words in ordinary conversation the speedy and nondeliberative process that it is.

If one contrasts the normal speech perception situation with its multiplicity of factors contributing to word recognition with the situation confronting the child on the Auditory Closure subtest, the irrationality of its design and the injustice of the inference made from the child's performance are strikingly manifest. For on the Auditory Closure subtest the child is faced with a task that he never is faced with in ordinary conversation--the task of identifying words given portions of their phonological data only. Quite simply the child is given a word recognition task while being deprived of a major and important portion of the information which is customarily brought to bear on such a task. It is obvious that the collective input of this information must be regarded as critical in any serious hypothesis concerning the ease with which such word identifications are usually made. Yet the child's performance on this ITPA subtest in which he does not have access to this data is the basis of inferences concerning his ability to make such identifications in the very situations where he does have it. Herein lies the irrationality and injustice of the test inference. Quoting Kirk:

The supplementary Auditory Closure test attempts to measure the phenomenon of closure in a different way. It attempts to measure the child's ability to grasp a word when only part of the word is presented to him. He must utilize closure to recognize the word. It is the same kind of ability needed to grasp a telephone conversation when there are background noises interfering and blotting out part of the sounds heard. It is probably related to the ability to understand speech with a foreign accent or poorly articulated speech. (Kirk and Kirk, 1971: 113.)

I am arguing that the child's performance on this subtest has no relevant parallels with her understanding of inarticulate or distorted speech in the ordinary conversational context. (See Darwin, 1976.) On the contrary, on this subtest the child is given the utterly novel

task of identifying words on the basis of a portion of their phonological data (and perhaps one might say a portion of their prosodic data). In fact, she is not even working with the correct phonological data in this unusual linguistic puzzle since the sounds presented on the test in isolation from the adjacent elements in the words of which they are a part are different from those encountered in the flow of speech wherein the acoustic features are "contextually conditioned" by the surrounding material (See Studdert-Kennedy, 1975; Liberman et al., 1977). This is the matter of "shingling" mentioned earlier in regard to the Sound Blending subtest. But such detail is of little importance here. The central point is that there is a radical discontinuity between the situation faced by the child on the test and the ordinary speech perception situation. The lack of any warrant for inferences from the child's performance in the test situation to his ordinary processing of speech should be as evident here as it was on the Sound Blending test where a similar discontinuity obtained. The same logic which guided the construction of this subtest would lead one to assess the effortless and "automatic" ability of a jigsaw officianado to put the last few pieces in place by handing him the last two pieces at the start of his work and asking where on the table they should go! As with both the Sound Blending and the Auditory Closure subtests, the required test behavior differs from the target behavior about which inferences are made in every relevant respect. In demonstrating the gross disparity between test and target behaviors, the rationale of the test design and the justification of the test interpretation have been simultaneously undermined. We have no reason to accept that the ability of a child to figure out what word is hidden in the sounds "ee / ter / unny" or "a / tronau / " is the same kind of ability "needed to grasp a telephone conversation... to understand speech with a foreign accent...etc." Neither telephone callers nor foreigners speak in such a cryptic code, and even if they did we would have a great deal more at our disposal to figure out what they are saying than is available on this subtest. (See Spolsky, 1968.)

In concluding, three lesser difficulties deserve mention. First, the ordering of the items is obviously based on test trials--the later items most assuredly being those which tripped children up most often. No attempt is mentioned nor apparently was made to relate the ordering of items to the frequency of the target word's occurrence in the child's experience. Yet the frequency metric of difficulty is demanded given the habit mechanism explanation of the child's performance wherein

redundancy is a critical factor. It simply cannot be accepted that such items as "macaroni," "fingernail," and "elephant" which occur much earlier on the test than such items as "refrigerator" and "newspaper" have been heard significantly more often than the latter by most children. Second, in some items, whole syllables are omitted while in others it is a single vowel or consonant; in yet others, two or three sounds are omitted. This random feature alone introduces enough variables in the required performance to make insecure any uniform interpretation of it. A further complication is that it forces us to accept that the intra-verbal habit chains which are supposedly responsible for performance are in some cases the product of phoneme-phoneme redundancies while in others they are syllable-syllable. In what principled way is the effective stimulus unit in the presumed habit formation to be identified? Finally, it must be noted that the question accompanying the presentation of each mutilated word is "what am I talking about?" Nevertheless, such replies as "T.V.," "phone," "car," and "Santa," are incorrect responses to the items "tele / ision," "tele / one," "auto / o / ile," and "an / a / aus/" respectively. The children making such replies are, I submit, observing a distinction between the questions "What am I talking about," and "What word might I be saying" which the authors both ignore and penalize.

VIII. CRITIQUE OF THE TRAIT INTERPRETATION

As explained earlier, the logical character of the Trait Interpretation of the ITPA, wherein various decoding, encoding, and association abilities (or disabilities) are attributed to the child, is the same as that which obtains when we ascribe musical ability, mechanical ability, etc. In ascribing such traits, we are not making an inference regarding whatever processes are involved in bringing about the overt behavior upon which the ascription is based. Rather, the overt behavior itself is the basis of ascribing to the child some "postulated attribute of people, assumed to be reflected in test performance." (Cronbach and Meehl, 1955: 283.)

The general character of the critique of the Process Interpretation has been that of showing the inference to language processing deficits to be unwarranted by the evidence of the child's performance. In showing this, it has frequently been argued that the preferred interpretation is not merely weakened by the presence of competing factors, but that these other factors may well be what actually determine differences in performance. In general, I have argued that it is the child's knowledge of his language and/or of the physical and social world that is the principal determinant of his test performance, with the consequence that this is what is actually being assessed, and this is not the same as assessing the processing of language. It is again this matter of to what we may most reasonably attribute test performance differences that is of importance in evaluating the Trait Interpretation of the test. For since the trait is being ascribed on the basis of test performance, it is essential to know what is determining that performance. Only then will we be able to understand what such attributions actually tell us about the children to whom they are ascribed. There is, therefore, this logical connection between the Trait Interpretation of the test and an understanding of the factors relevant to test performance that links the present section with its immediate predecessor.

Since the terms for the various psycholinguistic abilities or disabilities are not part of our everyday language, we are dependent upon the authors for an explanation of their meaning. This obligation is fulfilled by the authors when they provide the characterizations of

the psycholinguistic abilities which they do. Thus, we learn that Verbal Expression ability (Vocal Encoding ability) is "the ability of the child to express his own concepts vocally"; Auditory-Vocal Association ability is "the ability to relate concepts"; Auditory Reception ability (Auditory Decoding ability) is "the ability of a child to derive meaning from verbally presented material." (Kirk, McCarthy, Kirk, 1968: 9-11.) However, it would be a significant mistake to rest easy with this characterization. The problem is indicated by the fact that children who are patently able "to express their own concepts vocally" and "to derive meaning from verbally presented material," etc., may well be diagnosed as having disabilities in verbal expression, auditory reception, etc., the terms which the authors use to label those very abilities. Thus, for example, children who are manifestly capable of understanding the spoken word (indeed they must have this ability simply to take the Auditory Reception subtest, which consists of spoken questions) may well be diagnosed as having an Auditory Reception deficit, i.e., a disability in the understanding of spoken words. The problem which this raises is that while there may be agreement on what these trait labels mean, there is not agreement on what is to govern their attribution in particular cases. We have agreement in meaning but not in judgments.

It must be appreciated that all of the psycholinguistic abilities are to be taken as admitting degrees or levels. On this understanding, the ability to understand the spoken word (Auditory Reception ability) is something someone can have to a greater or lesser extent and the test is to be regarded as assessing different degrees of ability to understand the spoken word among those who are known to have the ability in an absolute sense. It is in this sense, for example, that we understand tests of cooking or horsemanship or typing; not as a means for separating those who can do from those who can't, but for determining differences in degrees of competence among those who can do. Disability or deficits in ability, not inability, are asserted of some children on the basis of poor performance.

But while in everyday discourse we use such phrases as "the ability to express ideas in spoken words," in both absolute and degree-admitting senses, and make such judgments as, e.g., "That child is better able to express himself than this one," the metric for such differentiation is not the children's performance on the ITPA. What criteria are operant when such judgments are customarily made is not

important--they may have to do with vocabulary, complexity of sentences, fluency, etc. They are not, in general, explicit. But the important consideration is that in everyday situations such judgments are not governed by a child's performance on the ITPA, while in the context of the test, they are so governed. To say that a child has a disability in X, Y, or Z is to say that she has obtained such-and-such a score on the X, Y, or Z subtest. It is for this reason that the authors can rightfully claim that the various abilities have been operationally defined (Paraskevopoulos and Kirk, 1969: 29). The operation which we perform which determines the application of the ability or disability trait names is the administration of the appropriate test. The child's performance on the appropriate subtest governs the ascription to him of the respective psycholinguistic ability or disability as the case may be, in the same way that a liquid's performance on the litmus paper test governs the attribution of 'acid' or 'base'. The application of these terms for the psycholinguistic abilities is governed by test performance.

But in the same way that we may well ask and may well be interested in knowing what it is about the liquids that brings about the changes in the litmus paper, we may also ask what it is about children that produces differences in their test scores. That is the very issue that was taken up in the consideration of the individual subtests. If the arguments there were correct, then the factors which determine test performance and therefore govern the attribution of the various psycholinguistic abilities as traits, are significantly unlike what we are given to believe. (Most importantly, the attributions are not indicative of deficits in language processing--decoding, encoding, association--in different sensory-motor channels that the trait names suggest. And the predictions of difficulty and the programs of remediation that are predicated upon the belief that channel-specific language processing disorders have been responsible for performance differences, which is what the trait names suggest, are not supported if our arguments are correct.)

Thus, given the soundness of those arguments, to diagnose a child as having an Auditory Reception disability is to say no more than that his vocabulary is limited for a child of his age. This is in no sense a language processing deficit nor is it an auditory problem. No remediation activities such as "identifying everyday sounds in blindfold guessing games," (Kirk and Kirk, 1971: 139) are warranted

by such a finding. We have argued that to diagnose a child as having a Grammatical Closure deficit is to find that his knowledge of standard English is deficient. Nothing has been learned of a deficiency in his processing of speech at the syntactical level and remedial practices based on this conclusion are again unwarranted. In the same way, we have argued that the determination of a Vocal Encoding disability does not mean that the child has difficulty putting ideas into speech; it simply means that he didn't have or recall as many ideas of a certain sort about the four test objects as did the children in the standardization group. This again is not a language processing disorder. Yet again, there is the diagnosis of an Auditory-Vocal Association deficit. If our arguments are correct, what governs such a diagnosis is the child's limitation in some combination of his conceptual categories and his knowledge of the social and physical world. This is not a deficit in thinking in language--"manipulating verbal concepts internally"--nor is it an auditory or vocal shortcoming. Perfectly sound "auditory-vocal channels" are compatible with vast differentials in performance on this subtest. The same sorts of appraisals could be continued through the full set of subtests, but the general point should be evident. It is our argument that in no case do the attributed disabilities refer to disorders in language processing specific to some sense modality. Rather, it is cognitive factors, specifically the child's knowledge of his language and of the social and physical world, which are not susceptible to the qualifiers auditory, visual, motor, or vocal, which are the determinants of test performance differences. The modality qualifiers, in our view, do nothing more than specify the sensory mode in which these essentially cognitive tasks are set, and in which the response is given. Realizing this, one becomes aware that the trait names of the various psycholinguistic abilities, e.g., Visual-Motor Association, are as misleading as calling a written history exam a test of Auditory-Manual Historical ability.

This critique of the Trait Interpretation of the ITPA may be summarized in this way: Determining what it is about the child that is primarily responsible for his test scores is to determine what the trait names "Auditory Reception ability," "Vocal Encoding ability," etc., tell about him. That is the matter which must be fully appreciated. One knows next to nothing about a test of "Aquatic Ability" in being told that aquatic ability is "the ability to swim." For

the tests upon which this attribution is to be based could be measures of (a) how fast a child swims, (b) how long he can swim, (c) how many different strokes he knows, (d) how well he executes some stroke or set of strokes, (e) how much he knows about water safety. Clearly what is being asserted of a child in describing him as having "Aquatic Ability" will vary from test to test, despite sharing the same label. What is being measured is different in each case, and most importantly, it is different sorts of abilities that are responsible for the child earning these trait attributions. I have argued that in every ITPA subtest considered, it is not the child's psycholinguistic processes, but the information of one sort or another which the child has at its disposal that is the principal determinant of performance; that even though the tasks involve the processing of language, the scores do not tell us about the processing of language, i.e., about psycholinguistic processes. For precisely the same reasons that we would not accept differences in scores on an oral geography exam as indicative of differences in children's ability to process spoken language, or differences in scores on a written geography test as indicative of differences in the processing of written language, we ought not to accept any of the ITPA subtests as providing such information. I am maintaining that there is no difference in the two cases. In both, the information variable is central with the language processing variable being relegated to the category of factors which are acknowledgedly involved in performance but not reasonably taken as principal determinants of it--such factors as motivation, attention and recall. The only significant difference between a geography test and the ITPA subtest is that in the case of the former, the priority of the knowledge factor is evident, while in the case of the ITPA it required the sort of disclosure that has been the central concern of this paper. What all this means for the Trait Interpretation of the ITPA is that the ascribed psycholinguistic disabilities are circumlocutions for various evaluations of the child's informational states--circumlocutions for assessments of his vocabulary, his knowledge of how certain objects are used, etc. As such, the ascription of such traits tells us nothing of importance regarding the children's ability to process language.

IX. CONCLUSION

In concluding this paper I would like to turn my attention from the Illinois Test of Psycholinguistic Abilities to say a few things about the character of my undertaking. A question which has crossed my mind more than once in the course of writing this paper has been what sense or force is borne by the word 'philosophical' in my description of the paper as a philosophical critique. Certain trial sections which have been eliminated from the final draft certainly had more recognizable philosophical character than many that have seen their way through to the end. A section on theories of meaning, a critique of Osgood's equivocation on the concept of meaning, a section on the concept of 'construct validity'--these absent sections would have rested comfortably under the umbrella of philosophy of language and philosophy of psychology--as some of the remaining sections do. But some others, e.g., the critique of the Sound Blending or Auditory Closure subtests, appear basically empirical in character, based as they are on challenges to various empirical assumptions implicit in the respective test's design. But to return to the opening question, in what sense is this paper philosophical?

I am certain of one sense in which it is not. It is not philosophical in the sense of making a contribution to the discipline of philosophy. None of the traditional problems of philosophy are advanced upon in this paper; few are even aired. If the effort proves to be of any importance at all, it will be of educational importance. That much, at least, was intended from the beginning and has remained stable throughout. I did want a paper that made a contribution to education. Whether or not it proves to be so, only time will tell. But I know now that it will not prove to be a contribution to philosophy.

In any case, 'contributing to philosophy' is a remote if not contrived meaning to give to the term 'philosophical'. We quite customarily connect the term with notions of logic. If one is thinking philosophically, one is thinking logically about matters. But thinking logically does not itself pick out an activity as philosophical--after all, the postman, the doctor, and the accountant are also given to logical thinking. It would appear that, as with

many (all?) human activities, what will distinguish philosophical reflection, a philosophical critique, from other sorts of critique, is the point of the activity. That point is expressed by Scheffler, with specific reference to the educational context, in terms with which I fully agree:

(T)he application of philosophical methods to education...aims explicitly at improving our understanding of education by clarification of our conceptual apparatus--the ways in which we formulate our beliefs, arguments, assumptions, and judgments concerning such topics as learning and teaching, character and intellect, subject-matter and skill, desirable ends and appropriate means of schooling....In applying philosophical methods of analysis, we are, then, concerned directly with solving intellectual rather than practical difficulties--with removing the perplexities that arise in our attempt to say systematically and clearly what we are doing in education and why....Such an analysis, indeed, exemplifies the positive contribution the philosopher of education can make...He can try to clarify our fundamental ways of thinking about education: the concepts we employ, the inferences we make, and the choices we express....In sum, he can improve our understanding of educational contexts and the problems they generate. (Scheffler, 1966: 4-5.)

I certainly would like to think that this paper is a philosophical "contribution" of the sort characterized by Scheffler. It has been centrally concerned with understanding just what the ITPA claims to be telling us about the children to whom it is administered, with the validity of arguments and the justification of inferences. I have taken these matters very seriously for reasons which seem very obvious. But in characterizing this as a philosophical enterprise, I feel that I am saying nothing more than that the straightforward demands of common sense were taken seriously. For how else could one determine whether the test succeeded unless one was clear about what success in this case consisted in? How else could any researcher declare that the ITPA does or does not measure what it purports to measure--that it has or does not have construct validity--unless he understood clearly and precisely what was being assessed, what the construct to be validated was? Is such an understanding not a logical prerequisite of such evaluations?

As a matter of fact, the construct validity research on the ITPA has been in disarray from the very beginning. The terms 'confounding', 'confusing', 'inconclusive' abound in the literature. McCarthy (1964) acknowledges the shortcoming of his own work in this respect, remarking that "it leaves much to be desired." And a recent discussant of the ITPA had this to say about its validity:

Frustrated by their inability to administer a specific test for learning disability, psychologists and others were quick to adopt instruments--such as the ITPA--whose title promised that it could assess the ability to use language. Though there remains much question as to whether this test does indeed test "psycholinguistic ability"--validity studies are contradictory and inconclusive--its popularity belies its weak psychometric foundation. (Ross, 1976: 13.)

I suggest that this state of affairs could well have been avoided had closer attention been paid to the clarity of what was under assessment. If this is what is meant by philosophical concern, then there should have been more philosophical concern. As it is, because of the vagueness of the formulation of the test, it has most predictably come to mean different things to different researchers. If one adds to this mix the fact that construct validity itself is a vague notion, used in a variety of different ways by psychometricians, one has, I suggest, a clear recipe for nothing else but confusion and inconclusiveness.

But the philosopher's concern does not stop at clarity. As Scheffler notes the philosopher attends also to the validity of and justification for various arguments, inferences and judgments. He writes elsewhere that some philosophers have developed "as their basic task, the logical evaluation of assertions--the examination of ideas from the standpoint of clarity and the examination of arguments from the standpoint of validity." (Scheffler, 1960: 7.) From start to finish my paper has been concerned with the validity of arguments and inferences. To that same extent it has been philosophical.

If puzzlement exists over the character of a philosophical critique of an explanatory theory or a diagnostic test, it should not be difficult to dispell. Manifestly, theories and tests can be rejected on grounds other than those provided by empirical findings. False assumptions, contradictions, vagueness, equivocation, circularity, and fallacious arguments can also serve to scuttle such enterprises; and the philosopher simply as a product of the character of his study, is especially attentive to problems of this sort.

That library research such as his, yielding a philosophical critique, can have profound effects is exemplified by Chomsky's well-known critique of Skinner. The behaviorist paradigm for the explanation of verbal behavior collapsed permanently upon Chomsky's meticulous disclosure that it was merely the illusion of theory, sustained by the equivocal use of principal theoretical terms. The S-R giant

was felled, not by amassing empirical data, but by presenting a logical analysis of what knowing a language involves and by a philosophical argument demonstrating equivocation and circularity in reasoning. This was a brilliant piece of philosophical work made possible by Chomsky's sophisticated understanding of the empirical issues involved, the impact of which on psychological research was immediate and far-reaching. So much for the character and potential force of philosophical critique in an area which might appear to be subject to empirical challenge alone. My work on the ITPA and its theoretical base, Osgood's mediated response theory, is an effort similar in form to Chomsky's.

Nevertheless, in raising one argument after another from psycholinguistic research in the latter portion of this paper, I have probably left the philosopher's province as many would define it. Some certainly maintain the view that one can do the philosophy of X without being terribly informed about X, whatever it is. This notion may well have some truth to it, but it has also led philosophers to say a lot of silly things about X. Vague and irrelevant commentaries are also a product of this view. I hope that by immersing myself in the literature of psycholinguistics I have at least been able to avoid some follies of that sort. Perhaps the healthier view in this domain is that the philosophical questions stop where the conceptual questions stop, and the philosopher should stop there too.

But I know I disagree. When all is said and done, I am unsure whether I am a philosopher with psycholinguistic proclivities or a budding psycholinguist with a philosophical turn of mind. Of this I am sure, however: many of the problems of education are characteristically of a hybrid nature and there ought to be opportunity for those who wish to do so, to pursue them unconstrained by the traditional disciplinary boundaries. In the area of language research these boundaries are notoriously hard to draw--such that, while polymaths like John Lyons, Noam Chomsky, and Jerry Fodor are undeniably remarkable--that there should be such figures is not remarkable. The problems which intrigue them demand the breadth of understanding they bring to them. Education would do well to have similar figures. And one could safely say that the eminence of such thinkers rests in their ability to keep the distinctions between different types of issues clear, while considering the questions at hand comprehensively. There is no mistaking that sort of mastery for the blurring of issues which

so often occurs, especially in education. I know of no such poly-maths in education--but I would welcome the appearance of some. At worst, to paraphrase Fodor, they'd have trouble deciding what department they are in and be an embarrassment to deans. At best, they might turn the study of education in a direction which would prove both refreshing and fruitful.

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