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ABSTRACT

This review of research, developed in cooperation with the National Association for Research in Science Teaching, was designed to analyze and synthesize the research related to developmental psychology and its relationship to science education. The review is divided into five parts: (1) Introduction; (2) Historical Precedents of the Developmental Movement in Education; (3) Piaget's Theory; (4) Piaget and Education: Implications and Contradictions; and (5) Recent Traditions in Instructional and Cognitive Psychology. The reference section contains 266 entries. (PEB)

DEVELOPMENTAL IMPLICATIONS OF SCIENCE TEACHING: EARLY ADOLESCENCE

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PREFACE

This review was developed to analyze and synthesize the research related to developmental psychology and its relationship to science education. This was a cooperative effort of the National Association for Research in Science Teaching and the ERIC Clearinghouse for Science, Mathematics, and Environmental Education. It is hoped that such reviews will provide information for science education researchers, practitioners, and development personnel.

Stanley L. Helgeson
and
Patricia E. Blosser
Editors

CONTENTS

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INTRODUCTION

This review is intended for science education researchers interested in the relationship of developmental psychology to education. My knowledge of the field is based on my research efforts as well as my three years spent in Geneva working with Piaget's research team at the International Center for Genetic Epistemology. What understanding I believe myself to have of Piaget's theory comes from having tried very hard to see how his theory specifically relates to data. Most science education researchers have been content with or have chosen to accept rather loose connections between Piagetian theory and data. In addition to working directly with members of Piaget's Center, I had considerable direct interaction with Piaget in discussions.

Though a Piagetian developmentalist at heart, my head has always been rather critical. (Indeed, at Piaget's request, I wrote out some of my objections to his equilibration theory in a short critical essay.) The reader will find in this review a great many objections to Piagetian theory, particularly as it is construed and used by science education researchers who have not been so fortunate as I to have spent three formative years in Geneva. In addition, the reader will find much that is positive. I have accentuated the negative only to the extent I feel necessary in order that the positive be accepted as a needed alternative.

I wrote this review with the practitioner as well as the novice in mind. Most practitioners have only a superficial understanding of developmental research. One of my major goals has been to show that the practitioner, no less than the novice, needs to look at primary sources instead of secondary popularizations.

The dream of developmentalists has always been the optimal harmonization of educational processes with the natural growth processes of intellectual, social, and emotional development. Currently, nearly all educational developmentalists turn to Piaget or rather to his popularizers for inspiration and guidance. The novice will have to understand Piaget's theory, at least in general, to be able to evaluate the large body of education research which claims to be based on Piagetian ideas.

The general framework of development in education was delineated long before Piaget came upon the scene. Thus, Piaget and development are not synonymous. I have tried to show that the strengths and weaknesses of the Piagetian approach are often independent of the developmental approach. This may surprise many developmental practitioners who seem to feel that their case rests wholly on Piaget. The reasons for this degree of independence are that Piaget's theory is only one of many that can be elaborated within the general developmental framework, and that most education researchers, especially with regard to teaching practices, operate at the level of this general framework more so than at the specific level of Piaget's theory.

It may surprise both practitioner and novice to discover that whereas contemporary cognitive psychology regards Piaget with great respect and recognizes him as one of the first to presage the current cognitive domain, he is respected more for the questions he raised than the answers he has given. The questions reflect a philosophical framework shared by cognitive psychology.

Science education research has accepted the answers, however. I will argue that the consequences have been damaging to the research effort. I believe they have engendered an erosion of faith in the relevance of developmental psychology. As a developmentalist, I will suggest ways for science education research to profit from current developmental research more than it has in the past.

One of the great problems facing the science education researcher interested in developmental psychology is a lack of training in this field. As a result, science education researchers have relied too much on simple (and probably simplistic) introductions to Piaget, often written by other science education researchers lacking training in psychology. Thus, in spite of the great and widespread emphasis on Piaget, few have read his theoretical writings.

For example, The Growth of Logical Thinking (Inhelder and Piaget, 1958) is the seminal work on adolescent reasoning in science and mathematics. It contains descriptions of experiments, a few glimpses of data, and a great deal of theory. Much has been made of this work by educators, but virtually none give any sign of having tried to understand the theory in its relation to both the data and the implications for education that researchers are so quick to draw.

Piaget, in his general speculations on education, has encouraged educators to draw these implications from a theory which is so complex that I can only sympathize with those who have preferred to accept it as given rather than make the considerable effort required to critically evaluate it. Nevertheless I do not sympathize with the reluctance of science educators to keep abreast of current developmental research. Review articles and introductory texts abound. Colleagues in psychology can be readily consulted.

It is absolutely necessary, I believe, for science educators to become better informed if they are to successfully absorb and apply the ideas of developmental psychology. This discipline was not elaborated for the direct consumption of educators. There are no simple handbooks listing step-by-step applications. Application requires basic research over and above that of the parent discipline.

Basic research must be directed at the individual, one of my major points. Following the lead of Piaget, and adopting modern measurement techniques, researchers must aim at detailed descriptions of the learner's knowledge and performance as well as his reactions to well-defined instructional procedures. The current emphasis in science education research is on group performance levels, grossly measured under ill-defined conditions. Some of my own research provides a good case in point. If the tenets of Piagetian psychology are accepted as given (as they were and are by many), then this kind of research approach is not only to be expected, it is expected to succeed. It has not succeeded.

Cognitive psychology has provided ample reason for the rejection of much of Piaget's theory, particularly his stages of logical development. By the same token, the foundations of much Piaget-based education research has been undermined. Most of the details of results of this research are of little value I believe, and I shall not discuss them. On the other hand, contemporary cognitive psychology appears to be on firmer ground. I will therefore discuss this research in some detail.

Fortunately for education, the temper of the times is such that psychological theory is being developed for increasingly practical domains. To give but one example, language research has largely turned away from the study of memorizing lists of nonsense syllables to the study of understanding written material of the kind found in, say, science, mathematics, and history texts.

This research is still very new. Thus, the principal value of the new research lies not so much in its specific findings as in its theoretical framework and research methodology. I have called this framework "the computer connection." Briefly, it holds that any process model for solving a task, as embodied in, for example, a computer program, is thereby a candidate for a process model of a person solving that task. Within this framework, the goal is to write programs which simulate human performance, hence capture as much of real human performance as possible. Programs are based on and evaluated against human performance data. I will not be giving too much away if I point out at this time that Piagetian models are static structural models rather than dynamic process models. Herein lies the decisive advantage of the contemporary approach over the Piagetian approach. In addition to the framework, some of the specific findings of cognitive psychology also have great potential value for science education and I emphasize these where they arise.

HISTORICAL PRECEDENTS OF THE DEVELOMENTAL MOVEMENT IN EDUCATION

Although developmental psychology is a recent branch of psychology, educators have always had informal observational knowledge of children in and out of school. It has long seemed to many that (a) children were really different from adults, not just less knowledgeable, (b) the differences were fairly stable and took all the years of childhood to vanish, hence (c) educational practice should adapt to children rather than the reverse.

Of course, since education provides challenges, children must adapt, too, but the challenges would be scaled down to the child's level and cast in terms suitable for children. This meant more than just making things easier for children; it meant respecting differences between adults and children regarding attitudes, motivation, interests, affect, and attention, in addition to differences regarding knowledge.

There was (and still is) the feeling that in spite of all the variability that makes each child and each childhood unique, there is, nevertheless, a profound similarity among children. Moreover, the process of transformation from child to adult is slow in every respect, cognitive and affective as well as social and physical. Finally, this process, unique for every child, is nonetheless sufficiently similar for all children that one could speak of <u>the</u> process of development.

Educators, indeed all adults, share these feelings and accord them at least some importance. Today, educators can rely on more than informal observational knowledge of children. There is a vast and rapidly growing body of theoretical and empirical research into the nature of childhood and the events and processes by which child transforms into adult.

It may turn out that these processes are the same ones which govern changes within adulthood, in which case the differences between child and adult would be more in degree, less in kind. Or the alternative hypothesis might be sustained: childhood is qualitatively different, with qualitatively different needs rather than with the same kinds of needs as adults', only quantitatively different.

Between two extreme hypotheses, two all-or-none conjectures, there usually can be found a third. In this case, the tertium posits both qualitative and quantitative differences. In any case, we are dealing with broad hypotheses, and a great many talented men and women are trying very hard to sort these matters out.

Nevertheless, educational philosophers have long been drawn to the idea of qualitative differences. Developmental educators have opted for Dewey, Montessori, and Piaget instead of Watson, Thorndike, and Skinner. In earlier times they would have warmed to Rousseau, Pestalozzi, Herbert, and Spencer. What did these individuals have to say?

Rousseau (no date, pp. 29-30): "The only habit the child should be allowed to contract is that of having no habits."

Kant (Bucher, 1904, p. 146): "The more habits a man has the less free he is and independent...The child must be prevented from habituating himself to anything, and he must not be allowed to form any habits." Kant was much in sympathy with Rousseau's Emile. Kant advocated the education of the intellect "according to 'nature'." He favored "self-doing" and "self-education." He would have supported the aims of progressive education and discovery learning. He would have argued with Piaget over philosophical questions, but nodded in assent when Piaget (1973) wrote "to understand is to invent."

Herbert Spencer (1910, p. 52), a giant of the 19th century though mostly forgotten now, writing with approval of various changes in educational practice, asked, "What now is the common characteristic of these several changes?" "Is it not an increasing conformity to the methods of Nature,?" he immediately answers. According to Spencer, "Nature" was being served by "the superseding of rote-learnt lessons [habit?] by lessons orally and experimentally given...;" by the "disuse of rule-teaching, and the adoption of teaching by principles— that is, the leaving of generalizations until there are particulars to base them on..."

Was it Piaget who wrote that "in choosing the succession of subjects and the modes of instruction which most interest the pupil, we are fulfilling Nature's behests, and adjusting our proceedings to the laws of life?" No, it was Spencer (1910, p. 52).

Surely then it was Piaget who wrote that

> ...education must conform to the <u>natural</u> process of mental <u>evolution</u> -- that there is a certain <u>sequence</u> in which the faculties <u>spontaneously develop</u>, and a certain <u>kind</u> of knowledge which each requires during its development; and that it is for us to ascertain this sequence, and supply this knowledge (emphasis added).

It could well have been Piaget, but Spencer (1910, pp. 52-53) wrote these lines too. Moreover, he was only paraphrasing "the doctrine long ago enunciated by Pestalozzi" (Krüsi, 1875, p. 52).

Pestalozzi, who was also influenced by Rousseau, wrote

> Sound education stands before me symbolized by a tree planted near fertilizing waters...The whole tree is an uninterrupted chain of organic parts...It is not the educator who puts new powers and faculties into man, and imparts to him breadth and life. He only takes care that no untoward influence shall disturb nature's march of developments (Krüsi, 1875, pp. 159-160).

Dewey, Montessori, Piaget, and Bruner could have been quoted as much. At this level of generality, 20th century developmental ideas on education hardly differ from past ideas. This is the pre-theoretical level of science, the level of frameworks within which to build theories.

The biological metaphor unites the philosophical pronouncements of the developmental approach. Habit is opposed to intellectual growth, to change, to adaptation and evolution, to the principle of life, the "elan vital" of Henri Bergson, so influential during Piaget's formative years (Piaget, 1952). Bergson wrote of "creative evolution" while Piaget wrote "to understand is to invent." Discovery learning, unstructured learning, free-choice environments, the developmental curriculum—so much of what is done today seems based on age-old principles.

In summary, developmentalists in education believe that intellectual development, like physical and emotional development, is a gradual process which cannot be substantially accelerated. We all function within the limitations of our abilities, but these limitations are greater in the young. Educators must recognize and respect these limitations if the educational experience is to produce optimal results. These limitations will only gradually decrease. Since we cannot accelerate development, or since to do so is contrary to nature, we must work within the limits imposed by developmental level and the slow rate of developmental change. "To present an adequate notion of learning, one must first explain how the subject manages to construct and invent, not merely how he repeats and copies" (Piaget, 1970b, p. 714). Thus, to understand learning, we should first understand development. As for teaching "advanced" concepts, i.e., accelerating development, "Acceleration is certainly possible but first we must find out whether it is desirable or harmful" (Piaget, 1970a, p. 31).

If the content of learning and the method of teaching should conform to the course of development, then the question one must ask is "What is development?" Virtually all science education developmentalists would answer by referring to Piaget.

Piaget is certainly not the only influential developmental theorist, but he is more than the most dominant one: Piaget is the point of departure even for those who make an issue out of differences with his theory. For example, Novak (1977a) has proposed Ausubel's theoretical ideas as alternatives to Piaget's. Differences do apparently exist between Piaget and Ausubel. However, the similarities between the two theories are striking (compare Ausubel's "subsumption" with Piaget's "assimilation" and also with any learning theory approach to meaning to see where the major general differences lie). I am not belittling the differences which do seem to exist and which may be important. Rather, I am stating that if there were no Piaget, Ausubel (and others of great stature, such as Bruner) would be greatly diminished. I am reminded of a famous remark of Newton's in a letter to Robert Hooke: "If I have seen further (than you and Descartes) it is by standing on the shoulders of giants" (Bartlett, 1968, p. 379).

Piaget is the 20th century giant of developmental psychology. For better or for worse, we begin to answer "What is development?" by referring to Piaget.

PIAGET'S THEORY

The General Framework

Piaget is an epistemologist: he asks the two great questions of epistemology: (1) What is knowledge? and (2) How is knowledge possible? Epistemology is a branch of philosophy and is often called the philosophy of knowledge. Piaget, much taken with philosophy as a youth, rejected philosohpical analysis in favor of scientific investigation as the way to answer the traditional questions of epistemology (1968).

The above questions must seem at first to be too broad for any one individual to attempt to answer. To reconcile the immensity of the questions with the modest resources of a single lifetime, Piaget followed Kant's method of breaking the questions down into simpler ones.

Kant, reflecting on the diversity of knowledge, decided that knowledge could be grouped into a small set of categories such as number, space, time, object permanence, and causality. These categories were regarded as fundamental because (a) none could be reduced to any of the others, and (b) each was necessary. For instance, since all physical events are interactions (causality) among some things (number, object permanence) occurring somewhere (space) at some time (time), then if we understood the nature of the categories involved we would thereby understand in some general yet profound way the underlying structure and manner of composition of physical knowledge.

The philosophical concern is with rather general entities and with the establishment of logical links between those entities and the physical knowledge familiar to lay people as well as to scientists. The concern is <u>not</u> with any particular law or fact of nature such as Boyle's law or Dalton's law, although Kant was particularly impressed with, and accorded special status to, Newton's laws of motion and Euclid's geometry, two of the landmarks of human thought.

The links between categories and actual specific knowledge were tenuous and elliptical. For example, algebra involved numbers, while number entailed certain notions of classification, seriation, and permanence. Where did these latter notions come from? Are ideas to be forever reduced to more fundamental ones in the search for <u>the</u> fundamental ones?

To avoid this infinite regress, Kant posited that certain concepts, or "schemes" as he called them, were further unanalyzable because they provided the foundations of analysis. Thus, to claim they were analyzable was to reason in a circle. From this argument came the notion of "innate" ideas, but I doubt that Kant meant to imply that the fundamental schemata of thought were innate in the current sense of that term. Enter Piaget.

Piaget undertook to empirically investigate the Kantian categories, hence his research on number, space, geometry, time, object permanence, and conservation. His biological and evolutionary perspective led him to formulate the general research approach wherein something is to be known by its manner of formation, its genesis. Hence, Piaget adopted the label "genetic epistemology" or rather, he borrowed it from the American developmental psychologist and epistemologist, James Mark Baldwin (1915). Baldwin's work was well known to Piaget who cites him in his early books. (To the best of my knowledge, it was Baldwin who first proposed the famous conservation of number experiment, although I do not know whether Piaget was aware of this.) For Piaget and Baldwin, "genetic" comes from "genesis" and means coming-into-being. Piaget rejects modern concepts of innate ideas (1967). Instead, knowledge is considered to come into being as a result of the constructive activities of the subject in interaction with the object of knowledge. Interaction and construction are the key words. They are also key words of discovery learning.

The Structures of Knowledge

Genetic epistemology must seem a confusing synonym for developmental psychology. I do not want to draw fine distinctions here, but I should point out that Piaget sees his work as distinct from, yet complementary to, the rest of developmental psychology (Beth and Piaget, 1966). He feels that his goals are general and fundamental while the specifics are left to this or that particular branch of psychology. For that reason, Piaget has never been interested in individual differences and, for most of his career, has ignored cross-cultural differences. Again, for that reason, Piaget has sought to describe and explain his data in most general terms. Consider the ability to seriate (Inhelder and Piaget, 1964).

Seriation tasks require that the subject arrange a group of objects in some linear sequence. A group of sticks of different lengths might have to be arranged in a row starting with the shortest, then the next shortest, and so on, up to the longest. Other objects or "content" can be seriated along other dimensions: weight, thickness, hue, and many others come readily to mind. Some psychologists would be interested in knowing whether and why performance is affected by changes in the content. Not Piaget, at least not enough to systematically investigate the question.

Piaget's concern is with the fundamental underlying organization of performance on tasks such as the seriation tasks. He feels that all such tasks reveal an identical organization or "structure," that is, successful performance on all such tasks involves one basic structure or scheme, the seriation scheme.

Theoretically, structures are psychological organizations. One way to describe organized knowledge is to use symbolic logic. This method emphasizes the relational structure of knowledge while eschewing its content. Actually, in a sense, for Piaget there is only structure and no content. The structure of the seriation task has been described (Inhelder and Piaget, 1964) in terms of the asymmetrical transitive relation '>'. A lucid introduction to the methods and goals of symbolic logic and '>' in particular can be found in Susanne Langer's classic volume (1953).

From a logical point of view, the seriation structure is an example of an entity which Piaget calls a "grouping" ("groupement" in French). It is considered to be related to, but simpler than, a mathematical group. Groupings are the theoretical descriptions of a variety of tasks, including conservation and classification in addition to seriation (Inhelder and Piaget, 1964; Piaget, 1965a). These tasks, superficially so dissimilar, are thus revealed to depend for their solutions on similar, closely related, psychological structures, according to Piaget. Knowledge, as manifested in the ability to solve Piaget's classification, seriation, and conservation tasks (among others), is made possible by the grouping structures. Without them, the information needed to solve these tasks could not be properly organized.

Structures such as the groupings, when applied to a particular task such as the seriation of sticks, entail the psychological activity of "chaining" ("enchainement" in French). This activity consists of an organized set of mental operations. The number of operations depends on the number of objects to be "chained." However, the nature of the operations is quite limited: in the case of seriation, each operation involves use of one and the same relation, namely '>'. To borrow an information-processing idea, it is as if the individual sticks were represented as a linear series of nodes and the '>'-operator advanced you from one node to the next. Class-inclusion relations also involve '>' as each node (class) "includes" the previous one. It is only a little less obvious that conservation tasks involve chaining too.

In a conservation task such as the conservation of liquid quantity, there are two identical glasses filled to the same level with, say, water. A third glass, empty, is adjacent to the other glasses: it is both taller and narrower (or the reverse). One of the filled glasses is emptied into the third glass and then put aside as attention is focused on the two filled glasses. Question: Is there as much to drink in each glass? Since the subject judged the two original glasses to contain the same amount, and since nothing was added or taken away, it would seem that the correct answer should be the deliverance of common sense. Much to Piaget's surprise (1968), this task is not likely to be solved before a normal child reaches 7-8 years. How is chaining involved?

According to Piaget, the child can solve the conservation task when he can form a one-to-one correspondence between two sets of elements, one set composed of *seriated* water levels, the other composed of *seriated* glass widths. This double-seriation structure allows the subject to infer that an increase in the height of the water level is necessarily associated with a decrease in the glass's width. Each time the '>'-operator takes the subject to a higher level, a '<'-operator takes him to a narrower width. A long discussion can be found in *The Child's Conception of Number* (Piaget, 1965a).

I have gone into this much detail because, to the best of my knowledge, it is absent from the accounts of Piaget's theory that are found in science education journals. Without at least some such detail, it becomes impossible to distinguish a Piaget from, say, a Pestalozzi, a Spencer, or a Dewey. Quite frankly, my distinct impression is that many of my colleagues do not, in fact, clearly distinguish between Piaget and Dewey and for precisely this reason; they are insufficiently familiar with the details of Piaget's theory. Given the opaque density of Piaget's writings, I can and do sympathize with my colleagues, but there is no excuse for lack of a sound theoretical grasp.

Interim Summary

Summing to this point, and anticipating what follows:

(1) Piaget seeks to discover the fundamental bases of knowledge.

(2) Piaget follows Kant's lead in the selection of these fundamental bases, now called structures or schemes.

(3) The structures of thought are not innate, rather, they are the result of spontaneous constructions on the part of the individual.

(4) Constructive activities are motivated by interactions with the environment (social as well as physical).

(5) These psychological structures operate like mechanisms for organizing information.

(6) The structures are described in the completely general terms of symbolic logic.

(7) The use of symbolic logic allows Piaget to give a unified description of ostensibly different kinds of knowledge.

(8) The use of symbolic logic reflects an inability or perhaps a lack of desire to account for content-related factors, so important in education.

(9) The logical structures of thought are held to be composed of operations which are the internal representations of overt physical actions and their perceived consequences.

(10) The logical structures of thought become increasingly complex as the child interacts with more and more of his environment.

(11) The structures manifested during the school years fall into two broad classes, groupings and groups, with groups being the "logical" and psychological extension or completion of the groupings.

(12) The groupings characterize the "concrete" stage of development while the group characterizes the "formal" stage.

Assimilation, Accommodation, and All That

The Piagetian scenario of development is rich with jargon. This jargon has gained wide currency, even in contexts where Piagetian psychology is not being discussed. Since this jargon is omnipresent and since it often carries theoretical connotations, it would be useful to see what it consists of, how it is used, and what it does (or does not) say. Also, other theorists (e.g., Ausubel, Novak and Harresian, 1978; Mayer, 1977) have adopted some of this terminology. Previous examples discussed above can serve to illustrate its use. Some of the jargon has already been encountered, e.g., structure, scheme, operation.

Piaget places assimilation at the cornerstone of his theoretical framework "...assimilation [is] the fundamental fact of psychic development" (1963, p. 42). Assimilation refers to the functioning of mental structures or schemes. If a child solves a seriation task, one says that he has assimilated the task to the seriation scheme or that the seriation scheme has assimilated the task. If the child had failed the task, then one would say that he had assimilated the task to some other schemes which could not do the job.

Whatever the child does, he is assimilating. Even if his behavior is confused, incoherent, erratic, and hesitant, he is nevertheless assimilating. However, in these cases, assimilation is not going well. This may be because the child is aware that goals are not being attained or approached. Alternatively, the situation may have evoked several incompatible schemes leading to internal confusion and erratic behavior. Think of a computer being asked to perform incompatible actions.

What does assimilation explain? Can you challenge Piaget with data showing that no assimilation whatsoever took place in some situation? Assimilation signifies nothing more nor less than a hope. If one uses the term, then one hopes to empirically demonstrate the plausibility of a theoretical model of the internal mechanisms of intelligent behavior. Without the model, there is only a hope or a promise. Thus, assimilation can at best refer to an explanatory model. It suggests what to look for: Alone, it explains nothing. "Mental assimilation is thus the incorporation of objects into patterns of behavior, these patterns being none other than the whole gamut of actions capable of active repetition" (Piaget, 1966, p. 8). By definition, intentional behavior always involves assimilation, and accommodation.

According to Piaget, "...the pressure of circumstances always leads, not to a passive submission to them, but to a simple modification of the [assimilatory] action affecting them" (1966, p. 8). This simple modification is called accommodation. Every action displays this double aspect of the functioning of schemes: assimilation and accommodation. Koestler described the functioning of a scheme in terms of two general factors: (1) the rules of the game, and (2) the lay of the land (1964). The basic logical organization of the scheme corresponds to the rules of the game, whereas the specific way these rules are used depends on the specifics of the situation, the lay of the land.

Secondary sources sometimes give the impression that you can have assimilation without accommodation if the assimilating scheme is perfectly adapted to the task at hand. Piaget explicitly rules out this interpretation (1965b, p. 148). Both must be present. For a modern, explicit discussion of schemes, I suggest Minsky's important contribution to the clarification of how knowledge can be represented (1975). His refreshingly concrete examples are drawn from the artificial intelligence field where researchers are not free to casually speculate on schemes of this and schemes of that: eventually, they must write programs and the programs must run.

When novel situations or tasks are successfully assimilated to (or by) existing schemes, then Piaget would say these schemes are thereby enriched or elaborated and become more stable. The behaviorist construct of reinforcement enters here. If the subject's use of a scheme satisfies his needs, then use of the scheme is reinforced and becomes more likely in the future. Also, the scope of application of the scheme has been broadened. Thus, new associative links have been established between the newly assimilated situation and situations previously assimilated by the same scheme. The additional links, like the threads of a web, make the scheme more stable, less likely to be forgotten or to become unstructured.

So far I have described the best of circumstances from the subject's view: successful assimilation. From an observer's point of view, the subject's behavior might not seem successful, but this matters little. However, when assimilation is unsuccessful, again from the subject's view, Piaget speaks of disequilibrium or disequilibration. The subject may try alternative schemes or oscillate between alternatives or even abandon the task. All such self-corrective behaviors occur spontaneously in an attempt to reequilibrate, as Piaget would say. The whole process is called self-regulation or reequilibration, with the latter term suggesting a positive goal, a new equilibrium state.

A more technical discussion of equilibration can be given. Piaget is aware of various scientific meanings of equilibrium, drawn from mechanics, thermodynamics, and biology. He sets his own technical meaning apart from the others (1975, 1977). Frankly, I find it somewhat incomprehensible. The preceding paragraph suggests a looser definition which is illustrated by this anecdotal example.

An example of a spontaneous constructive activity from common experience is that of learning to swim over the winter, i.e., away from water. Many children end a summer having almost learned to swim. They spend the winter probably thinking that they will have to start learning all over again, only to find the next summer that not only have they not forgotten anything, they swim better than when last summer ended. This suggests that spontaneous structuration is apparently an unavoidable phenomenon, not necessarily under the conscious control of the individual.

In Piagetian jargon, the novice swimmer was in a state of disequilibrium. He had not yet succeeded in properly coordinating the various motor schemes required for swimming. Apparently, however, the necessary schemes were there, but lacked coordination and nothing more.

Spontaneous, constructive, self-regulatory activity was initiated and, in the absence of further practice, this activity resulted in the necessary organization. The following summer, this newly equilibrated motor organization would be exercised in various ways (side-stroke, crawl, treading water, etc.), would become increasingly elaborated, increasingly stable, increasingly equilibrated.

A more famous example of self-regulation is described by Poincare (Koestler, 1964) who completely set aside a difficult mathematical problem only to become suddenly conscious of its solution several months later. Koestler (1964) describes other similar anecdotes from the history of scientific creativity. Mundane examples abound in everyday life.

If I seem to insist on the spontaneous and autonomous constructive activity of the intellect, it is because Piaget and his colleagues do so and because it is for precisely this emphasis that Piaget is the current hero of discovery learning advocates.

Self-regulation can lead to learning (elaboration or enrichment of a given structure) or to development (new structural growth). As briefly introduced in the interim summary, the structures usually present during the school years belong to two broad classifications, the concrete and the formal operational structures. Learning occurs within structures while development occurs across structures. Development in the school years consists in getting from concrete to formal. Since this is a slow process, Piaget speaks of stages which suggest discrete periods of development.

However, throughout the concrete stage, constructive mental activity is constantly preparing the formal stage in some as yet unspecified way. Stages are discrete, but change is continuous. Thus, the stage concept is primarily an analytical device for rationally representing the progress of development in broad terms. In this sense, concrete and formal are abstract terms, idealizations, to be applied with caution to individuals.

The formal stage is of particular interest for science education at post-primary school levels. Researchers have suggested that the majority of significant concepts in secondary school science requires formal operations for their understanding (e.g., Cantu and Herron, 1978; Shayer, 1970). They then point out that most students do not develop formal operations prior to college age and perhaps not even then. Thus, they go on to conclude, (a) educators must help students overcome the limitations of concrete operational structures, (b) this can be done by either promoting development of formal structures or (c) translating science concepts into terms assimilable by concrete operations.

In what follows is a discussion of (1) an initial state, the concrete stage; (2) a final state, the formal stage; and (3) general Piagetian suggestions on how to go from concrete to formal. Along the way the theoretical and empirical bases for believing that the preceding (1-3) has educational relevance are evaluated.

The Concrete Stage

From the age of about five years, children are increasingly able to handle tasks requiring the "concrete" logic of groupings. The term 'concrete' signifies a relative limitation of the application of groupings to the here and now, to what is real and present, to the familiar. Dewey used 'concrete' in the latter sense of 'being familiar,' but then even higher mathematics could be concrete for some rare individuals. The key restriction for Piaget is that subject matter requires the concrete schemes of organization.

The number of tasks assimilable by concrete operations is almost endless as is the number of books about such tasks authored or edited by Piaget. Piaget's books make for fascinating and stimulating reading regardless of one's theoretical bias. In spite of the great number of concrete tasks, a unified theoretical account of them is presumed possible via a small number of concrete operations. The power of the theory is potentially immense in this sense. (The qualifiers "presumed" and "potentially" are justified and should be tacitly assumed even when I omit them.)

Continuing with the nature of the concrete stage, application of the concrete operations is typically dependent on presumably incidental task variables. Although a given task may embody, say, the conservation structure, the subject may or may not solve the task even though he has the structure, i.e., even though he has solved other tasks requiring the structure. Inconsistency in applying a given structure is often referred to as an example of "horizontal decalage" (Piaget, 1970b; Piaget and Inhelder, 1962). (The modifier "horizontal" will be dropped henceforth since I shall not discuss "vertical" decalage.) The most extensively studied examples of decalage concern the conservation scheme. In fact, conservation is the most extensively studied scheme for any reason.

The crucial interest of decalage for psychology is that it raises the question of the relative importance of Piaget's structures as descriptive explanatory concepts. Indeed, decalage casts doubt upon the validity of Piaget's notion of structure. For education, the questions raised by decalage are similar.

Educators aim for transfer of learning, that is, application of skills and concepts across tasks. Transfer is from what is specifically taught and encountered to what is novel. Piaget's theory suggests an answer to the question of what is transferred: the underlying schemes. But if these schemes are insufficient to guarantee transfer, then what else must be done? If transfer of performance turns out to be primarily a function of spurious variables, that is, variables extrinsic to the schemes, then perhaps educators should focus on these other variables. Thus, the very possibility of using Piaget's theory turns on the resolution of the decalage problem.

Decalage in conservation is illustrated by the amount-weight-volume sequence. First, the subject understands, in some particular context, that changes of shape leave invariant the amount of some substance (water, clay, etc.). Then, a year or two later, the subject realizes that weight is also an invariant under transformations of shape. Finally, a year or two later (at the least), volume is recognized as an invariant (for relatively incompressible media such as water and modeling clay). Only part of this sequence has been observed outside of Geneva (Elkind, 1961): it appears that volume conservation is quite rare. Piaget now assigns it to the beginning of the next stage of development, although he often discusses it is the context of concrete operations.

Why the decalage? When children say that there is the same amount as before, they usually say that nothing was added or taken away. The very same statement is used one or two years later to justify weight conservation. Why the delay? Piaget has answered by simply pointing out that some concepts such as weight have extraneous and misleading associations which confuse and distract the child. Weight is more confusing in this sense than is amount. Thus, the child more easily assimilates amount to the conservation scheme.

During a symposium in Geneva, Piaget remarked, in essence, that the scheme is the "signal," but there is "noise" too. The child must learn to ignore the noise before perceiving the signal. Granted. How is this to be done? Unless an answer can be found, and a practical one at that, Piaget's psychology will be of little value to educators. Answers have been given, good ones I believe, but they have emerged from the recent information-processing and neo-Piagetian traditions of North America. These are discussed in a later section. The Piagetian answers, less satisfying, are discussed after the section on formal operations.

In addition to decalage, concrete operational subjects display another limitation, one which educators have recognized for hundreds of years. The child finds it rather difficult to sustain reasoning in the absence of the object of reasoning. Primary school pupils in particular lose track of verbal arguments of even modest length. They do much better with short statements and when words refer to objects or pictures present before them, and better still if they can manipulate these objects or otherwise gain familiarity with them. At the extreme, objects are more than helpful, they are necessary. Hence the term "concrete." Piaget (Piaget and Inhelder, 1967) wrote,

> The operations in play in this type of problem can be said to be 'concrete' in the sense that they bear directly on objects and not yet on verbally stated hypotheses as will be the case with the propositional [i.e., formal] operations...(p. 79, my translation).

This turns out to be an overstatement. Objects are not necessarily providing that verbal statements are simple, succinct, and refer to sufficiently familiar objects, situations, and concepts (Inhelder, Sinclair and Bovet, 1974, p. 2). Again an important question for educators: When must objects be used and when can one do without them? Piaget's theory cannot answer this question. It is an empirical matter at this time.

Although most junior and senior high school students are considered to be at the concrete stage of development, science educators feel compelled to focus on the next developmental stage. Many (e.g., Cantu and Herron, 1978; Shayer, 1970) believe that science courses in junior high school and beyond involve many concepts which require the kind of reasoning Piaget calls formal. Even if Piaget did not exist, interest would remain in the formal stage tasks investigated by Inhelder and Piaget and discussed in The Growth of Logical Thinking (GLT) [Inhelder and Piaget, 1958]. This has surely been the most influential work for science educators interested in development beyond the primary school. The formal stage is considered to be the apex of development and an indispensible goal for science education (Piaget and Inhelder, 1967).

The Formal Stage

The formal stage is so named because when the subject is operating at this level, he is responsive to the form, the underlying structure of the task: form and content become differentiated. According to Piaget, content in the sense of physically present reality is no longer usually necessary to the subject's reasoning. Neither is familiarity, indeed the subject can reason correctly about purely hypothetical statements. Concrete props might often be very useful, but they are no longer always necessary.

In GLT, Inhelder and Piaget report subjects' performance on 15 tasks, most of which deal with ratio and proportions, controlled experimentation, and combinatorials. Ratio and proportions tasks require that the subject generate numerical data and realize that these exhibit linear or inverse proportional relations. Controlled experimentation tasks require that the subject determine the effect of each of several independent variables on a dependent variable by systematically varying them one at a time. Combinatorial tasks require that the subject investigate the effects of variables considered not just singly, but in combinations of two, three or four at a time. The subject is never told how to proceed except in the most general way, e.g., the subject may be told to find out how the balance beam works, but he is not told to look for linear or inverse numerical relationships. Nor is he told to examine variables one at a time or in combination. The subject alone must decide when one or another procedure is to be used.

In the balance beam task for instance, the subject is asked how to restore a balance or where to put a given weight to balance a different weight. Subjects are free to experiment as they see fit. The experimenter asks for reasons and predictions (Why will you do that? What do you expect? Why?).

Inhelder and Piaget claim that successful performance on this and the other formal tasks requires the use of mental operations which form a mathematical group, not a grouping as with concrete tasks. The best that concrete stage subjects can do, of course, is to assimilate the information to the groupings of concrete operational structures. For instance, on the balance beam task, concrete subjects construct a double seriation of weights and distances. In qualitative terms, this "double serial ordering" allows formulation of the "hypothesis that the same object 'will weigh more' at a greater distance from the axis..." (Inhelder and Piaget, 1958, p. 172). The subject can also structure inverse as well as direct correspondences, i.e., the subject can generate and work with the hypothesis that the greater the weight, the closer to the axis it needs to be put in order to achieve a balance (see eq. (5), Inhelder and Piaget, 1958, p. 172). At the level of concrete operations, this is the best the subject can do. To do better, formal operations are presumably required. Formal operations are supposed to allow the subject to pass from "simple qualitative correspondences" to "metrical proportions." These metrical proportions entail the setting up of an equation of proportions and its solution via a cross-multiplication algorithm.

Piaget tries to show that (a) the information obtained from experimenting with a balance beam can be cast into the form of simple propositions or statements, (b) these simple propositions are logically combined, (c) when taken as a whole, the sets of logically combined propositions can be transformed into one another by the actions of the group of formal operators (the elements of the INRC group discussed below).

Once again, operations have the effect of mentally moving the subject from node to node, each node now being some logical combination of propositions. By this means, it is possible to generate new combinations of propositions, new statements about the balance beam, without having to make or see or believe in the physical embodiment of these propositions. Form is dissociated from content. What does this imply?

Consider a subject who can solve the balance beam task. That is no mean feat. What does this lead Piaget to expect? Simply this, that if the subject can solve one formal task then "the subject has become capable of solving all similar problems" (Inhelder and Piaget, 1958, p. 267, emphasis added). It is difficult to resist interpreting this to mean that the formal stage subject can transfer concepts and skills from task to similar task. Moreover, "similar" is apparently taken to mean structurally similar, particularly since form is now dissociated from content, and structure is defined in terms of the INRC group. If, in fact, Piaget never intended these interpretations, we are faced with the theoretical possibility of decalage.

Piaget claims that tasks of the formal stage are solved approximately synchronously (Piaget and Inhelder, 1958; Piaget and Inhelder, 1967; Piaget, 1953). According to Piaget's unseen data, this approximate synchrony "seems to indicate that there exists a liaison among them" (i.e., among the formal tasks)(Piaget and Inhelder, 1967, p. 111). The liaison is claimed to be effected by the INRC transformations as can be seen, according to Piaget, once each task is properly analyzed (Piaget, 1953). Moreover, the variety of schemes embodying the group of transformations, e.g., notions of proportions, double systems of reference, hydrostatic equilibrium, in short all the formal notions, "shows the generality of its [i.e., the group's] use" (Piaget and Inhelder, 1967, p. 112).

Piaget claims moreover that each of the formal tasks begins to be solved by a noticeable minority of subjects at about 11-12 years and that by 14-15 years, subjects have reached the formal stage (Inhelder and Piaget, 1958). Since, as a rule, Genevans use a different sample of subjects for each task, recurrent age norms can only lead to the reasonable expectation that any randomly selected normal subject of 14-15 years or older has at least a 50 percent probability of performing at about a formal level on any formal task. If one uses the same sample for all the tasks, then for this sample, performance correlations should be quite high.

Everything is theoretically grounded in the INRC group. One would therefore expect that the INRC group has received close scrutiny. It has not, at least not by science educators who have tended to accept it and then ignore it. It seems necessary to describe in detail the use of the INRC group in order to compensate for a serious dearth of discussion in the science education literature.

The INRC Group

The hydraulic press task (Inhelder and Piaget, 1958, Chap. 10) can be used to illustrate the function of the INRC group. In this task, a kind of U-tube is partially filled with water. As weights are added to a piston on one side, the water level descends on that side while it rises on the other side. The difference in water levels is equivalent to a quantity of water whose weight is the same as the sum of the piston's weight and the added weights (assuming equal cross-sectional areas of the columns: This simplifying assumption does not restrict the generality of the following discussion). Observations of the relations among variables are expressed as propositions (Inhelder and Piaget, 1958, pp. 160-161).

- p = a given weight, W1, exerts a pressure
- q = another weight, W2, exerts a pressure
- $\bar{p}$ = the removal of W1 eliminates the pressure it exerted.
- $\bar{q}$ = $\bar{q}$ means the same as $\bar{p}$, but refers to W2.
- p' = the weight of the water level difference caused by W1 is equal to W1.
- $\bar{p}'$ = the resistance or pressure due to the water level difference caused by W1 can be diminished by reducing the difference (i.e., "eliminating part of the liquid or by substituting a liquid of less density (p. 161).

q', $\bar{q}'$ mean the same as p', $\bar{p}'$, but refer to W2. For example, the meaning of $\bar{q}'$ differs from that of $\bar{p}'$ in that $\bar{q}'$ refers to a water level difference caused by W2 or, if two weights are simultaneously added, $\bar{q}'$ refers to that portion of the water level difference due to W2. (N.B. One of the weights is always the piston weight.)

The reader may have noticed that $\bar{p}$ and $\bar{p}'$ are not the logically derived negations of p and p'. In particular, $\bar{p}'$ includes substantial physical knowledge unhinted at in p'. Also, in case the reader is trying to imagine what physical events occur during the experiment and what their physical explanations are, the proposition $\bar{p}'$ corresponds to comparisons made when the experiment is performed with alcohol instead of water. In addition, $\bar{p}'$ means that a given difference in levels corresponds to an excess pressure (from the higher column) which varies with the density of the liquid, being less for alcohol than for water because alcohol is less dense. Thus, at equilibrium, a greater level difference is required to compensate for the lesser density. In fact, this is predicted by the subjects classified as formal (p. 158).

There is, however, no particular connection between $\bar{p}'$ and W1 except that in order to observe the effects of using alcohol, some given weight must be acting in one of the columns of the U-tube. As for "eliminating part of the liquid" in the higher column, this is a mental action. It is as if the subject imagines that were some of the liquid removed while the weight was held in place, then the resistance of the higher column to the weight would be diminished or eliminated entirely. This mental action corresponds to changing one variable of the system while holding the others constant. In the actual apparatus, removal of liquid would simultaneously lead to a readjustment of both liquid levels: the weight would descend and the difference in levels would be reestablished. We shall return to the physical conceptions Piaget attributes to formal stage subjects after illustrating Piaget's use of the system of formal operations.

Piaget's method consists of selecting aspects of successful subjects' knowledge, not from any one subject, but the sample of all successful subjects even though not every such subject manifests these aspects. Piaget then represents these aspects by several pairs of propositions which he calls "binary operations," a confusing terminology. Piaget goes on to "demonstrate" that the several binary operations can be transformed into one another by action of the formal operators called I, N, R, and C. Thus, given only some of the binary operations, that is, given only some of the aspects of formal stage knowledge, the formal operators can generate the remaining aspects or "complete" the system of knowledge. The system is presumably complete in the double sense of (1) being sufficient to describe subjects' understanding of the hydraulic press task, and (2) being closed under transformations of the formal operators, i.e., no matter how many times and ways these operators are used, the same small set of binary operations appear. Before showing Piaget's work, a word about symbolism: the logical connectives "and" and "or" are symbolized by "." and "V" respectively. Also, the negation of a proposition p is written as $\bar{p}$. Now to the use of the INRC group of operators.

Piaget considers the case of two weights, hence pairs of propositions, p and q, $\bar{p}$ and $\bar{q}$, and p' and q', and $\bar{p}'$ and $\bar{q}'$. The subject is supposed to first reason that either weight (or both) causes a pressure in one side of the apparatus. In terms of Piaget's logical symbolism, this becomes (p or q), i.e., (pVq). "The inverse operation consists of stating the cancellation of this action; let this be($\bar{p}.\bar{q}$)" (p. 161). Similarly, the fact of the resisting pressure in the higher column reacting against the two added weights is logically represented by ($p'Vq'$) while its "cancellation" is given by ($\bar{p}'.\bar{q}'$). As previously noted, the "cancellation" expressed by $\bar{p}'$ differs from that expressed by $\bar{p}$.

In the preceding paragraph, four pairs of propositions were described. These pairs, called binary operations, represent four aspects or components of the formal stage subjects' knowledge. The term "binary operation" is confusing since these propositions pairs do not represent an operation in the sense of "that which operates." Instead, the binary operations are operated upon and transformed by the formal operations, as described below. The four binary operations described above were:

(1) pVq

(2) $\bar{p}.\bar{q}$

(3) $p'Vq'$

(4) $\bar{p}'.\bar{q}'$

These four knowledge components are presumably culled from interviews with successful subjects. (Other experiments involve other binary operations, including more complex combinations, e.g., ($p.q$)V($\bar{p}.\bar{q}$).) Some subjects may explicitly reveal only some but not all of these four binary operations during the hydraulic press experiment. Thus, this set of four is a mosaic formed from the responses of successful subjects rather than the average response. "Averages have nothing to do with it," Piaget once said. More about this later.

"The discovery unique to" the formal stage is that "the intervention of a resistance" in the higher column is "equivalent to the elimination of a pressure" caused by weights in the first column (p. 161). There is no "elimination" of course; Piaget simply means that at equilibrium, the forces due to the added weights in one column and the resultant extra liquid in the other, higher column are equal and opposite. They do not go away, they just balance. In this sense, they cancel one another so that there is no net unbalanced force. To truly eliminate a pressure, i.e., reduce it to zero, one would have to remove the weights (as expressed by $\bar{p}$). (Perhaps Piaget had pressure differences in mind rather than pressures. This possibility does not affect the rest of the discussion.)

The "unique discovery" of formal stage subjects, just described, can be simply put: to each increase in pressure due to added weight, there is an increase in the liquid level difference, hence an increase in resistance to the weights (action-reaction). In symbolic notation, to each p there is a balancing p'. Since p' refers to a sort of opposition to p and since $\bar{p}$ also refers to a sort of opposition to p, Piaget feels that p' and $\bar{p}$ are logically equivalent (as are $\bar{p}'$ and p, q' $\bar{q}'$ and q): he believes one symbol can be used in place of the other in logical expressions. Thus, for example, ($p'Vq'$) can be rewritten as

$(\bar{p}V\bar{q})$. This allows Piaget to rewrite the third and fourth of the four binary operations basic to an understanding of this task.

(1) pVq

(2) $\bar{p}.\bar{q}$

(3) $\bar{p}V\bar{q}$ instead of p'Vq'

(4) p . q instead of p'Vq'

At this point, Piaget shows that these four binary operations can be transformed from one to another by the formal operators. If each of the four binary operations is represented by a point on a plane, then the formal operators will move you from point to point. But you will be moved to no points other than these four: in this sense, the system of binary operations is closed. The mathematically informed reader will recognize that I have just described the action of a mathematical group of operators. This reader will also have recognized that much of Piaget's procedure makes no logical sense whatsoever. Please bear with me.

To illustrate the actions of the formal operators labeled I (identity), N (negation), R (reciprocal), and C (correlative or, as most logicians call it, dual), note that

(a) N operating on (pVq) yields $(\bar{p} . \bar{q})$.

(b) R operating on (pVq) yields $(\bar{p}V\bar{q})$.

(c) C operating on (pVq) yields (p . q).

The identity operator I preserves the identity of whatever it operates on. If N, R, or C were applied to a different binary operation, the result would be one of the four binary operations already listed. For instance, C operating on $\bar{p}V\bar{q}$ yields $\bar{p} . \bar{q}$, a result already obtained from the action of N on (pVq) and given in (a) above. Different binary operations can arise in other contexts.

The principal difference between the formal and concrete subject is that the formal subject accounts for equilibrium by the reaction or opposition of the increased liquid level to the action of the weights. This is no mean feat. The subject must mentally separate the excess height from the rest of the column, attribute to it a weight acting down and "back" towards the other column, and an exact equality of this weight with the added weights (W1 and/or W2) acting in the opposite fashion. The equilibrium is conceived of as the conjunction of equal and opposing forces, the magnitude of one varying with the magnitude of the other. As one subject put it, "the water comes into equilibrium if it communicates by a tube and the pressure is transmitted in full" (p. 160). In the words of another subject, the pressures due to the weights and the water level difference are equal because "the apparatus [piston] doesn't fall and doesn't rise, and, reciprocally, because the water neither rises nor falls" (p. 159). Piaget notes that "the influence of acquired [e.g., school] knowledge may occasionally be perceived" (p. 160).

In like manner, Piaget analyzed successful performance on the various formal tasks and always came up with the INRC group at the heart of subjects' understanding of apparently rather different concepts and physical phenomena.

I wish I could convey to the reader the excitement I felt when I first encountered Piaget's logical model of formal operational thought. Not only did he appear to provide an unexpected data base, synchronous development of the ability to succeed on the ostensibly different formal tasks, he also appeared to provide a coherent theoretical model which explained the surprising unity of the data base. I quickly delved more deeply into the contents of The Growth of Logical Thinking. If I am presently discussing the INRC group in such surprising detail, it is because so few of my colleagues have been willing to come to grips with the relation of data to theory in this book, as if this were irrelevant to the monumental claims about formal reasoning which followed and which provided the basis of most of the subsequent research in the science education developmental literature.

Problems with Piaget's Use of the INRC Group

Piaget's use of the INRC group has been severely criticized by several logically astute authors (Wason and Johnson-Laird, 1972; Byrum, Thomas and Witz, 1972; Parsons, 1960; Lunzer, 1973), each approaching the task from a somewhat different perspective, each focusing on somewhat different aspects of the situation, each coming to the same conclusion. In Ennis' words, "His [Piaget's] logical principles look unsatisfactory; his generalizations seem defective; and his basic categories and theorizing in this area appear to be devoid of empirical interpretation" (1978, p. 202). Rather than review these critiques, I will present a different one. Consider again the problem of equilibrium in the hydraulic press.

First, the propositions which Piaget combines into binary operations are not all simple; p' and $\bar{p}'$ already contain the sophisticated information of the kind reserved for the formal stage. Thus, without yet appealing to the INRC model, Piaget has injected what he hopes to extract with the aid of the INRC group, namely the principal insights of successful subjects. Piaget thus argues in a circle.

Second, the propositions $\bar{p}$ and $\bar{p}'$ are not the logical negations of p and p' although the symbols say that. In particular, $\bar{p}'$ contains information unrelated to p', such as the role of density. These observations should bring a halt to any further manipulations of the symbols until their meanings become logically consistent with one another.

Third, independent of the preceding, Piaget focuses on two weights. Why two? It is clear that conceptually all that matters is the total weight (of piston plus optional added weights). To focus on two weights as if that were somehow important, worse still, to focus on two weights as if the subjects did, is both theoretically arbitrary and empirically ruled out. In fact, Piaget's motive is obvious. The INRC operators act on binary operations, hence pairs of propositions are necessary. Thus, Piaget has assimilated Inhelder's data to his logical schemes and has thereby completely distorted the situation. Piagetians call this "play" (Piaget, 1962).

Fourth, Piaget makes p' and $\bar{p}$ logically equivalent because they represent situations that are (loosely) physically equivalent. For example $\bar{p}$ "refers" to a situation in which an opposing pressure is increased by increasing the water level difference. To see the physical equivalence, imagine two phenomena. In the first, some amount of weight W is removed and the system adjusts to a new equilibrium state. In the second, some water of weight W is added to the higher column and the system adjusts to a new equilibrium state. As measured by the water level differences, these two equilibrium states are identical. Hence, a sort of physical equivalence: the dependent variable (water level difference) takes the same value in both cases.

Probably a competent logician could find two ways, one for each of the above situations, to derive the same conclusion, i.e., the same final equilibrium state. It is equally sure that the two logical derivations would be rather different, involving different propositions. Piaget's approach was simply to take p' equal to $\bar{p}$. Piaget's "method" invokes the circularity mentioned in an earlier critical comment. Also, the propositions "refer" to situations, but more than loose correspondence is required if we are to treat these as logical propositions. The equating of unprimed and primed propositions is a completely invalid step. Again, the reason for it is obvious.

The INRC operators are defined on a closed set of binary operations. Either all the propositions must be unprimed or all must be primed. The INRC operators cannot magically transform the former into the latter. For this, other "methods" are employed.

These four criticisms apply in more or less equal force to all of Piaget's logical legerdemain. When Piaget introduces numerical computations, new problems arise, as in the "explanation" of successful performance on the balance beam task, discussed below.

In general, all the knowledge which separates the formal subject from the concrete subject, that is, which distinguishes successful performance from unsuccessful performance on the formal tasks, all this knowledge has been injected by Piaget into (a) the initial descriptions of the (unprimed and primed) propositions, and (b) the positing of an equivalence between them ($\bar{p}$ is substituted for p' wherever it appears).

In both (a) and (b), the formal subject is posited to have just those qualities of knowledge which can only be had thanks to the INRC group or formal operations. But to this point, the INRC group has not yet appeared. Nor need it ever appear. It adds absolutely nothing new; at best it only describes what already exists. That it can fill this descriptive role is likewise a hollow achievement because Piaget artificially assimilates what he claims to be subjects' knowledge to precisely those combinations of propositions which the INRC group can describe. Circularity again: the INRC group describes relations among the binary operations because they were arbitrarily chosen to fill this need. Piaget avoids the only interesting questions the data suggest: How does the subject come to possess the knowledge Piaget posits prior to introducing the INRC group? and, Why are younger subjects less able to handle these tasks?

Somehow, the thought is irresistable that there is a connection between the criticism and the indifference which the formal operations theory has generated, in Geneva as well as elsewhere, and its omission from a recent summary of Piaget's work written under his supervision (Cellerien, 1973). Outside of Geneva, it is not the theory, but interpretations of its significance which have stimulated so much research in science education. But what use are interpretations of an unacceptable theory?

The problems with Piaget's approach go beyond the specifics of the INRC theory. If the INRC group is not useful then perhaps another logical description will do the job. In fact, no logical description is likely to do the job. Worse still, there may be no job to do, that is, there may be no empirically unified body of data awaiting theoretical unification. The whole of Piaget's theory of logical development is in question. However, his general framework, his pre-theoretical stance is on firm ground to judge by the number of cognitive theorists who reject Piaget's theory while working within his framework. Before discussing the relatively minor and largely exploratory Genevan research on training, which is fairly atheoretical, Genevan claims notwithstanding, Piaget's theory will be criticized and evaluated from an educator's viewpoint.

Criticisms of Piaget's General Approach

Both theory and data are vulnerable to criticism for a variety of reasons. Problems arise from Piaget's use of symbolic logic, his classification of tasks, his research methodology and his data base.

Piaget uses symbolic logic in order to find a general way to describe and classify tasks. He focuses on the logical relations he believes to be essential to each task while ignoring the specific content of tasks. Since Piaget's theoretical description of performance omits mention or even consideration of content, his theory cannot, by design, account for the role of content. The effects of content are details left for others to work out. Only a moment's reflection should be needed to see that the omission of these details leaves a great and vital gap from the educator's point of view. Educators are concerned with content at least as much as with structure. Transfer of learning, a universal goal, means transfer of structure to new content. Thus a word of caution: even if Piaget's theory were perfect and complete, there would remain the problem of application to the diversity of content, thus the need for research, a need anticipated by Piaget (Beth and Piaget, 1966).

The problem of decalage, off-handedly dismissed by Piaget in his discussion of the conservation decalage (Piaget and Inhelder, 1962), has proved decisive in the sollapse of faith in the usefulness of his theory, if not the theory itself (Odom, 1978). Piaget has argued that decalage results from confusion between signal and noise: subjects are distracted by aspects of content (noise) and fail to see the applicability of their existing adequate concepts (signal). Without further ado, Piaget turns away from the problem of performance inconsistency and continues to classify under the same heading tasks of widely different levels of difficulty. For Piaget, decalage is the noise obscuring the signal which is his logical classification scheme.

Piaget's classification scheme is useful only to the extent that it brings order to our observations. To use a metaphor from statistics, it is potentially a way of reducing variance, of partitioning tasks, performances, and pupils into fairly well-defined and internally homogeneous groups in order to reduce complexity. But it turns out that within-group variance is still too large: the predictive power of Piaget's theory is too weak. This may be less a criticism of the theory than of the attempt to use it to achieve ends for which it was not designed. Be that as it may, the problem remains.

Decalage is the signal, not the noise. The criterion of logical operations fails to capture the process variables which determine actual performance and thus performance variability, the prime concern for educators. The reasons for such negativism are empirical. The psychological research literature of the past ten years has amply demonstrated the insufficiency if not the non-existence of concrete and formal logical operations (e.g., Falmagne, 1975; Siegler, 1978a; Siegel and Brainerd, 1978; Revlin and Mayer, 1978; Brainerd, 1977b, 1978c). Other models have been hypothesized as research continues in the domain originally charted by Piaget and the most promising of these will be discussed in a later section of this paper. In a word, the problem with using logical operations to classify tasks and pupils is that empirically, decalage is the rule, not the exception.

It is tempting to list some of the kinds of tasks handled at the concrete stage. Introductions to Piaget written by and for workers in science education always do this and thereby convey the impression, gained from Piaget, that it is important to classify tasks a la Piaget. There seems not the slightest doubt that it is not important to do this.

Educators cannot even rely on general experience to insure that students reach the formal stage, if only on unpredictable occasion. Many students seem to remain consistently at the concrete stage. That is, many students consistently fail to perform reasonably well on school tasks classified as formal. In spite of Genevan age norms, English and North American adolescents do not spontaneously develop the ability to handle formal stage tasks by 14-15 years. This has caused some to speculate on cultural differences between Geneva and the rest of Western society. Others have placed the blame on educators, as if American educators had failed where their Genevan counterparts had succeeded (McKinnon and Renner, 1971). While educators could certainly be doing a better job, in Geneva as well as elsewhere, the problem is not the stunting of natural intellectual development. This would assume that Genevan age norms were "natural." On the contrary.

The Genevan age norms do not exist. I cannot prove this startling claim, but I have good reason to believe it is true, based on my years in Geneva and my conversations with Piaget.

For one, the word "norm" suggests a statistically valid concept, but Genevan sampling procedures are anything but statistically valid. Genevan experimental samples usually include several very able adolescents and pre-adolescents. Genevan children were not required to continue in school beyond 12 years until fairly recently. At the time Piaget was receiving formal stage data, the few students who did go on to high school were a very select group. They still are, though less so.

Second, the behaviors classified as beginning formal and fully formal were not observed, even for this select group, to emerge on the average at 11-12 years and 14-15 years respectively. Of course the more adequate responses are to be expected from older subjects. But the so-called age norms were just rough guesses.

Third, the responses called formal were not even typical of these age levels. Rather, Piaget made a judicious selection of responses he considered more or less correct and he fitted them (or parts of them) together like the tiles of a mosaic to form prototypic responses. No one subject has exhibited all the components of formal operations, nor has any group of subjects taken collectively (Bynum, Thomas and Weitz, 1972).

When I asked Piaget whether the formal stage responses were in any sense the average response of an age group, he replied, "Averages have nothing to do with it. Absolutely not! I defined the stages that way. It's sort of a tautology, an hypothesis." Piaget was perfectly entitled to do this, but I do not think he made this clear in his writings. He still feels that the age norm of 14-15 years will hold up for academically exceptional students, interested in science and mathematics (personal communication). Shayer's (Shayer, Küchemann and Wylam, 1976) surveys support this conjecture. But this raises another problem.

Formal stage performance, i.e., success on the formal tasks, may reflect acquired knowledge to a great extent. Consider the use of "metrical proportions" for solving various proportions tasks. Piaget claims that this is what subjects do: they spontaneously set up and solve an equation of proportions. Moreover, they use the cross-multiplication algorithm. On empirical grounds, this characterization of formal stage performance is tantamount to a definition of the null class. Personally, I know of no one who has ever spontaneously invented an equation of proportions and then, again spontaneously, has invented the cross-multiplication algorithm to solve it. There may be exceptions, some rare individuals, but one cannot have a science of the exception. One might as well define a stage of development corresponding to the invention of Einstein's equations and another one for Euclid's Elements.

The gross disparity between theory and data cannot be reconciled by appeal to the performance/competence distinction. The original competence notion referred to a subset of subjects' actual performances. Piaget's INRC theory refers either to a miniscule subset heavily tainted by "acquired knowledge" or to a null set, i.e., unobserved data. The current competence notion (Miller, 1975) refers to subjects' actual performance only in an indirect way: a competence theory must only be compatible with actual performance. Piaget's INRC model fails on the second account as well. Consider again the balance beam task.

Referring to the INRC derivation of cross-multiplication, Piaget says (Inhelder and Piaget, 1958, p. 180), "[the] formulae may seem much too abstract to account for the actual reasoning of our subjects...we are justified in considering the preceding formulae symbolic expressions of the actual reasoning of our subjects" (emphasis added). But subjects' actual reasoning is quite different from Piaget's descriptions. He can not have it both ways, i.e., either he models competence or performance, but he cannot do both at the same time. Indeed, he has it neither way.

> The INRC model is at best remotely related to what it purports to explain. Piaget's use of INRC is less an attempt to describe behavior than an attempt to describe tasks. Moreover this attempt ranges from implausible to incoherent. It would not be far wrong to assert that INRC has explained nothing and can explain nothing (Wollman, 1979).

But some students do succeed on formal stage tasks. Equations of proportions and the cross-multiplication algorithm, for example, are found in some texts as early as the 6th grade, in all texts by the 8th grade. So someone did invent this mathematical technique, some rare individual from the unwritten history of mathematics. Many students, albeit a minority, do learn about proportions and other aspects of the formal tasks. And that is the point. School learning is confounded with spontaneous development.

Success at the highest levels might surely be due to school learning. The exceptionally able student of 14-15 years will have taken some of these courses: biology, chemistry, physics perhaps, algebra, and geometry. By definition, he or she will have done well. Since he or she is likely to have been particularly interested in science and mathematics, according to Piaget's current guess, extracurricular learning was likely, even prior to the taking of these courses. To claim, as Piaget and Piagetians do, that these students are academically able to perform well in science and mathematics because they are formal merely begs the question. At the present, this kind of circular reasoning has typified definitions of who is formal (Brainerd and Siegler, 1978).

It would appear that Piaget's constructs have failed to reduce or explain the variance in our observations. The differences between those who do and those who do not succeed on a given task have not been adequately explicated by the terms concrete and formal. Another mystery which remains is performance inconsistency, particularly of subjects classified as formal. Finally, there seems to be no accepted way to decide who is formal. Subjects failing one task or many tasks may succeed with the next. Also, tasks which on the face of them clearly embody some formal stage concept are sometimes solvable by very young subjects who are unable to do some concrete tasks. Piagetians then object to calling these tasks formal. By refusing to allow these tasks to be classified as formal, one engages in the circular reasoning mentioned above. Not surprisingly, performance correlations on formal tasks are low to modest in general. (The rare cases of high correlations are discussed in the next section.)

In a word, Piaget's logical classifications are so broad as to include instances whose differences are more interesting than their similarities. Regarding science education, Piaget claims that "the spontaneous formation of an experimental spirit [i.e., a spirit of systematic empirical inquiry], impossible to constitute at the level of concrete operations, is rendered accessible to subjects by the...propositional structures to the extent that one furnishes them [i.e., the subjects] the occasion [for using propositional structures]"(Piaget and Inhelder, 1967, p. 115, my translation). This underscores the relevance of formal reasoning for science education from a Piagetian educator's point of view.

Piaget has provided us with a vast collection of descriptions of performance and explanations purporting to unify and make sense of the descriptions. I have claimed that the theoretical accounts of performance data are unacceptable, insufficient at best, and invalid at worst. The performance data persist, however. The youngest pupils in primary school do have difficulty with most concrete tasks although they usually can solve a few. As they grow older, they solve more and more of these tasks even though no one apparently instructs them. By middle to late adolescence, the chances are no better than 50-50 that a student will solve Inhelder and Piaget's formal tasks. The probability that most of these tasks are solved is approximately zero unless the student is exceptionally able academically. In this case, however, success on formal tasks may be due in large measure to school learning. Piaget claims, however, that formal operations are necessary for doing well in science and mathematics.

A widespread view among developmentally inclined educators is that students do not do well in science because they lack formal operations, however these may be defined. Theoretical models aside, the question is, "How and why do some subjects ultimately develop the abilities to handle formal tasks?" The educational challenges are, "How can schooling help students succeed on formal-type tasks?" and "For those students who are still 'concrete,' how can we adapt course content and instructional methods to their intellectual level?"

The Transition from Concrete to Formal

Although Piaget has sought to understand development, he has focused almost entirely on what children know or do not know at a particular time. As an initial research strategy this makes sense: first determine states, then study transitions from one state to the next. Piaget has maintained his initial strategy for over half a century. Thus, he can only speculate on the dynamics and the processes of development. Of course, given his vast knowledge, his speculation is informed.

About a decade ago, Genevans began investigating change more directly. Their general views are briefly stated and quite consonant with the tenets of discovery learning. Development requires interaction: the learner must be mentally or physically active (or both) on a problem. For motivational reasons, the problem should be not too easy, not too difficult. The learner requires a variety of such problems, each demanding a variety of actions, both mental and physical. The resultant knowledge is composed of complex relational structures, not a list of facts or associations. The learner should interact with others, with peers especially, and try to communicate his thoughts about the problem at hand. Finally, the learner needs time. Structures by their very nature develop slowly. There is little else to add.

Quite generally, development is due to four factors: maturation, interaction with the physical world, social interaction, and equilibration. All four are important, but Piaget has written almost exclusively of equilibration. Educational research however is almost exclusively occupied with the interactional factors (social interaction includes interaction with teachers). Moreover, Piaget's accounts of equilibration are rather vague (Bruner, 1959; Furth, 1977). Thus, when some of his Genevan colleagues turned to training research, they had little to go on other than a broad philosophical position.

In 1974, Inhelder, Sinclair, and Bovet (1974) published the results of a brief series of training studies, the only such studies in half a century of Genevan research. They wrote that "little is as yet known about the mechanisms of transition from one major stage to the next and about the passage between two successive substages. In order to account for the transition mechanisms, structural and dynamic models have to be combined" (1974, p. 14). They and Piaget realized that his theory at best could make sense out of the direction of development—the logical structures were presumed to have a natural order, the formal stage group following and "completing" the concrete stage grouping. They also realized that the logical structures could not account for the transitions between substages, the decalage problem.

Lacking specific and detailed theoretical guidelines, they accepted Piaget's speculations at the outset. They adopted "the idea that under certain conditions an acceleration of cognitive development would be possible, but that this could only occur if the training procedures in some way resembled the kind of situations in which progress takes place outside an experimental set-up" (1974, p. 24). This is an interesting hypothesis. It is a little mind-boggling that they committed themselves to years of research based on an untested and highly speculative premise. Fortunately, the psychological tradition outside of Geneva insists upon the evaluation of hypotheses. The Genevan results were modest whereas North American training studies have produced exceptional results (Brainerd, 1977c; 1978a).

Another methodological point: the Genevans sought to model natural situations in which progress is made. How could they evaluate the accuracy of their model? They had never investigated such natural learning situations before. In fact, the best they could do was let themselves "be guided by what was already known about very general trends in development and about the types of difficulties young children encounter"(Inhelder, Sinclair and Bovet, 1974, p. 24).

Another point of interest to educators concerns the role of language. Piaget has always been eager to show that action, not language, is the basis of knowledge. Genevans have tried to support this position by showing that improvements in language are insufficient to guarantee development or even improvement on a given task, e.g., "verbal training...does not ipso facto lead to an understanding of the concept of conservation of quantities"(Inhelder, Sinclair and Bovet, 1974, p. 270). Occasionally, they have been embarrassed by their own success in producing training effects from "mere" verbal training. In typical Genevan fashion they refused to accept the obvious and instead did what they usually do: invented an ad hoc hypothesis. In this case they stated that the linguistic exercise "verbal in appearance, since the subject was not manipulating anything, constitutes in truth a [cognitive] operational exercise" (Piaget, 1974, p. 260). No serious attempt is made to assess verbal training effects. Thus, those training experiments concerned with the role of language simply assessed the effects of verbal factors in isolation.

On the other hand, no attempt was made to assess the effects of verbal input in the context of problem-solving activity. The latter use of language in conjunction with activity is in fact found in educational settings, particularly with laboratory activities and discovery learning programs such as SCIS. Also, the conjunction of adults' language and children's action characterizes much of the learning experiences of children in the home. Parents are (often) correct when they describe their children's learning as rapid and substantial. Thus, the Genevans failed to investigate an important type of situation in which progress takes place, a type of situation which is also often encountered in school settings. Otherwise, their method resembles that of relatively unguided discovery learning with an accent on cognitive conflict (to produce disequilibrium followed by reequilibrium).

Inhelder, Sinclair, and Bovet tried to train learners on concepts from the concrete stage such as conservation and seriation. They were very non-directive to avoid "coercing" the learner so they never provided the learner with direct evaluative feedback on the progress he was or was not making. Learners had to infer progress or the lack of it indirectly, e.g., when asked to do something over again, a child can tell that he or she did not do it right the first time even though it may be quite unclear as to what was wrong. This has been observed repeatedly in Geneva. The child is both mystified and unhappy.

They also failed to sufficiently provide for motivation, or at least it seemed that way to me during the training sessions I observed while in Geneva. The questions put to children were removed from their interest in the sense that there was no point or goal from the child's perspective. The absence of a desirable goal is common in testing procedures in America and in much of the Genevan work on assessment of knowledge. The conservation task, for instance, has no goal except for assessment techniques, but it goes counter to the Genevans' avowed interest in reproducing or simulating natural experience.

The natural experience of children in spontaneous learning situations is surely goal directed. What is learned may or may not be the initial goal of the activity, but there would be no activity without some desirable goal. Thus, the Genevans are guilty of "coercing" children into pursuing goals known only to the researchers. As we shall see later, methods have been developed which allow the child to unambiguously evaluate his or her progress in the context of goal-directed training activity.

To conclude this necessarily brief section, Genevans claim that true learning results only from active self-discovery methods. Adult intervention such as pointing out errors or giving answers is strictly forbidden: this is not the way to true learning. This laissez-faire policy is not an empirically verified conclusion, but rather an article of unquestioned faith. Although some substantial learning resulted from their technique, it was restricted to individuals who already knew something of what they were supposed to learn. Brainerd (1978) has severely criticized this and other aspects of their methodology and theoretical framework. One should not read the Genevans' work without also reading Brainerd's.

The Genevans conclude that whereas they have only made a beginning and much is left unclear, they are at least sure that S-R theories and nativist theories (e.g., Skinner and Chomsky) as well as any direct tutorial methods are unable to complete the task they have begun. This dogmatic assertion has been criticized as being uninformed by a Piagetian colleague of the Genevans in a paper entitled "The Genevan Training Experiments: A Hardly Convincing Collection (Even for Piagetian)" (Lefebvre-Pinard, 1976; my translation of the title). The author, Lefebvre-Pinard, working within her Piagetian framework had designed some excellent training procedures as have others who can be considered Piagetian or neo-Piagetian researchers (Brainerd, 1973c, 1978a). It is to Piaget's everlasting credit that so much that is positive has evolved from his work. It should not be surprising that research has been unpromising where it has clung to the past to the rigourous exclusion of the present (Lefebvre and Pinard, 1972, p. 103).

Other Developmental Theories

Most science education research has been based on Piaget if it has been based on any developmentalist (Novak, 1978). Regardless of the worth of other theories, they have not had much impact on research, although the situation may be changing. Two theorists, well known, but not often cited are Bruner and Ausubel. Both are very Piagetian even though both sharply disagree with Piaget on some issues. Bruner and Piaget have feuded in research journals, while Novak, Ausubel's champion, has often jousted with Piagetians. The debates have been salubrious though overextended. I will briefly consider the main areas of disagreement.

Bruner (Bruner, Olver and Greenfield, 1966) reanalyzed and gave altered meaning to Piaget's notions of sensori-motor, concrete, and formal operations. Bruner speaks of three kinds of knowledge: enactive, ikonic, and symbolic. These terms refer to mental codes or representations. The enactive mode of representation involves physical action, the ikonic mode involves imagery and perception, while the symbolic mode principally involves language. All modes may be used at <u>any</u> time by subjects of school age. Concrete subjects would be weaker than formal subjects at representing knowledge, particularly in the symbolic mode, but this mode is not denied them. On the contrary, Bruner's technique for producing cognitive conflict and presumably subsequent conflict resolution capitalizes on the younger subject's ability to use symbolic representation in the absence of distracting concrete elements.

For example, Bruner observed that 5-year-olds would predict conservation provided they did not see the water levels (Bruner, Olver and Greenfield, 1966). This prediction conflicted with erroneous inference drawn from their ikonic (mis-) representation of quantification: when they saw the results of pouring, the resulting conflict initiated changes in the direction of higher functioning, i.e., towards the symbolic mode. But one can easily imagine that there are cases where experience is not deceiving and it is the symbolic representation, not the ikonic one, which undergoes change. In any case, the disequilibrium is between representational modes. If this disequilibrium is of educational value, then the teacher must find ways for the pupil to represent knowledge in several ways. This might require great ingenuity, particularly if the technique is to be applied individually and spontaneously in the classroom.

Scientific and mathematical knowledge involves all modes, but the symbolic mode is the most powerful. The symbolic mode can represent in language and symbols the knowledge represented in the other modes. The other modes are closer to direct experience whereas the symbolic mode can go beyond direct experience. This is Piaget's form-content differentiation. (Actually this differentiation is as old as rationalist philosophy and goes back to Plato.) In addition to going beyond the record of past experience, the symbolic mode allows a projection into the potentialities of future experience. Finally, the symbolic mode is the mode of choice for communication of ideas although the ikonic (pictures, graphs) and enactive modes (laboratory activities) cannot be entirely supplanted.

Bruner also suggested that action and perception could provide the basis for a symbolic representation that would be relatively general and applicable to a wide range of situations in addition to the ones initially encoded. Bruner makes no mention of concrete and formal; in his applications of this instructional method, concrete and formal are irrelevant concepts. Instead, Bruner takes care that (a) the required actions and perceptions are simple and performed by the learners, and (b) the symbolic representation falls within the scope of learners' language ability. Whether either constraint is met can be gauged only on the basis of experience with the learners. The method seems to require a level of facility with mathematical notation to be expected of junior high school students of at least average ability. Bruner (1964) describes his technique in an application to the induction of the formula $(x + n)^2 = x^2 + 2nx + n^2$ with primary schoolers of exceptional ability. Tony Lawson and I conducted a training study designed along similar lines for teaching ratio and proportions to 12 year olds (Wollman and Lawson, 1978). The method involves evaluative feedback and while it allows for self-discovery, it does not rule out direct rule instruction. Lawson and I refrained from using rule instruction and found that our method was clearly superior to a rule-instruction tutorial based on a textbook presentation.

Bruner's second main point is that each culture transmits to its youth a set of intellectual tools which may be in part unique to that culture (Bruner, Olver and Greenfield, 1966). The young follow the lead of their elders in acquiring those intellectual skills which are in current and frequent use. This is reminiscent of Dewey's (1910) claim that the division between concrete and abstract ideas depends on familiarity. Those ideas frequently encountered are called concrete while uncommon ideas strike one as abstract. Ausubel (1964) took this position in an early pro-Piaget article. Dewey points out that familiarization with mathematical ideas will transform one's reactions to them from a feeling that they are abstract to a feeling that they are concrete. The educational implication is that, wherever possible, try to make the use of basic intellectual skills part of students' everyday experience.

Beni Chen and I tested the feasibility of this idea in a training study with very average fifth graders. We tried to "teach" the controlled experimentation concept by simulating the kind of social interaction that leads one to question and carefully examine causal explanations. Although we could not initiate this kind of interaction every day, we tried to make it appear quite natural, that is, we tried to convey the idea that this sort of give-and-take method of inquiry could be part of one's daily intellectual experience. The method succeeded far beyond our expectations (Wollman and Chen, 1978).

Why so little has been made of Bruner's ideas by science educators is puzzling. His book, The Process of Education (1960), is a classic and was much quoted by curriculum developers and advocates of discovery learning. Perhaps his methods require too much of teachers.

The second developmentalist, Ausubel, has taken issue with Piaget on the role of language. Ausubel (1963) believes in the possibility of meaningful verbal learning, while Piagetians apparently do not because it involves "coercion" and robs the student of the "full" opportunity for self-discovery. Since there is no accepted definition of "meaningful," the debate may be theoretically hollow. Piagetians have their definition and it certainly differs from that of others (Inhelder, Sinclair and Bovet, 1974, Introduction; but see also Brainerd, 1978a). The definition of meaningful is largely a matter of taste.

The empirical record indicates that many researchers are satisfied that meaningful verbal learning is possible (see Novak, 1977a for a review). But, for now, the main point is that words must refer to something tangible even when words are not completely reducible to tangible referents. For meaningful verbal learning to occur, students must have had relevant experience with tangible referents. Some of that experience can occur at the time of learning. Novak's students have utilized these ideas in training studies with young children (Novak, 1977a).

Ausubel and Novak also take issue with Piaget on the usefulness, if not the validity, of Piaget's stage notions. Novak (1977a) in particular feels that the evidence does not support stage notions and only forces Piagetians into constructing convoluted "Catch-22's." Recent work in psychology converges on the same conclusion though perhaps not for the same reasons (e.g., Brainerd and Siegler, 1978; Wohlwill, 1966).

Ausubel's principal theoretical constructs owe much to Piaget, just as Bruner's do. For example, Ausubel writes that "The most important single factor influencing learning is what the learner already knows" (1968, p. vi). Novak calls this "the most important idea in Ausubel's theory"(1977a, p. 455). But Piaget wrote that "assimilation is the fundamental fact of psychic development"(1963, p. 42), that learning should "vary very significantly as a function of the initial cognitive levels of the children" (1970, p. 715). Thus, in first approximation, Ausubel = Piaget. Differences emerge when theoretical descriptions of knowledge are given.

Are there differences in the description of the conditions of learning? Novak writes, "Meaningful learning involves a conscious effort on the part of the learner to relate new knowledge in a substantive, non-arbitrary way to relevant existing concepts or propositions in the learner's cognitive structure" (1977a, p. 456). Given the Piagetian distinction between learning and development, namely that learning is the application or "elaboration" (Strauss, 1972) of cognitive structure (Piaget, 1970a; Inhelder, Sinclair and Bovet, 1974), there is no difference between Novak/Ausubel and the Genevan position stated by Inhelder, Sinclair, and Bovet: "In terms of successful training procedures...the more active a subject is, the more successful his learning is likely to be" (1974, p. 25). Genevans have mental as well as physical activity in mind, as I have noted before.

Novak goes on to use the term "assimilation" to explicate Ausubel's "subsumption" construct: new knowledge is "subsumed" by "anchoring concepts" in an interaction "altering the form of both the anchoring concept and the new knowledge assimilated" (1977a, p. 456). My Ausubel-Piaget dictionary translates "anchoring concept" and its synonym "subsumer" by "scheme." Similarly, "subsumption" becomes "assimilation" in the necessarily broad sense of both assimilation and accommodation.

Novak cannot get off the Piagetian hook by pointing out that in Ausubel's theory, "specifically relevant concepts" rather than general schemes do the assimilating. It has been argued that the same is true in Piaget's theory, but that Piaget has chosen to focus only on what is common across concepts rather than on their specific differences. This unavoidably resulted in the decalage phenomena which Novak so correctly took to mean that Piaget's stage notions do not help us understand what has or can be learned. It is tempting to say that Ausubel tried to bring Piaget's general theory closer to reality.

Indeed, Ausubel in 1964 anticipated Piaget's partial attempt in 1972 to accept reality. In that 1964 article, Ausubel said that a subject's cognitive level of functioning would regress in the face of an unfamiliar or very difficult problem. In 1972 Piaget said the same thing. Given subjects "capable of thinking formally in their particular field," when they are "faced with our experimental situations [i.e., the formal tasks], their lack of knowledge or the fact they have forgotten certain ideas that are particularly familiar [to others]...would hinder them from reasoning in a formal way, and they would give the appearance of being at the concrete level" (Piaget and Inhelder, 1967, p. 10). Thus, both authors rejoin Dewey who pointed out that unfamiliar material was abstract and difficult because, being unfamiliar, it held relatively little meaning or lacked concreteness (in Dewey's sense of the term).

Ausubel also anticipated Piaget when he wrote that an individual might be formal in one field, concrete in another: "abstract thinking, for example, generally emerges earlier in science than in social studies because children have more experience manipulating ideas about mass, time, and space than about government..." (1964, p. 49). This is also Dewey's position. Ausubel goes on to point out, "However, in some children, depending on their special abilities and experience, the reverse may be true" (1964, p. 49). Piaget intended precisely this meaning when he wrote that although "all normal subjects attain the stage of formal operations..." one can expect that "they reach this stage in different areas according to their aptitudes and their professional specializations..."

Ausubel anticipated Lawson's article of 1977 as well. In that article, Lawson came to grips with the problem of decalage at the formal stage. Rather than expect across-the-board formal stage performance from a student who is formal on one task, Lawson suggested that one should expect instead that this student will reach a formal level of functioning on other tasks sooner than the student who is not yet formal on any task. Lawson illustrates his idea with examples from athletics:

it is easier to pick up a new sport if you are already adept at other sports than if you have never participated in any sport. The more you participate, the easier it gets because you have developed transferrable elements. The hand-eye coordination involved in hitting a baseball incorporates some elements transferrable to tennis or golf, etc.

Ausubel (1964) nicely put Lawson's case when he wrote that a formal individual starts out concrete with a new subject matter area "but since he is able to draw on various transferrable elements of his more general ability to function abstractly, he passes through the concrete stage of functioning in this particular subject matter area much more rapidly than would be the case were he still generally in the concrete stage of development"(p. 51). There is nothing wrong with reinventing the wheel: the main thing is to put it to good use. In the next chapter we will see whether this has been done. For now, I shall continue with the parallels between Ausubel and Piaget.

Another key Ausubelian concept allied to assimilation is that of "progressive differentiation." It refers to the process whereby "concepts and propositions become more elaborate" as "new knowledge is acquired." Thus, "any given concept...is in the process of being differentiated." Differentiation and elaboration are synonymous. An everyday term such as refinement would also seem to catch the meaning: the more a concept is used in different situations, the finer the shades of meaning which attach to it. For when a "subsumer" or "schema of assimilation" is applied to many things "it is necessary for it to become differentiated." As a result, "new linkages form between concepts, thus codifying in part the whole matrix of inter-connected concepts." In other words, the subject "assimilates to himself the whole universe, at the same time that he accommodates him-self to it." Indeed, "it is the infinite variety of combinations possible among schemata which is the big factor in differentiation." No wonder that differentiation is an integral part of "assimilation theory" as <u>both</u> Novak (1977a, p. 457) and Piaget (1967, p. 407) call it.

The quotations in the preceding paragraph are drawn from Novak (1977a, p. 457) and Piaget (1966, p. 150; 1963, p. 408). It is left to the reader to judge whether it is worthwhile to sort them out. Will the real theorist please stand up?

Science education research has used Piaget's ideas more than those of any other developmentalist's, for two principal reasons. First, a part of the educational community always has looked toward some contemporary champion of development and Piaget currently fills that role. Second, Piaget's theory seems to hold out promises of unparallelled magnitude. These promises are based on the belief that learners, subject matter, and teaching strategies are in some way commensurate and can be measured or compared on a common developmental scale. At last there appeared a scientific basis for the dream of optimally harmonizing learner, subject matter, and teacher.

It has been repeatedly pointed out that the theoretical bases of the concrete and formal classifications are very weak, that the weight of empirical evidence goes against the most educationally relevant aspects of Piaget's theory, and that the data he reports or claims to possess are often suspect, sometimes replicated, sometimes not. This discouraging state of affairs does not preclude the possibility that useful research can be conducted under the alternative assumption that Piaget is essentially correct. Let us consider them both the educational implications of Piaget's theory, as drawn by educators, and the research that followed.

Educational Implications

The general implications of the developmental psychology of Piaget fall into three broad categories (Brainerd, 1977b):

(1) Sequencing of subject matter
(2) Choice of subject matter
(3) Method of instruction

Sequencing of Subject Matter

The sequencing implications include both learner and subject matter, for both can presumably be placed somewhere along a developmental scale. Ideally, once you have located the learner's position, you "read off" the available subject matter at that position, make a selection, attempt to teach it and, depending on your goals, you stay at that level or move up on the scale. In a less than ideal world, the learner is not simply located at a particular point on the developmental scale: a second coordinate, the concept type, is needed. First one must choose the type of concept to be taught and only then should one developmentally assess the learner. If the concept is transitivity or seriation, proportional reasoning or separating variables, etc., you will discover that the world is even less ideal than just described: a third coordinate, the concept setting, is needed. In addition to specifying the concept, you must also specify the specific context in which it is used, the problem in which it is embedded. The learner who has mastered transitivity of length may find weight transitivity problematic. The learner who solves a ratio task involving a 2:1 ratio with small numbers may fail when the numbers are in a 3:1 ratio or are large (e.g., 34:17). And so it goes.

Choice of Subject Matter

Choice of subject matter has been influenced by Piaget's findings (see, e.g., Kamii and DeVries, 1974). For instance, elementary school science and mathematics curricula have included classification, seriation, conservation, and reference frame exercises. Recommended subject matter includes aspects of topology and geometry studied by Piaget and his associates. As Brainerd puts it, "advocates of cognitive-developmental curricula almost invariably interpret the theory's statement that we could teach certain things as mandating that they should be taught" (1977b). It seems clear educators believe that by including Piaget's tasks, they are providing for learners' intellectual development while ensuring that their curricula show up well on evaluations which include measures of development. On the other hand, curricula should presumably exclude any subject matter that cannot be learned via active discovery methods with minimal teacher intervention. Formal stage tasks in particular are not to be included in elementary school programs (e.g., Elkind, 1972).

Method of Instruction

The methodological implications include active self-discovery methods, use of concrete materials and graphics, peer interaction, and modeling behavior. These implications are not unique to Piagetian psychology, but they are accorded particular importance in his theory. Instructional techniques are presumably classifiable along a developmental scale as being more or less appropriate for a given developmental level. For post-elementary school levels, concrete props or realistic, easily imaginable settings are considered important for introducing new ideas.

Typical of the developmental curriculum guidelines are those offered by Karplus (1977) in his three-phase learning cycle:

> During exploration, the students gain experience with the environment--they learn through their own actions and reactions in a new situation. ...The new experience should raise questions...they cannot resolve with their accustomed patterns of reasoning...As a result,mental disequilibrium will occur and the students will be ready for self-regulation.
>
> ...concept introduction...starts with the definition of a new concept or principle...The concept may be introduced by the teacher, a textbook, a film, or another medium...

> In...concept application, familiarization takes place as students apply the new concept...to additional situations... physical experience with materials and social interactions with teacher and peers play a role (pp. 173-174).

Karplus' exploration concept introduction concept application cycle [formerly the exploration-invention-discovery cycle (Atkin and Karplus, 1963)] parallels the following:

(1) Preparation

(a) "Statement of the aim:" providing learner with a clear goal.

(b) "Providing the apperceptive basis:" insuring through "thoughtful grading or sequence of materials" that the learners are in possession of the "knowledge or experiences...that are absolutely necessary...to understand and master" the material.

(2) Presentation

Learners receive "the concrete or particular, new material that involves the generalizations that are ultimately to be reached."

(3) Comparison

"This third 'step' in the inductive plan is intended to bring together related elements in such a way that the fourth 'step'—the generalization—shall proceed out of it as a flashing discovery..."

(4) Generalization

Here "the climax to the inductive process is reached...The rule or definition, the formula or principle, has been evolved and is stated."

(5) Application

"The generalization...is now applied to new particulars..."

This learning cycle is quite similar to Karplus. Preparation and presentation are like exploration, comparison and generalization like concept introduction (or invention), and application is identical to concept application. Karplus' approach is less idealistic because he intends for the teacher or text to introduce the new idea. Although he allows for the possibility of spontaneous invention, he does not insist upon it, whereas in the other version of the "induction-deduction" cycle, "the generalization must, by all means, be made and stated by the pupils themselves—never by the teacher." The latter is a more thorough-going Piagetian position than is that of Karplus.

The method is called inductive-deductive because after the concept is induced from particulars, applications are deduced. In Piagetian style, "these reciprocal logical processes [of induction and deduction] go on unconsciously as we are confronted with new experiences, but the genesis of new ideas...may be developed by a formal application of the natural logical steps involved" (emphasis added).

This close and very Piagetian alternative to Karplus' approach was developed by Herbart about 150 years ago (Spencer, 1910).

Methodological principles and catch-phrases from other sources are "learning has to be an active process," "intellectual activity [should be] based on actual experiences rather than on language," that is, "the process of self-development should be encouraged to the uttermost." "Children should be led to make their own investigations, and to draw their own inferences. They should be told as little as possible, and induced to discover as much as possible." Progress is made from the "concrete to the abstract," from "the empirical to the rational." The preceding is a mixture of old (Spencer, 1910) and new (Kamii, 1973).

Teachers and curriculum planners have little to choose between past and present as regards general pedagogical principles. These principles may nonetheless be excellent, as I believe they generally are. However, if after hundreds of years, teachers are still being exhorted to use developmental principles, one can wonder whether Piaget will go the way of Pestalozzi, Herbart, Spencer, and Dewey, respected but out of vogue, if not forgotten. The fate of Piaget's relationship to education will depend on the research findings stimulated by his theory and observations. If this research does not advance us in a practical way beyond the speculations and maxims of the past, then we can write the future now.

The Research Program

The questions asked by most developmentally oriented science education research fall into one or more of four categories. Previous reviews (Modgil and Modgil, 1976; Athey and Rubadeau, 1970; Chiappetta, 1976; Haley and Good, 1976; Helgeson and Blosser, 1976; Rowe and DeTure, 1975; Phillips, 1974; Voelker, 1963; Novak, 1973; Trowbridge et al., 1972) analyzed the literature differently, of course.

(1) Test construction and surveys: (Lawson and Nordland, 1977; Joyce, 1977; Hale, 1976; Haley and Good, 1976; Herron, 1976; Carlson, 1976; Shayer, 1976; Bennefield and Capie, 1976; Sayre and Ball, 1975; Rowell and Hoffman, 1975; Raven and Guerin, 1975; Grant and Renner, 1975; Nordland et al., 1974; Lawson and Renner, 1974; Shayer and Wharry, 1974; Lawson et al., 1974; Renner and Lawson, 1973; Shayer, 1970; Piburn, 1977; Ruud, 1976; Wollman, 1976; Karplus et al., 1975; Bady, 1977; Smock and Belovicz, 1968; the following also fall into the second category, Kolodiy, 1977; Bauman, 1976; Albanese et al., 1976; Lawson and Blake, 1976; Sayre and Ball, 1975; Lawson et al., 1975; Wheeler and Kass, 1977; Ball and Sayre, 1972).

(2) Relationships of Piagetian measures with other measures (science achievement, reading achievement, and IQ, among others): (Lawson and Nordland, 1976; Lawson et al., 1975b; Lawson and Renner, 1975; Lawson et al., 1975a; Raven et al., 1974; Raven and Polanski, 1974; Leon, 1971; see also category (1)).

(3) Developmental classification of subject matter: (Shayer, 1972, 1974; Lawson and Renner, 1975; Grant, 1976).

(4) Evaluation and comparison of teaching methods and curricula: (Wollman and Lawson, 1977, 1978; Vanek and Montean, 1977; Rowell and Dawson, 1977; Raven and Calvey, 1977; Boulanger, 1976; Renner and Lawson, 1975; Bredderman, Ted, 1974; Nous and Raven, 1973; Wheeler and Kass, 1977; Rastovac, 1977; Cleminson, 1970; Emery, 1973; Andriette, 1970; MacBeth, 1972; Weber, 1972; Wheatley, 1975; Stafford and Renner, 1971; Schmedemann, 1970; Price, 1968; Allen, 1967; Raun and Butts, 1966; Voelker, 1968).

Researchers wanted to find out who is formal, who, concrete. Wanting to conduct large-scale surveys, they designed either rapid interview methods or group tests. Concerned with the developmental match between learner and subject matter, they sought ways of classifying the latter as well. Given instruments for assessing the developmental level of learner and subject matter, they evaluated science programs and teaching efforts. Since other ways already existed for assessing performance and otherwise classifying learners, researchers looked for relations between the new methods and the old. On rare occasion (Erickson, 1977), researchers have tried to remain faithful to Piaget's clinical interview method when assessing students' knowledge. Attempts to replicate some of Piaget's findings are also quite rare. Research has aimed primarily at primary school levels, although there is a recent upturn of interest in college education. Consider findings in the principal categories will be considered in turn, focusing on post-elementary school research findings as much as possible.

Test Construction and Surveys

The majority of junior and senior high school students generally do not do well on Inhelder and Piaget's formal operations tasks, particularly on those requiring computation. Although performance levels increase with age, the average beginning college student has at most about a 50-50 chance of success on formal tasks in general, somewhat more on the controlling variables and combinatorial tasks, somewhat less on the proportional reasoning tasks. Of course, science majors do much better as do gifted non-science majors. Performance on original tasks, ostensibly requiring formal operations, ranges from floor to ceiling, from less than 5 percent to more than 95 percent success. Wason and Johnson-Laird discuss possible reasons for performance variability in an important book (1972).

Researchers have developed test batteries whose items scale in some way, correlate reasonably well with tasks from GLT or elsewhere, give statistically reliable results, and roughly identify students as more or less able in science courses. However, there is no evidence that these tests can tell teachers more than they already knew.

Piagetian and neo-Piagetian psychology has not given much attention to tests and surveys since the early days in the 1960s when investigators were trying to replicate Piaget's findings. Even the Genevans abandoned or at least shelved some years ago their own attempts to construct test batteries (personal communication). These tests were supposed to sharply delineate a subject's developmental profile. It is possible that research was suspended because no sharp profiles were emerging (decalage, of course). Nevertheless, developmental test construction continues to have great appeal to education researchers. As always.

Relations of Piagetian Measures with Other Measures

The general finding in this area is that Piagetian tests sometimes correlate moderately with IQ, scholastic achievement tests (in reading and language as well as math and science) and other measures. Occasionally very high correlations are reported, but these probably were obtained with unacceptable sampling procedures. Test items are not usually reported in the literature, much less the difficulties of test construction. With great reluctance, discussion of these matters is postponed (see below). Other findings are that formal stage tasks often do not correlate even moderately well with each other (e.g., Arlin, 1975). Piaget (1972) now tells us that this should not be surprising and is no cause for alarm. As a result, decalage has been recognized and sanctioned as the rule rather than the exception. Perhaps Piagetian science educators do not realize the extent to which this reduces the usefulness of Piaget's theory. Novak (1977b), discussing Piaget's theory in the light of Wollman's work (1977a, 1977b) and others', was reminded of epicycles within epicycles. We all remember the Ptolemaic theory. Or do we?

Developmental Classification of Subject Matter

There have been heroic attempts to classify science concepts as more or less concrete or formal (Shayer, 1972), but these classifications have little or no theoretical and/or empirical basis. The methods have relied on either the prima facie similarity of a school concept with a Piagetian one or the researcher's best guess as to the difficulty of the concept. Since most school science concepts are not very similar to Piagetian concepts, informed guesswork has been the method of choice. On the basis of this guesswork, high school physics has been identified as being more demanding than biology, which will surprise no one, least of all high school students and teachers.

When test items have been classified, then administered, the results have generally supported the classifications to the extent that item difficulty increases, on the average, with developmental classification (along a three-or four-point ordinal scale, usually). Thus, some investigators have been proved adept at predicting whether a test item is relatively difficult, easy, or in-between. Relatively difficult items are longer and more computationally demanding than are simpler items. The simplest items include recall of information, perhaps simple qualitative inferences. The in-between items resist dichotomous classification. This type of classification is much more an art than a science.

Lacking in this area is research comparing the psychologically naive classifications of teachers and their students with the psychologically informed classifications of investigators. Such research would be extremely interesting. It might determine whether there is any need for further work of this type, an essential consideration for funding agencies.

Evaluation and Comparison of Teaching Methods and Curricula

About as often as not, relatively Piagetian curricula are no more effective than more traditional alternatives (Vanek and Montean, 1977; Raven and Calvey, 1977; Cleminson, 1970; Andriette, 1970; Wheatley, 1975; Schmedemann, 1970; Allen, 1967; Raun and Butts, 1966; Voelker, 1968). This rather generally stated finding has been replicated at essentially all grade levels for a variety of criteria. Nevertheless, several of the studies report some evidence in favor of developmental curricula (Raven and Calvey, 1977; Weber, 1972; Stafford and Renner, 1971; Marks, 1966). The differences are statistically significant of course, but are they educationally significant? The research literature does not address this question, e.g., there is no concern with whether differences between learning outcomes are large when compared with the cost of achieving them.

When learning outcomes of curricula are "explained," obvious problems must arise. Different curricula differ in so many uncontrolled-for ways, that one cannot be at all sure why learning outcomes do or do not differ. Relatively unambiguous causal explanations can only come from carefully controlled comparisons and, for practical reasons, this means small scale training studies.

Training studies are rare and have given mixed results, success and failure (Rowell and Dawson, 1977; Wollman and Lawson, 1977; Boulanger, 1976; Renner and Lawson, 1975; Bredderman, 1974; Nous and Raven, 1973; Wheeler and Kass, 1977; Rastovac, 1977; Emery, 1973; MacBeth, 1972; Price, 1968). But the reasons for success or failure are still lacking, because these studies have deliberately confounded variables which might affect learning outcomes. The justification has been that such studies were primarily concerned with the feasibility of teaching certain concepts, leaving for a future date more analytical research, if need be. The widespread feeling, due to Piaget and his colleagues, that acquisition of Piagetian concepts necessarily requires broad "multivariate" experience probably discouraged analytical laboratory techniques while encouraging "messy" experiments. The analytical research of instructional psychology (discussed later in this paper) has yielded clear and eminently successful results.

One particularly interesting result from the science education literature was reported in an acclaimed paper on the training of the formal concept of controlled experimentation (Lawson and Wollman, 1976; see also Wollman and Lawson, 1977). Responsiveness to training was shown to vary significantly with developmental level. Had this level been determined with respect to the concept in question, the Genevan's technique, the result would have been uninteresting. In that case, all one could say was that the better a subject performed initially on a closely similar task, the better the subject performed after training. Barring ceiling effects, this is not surprising. However, the developmental scale was based on conservation task performance. These tasks were not similar to the criterion measures of causal inference.

The authors interpreted this evidence as confirming Piaget's claim that conservation performance indexes development. This is probably true, but not for Piagetian reasons. It has been shown that conservation performance also indexes memory or attentional development which has clearly and often been related to training responsiveness and overall performance level for many different tasks. More about this in the next chapter.

One study (Wollman and Lawson, 1978) compared a Piagetian tutorial with a textbook tutorial. The Piagetian approach was superior in teaching the proportionality concept, although the textbook tutorial also enjoyed modest success. Studies such as these are considered by their authors to confirm the general superiority of the developmental approach over traditional alternatives. "Developmental" means active involvement of subjects with concrete materials guided by low-profile tutors.

Since this type of research is conducted by people committed to the developmental approach, perhaps different results would be obtained by investigators with a different bias. This has indeed been the case. Such research, drawn from the instructional psychology literature, is discussed later in this paper.

In conclusion, the dozen or so studies in the science education literature on teaching outcomes support the contention that teaching Piagetian concepts is feasible. However, when such attempts have worked, it has not been clear why. Failure is as poorly understood. Thus, no clear pedagogical guidelines have been empirically delineated. The science education literature has not advanced us beyond Herbart. The reasons are clear.

Weaknesses of the Piagetian Developmental Approach

In a word, the principal weakness of developmental science education research is its reliance on dubious implications of a speculative theory. The specific weaknesses of the four principal research categories will be discussed and some general observations will be made.

Test Construction and Surveys

Most problems stem from two sources, the concept of general stages and their characterization in terms of logical structures. As construed by the science education community, these theoretical cul de sacs have undermined attempts to find general and useful descriptions of students and tasks. These theoretical shortcomings, for which science educators are of course not responsible, also render inconclusive, if not pointless, correlational studies of various measures. On the other hand, most tenets of developmental curricula and most aspects of discovery learning instructional strategies are unaffected by the defects of Piagetian theory. Developmental and discovery notions were around long before Piaget and probably—and it is hoped—they will remain long after. Only some specific Piagetian suggestions regarding teaching methods and curriculum content are affected, e.g., the laissez-faire approach, the inclusion and sequencing of certain Piagetian concepts, and the exclusion or delay of introduction of other concepts.

The stage concept has been carefully examined in the psychological literature and found wanting. The details of the arguments will not be presented, but one issue deserves mention since it goes to the heart of the usefulness question and science educators are aware of it. The issue is that of decalage, performance inconsistency.

The main Piagetian promise or hope was to bring order to chaos. To use the statistics metaphor again, Piagetian concepts were supposed to allow classification of learners and subject matter into large yet distinct groups such that the within-group variance was significantly less than the between-group variance. Within each group, learner abilities and subject matter demands were presumed to be closely matched. While this can always be done empirically, via trial and error, Piagetians believed they could now do it theoretically. But it has not been possible.

Theory has been unable to predict performance. Decalage, within-group variance, remains important even at the level of formal stage performance. In the end, we are back to empirical relations: we observe that some tasks are of comparable difficulty while ostensibly similar ones are either much easier or much harder. And we do not know why.

It appears that test constructors mislead when they focus on tasks of comparable difficulty and omit mention of all the others they weeded out during pilot work on the test construction. This "weeding out" procedure is appropriate for IQ test construction since the goal here is a reliable age norm along with a certain variance. Items which satisfy these demands are identified empirically; they are not derived theoretically (hence the great IQ debate). But Piagetian test items are supposed to be theoretically grounded. The Piagetian test constructor has not the luxury of the IQ test constructor in being able to discard without further ado the items which do not scale properly. Internal validity is thereby purchased at the cost of external validity.

All initially chosen test items have face validity by definition. Those weeded out during pilot work should be treated as warning signals that either the theory does not work or that the test constructor does not quite know how to use it. It is suggested that one or both of these alternatives is always true and thus, for all practical purposes, the theory of logical stages of development is not useful and test construction should be abandoned.

Test designers conceal performance differences between "structurally similar" tasks. Perhaps they have it backwards. The most interesting data are large performance differences on slightly different tasks. These differences, when noted, have typically been shoved under a rug or explained in some ad hoc fashion.

Other disquieting data have been treated as rudely. For example, when American investigators failed to replicate the Genevan age norms regarding the emergence of formal operations, they gratuitously appealed to some difference between American and Genevan adolescents. This reminds one of a stimulus-response account of the emergence of conservation of liquid quantity. The author, unable to easily assimilate the data to his conceptual framework, posited the ad hoc hypothesis that about the age of six years, parents for some unknown reasons begin to drop all sorts of cues (stimuli associated with the response) suggesting conservation.

Surprising performance inconsistencies or complex performance patterns should be the prime data. Tests whose purpose is to identify concrete and formal students have been designed to eliminate such data since "concrete" and "formal" indicate kinds of consistency.

Piaget proposed decalage in the forties and thus Piagetians have grown accustomed to this idea over the past 30 years. Depending on how one interprets Piaget's writings, decalage describes a more or less important aspect of formal stage behavior as well. But consider how decalage limits our knowledge.

If a subject solves one task involving, say, the controlled experiment idea, does the subject "have" the underlying logical operations and if so, What can you predict? i.e., What do you now know, thanks to Piaget's theory, that you did not know before? The answer is nothing. Even if solving the task means "having" the operations, you will not be able to theoretically predict whether the subject will solve other similar tasks. What if the subject fails the task instead? He might pass the next task, for all you know. What if you have the time and resources to administer many tasks embodying the same "operations"?

You will surely be able to make more reliable empirical predictions regarding the next task, but these predictions will be independent of Piaget's theory. Subjects who consistently pass or fail will be expected to remain consistent while inconsistent subjects will be assigned an intermediate probability of success. Psychology has nothing to do with making these predictions.

Logical operations, by their very general nature, describe all tasks of a given kind equally well and equally bad. Having put a whole group of tasks into the same logical basket, you find that you must sort them out again because, from a performance perspective, they are not all equivalent. Since performance is the ultimate criterion for judging learning outcomes, we are once again at a loss. We cannot make informed decisions about what to teach. Passing one task of a given logical variety does not mean passing all such tasks, hence does not mean there is no need to teach. Failing one task does not mean failing all such tasks, hence does not mean the subject who fails is lacking the "operation."

Developmental science educators seem to think that if only you could teach the operations, the rest would follow, would be assimilated to and appropriately organized by the operations. But tests do not reveal who in general has the operations and, even if they did, it would not matter because the Piagetian logical operations are not clearly related to performance. The psychological literature has not only failed to turn up these operations, it has reported a large body of evidence inconsistent with Piagetian logical operations (e.g., Falmagne, 1975; Siegler, 1978a; Brainerd, 1977c, 1978; Revlin and Mayer, 1978; Siegel and Brainerd, 1978).

Piagetian tests have served another function—that of alerting the educational community to a serious state of affairs: students are supposedly being denied their birthright, namely normal intellectual development of the stage of mature reasoning, the formal stage. Presumably, the schools are not doing their part. To support this propaganda effort, authors cite the fact that in Geneva adolescents develop into fully formal thinkers by 14-15 years. This "fact" is not supported by any evidence. At least, Inhelder and Piaget have reported none. Indeed, this "fact" is only speculation, as previously noted.

I spoke to Piaget about the formal stage age norms. I asked whether on the average subjects of 14-15 years give the fully formal responses described under the IIIB heading in the chapters of *The Growth of Logical Thinking*. "Averages have nothing to do with it," he said, as already reported. At the time, I interpreted this to mean that the exact empirical determination of age norms was not yet accomplished and that Piaget, concerned more with sequences of performance than with absolute age norms, had simply made a best guess regarding an inessential parameter. Presently, I do not think any useful age norms for Piagetian stages can be found. For this reason among others, e.g., decalage, I saw no point in reporting any detailed findings from the test construction and survey data.

I am not defending current educational practice. Schools may not be doing their best to promote intellectual development. Indeed, this should be a routine assumption if we want to improve matters. I am suggesting that although Piaget has done more than anyone to begin finding ways of improvement, Piagetian research is fundamentally ill-suited for completing the task. Moreover, I have suggested that the key to understanding intellectual development may be found by looking at tasks which hardly differ yet which elicit very different performances. Virtually no test surveys have had this goal. The few surveys that have had this goal are interesting because they suggest a neo-Piagetian description of development. They will be discussed in the next section except for the least interesting one which was, however, reported in the science education literature (Wollman, 1977a, 1977b) and which will be briefly discussed now.

A set of tasks were designed to assess varying degrees of difficulty associated with one Piagetian concept in one task setting. The concept was controlling variables and the setting was the collision of spheres. Each task described situations in which a sphere is to roll down an incline and strike a target sphere. The target sphere recoils, the degree of recoil depending on several variables according to the task. The tasks were "hypothesized to differ primarily along one dimension relevant to task difficulty." This dimension was not named, but was described: "each task offered a different number of ways to conduct an experiment—the more ways, the more expected difficulty for a student lacking general criteria to judge the validity of each way." Thus, although simple for an expert, these tasks were supposed to be difficult for novices, with difficulty depending on the amount of information to consider or bear in mind. It was suggested "that the concrete-formal distinction is relative and that a given student may respond to tasks at one level, then to similar tasks at another, depending on circumstances <u>unrelated</u> to the logical character of the tasks, hence unrelated to what Piaget considers to be the main determinant of task difficulty"(emphasis added). It was found that a substantial proportion of primary school pupils correctly responded to some of the tasks. There was no reason to believe that these tasks did not involve in some way the controlling variables concept. Performance level of eighth graders varied with math ability level as determined by the teachers. The best math students were the most "formal," the poorest were the most "concrete." (Can science education researchers really do a significantly better job at present than teachers at ranking students according to intellectual ability?)

The purpose of the survey was <u>not</u> to identify concrete and formal subjects, but to "suggest a generally useful technique for designing sequential learning experiences and assessment instruments consonant with the course of intellectual development as seen from a Piagetian viewpoint." This should have been written "neo-Piagetian viewpoint," but the intent is understandable.

The key ideas contained in these two papers are (1) even very young children have acceptable strategies ("developmental precursors") for solving some controlling variables tasks, (2) a principal dimension of difficulty may be <u>amount</u> of information simultaneously demanded,

(3) this may be true in general, and if so (4) learning sequences should be designed with quantitative informational demand as a key sequencing dimension.

This research was conducted in ignorance of the information-processing tradition and new psychological techniques for describing and comparing performances and task demands. Had he been conversant with the techniques, the author might have been able to quantify or specify the informational demands of the tasks. The instructional psychology literature, described in the next chapter, has gone a long way towards achieving these goals.

Relations Among Piagetian and Other Measures

I have already described weaknesses of attempts to construct tests. These weaknesses carry over to correlational studies. An additional weakness will be described.

Generally, correlational studies have revealed only moderate correlations among Piagetian instruments and between these and other instruments. However, large correlations have sometimes been found and these are taken as confirming the stage notion of Piaget's theory or the ubiquitousness and importance of formal reasoning or some such ideas. I suspect that the correlation coefficients may have been artificially inflated by using unacceptable sampling procedures.

Large correlations can always be obtained by using very heterogeneous samples. If half of the subjects are very able students while the other half are much less able, then (barring ceiling effects) most any intellectual tasks will correlate highly. The abler students will perform well, the less able less well. If the abler students are also older, then it is plausible to attribute the results to the increased store of knowledge and skills (including testwiseness) that accrue to older subjects. Age heterogeneity is unacceptable.

If subjects are of the same age, but of vastly different intellectual and/or cultural milieux, then this heterogeneity is likewise unacceptable unless the measures are culture fair and do not require items of information not common to all. Piagetian tests can fail on both accounts and should be used within a given cultural setting, e.g., middle class Western society.

When researchers have controlled for differences in age, background experience, task relevant knowledge, and other variables, then the principal developmental dimension along which task performances correlate is the quantitative informational demands of the tasks. This is discussed in the next chapter.

Developmental Classification of Tasks

The main problem with "research" in this area is that it is only loosely related to developmental theory and could perhaps be done as well by teachers ignorant of psychology. This kind of classification is more an art than a science for the following reasons.

Classification has relied almost entirely on guesswork. Some school tasks resemble Piagetian tasks of a given stage. With decalage in mind, the investigator then guesses as tohow the school task will compare with the Piagetian tasks in difficulty. However, most school tasks do not clearly resemble Piagetian tasks. In this case, the investigator again guesses, this time on the basis of his and others' experiences as students and as teachers.

By means of this guesswork, the investigator decides which tasks are of comparable difficulty. He classifies and rank orders them in several groups. In each group, there is likely to be at least one task similar to a Piagetian task. The groups are labeled early concrete, late concrete, early formal, etc., according as they contain one or more tasks similar to Piagetian tasks with these labels.

How do these investigators decide on classifications in difficult cases? No one seems to know. Stimulating encounters with colleagues trying to decide which of two tasks was more formal or more concrete has only resulted in learning that these labels cannot generally be applied in any clear fashion. Classroom teachers could probably do as good a job at ranking tasks on difficulty. Someone should test this hypothesis before a great deal more energy and resources are spent on these efforts.

Piagetian task classification once seemed to be a very useful and reasonable thing to do. What has it gotten us? We now know that physics is "developmentally" harder than biology. What else?

An alternative to guesswork and empirical trial-and-error is the neo-Piagetian task analysis approach, developed by psychologists and discussed in the next chapter. This approach also involves guesswork and trial-and-error, all science does, but it is more theoretically coherent than the Piagetian approach. Moreover, this approach deals directly with the decalage problem by accounting for specific content and process variables overlooked by Piaget's logical schemes.

Evaluation and Comparison of Teaching Methods

Developmentalists have not gone beyond Herbart in specifying how to teach. This research is plagued by methods which confound many variables which might affect learning outcomes. Thus one cannot interpret its results with any precision. If complexes of variables had proved consistently and dramatically superior to reasonable alternatives, so be it—stick with a winner! But learning outcomes have been mixed. To find out what affects learning and by how much, researchers will have to adopt properly experimental techniques, systematically varying aspects of the learning environment. Again, instructional psychology has moved ahead in this area, while Piagetians have continued to juggle large combinations of variables, looking for the right chemistry.

Another weakness with Piagetian-oriented research in this area is that it fails to sufficiently focus on what may be the unique advantages of discovery learning methods. Specific concepts or skills might be taught as effectively or even more effectively by more direct methods. But it is difficult for me to imagine how, by direct methods, one can teach someone to be active, independent, critical, and eager to play a role in the acquisition of knowledge. Success enhances motivation, but there is nothing like "doing it on one's own." To a greater extent than direct tutorial methods, varieties of discovery learning allow the learner to "do it on his own." Where students' motivation and enhanced self-concept are important goals, or where independence is a desired outcome, then some autonomous discovery activity may be essential. Measures should be employed to assess these learning outcomes.

Concluding Remarks

Science education researchers have done well to turn to developmental psychology, but they must keep in touch with it if they are to avoid being misled. There is no way to guarantee being misled; psychology is a rapidly changing field and the present is always uncertain. Perhaps a sure way to be misled is to stay in the past. As long as science education investigators are going to avail themselves of developmental psychology without thereby testing its assumptions, methods, and conclusions, they should at least be aware of the work of those that do. Piagetian psychology has contributed to a neo-Piagetian movement whose other source is the information-processing field. The advances of the neo-Piagetian movement could translate into advances in science education research.

Cognitive developmental psychology at the present time differs in important ways from the Piagetian tradition which dominated the field for so long. Of course similarities also exist; much of contemporary developmental psychology had evolved from the Piagetian lineage. The principal novelties of contemporary research are particularly important for science and math education. These new developments are an interest in the detailed nature of children's (and adults') problem-solving strategies and in their memorial abilities. Most of the new work has been done in the seventies although an important source of impetus goes back to Simon's (1962) paper in which he advocated simulating or modeling task performance as if one were writing a computer program or a flow chart representing the functional structure of a program. That was in 1962, a time when Elkind (1961) was just beginning his series of replications and refinements of some of Piaget's conservation experiments which had first been conducted more than a generation before. The Growth of Logical Thinking had appeared in English only four years before Simon's paper. Indeed, Craik (1967) had long ago advocated essentially the same theoretical approach as Simon's simulation of performance by process models. The time was just not right in Craik's day and Simon, too, was ahead of the times. That is no longer the case.

Instructional Psychology

In Glaser and Resnick's (1972) review of research in instructional psychology, they found only three studies attempting to train children on formal operations type tasks. Although there are more now, most studies still deal with concrete stage tasks as Resnick noted in 1976. The same general methodology to be discussed is used for both kinds of tasks although it is more difficult to apply to formal type tasks if for no other reason than their relative complexity. The general methodological approach first seeks to ascertain detailed task analyses or process descriptions of what the subject knows and does. Process descriptions, as opposed to structural descriptions such as Piaget's, are used since the subject's knowledge is taken to be knowledge-in-action. Although this was always Piaget's goal, his choice of symbolic logic as a descriptive language prevented him from generating theoretical descriptions of processes unfolding in real time (symbolic logic is atemporal).

After hypothesizing a set of process rules characterizing ever-increasing levels of performance, the second methodological step "is to create problem sets that yield sharply differing patterns of correct answers and errors depending on what rule is being used" (Siegler, 1978b). Having validated a task analysis, i.e., the hypothesized rule progressions in steps one and two (and recycling the steps if need be), the third general step is to measure the subject's responsiveness to potential learning experiences. This responsiveness will depend in principle on the current level of the subject's knowledge, as Piaget has long held, but also on the subject's memorial abilities, particularly his abilities to encode, organize, and store information during the course of the experiment. The label "short-term memory" captures the spirit of these kinds of abilities although its precise meaning varies among authors and need not concern us at the present moment. By systematically varying the components of the learning experience, it is possible to discover much about the detailed nature of learning processes. Having made this apparently gratuitous remark, I must point out that workers in the Piagetian tradition have, until very recently, eschewed precise systematic variation of experimental factors, partly for philosophical reasons, partly in deference to Piaget's personal disdain for this type of research, at least as far as his own research program is concerned.

Thus, the most promising lines of current psychological research in cognitive development utilize some form of a task analysis followed by some kind of instructional procedure. The goal of this research is to discover what knowledge is and how it changes. Knowledge is considered to be that which is revealed in action; the action of performing tasks (cognitive tasks, classroom tasks). However, a theory of cognitive development is not the same as a theory of instruction. An instructional theory must go beyond cognitive developmental theory just as an engineering discipline must go beyond its allied scientific discipline. Thus, developmental research cannot be simply carried over to educational research. There is a definite difference between the goals of the two domains. Fortunately, that difference has never been smaller than it is today. Increasingly, psychologists are investigating the development of performance in more or less real-world task environments.

The beginnings of this research are traced in Glaser and Resnick's review (1972). The complexity of real-world tasks has required the development of new task analysis methods. The computer provides a ready-made illustration of how this might be done. Before engaging in instructional research, it is very helpful "to define clearly what it is that an expert in a subject matter domain has learned." A developmental approach attempts to define the knowledge of the novice as well. Task analysis, in Glaser and Resnick's terms, "is characterized by the description of tasks in terms of the demands they place on such basic psychological processes as attention, perception, and linguistic processing." These analyses reflect "the processes available at different stages of learning or development."

Each of the above named psychological processes (and there are more, e.g., memory) define a domain of psychological research. However, the level of analysis will determine how deeply one must delve into these domains. For the purposes of education research, as will be demonstrated, one need not go very far in order to obtain rewarding results. Nevertheless, problems of instructional design may arise which can only be adequately resolved by returning to a more basic discipline. For the present, such problematic cases would seem to be exceptional in the sense that there appears much that could be done by education researchers without requiring extensive competence in psychology. (Perhaps I am being too optimistic. It would help to have a psychological consultant handy.)

One common feature of task analyses is their hierarchical nature. Task hierarchies of a certain kind have been described and used for instructional purposes by Gagne (1970) and others. Gagne's analyses were apsychological in a sense, certainly adevelopmental in that they referred to a kind of expert performance. For example, any computational technique must involve numbers or presuppose them. This has nothing to do with psychology. On the other hand, this has an obvious implication for instructional psychology and education: numbers must be understood (in some sense) before computation can be taught. Gagne's hierarchies conform to these kinds of constraints.

Developmental task analyses, on the other hand, are empirically derived since they essentially describe the task from the learner's point of view. Empirical hierarchies must be compatible with a priori ones, but they can also be different. Brainerd has taken Piagetians to task over the conservation hierarchy (e.g., substance, weight, volume) because they have, in his opinion, failed to differentiate between these two kinds of hierarchies, the a priori and the a posteriori (Brainerd and Siegler, 1978; Brainerd, 1978a).

It may be objected at this point that Gagne's hierarchies are not as characterized here, that they are indeed the results of empirical research rather than a priori analysis. After all, these analyses must be and have been validated empirically before being used for instructional purposes. This is quite true. However, the original analyses are not empirical. They do not attempt "to describe internal processing" (Resnick, 1976). Unlike the work to be described below, the Gagne-type hierarchies do not arise as psychological descriptions of performances on a variety of tasks. Psychological performance models are "left entirely implicit in Gagne's work" (Resnick, 1976). It is for this reason that they need validation.

Typically, the analyses resemble a logical sequencing of subject matter from the viewpoint of the expert. The subject matter is broken down into components whose meaning is clear, i.e., which form a coherent whole only after mastering the hierarchy, after reaching its pinnacle. The empirical developmental hierarchies however, are formed from components whose meaning is already clear, i.e., which form a coherent whole from the point of view of the learner at that level of the hierarchy. Learning a Gagne-hierarchy is like watching a mosaic being assembled; the picture becomes clear only near the end. The empirical hierarchy is more like a series of pictures, each successive one a closer approximation to the last one. At least, the hierarchies to be reviewed fall into this category.

The Early Research

The early task analytic studies dealt with simple verbal reasoning tasks (e.g., Clark, 1969) such as the two-term and the three-term seriation tasks. Piaget was perhaps the first to ask children questions such as, "If Catherine is darker than Angelica, and Catherine is fairer than Odette, then who is fairest?" This question asks for an inference based on two propositions about a three-term series

(Angelica, Catherine, Odette). By varying the linguistic form of the propositions and their order, as well as by varying the semantic nature of the relational term (fairer than, taller than, etc.), it has been found that performance is affected by both the linguistic structure of the problem as well as its semantic content and that there is developmental change with respect to both dimensions (Trabasso, Riley and Wilson, 1975). By way of contrast, Piaget considered only the logical format of the problem. For him, all problems of this type are adequately described by the logic of transitive relations (if $A > B$ and $B > C$, then $A > C$ where '>' is any asymmetrical transitive relation). The presentation and solution of these tasks at the verbal level, i.e., without objects or pictures as possible aids, was supposed to be a relatively mature accomplishment (Piaget, 1974, Chap. 15). However, children as young as five years can solve some kinds of purely verbal 3-term tasks. The reader can easily verify this using 'taller than' in place of '>'. I found most six-year-olds (and above average five-year-olds) could handle this task but not an isomorphic task using 'heavier than' for '>'.

School tasks were also investigated. Algebra word problems received particular attention from Simon and his associates (Piaget and Simon, 1966). A general and important finding was that the problem format could greatly influence the way a solution was attempted. Another finding was that auxiliary information, not in the explicit problem statement, also played an important if not essential role, and that whether or not such information was used depended on who was doing the problem. Some problem-solvers consistently went beyond the explicitly given information while others behaved as if this information, available to them, were irrelevant. Other individual differences emerged with respect to mental imagery and verbal proficiency in representing the problem. Snow (1977) has reviewed the work on such individual differences. Regarding the possibility of improving, say, the ability to form mental images, given verbal information, Snow is not encouraging.

Even if we cannot substantially change abilities to form mental images or more abstract representations of information useful for problem-solving, we may succeed in cueing novices with these capabilities to the potential benefits of using various representations. Rohwer (Rohwer, 1970; Rohwer and Dempster, 1977), for instance, improved the performance level of six-year-olds to about the level of 12-year olds in a word recall task by simply asking them to think of a picture in which each word was represented (the words referred to objects or perceptual features of objects). Similarly, performance could be improved by asking subjects to think of a sentence that uses all the words. Clearly, Rohwer's subjects possessed useful skills, but were not aware of this usefulness. Note that Rohwer did not teach how to make a mental image or how to make up a sentence. Rohwer calls these mnemonic strategies "elaboration strategies." They are particularly prevalent starting in adolescence. Glaser and Resnick discuss other aptitude training studies.

Other school tasks as well as Piagetian tasks have been investigated by Resnick and her associates (Resnick, 1976). The focus here is on early development, e.g., the development of the number concept. These studies are quite useful even for researchers interested in secondary school subjects because the methods of contemporary task analysis research are clearly illustrated. In particular, Resnick gives a "selective history" of task analysis which should be useful to education researchers who have only a limited acquaintance with only one or another psychological school of research.

One of the general findings of Resnick and her associates was that once a task hierarchy had been determined, positive transfer would most likely be obtained when the tasks were taught in hierarchical sequence from simplest to most difficult, rather than in reverse order. Another interesting finding was that a minority of subjects could "skip" some elements of the sequence. This suggests a possible important difference between this kind of developmental sequence and Piagetian sequences. There can be no skipping of steps in the latter. Resnick's interpretation of the data allows for a compatible interpretation: "What these children apparently did was to acquire the prerequisites in the course of learning the more complex tasks" (1976, p. 68). This, in turn, raises an interesting question.

Can we "match instructional strategies to individuals' relative ability to learn on their own...?" Resnick felt that there was insufficient research at that time on how learning occurs with minimal instruction. The situation has not changed. (Most education researchers appear to be more concerned with understanding how learning occurs than with "maximal" instruction. Considering the results of national assessment tests and Piagetian tests of college students' abilities, some are concerned with whether learning or development occur at all.)

Rational vs. Empirical Task Analysis

Resnick's work illustrates the distinction between two approaches to task analysis which she calls rational and empirical. "Rational analyses are descriptions of 'idealized' performances" (p. 64). These are performances that succeed, but may not model successful human performance. Empirical analyses result in descriptions or models of human performance. Clearly a rational task analysis can be the starting point for a research effort aimed at deriving empirical analyses for the same tasks. By the same token, rational analyses are informed by one's intuition, observation of others, instrospection, i.e., by what one already knows of how people perform on the tasks in question, on similar tasks, and on tasks in general. The more we know about performance, the more fuzzy the distinction between the two types of analysis. Although Resnick writes, "Typically a rational task analysis is derived from the structure of the subject matter and makes few explicit assumptions about the limitations of human memory capacity" (p. 65), this statement must be understood in an historical perspective. It was made at a time when few were willing or able to make such explicit assumptions. A number of studies, to be described, will indicate how the situation has changed. Briefly, the more one knows of performance patterns, memory limitations, and the like, the more one's rational task analyses are empirically constrained. No one would propose a task analysis that could not be understood by learners. The important point here is that Resnick was motivated to make empirical analyses because of inadequacies in the early rational analyses.

The rational analyses led to a verifiable sequence of task complexity which was useful for instructional purposes, but certain questions were left unanswered, e.g., how was performance affected by memory load? Also, hierarchy data cannot always tell us why certain tasks scale in difficulty as they do. Logical analysis can provide some answers, but tasks that are equivalent with respect to a logical analysis may still be unequivalent with respect to difficulty. Piaget's conservation tasks provide a famous (infamous?) example.

These are the kinds of questions which may or may not be of interest to an education researcher; after all, there were some educational benefits derived from Resnick's analyses. Depending on the goals of research, such benefits might be considered sufficient.

On the other hand, one might need more detailed theory and data. For example, Resnick's rational analyses identified "general information-processing abilities, such as perceptual processing, ...memory, ... that are called on in performing a specific complex task" (p. 98) (emphasis added). Emphasis is placed on the general nature of the abilities for two reasons: (1) this illustrates a remark made earlier to the effect that one can obtain educational benefits without having to analyze psychological processes in detail, and (2) by not specifying any detail, other performance models might have led to the same hierarchy. If one is interested in more than a specific task, i.e., if one is after a domain of tasks such as the formal operations tasks, or if one is after across-task parameters, then some details are needed, e.g., information concerning memory capacity and memory load. The work of Siegler and of Case, described below, will show how much detail appears to be needed. The rewards of using such details are, in my estimation, great. But first, some examples from Resnick.

Use of Task Analysis

Woods, Resnick, and Groen (1975) studied simple subtraction processes (e.g., 5 - 4 = ?) used by second- and fourth-graders. The problems were simple enough to be done by all the subjects so the data of interest were response latencies. The pattern of latencies was used to evaluate a set of five performance models. Most subjects appeared to use one of two models. A minority of second graders used a decrementing model, while most second graders and all fourth graders appeared to use a choice model. The decrementing model for $m - n = ?$ sets a counter to m, decreases it n times, then reads the counter. For this model, response latencies are directly proportional to n and subjects assigned to this model produced this kind of data (except for problems of the type m - m = ?). As every adult knows, this model is not always the best. Apparently, most children know this too. The choice model, used by most children as early as the end of the second grade, chooses between alternatives. Depending on which has fewer steps, either use the decrementing model or count up, i.e., set a counter to n and then read off the number of increments needed to count up to m. For the choice model, latencies should (and did) vary linearly with the smaller of n and m - n. Three observations make these results intriguing.

First, neither performance model corresponds to the subtraction algorithm usually taught. Second, the performance models, suggest a spontaneous and surely naive attempt to reduce the load on short-term memory. Third, the performance models do not display the structure of the subject matter as clearly as the original algorithm.

Typically, pupils are taught subtraction as the creation of a set and then the removal of a subset from the first set. The decrementing model requires fewer steps than this and the choice model, by construction, requires still fewer steps overall. Although "the decrementing model is in fact derivable from the algorithm we assume is typically taught" by simplifying the latter, the choice model "cannot be derived from the teaching algorithm in so direct a way" (Resnick, 1976). An "invention," to use Resnick's term, must be made. In a subsequent study, Resnick and Groen confirmed that what children do differs from what they are taught once children have practiced using their new knowledge on a variety of tasks.

The principal result is that "what we teach children and how they perform a relatively short time after instruction are not identical —but neither are they unrelated"(Resnick, 1976, p. 68). The effect of teaching leads indirectly to skilled performance. Resnick's data "suggest that children seek simplifying procedures that lead them to construct, or 'invent,' more efficient routines that might be quite difficult to teach directly" (Resnick, 1976, p. 68). The "taught" routines, in Resnick's studies, were derived from the subject matter. After some practice, the subjects were performing other routines requiring fewer steps than the taught ones, but requiring more decision points. In other words, subjects spontaneously constructed new rules to adapt the given algorithm to different problems.

The algorithm would have worked too, that is the nature of algorithms, but for the sake of performing fewer steps, modified algorithms were constructed. Piaget's notion of self-regulation or reequilibration is suggested, but for non-Piagetian reasons. During practice, subjects apparently learned that some problems could be done more easily. Resnick felt that it would have been more difficult to teach the set of several modified algorithms than the one original algorithm (presumably in the same time, under the same conditions, etc.). Thus, the two kinds of analysis, rational and empirical, can both be useful and can complement each other.

Skilled performance need not coincide with the rational task analysis version of performance. There are cases where these differ, e.g., in solving equations of proportions. A rational task analysis could come up with, say, the cross-multiplication algorithm. Every reader no doubt knows this method and no doubt every reader uses other simpler methods, depending on the numbers appearing in the equation. When the equation is 2:5 = 4:x, the novice might use cross-multiplication to obtain 2x = 20, etc., but the skilled subject will note while scanning the equation that the right side numerator is twice the left side numerator, hence x is twice 5. Going beyond the algorithm when convenient indicates some measure of insight and this differentiates the (more or less) skilled performer from the novice. How does the novice become skilled?

Resnick's observations suggest that if novices are given the opportunity to practice using a newly learned algorithm with a variety of tasks, then very quickly novices begin turning into skillful performers on their own. How quickly and to what extent this occurs will depend not only on the novice's abilities, but also on the specific ways in which algorithms could be modified or even replaced for a given kind of task and algorithm.

Algorithms vs. Skilled Performance

Resnick raises the question of whether it might be preferable to teach the rules of skilled performance rather than the algorithm. After all, skilled performance is the goal and subjects will spontaneously move in that direction, so why not save time by directly teaching it? Her answer is an intriguing no. She suggests that it is preferable to provide "instruction in routines that put learners in a good position to discover or invent efficient strategies for themselves" (1976, p. 72). A potential problem with teaching skilled performance from the start is that being more flexible or less stereotyped, it takes longer to describe and teach. An algorithm treats all tasks as equals. Instead, it partitions the task domain into subsets of tasks and only treats members of a subset in the same way. To teach a skilled performance algorithm, it would thus be necessary to specify the defining characteristics of task subsets and the solution strategy appropriate to each subset. In addition, the original algorithm would also have to be taught: it would be used when a given task does not fall into any of the subsets for which alternate strategies are easier to use. As Resnick puts it, skilled performance is "often so elliptical as to obscure rather than reveal the basic structure of the task" (p. 74).

In my own research, I have preferred to let learners first develop some aspects of skilled performance, and then unify these aspects in a way that reveals the basic task structure. Because of its memory and verbal demands, this approach might not be suitable for very young subjects. The length of a didactic exposition of all the facets of skilled performance would be much greater than the length of an exposition of the original algorithm. For young learners in the early primary grades, shorter presentations are preferable because young pupils have shorter attention spans, poorer strategies, poorer reading ability, and no note taking ability. For older learners at, say, the intermediate levels (ages 12 to 15) there might still be serious problems keeping track during a classroom lecture although written text materials would allow going over the exposition as often as is necessary.

In addition to (a) teaching an algorithm derived from a rational task analysis, and (b) teaching skilled performance as determined by an empirical task analysis, there is the option of (c) teaching only a part of skilled performance, providing practice, then teaching another part, and so on. This last option can serve several purposes, e.g., it can be used to arrange matters so that learning follows an historical sequence, interesting in itself. The focus can then shift to the historical development of some skilled performance rather than (or along with) the endstate of that development, namely the skilled performance itself. This third approach is very neo-Piagetian and is discussed below.

When it is preferable to use a rational task analysis, three criteria are specified by Resnick for choosing a teaching routine:

(1) It must adequately display the underlying structure of the subject matter.

(2) It must be easy to demonstrate or teach.

(3) It must be capable of transformation into an efficient performance routine (Resnick, 1976, p. 74).

Resnick provides some examples and also some notable nonexamples of teaching routines meeting (or failing to meet) the three criteria: both kinds should be studied in order to understand how to design and evaluate this type of teaching routine.

Most science educators attracted to developmental psychology are also drawn to some kind of active, more or less guided discovery method. Is Resnick's approach compatible with the discovery philosophy? She thinks so: "the traditional line between algorithmic and inventive teaching disappears" according to her observations. "We are not faced so much with a choice between teaching by rules that will enhance the probability of discovery—rules that somehow invite simplification or combination with other rules" (p. 76). She goes on to suggest "that differences in learning ability—often expressed as intelligence or aptitude—may in fact be differences in the amount of support individuals require in making the simplifying and organizing inventions that produce skilled performance" (p. 78).

Accepting Resnick's arguments, does it still make sense to carry forward the current debate between Piagetians and Ausubelians? One issue is discovery vs. receptive learning. According to Resnick, the question is not either one or the other, but the relative proportions of both. You can teach some of the rules and guide learners to the discovery of others. Is it necessary to teach all of the rules? Or none? There appear to be no good arguments for either extreme. On practical grounds alone, neither method would be very effective. It seems to me that whether one takes sides with discovery or receptive learning, the goal is intellectually active and insightful learning. It is difficult, if not impossible, to differentiate between Piagetian and Ausubelian educators on the basis of their avowed goals, that is, their descriptions of students who have learned.

A second issue in this debate is generalized learning vs. content specific learning. Piagetians stress the former as well as the latter while Ausubelians only stress the latter. General abilities are suggested by Piaget's description of the formal operation stage. Piagetians dream of ushering students into this stage because so much goes with it. Recall Piaget's words: "the subject has become capable of solving all similar problems" having solved one formal task (Inhelder and Piaget, 1958, p. 267). Ausubelians point out that there is little or no evidence for placing faith in such broad all-encompassing abilities. And they are right. This is particularly so if we look for experimental evidence that any such abilities have been taught: transfer still eludes us (and students).

Task analysis bears on this issue too. Although work in this area is particularly scant, the technique of empirical task analysis has consistently suggested that both general and specific aspects of abilities and tasks need to be considered. In this sense, the Piagetians are correct. However, the general aspects do not recur the same way across task domains. This variability obscures the generality of these aspects which is revealed only at relatively deep theoretical levels. At the level of performance, task specific aspects (specific knowledge, specific ways of organizing general abilities) will usually dominate, particularly in semantically rich domains such as physics and chemistry. In this sense, Ausubelians such as Novak (1977a) appear to be correct. Once again, extreme positions in the debate seem untenable.

The Emergence of Skilled Performance

Skilled performance is analogous to using short cuts. When someone asks for directions, you are often faced with the choice between describing a simple, clear, easy-to-follow route and a much shorter route with ins and outs reflecting familiarity with the environs. If a stranger is asking for directions, you would probably say, "There's a real short way to get there, but to be on the safe side you'd better follow the main roads, etc." Piaget argued from the start that cognitive development proceeds by constructing both the main roads and the short cuts. Although individuals can learn these through direct instruction, they do in fact invent and discover much on their own. Others besides Resnick and her colleagues have observed this spontaneous constructive activity under experimental conditions (Wallace, 1972; Paris and Lindauer, 1977; Shaw and Wilson, 1976).

Shaw and Wilson (1976) report a study in which learners spontaneously constructed a rule for generating a class of items, each item being a certain spatial configuration of elements. The learners were not told to construct a rule, they were simply shown a subset of the class of items. They were not even told that they would be tested for recognition. However, during the recognition phase of the experiment, learners could not distinguish between items they had seen and items not seen but belonging to the same generative class. They could correctly identify new items as such only when these did not fit the generative rule. A control group was presented similar items which could not be assimilated to any generative rule, hence no rule was constructed. The control group was able to recognize which items it had been shown. Shaw and Wilson conclude that "the abstraction of the systematic relations between instances of the system appear to be automatic in the sense that it was not intentional" (p. 210). Self-regulation again? It is likely that the development of skilled performance by Resnick's subjects was also automatic. These are just a few of many examples of self-regulatory activity (to use Resnick's term as well as Piaget's) described by contemporary psychology. What is the motor of this activity?

One hypothesis of particular educational promise is that subjects spontaneously seek to minimize strain on their limited short-term memory capacity. This would account for the shift from algorithms to skilled performance, as mentioned above. It would also account for spontaneous rule generation since remembering a generative rule is usually easier than remembering all the generated instances. This hypothesis would also provide a more precise account of Piaget's equilibration model. According to this model, the development of new mental structures is instigated by inadequacies of the old structures. As subjects become increasingly aware of exceptions to their rules, spontaneous activity works toward the elimination of incoherence by the construction of new rules, new structures which admit of fewer inconsistencies or short-comings than the old rules. In addition to reducing errors, the new rules probably have the advantage of simplifying subjects' ways of conceptualizing certain tasks. As the number of rule exceptions increases, it becomes increasingly onerous to keep in mind the old rule plus the exceptions. A new rule with fewer or no exceptions would be a more economical way to store information relevant to a task. I am not aware, however, of any experimental attempt to determine how subjects would respond to a choice between two rules, each with drawbacks, but one with clearly fewer exceptions.

The role of limitations on short-term memory is not made explicit by Resnick. However, her second requirement that it must be easy to teach a routine derived from a rational task analysis may implicitly take into account memory limitations. If a routine were very taxing on short-term memory, it would surely be difficult to teach. The trick is to come up with rational task analyses which always lead to teachable routines. If that were an easy trick to perform, there would be little need for science education research.

A strong objection can be raised at this point to the suggestion that rational task analyses are or can be very useful. These analyses result in algorithms, stereotyped routines for solving well-defined tasks. Is this any different from what has been tried in the past and has failed? We tell students how to solve certain problems and then either they do not follow what we say or, having followed, they can go no further on their own. At this point, a defense may be tentatively made. If students lose track of a routine, it may be because of short-term memory limitations, in which case the problem could be solved by respecting memory limitations in the design of the routine. If students do not lose track, but fail to apply the routine to novel tasks, then other problems might be involved. Some of the work aimed at elucidating the nature of the second difficulty, applying a routine to new situations will be described. This description will be brief because the magnitude of the problem has limited the amount of research.

First, however, a detailed presentation of some research studies aimed at teaching rules for dealing with some of Inhelder and Piaget's formal operations tasks will be presented. Then the methods of empirical and rational task analysis will be illustrated for the balance beam task while the results of an empirical analysis will be discussed for a ratio task. The role of short-term memory limitations will be discussed in relation to spontaneous performance and responsiveness to instruction. This work appears to be the most promising and is perhaps the most accessible (to science educators) of current developmental research.

Instructing Formal Stage Performance

In a series of experiments, Siegler and his colleagues tried to teach preadolescents how to solve tasks similar to Inhelder and Piaget's formal reasoning tasks. The authors were interested in more than testing the Piagetian claim that such instruction would be "useless." Their methodology allowed an evaluation of the several components of the instructional procedure. Even if the overall procedure did not succeed, this approach could provide valuable information (providing the procedure produced some effect).

In the first of these studies, Siegler, Liebert, and Liebert (1973) taught 10- and 11-year old children an elimination-of-variables procedure as indicated by their success in applying it to Inhelder and Piaget's pendulum problem. Their tutorial method involved three components: conceptual framework (C), analogue problems (A), and measurement tools (M), hence the acronym CAM. The components were used all together (CAM) or in smaller combinations (CM, AM, and M). All but the M version were judged successful.

The conceptual framework involved the ideas of "dimension" and "level." Subjects were told that dimensions "can be thought of as a way in which different things can be measured like you can measure how long a line is with a ruler. There, length is the dimension.

Or when someone steps on a scale to see how much he weighs, then weight is the dimension..." Levels of dimensions were introduced similarly. Then subjects were given a rule for separating variables: "If one level of the dimension is always higher on the measure than the other level, then that is the important dimension."

The two analogue problems involving a measuring instrument were both similar to the pendulum problem (Piagetians would say, "structurally isomorphic" or something like that). In one, there were four balls, two heavy and two light, two orange and two white. Levels were factorially combined, i.e., one heavy orange ball, one heavy white ball, etc. Subjects were asked to name dimensions and levels and were told the correct answers if they could not do so. Each ball was weighed on a balance scale (the measuring instrument) to see whether it would tip over the balance (the "dependent" variable was the final state of the balance, either tipped over completely or not). Subjects were asked to decide which dimension was effective. They were cued to make use of the conceptual framework if they wished. Errors were corrected. Finally, the experimenter demonstrated how the rule could be used. This provided a model for skillful use.

After the second analogue problem, subjects were given the pendulum problem. The measuring instrument was a stopwatch. Subjects watched and listened to a demonstration of its use and then practiced on their own. As before, subjects were cued in that they were told that this problem could be solved the way the others were. A control group was simply given the pendulum problem.

There were 12 subjects in the treatment and control groups. While 8 of 12 CAM subjects succeeded in isolating the pendulum's length, only 1 of 12 control subjects did. The performance of the control group was preformal as expected from Inhelder and Piaget's observations. However, the subjects were old enough to be considered close to the age range where a small but real proportion of individuals solve the pendulum task without tutoring.

Alternate combinations of CAM components were tested, namely CM and AM. Since the use of some measuring device was necessary to the task, the CA combination was ruled out. The CM and AM methods were used with samples of six subjects. All the methods gave statistically equivalent results which were superior to the control group's performance. Next, the authors tested the effectiveness of instruction in measurement procedures alone (M). Again a small sample was used (N = 10). Four of ten subjects were successful. Compared with the performance of the control group, this difference failed to reach significance ($.10 < p < .15$). Finally, the control group was trained with the full CAM procedure just to check whether they could also benefit from this instruction. They could and performed as well (7 of 11) as the original CAM group (8 of 12).

This study was characterized by a methodological precision not found in most training studies. Regardless of these authors' results, their analytical approach provides a model worth emulation. Most training research confounds many factors. If authors are aware of this, they sometimes justify the approach on the grounds of ecological validity. Or they point to the exploratory nature of the research. Nevertheless, there is a genuine need for the more analytic approach since it is the best way to test the relative usefulness of competing educational philosophies. When many factors are confounded, successful training outcomes are compatible with competing theoretical positions. Since each positions are used to launch major curriculum efforts, clearly we need information to make informed comparisons and evaluations.

Before proceeding to Siegler's other studies, one particular comment should be made about the CAM study (general comments will follow). It concerns the eliciting of formal operations, as opposed to training. The M-treatment just failed to reach the IQ significance level for a sample size of only ten. It is quite possible that the M component was indeed successful, just less so than the other treatments. The M-treatment can be considered minimal. It is not even training in the sense that C and A are. It is plausible that such minimal treatment succeed with subject matter regarded as theoretically recalcitrant to instruction? Danner and Day (1977) reported similarly suggestive evidence on the eliciting of formal operations by indirect methods.

In a second study, Siegler and Liebert (1975) conducted a training study for the design of a factorial experiment. The task was constructed to bear a close formal resemblance to Inhelder and Piaget's chemicals problem (1958, Chap. 7). This time two "developmentally distinct" age groups were used, 10-year-olds and 13-year-olds. Based on observations such as Piaget's, a three-year difference was considered about the minimum needed to detect developmental differences. The two age levels bracket the age (11 years) which Piaget has always cited as the beginning of the elaboration of the formal operations stage.

Large gains in spontaneous performance during the period from 10 to 13 years had been reported by some authors. Siegler and Liebert point out, however, that some surveys reported substantial gains and others did not, but the survey methods did not permit a clear characterization of the differences between 10- and 13-year-olds. Once again, the need for analytical studies is demonstrated.

The treatment lasted less than half an hour, as in the previous study. Using a CA treatment, both 10- and 13-year-olds benefited (N = 10 for each age). The C treatment also benefited the 13-year-olds, but less than the CA treatment. However, the C treatment did not benefit the 10-year-olds. Regardless of treatment, 13-year-olds were superior to 10-year-olds. The performance of the control group was very low, with 13-year-olds performing only slightly better than 10-year-olds. The authors could find no particular reason why their control group revealed no dramatic growth over this three-year span while others had observed growth.

The principal finding of this study was that although 10-year-olds can acquire a formal operations skill after brief instruction, "the instructional conditions under which they do so may be more circumscribed than those from which older children can benefit." The authors discussed some interpretations of the differential effectiveness of the C treatment. They suggest that 10-year-olds, as a group, lacked the foresight of the older group and for this reason were unable to generate as many unique factorial combinations.

The subjects were told that keeping track of their work by recording each combination might be of help. Both CA and C groups were instructed on the construction of tree diagrams which could be useful for systematically generating all combinations. A relatively strong correlation was noted between use of written records and performance. The data revealed that the younger children failed to keep records in the C treatment. This may be why they did not perform well. The 10-year-olds in the CA treatment did keep records and did perform well. The authors suggest that the difference was due to a failure to foresee the need for records.

The A component of the CA treatment illustrated the usefulness of record keeping. The C treatment had no such illustrations. Only the 13-year-olds foresaw the need for records, according to this interpretation. In the previous study, the CM treatment provided the whole experimental outline in advance so foresight could be cued by hindsight. In the combinatorial study, there were no such cues. As the authors note, Neimark and Lewis (1967) also singled out the foresight variable as a clue to the limitation of 9 to 11-year olds' information seeking strategies.

But why the difference between the two age groups regarding foresight? The present study cannot answer this question. However, this study did in fact help provide the question and therein lies the strength of this analytical approach to the relation between development and education.

In a third study, Siegler and Atlas (1976) studied the effects of teaching subjects how to detect interactive patterns in data. These patterns are more complex than those produced when confounded variables simply produce additive effects. Thus, the training faced a greater challenge than in the previous studies. Again, there were two age groups, 10- and 13-year-olds. Once more training lasted roughly half an hour. The training method included presentation of a long algorithm with closely guided practice in applying it with corrective feedback. The data consisted of performances on three untrained tasks. Older subjects outperformed younger ones, but both groups outperformed controls. Performance patterns indicated all-or-none use of the algorithm. To check on this, two other tasks were given, one solvable by extension of the algorithm and one apparently easier problem not solvable with the algorithm. The data confirmed that subjects who solved the previous problems were indeed using the algorithm and not some other unknown method. The problem which appeared easier, at least on intuitive grounds, proved to be the more difficult one as would be expected if subjects were using the algorithm.

This study did more than demonstrate that 10-year-old subjects of average ability can learn to detect interactive patterns in data. The study also demonstrated "some of the advantages of creating formal models of solution strategies and using them as a basis for instructional procedures." This level of analysis is virtually unheard of in the science education training literature. Although the success of tutorial methods does not automatically translate into successful classroom methods, the careful research by Siegler and his colleagues "suggests that acquisition of formal scientific reasoning may be far more dependent on specific instructional experiences and far less dependent on general maturation than hypothesized by Inhelder and Piaget." The question is not whether children can learn these skills, but how can we teach them and is it worth the effort?

If one were to play devil's advocate, some fairly obvious objections to studies such as Siegler's could be raised. These are the kinds which come rather automatically from Piagetian investigators. For one, the subjects were taught; they were not allowed to discover. True, but they did learn. Second, the subjects learned algorithms; they were just imitating a mechanical procedure. This is not true. The criterion of success was always transfer to an untrained problem (or problems). Third, there was no proof of lasting retention. True, but the goal was learning, not learning for all time. Lasting learning most probably depends on the length and breadth of practice. Data should most certainly be gathered regarding factors which might influence retention. However, given Piaget's talk about the uselessness of instruction, even short-term learning must be hailed as a significant training outcome. Fourth and last, there is no proof that these subjects were advanced to the stage of formal reasoning because we do not know whether they can solve formal tasks differing in type from the trained tasks. True, but there is no proof that such a developmental stage exists, i.e., there is no record of spontaneous acquisition of all or most of the formal operations skills. Some rare individuals can handle most of the formal tasks, but I would maintain that all such individuals have received instruction on some of the underlying concepts, e.g., algebraic proportions. Let us return now to Siegler's research.

In all the previous studies, there is no clear understanding of the developmental differences between older and younger subjects. Siegler et al. ask why it is the case that "illustrative tasks are crucial to the success of training" (Siegler and Atlas, 1976, p. 369). The authors suggest that in addition to formal models of what is learned in one procedure, answers to these questions "will require models of the learner's knowledge before entering the experimental situation and also models of alternative approaches to solving the problems" (Siegler and Atlas, 1976, p. 369).

Piagetians and Ausubelians have tried to give useful descriptions of individuals' knowledge as a function of developmental level. These descriptions signalled an advance over research programs which steadfastly rejected the goal of coherent descriptions of the rules underlying individuals' behavior. Nevertheless, these descriptions have been too general, too abstract, or too complex.

Recall the balance beam task of Inhelder and Piaget. The empirical data were not gathered in a manner sufficiently systematic to allow a useful coherent and detailed description. Worse, the theoretical model, derived independently of the data, was hopelessly illogical. Moreover, the theoretical goal, set prior to data collection, was implausible in the light of subsequent data. The problem was not that theory preceded data, but that the data were at best loosely related to the theory as well as being internally incoherent. In defense of Inhelder and Piaget, it should be pointed out that their work was exploratory. It is the endemic weakness of Piaget's research program that it is forever exploratory. Half a century of exploratory research might prove consistently stimulating under the guidance of a Piaget, but at some point someone somewhere must dig a little deeper. For an exemplary model of the kinds of descriptive data we need, consider again a now classic paper by Siegler (1976).

Task Analysis: A Developmental Approach

The purpose of this study was "to characterize and explain developmental differences in thinking," in particular three aspects: "specific knowledge governing task performance, responsiveness to experience, and basic processes that underlie differences in the other two areas." This approach was applied to the problem of children's performance on balance beam tasks. The study included three generically related experiments.

The first evaluated the adequacy of several models of task relevant knowledge. Experiment 2 measured different aged children's responsiveness to training or experience. The children were equated with respect to task relevant knowledge as determined by Experiment 1. The third experiment examined a particular hypothesis put forward to account for the developmental differences observed in the previous experiment.

The balance beam tasks required that the subject predict which (if either) side would go down when weights were arranged in some manner on the four pegs on each arm of the balance. The first experiment tested the validity of several rules (see Figure 1) characterizing increasingly adequate levels of performance. The rules were based on data, some of them Inhelder and Piaget's, and introspection ("How would I do this?"). The data provided the basis for describing immature performance while introspection by skillful subjects permitted a description of mature performance. Note that data, in particular Inhelder and Piaget's, were <u>not</u> used to characterize mature performance. In fact, data suggest that mature performance for this task is exceedingly rare.

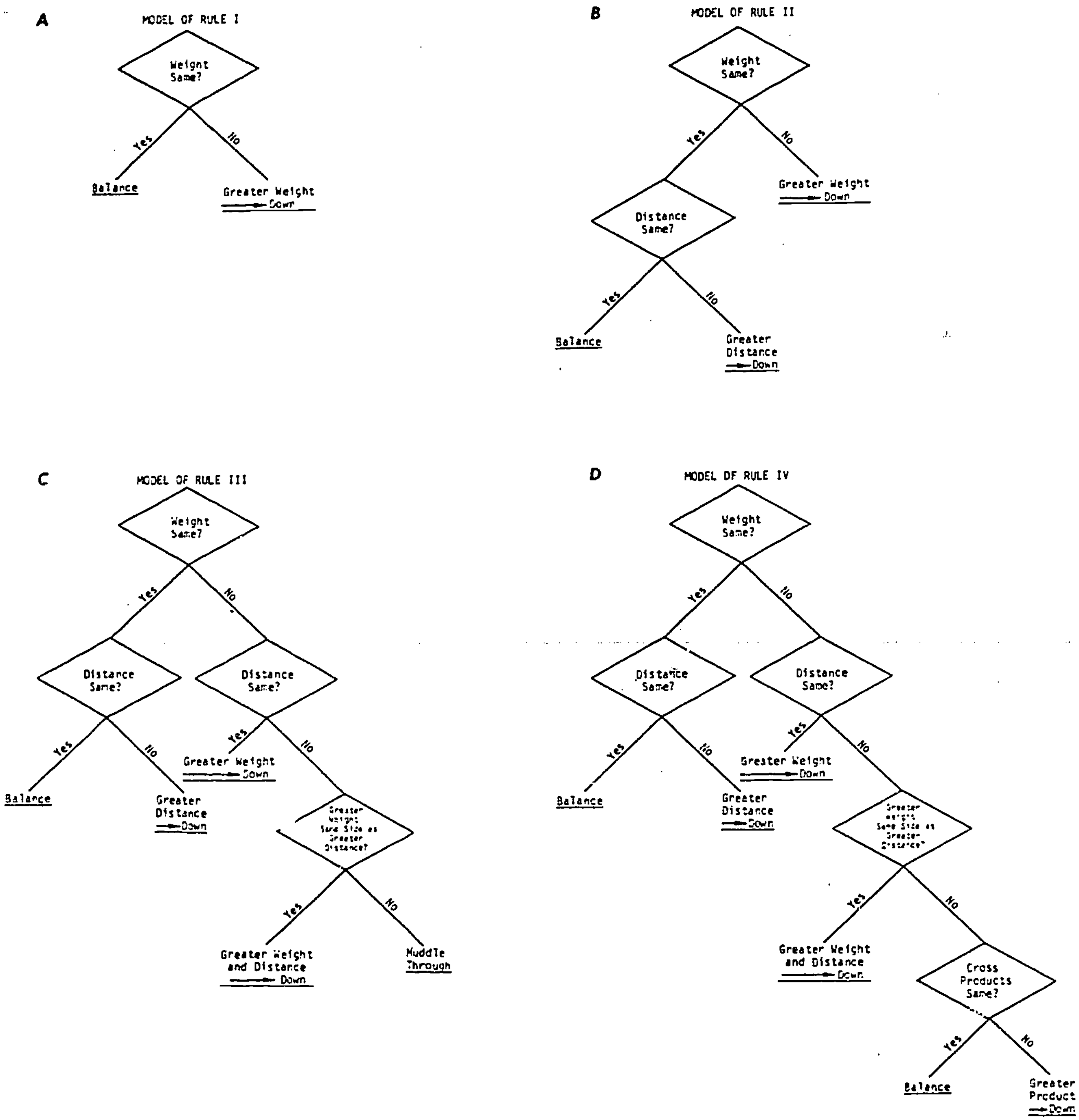

Figure 1. Decision Tree Model of Rule for Performing Balance Scale Task.

From R.S. Siegler. "Three Aspects of Cognitive Development." Cognitive Psychology, 8: 484-485, 1976.

The key to hypothesizing performance rules was a task analysis or flow chart of the processes and decisions apparently used by skilled subjects. A crucial point must be made here: skilled subjects do not necessarily always use the most sophisticated procedure available to them. For example, the cross-products algorithm for comparing two torques, though available, would not be used if there were two equal weights on the two balance arms. Instead, after noting the equal weights, the skilled subject would merely compare the lever arms. Similarly, the skilled subject would notice whether the lever arms were the same; if so, a simple comparison of weights would complete this sequence of unsophisticated computations. Only if weights and lever arms differed in opposing ways, would the skilled subjects use a sophisticated method. This solution strategy is not the most mathematically elegant nor does it lead to the most efficient computer program. Nevertheless, the strategy was considered to be psychologically the most plausible.

It turned out that except for the use of cross products or some other sophisticated method, Siegler's mature subjects performed in accordance with the rational task analysis. Like others before him, excepting perhaps Inhelder and Piaget, Siegler found few adolescents, 16-17-years-old (5 of 30) using sophisticated methods.

The performance models for less sophisticated performance not only conformed to previous data, they were derivable from the most sophisticated model. In all, four rules were posited. For logical reasons, the rules nested in sequence so that use of Rule IV (most advanced) could never precede use of Rule III which could never precede Rule II, etc.

For example, Rule II started by asking whether the weight distributions were the same. If not, Rule II predicted the side with the greater weight would tip the balance. So did Rule I. The two rules differed for the case of equal weights: Rule II compared lever arms when the weights were equal, but Rule I did not and simply predicted a balance. In this way, Rule I was less differentiated than Rule II, that is, Rule I considered fewer cases as different. In particular, Rule I always failed to differentiate the cases of equal and unequal lever arms.

Rule II could be improved upon as well: for unequal weights, it failed to differentiate the cases of equal and unequal lever arms. The advance of Rule III over Rule II was precisely this added differentiation. Rule III in turn failed to handle opposing differences in weights and lever arms, whereas Rule IV bettered Rule III in precisely this one situation.

Note that at even the least levels of sophistication, performance is described as rule-governed. Siegler points out that Inhelder and Piaget did not describe their pre-operational subjects as following any consistent rule. In general, this sequence of rules adheres to a much tighter organization than the performance descriptions characterized by Piaget's logical models. Siegler's rules also cover more cases. Both theoretically and empirically, Siegler's approach is more coherent.

The results strongly confirmed the task analysis approach: 117 children of 120 gave explanations which consistently fit one of the four rules. Moreover, the rule-system analysis clarified a complex pattern of results which included both performance increments and decrements with age for various problem types. In other words, simple classification of subjects was not the goal. This sets Siegler's study apart from most surveys of developmental levels in the education literature. Not only was a theoretical model found adequate to the task of classifying a broad range of subjects (5-17 years) and performances, the model was shown capable of quantitatively predicting a variety of performance patterns. Most researchers focus exclusively on performance increments with age, but Siegler correctly predicted a class of performance decrements as well. These resulted on certain tasks from younger subjects making relatively more correct predictions but for the wrong reasons.

The power of Siegler's approach resided in his use of task analyses of sufficiently fine-grained detail to allow unambiguous predictions of judgments and explanations for a rich variety of tasks. The research was not exploratory, rather it took as its point of departure the exploratory data of others, including Inhelder and Piaget. Furthermore, the tasks which were employed were designed to test the rule models, unlike Inhelder and Piaget's tasks.

There was no question of "validating" the tasks by comparing them with Inhelder and Piaget's tasks. Unless one is making definite comparative statements about whether two instruments are measuring the same thing and one has no a priori or prima facie grounds for asserting that they do, then the validation question is pointless. This calls to mind those researchers who succeed in teaching something, but then find that their results are "invalid" because certain correlations failed to obtain. When Smedslund (1965) taught conservation of wei[illegible] Genevans objected because the trained conservers were unable to handle some weight transitivity tasks, as well supposedly a concurrent ability of "natural" weight conservers. This "Catch-22" diverted attention away from what the subjects <u>could</u> do, as if that were not important.

The moral for education is that science education research should focus on ostensible and limited objectives of training and less on some generalized transfer of training <u>unless</u> the training was specifically designed to produce transfer. A sidelight of Siegler's Experiment 1 adds emphasis to this point.

Some of Siegler's subjects were allowed to engage in discovery-type activities. They were told to experiment with the weights to help "learn how the balance scale works." Other subjects, also told to look for rules, studied the experimenter's weight manipulations and the resultant dispositions of the balance. These two activities gave subjects no advantage on Rule IV problems over other subjects in a control condition. Was Rule IV so difficult to learn? Siegler briefly verified that it was not. Three ten-year-olds were quickly and successfully taught the method of computing torques.

But spontaneous formal stage adolescents know far more about the balance and other tasks than these 10-year-olds. As Siegler put it, "This finding suggests a need to carefully distinguish between the processes of learning and discovery" (p. 498). These processes need not produce the same results. Successful performance produced by learning need not be identical to successful performance arrived at "naturally." If the results of learning are more circumscribed than those of discovery, it may be because the antecedent experiences during learning are more circumscribed, more narrowly focused, more concentrated in time. But the results of learning are no less valid because of that. End of digression.

Learning as Dependent on Developmental Level

In Experiment 2, Siegler tested the hypothesis that developmental level could be used to predict responsiveness to experience. Not only Piaget, but cognitive developmentalists in general as well as learning theorists predict that knowledge closer to the subject's level would be easier to acquire. A more interesting prediction is that subject's responsiveness is a function of developmental level as opposed to age. In other words, equated for level, subjects of different ages will respond similarly to learning experiences. Siegler compared five and eight-year olds who, in Experiment 1, had not gotten beyond Rule I.

Subjects were presented with tasks in a prediction-outcome procedure. Tasks were at Rule II or Rule III levels, that is at one or two levels beyond the subjects. The first prediction was that subjects would learn more from the Rule II tasks than the Rule III tasks. The second prediction was that no age effect would emerge, that is, developmental level as defined by Experiment 1, would suffice to predict performance. This is a stronger version of the hypothesis confirmed by Wollman and Lawson (1978) because Siegler used subjects of very different age levels as opposed to subjects of about the same age. However, older subjects usually do outperform younger ones on tasks for which neither age group has been specifically prepared. Siegler noted this and, strictly speaking, made no prediction (other than the null hypothesis).

The most striking result indicated that five-year-olds "derived radically different lessons from experience" than did eight-year-olds. For one, the older children benefitted more: They progressed to Rules II and III whereas the younger children progressed to Rule II or not at all, usually the latter. For another, exposure to Rule III problems apparently had no coherent effect on five-year-olds. Either performance remained at Rule I or it was too confused to be categorized. On the other hand, Rule III problems led eight-year-olds to use Rule III. The simpler Rule II tasks led to use of Rule II. In a sense, eight-year-olds rose to the level of the challenge, learning the rule appropriate to the task at hand. Why did eight-year-olds benefit more than five-year-olds?

The traditional developmental answer, as Siegler points out, is "readiness." Piagetians would claim that five-year-olds lacked the appropriate structures. Siegler quickly adds that readiness "only labels the phenomenon, it does not explain it" (p. 504). At this juncture, developmentalists in education usually stop. Siegler moved on.

The Encoding Hypothesis

In search of a truly explanatory hypothesis, detailed protocol analyses were made for three five-year-old subjects. This led to the "encoding" hypothesis: "Five-year-olds are less able to acquire new information than eight-year-olds because their encoding of stimuli is less adequate" (pp. 504-505). Recognizing that, by itself, this post hoc hypothesis did little more than restate the data, albeit in a new way, Siegler devised a third experiment to see whether new and independent data could be predicted from the encoding hypothesis.

The methodological step of obtaining independent confirming evidence is absolutely essential if one is to proceed from a relatively descriptive level to a relatively explanatory one. Nevertheless, to the best of my knowledge, this is a step Piaget has never taken after more than half a century and more than a thousand studies. This may account for the inherent circularity of Piaget's stage constructs. Education researchers tend to ignore this circularity. Perhaps education researchers are unaware of the Genevan research program, and have assumed that the Genevan school uses the same methods as they.

It appears that most people in education read secondary sources rather than Piaget. Under the circumstances, this would be unwise. Too much rides on a commitment to a theoretical position. Careful reading of Piaget is an onerous chore, as Piaget would be the first to admit, but it is an undertaking well worth the considerable effort for anyone contemplating Piaget-based research.

Returning to the third aspect of cognitive development, namely the underlying processes that account for differential responsiveness to experience, in Experiment 3 Siegler found that the younger children did indeed fail to encode or encode as well information about the lever arms. Relatively poor performance persisted even when younger subjects were allowed more time to study the apparatus and were told what to encode. The possibility remained that the younger subjects did not know <u>how</u> to encode even though they knew what to encode and had ample time. Indeed this was the case. After being instructed on how to encode, not only did their pattern of encoding match that of the older subjects, they were now able to benefit from exposure to higher level problems. Virtually all the younger ones progressed at least as far as Rule II and some as far as Rule III although the older group did somewhat better. In what sense then were the younger children less "ready" than the older ones?

Several differences, from this and other research, are plausible candidates for characterizing different levels of responsiveness to experience: specific knowledge—not knowing what to look for; specific strategies—not knowing what to do with information; memory limitations—not knowing useful memorial strategies. In addition, younger children tire more easily and are more readily distracted. Missing from this list is any mention of global, stage-related, logical abilities. The theoretical focus is much sharper. Nevertheless, Siegler drew some general conclusions regarding cognitive development. These conclusions will be reproduced in some detail since they represent a significant advance over the Piagetian formulations which have loosely guided so much of education research.

A Neo-Piagetian Theory of Development

Siegler posits a three-step view of development. First, knowledge is at some particular point or developmental level such as Rule I above. The encoding of stimuli is limited by the constraints of that level as when Rule I subjects poorly encoded distance information which, in fact, was not relevant to Rule I. Second, new information is encoded, going beyond the immediate needs of a level as when, for some reason not made clear at this point, Rule I subjects begin to take note of distance relations. This expansion of the range of encoded dimensions need not immediately lead to new predictive knowledge such as a new rule. New rules presuppose new data, but not vice versa. However, once new data are encoded, a reorganization process is initiated. This is the third step: The subject's structures change to make use of the new encoding.

"As might be apparent, this formulation is closely related to the Piagetian construct of equilibration, with the present term encoding playing a similar role to the Piagetian term assimilation," to quote Siegler (p. 516). He goes on to point out that "there is one crucial difference between the two formulations; it is possible to independently measure encoding, whereas no means have been devised to measure assimilation" (p. 516). Nor can means be so devised because assimilation plays too global a role in Piaget's theory.

The assimilation-accommodation idea only provides a general framework within which one might formulate more specific hypotheses such as the encoding hypothesis. Piaget and, it appears, most Piagetians have preferred to remain at the more general level. Perhaps this is why Piaget's theory has so little predictive value. When it becomes possible to measure assimilation independently of knowledge level, then prediction becomes possible, according to Siegler. This is so because independent measurements avoid the circularity of Piaget's stage constructs.

Knowing subjects' knowledge level, the "rules of the game," and independently knowing subjects' encoding level should make it "possible to identify individuals who are in a state of disequilibrium on a particular concept and thereby to predict which individuals are and which individuals are not ready to benefit from experience" (p. 516, emphasis added). When encoding is in advance of rule use, the stage is set for producing disequilibrium and initiating the reequilibration process, the construction of new rules. Important long-standing questions remain, but some may be of interest primarily to psychologists rather than to education researchers.

Some Research Needs

Taking Siegler's three neo-Piagetian steps as a point of departure, the first problem for a developmental theory of education is to adequately describe the learner's current knowledge state. Labels such as concrete and formal tell us something, but they are inadequate. We need more detail, in particular with respect to performance on the specific tasks to be mastered. Rather than global descriptions of the individual, we need mini-theories of the kinds of rules most likely to be used in specific problem domains. Siegler's four rules for the balance tasks are an example in this sense, whereas Piaget's descriptions are not.

The next problem is getting from step 1 to step 2. This is actually the learner's first problem while our problem is, in part, to understand why. Why do children fail to attend to all relevant task variables? Why do they then begin paying attention to more aspects of a problem than are required by their current level of knowledge or repertoire of strategies? How can teachers help them get to step 2?

The last problem, in like fashion, is in getting from step 2 to step 3: What to do with the new information. Why are old rules modified or abandoned? How are new rules constructed? How can teachers be of help here?

Cognitive psychology cannot yet tell us with any degree of certainty why and how children <u>spontaneously</u> adopt new and more useful encoding habits. Education research, however, need not answer this question. Rather, the focus should be on finding ways to <u>teach</u> children to encode differently. Of course it might truly help to know which conditions initiate this process naturally. But speculation about the causes of untutored learning will not get us very far. Ultimately, we shall have to work with "experimentally manipulable explanatory factors, and factors that can be independently assessed," as Siegler remarks (1976, p. 517).

Once encoding does go beyond the constraints of a developmental level, disequilibrium presumably results and this entails a reequilibration process. Again, no one knows why this should be so. For example, no one knows why children (and adults) try to make sense out of new information rather than just record it. New information seems to usher in the awareness of a problem or what Dewey (1910) called an "unsettled" situation. Moreover, we do not know when or why the awareness of a problem should stimulate efforts to solve it. Nor do we know how problems are solved (at least, not very well).

These are fascinating questions for psychology, and although their answers would be useful for education, education need not necessarily raise them. Recall that Siegler simply taught five-year-olds how to encode distance information and they took it from there. Having encoded new relevant data, they generated new rules. It appears that educators can, to an extent, count on the workings of internal generative processes. The problem then becomes one of stimulating these processes.

One way of initiating generative processes is to improve encoding adequacy: As we have just seen, Siegler took a rather direct approach: he told children just what to do. Many people, particularly education developmentalists, object to this kind of direct approach on the intuitive grounds that it fosters passivity and parroting. This may or may not be the case. But other methods exist for inducing growth. We have seen that training on analogue problems succeeded in generating knowledge that was general enough to handle transfer tasks. It is left to research to discover and evaluate other methods. One neo-Piagetian method of exceptional promise has been used successfully and often by Robbie Case and his students. Although this method was developed independently of the work of Siegler, it is closely related to this work in spirit and can be presented, for logical reasons, as a more refined approach than Siegler's regarding encoding limitations and differential responsiveness to experience.

Accounting for Limited Memory Capacity

In a series of empirical studies and theoretical papers, Case (1974, 1975, 1978a, 1978b, 1978c) has developed an approach to instruction based in part on the pioneering work of Pascual-Leone (1969, 1976), a former student of Piaget. This neo-Piagetian instructional theory hinges upon two constructs in particular: limited attentional capacity and cognitive conflict.

Attentional capacity is always limited, even among brilliant adults (try to multiply two four-digit numbers in your head or imagine all possible chess configurations after several mid-game moves). The concept of a limited processing capacity was originated by information processing theorists and introduced to psychology by Miller's classic paper (1956; see also Broadbent, 1973; Pascual-Leone, 1970; Baddeley and Fitch, 1974).

Authors speak variously of attentional capacity, processing capacity, short-term memory, and working memory. These terms mean different things to different people, but they have one component of meaning in common: they all refer to limitations on the amount of information one can deal with at one time.

Information is viewed as being organized in relatively discrete "schemes." The number of such schemes active at any given moment is limited by intrinsic constraints on human information processing abilities. It is as if (a) only a relatively fixed amount of energy were available for activating schemes, and (b) a minimum or threshold amount were needed to activate any given scheme. This is Pascual-Leone's conjecture.

The schemes which are activated during problem-solving are determined by the available problem-solving strategy, as with Siegler's step 1. Modification of strategies is imitated by cognitive conflict. The transition to Siegler's step 2 begins with the learner's understanding that his present strategy <u>needs</u> modifying. In Case's system, the need arises from a cognitive conflict prediction of some phenomenon or arrives at two contradictory predictions. The conflict spurs the individual to consider the problem in new ways because the old way is now seen and felt to be inadequate. The educator's task is to facilitate production of the conflict.

Conflict produces heightened awareness (Kahneman, 1973), an important consideration when attentional limitations may be crucial. In fact, Case's method is designed to minimize attentional demands at every step in the instructional sequence. However, it is not clear why the learner <u>must</u> discover "on his own" the error of his ways, although Case feels that this is likely to be most productive, perhaps enhancing motivation. Siegler directly taught strategies without use of conflict. Direct and indirect methods might differ little if success is judged by performance on some limited number of tasks. However, Case's less direct approach might produce motivational effects such as enhanced self-esteem ("I did it without being told how!"). We will return to this issue later. For now, suffice it to say that the conflict technique has been used successfully. It should appeal to those such as myself who sympathize with the philosophy of discovery learning since one of the "discoveries" is the need to learn.

<u>An Example of Task Analysis: Accounting for Memory Capacity</u>

Case's technique relies on a careful task analysis, as does Siegler's. Siegler's analysis resulted in a flow chart description of the learners' rule systems and the mature rule system (to be taught). Case's analysis proceeds on a level of finer detail. Within the framework represented by a flow chart, Case's descriptions of rule systems entail a number of subroutines, each a linear sequence of mental activities such as making an observation, storing a datum, making a computation, etc. In addition, Case quantifies the attentional of each subroutine and compares this demand to the quantified working memory capacity of the learner. For instructional purposes, demand must not exceed capacity (1975).

A number of empirical studies has confirmed the possibility of carrying out this approach (Case, 1978a, 1978b, 1978c). Moreover, when classroom teachers plan instruction following Case's general developmental approach, or similar ones, it appears that they can "deal with the difficulties of individual learners must more quickly and effectively" (Case, 1978a). This is not the place for going into the detailed nature of the underlying psychological theory, but several applications of it will serve to illustrate some of the recurrent themes.

Case (1978b) analyzed children's strategies for Noelting's (1976, 1979) ratio tasks. In these tasks, th[illegible]ect must imagine that glasses of water and orange juice are [illegible] mixed to make something that will taste more or less li[illegible]ge juice, depending on the ratio of orange juice glasses [illegible]er glasses. The task is presented pictorially (see Figure 2): a large empty pitcher next to several glasses, some drawn filled with a clear liquid (water), others drawn filled-in (orange juice). In each task, there are pictures [illegible] empty pitchers, each next to several glasses of liquid. The [illegible] is to predict which pitcher (if either) will contain the mixture that tastes more like orange juice after the glasses of liquid are mixed.

Noelting carefully described the strategies used by children 3-10-years-old. There were four strategies that accounted for most subjects' responses, each subject using just one strategy. As with Siegler's balance beam rules, Noelting's orange juice strategies form an embedding sequence such that "each strategy represents a modified and more powerful version of the previous one." Again, as with Siegler's rules, even the most primitive strategy, that three to four and one-half year-olds, was coherent or rule governed as far as it went (not very far to be sure). Again, each strategy made an advance over the previous one by being more differentiated, i.e., by taking account of a single "new and relevant aspect of the task which was not dealt with previously." As Case (1974) put it, "In Piagetian terms, each successive strategy is both more differentiated and more equilibrated than the previous ones."

Noelting, in the Piagetian tradition, described the sequence of strategies in terms of logical structures as well in terms of processes. Case, redescribing and analyzing the processes in terms of information load on working memory, noted that each strategy required one more item of information than the previous one (see Table 1). For example, the first strategy judges each pitcher's set of glasses in isolation, looking only for the presence or absence of juice. If only one pitcher is to receive juice, this strategy works, otherwise it fails. In the second strategy, the amount of juice and not just its presence is noted, but no attention is paid to the water. This strategy works whenever it suffices to count and compare the number of glasses of juice for each pitcher. The third strategy notes the presence or absence of an excess of juice over water, but succeeds when only one pitcher has an excess. The fourth strategy goes a step further by comparing excesses (or deficits) if both sides have one.

The maximum number of items which must be held in working memory during execution of a strategy exhibits the following progression: strategy 1—one item—the presence or absence of juice; strategy 2—two items—the number of juice glasses for each pitcher; strategy 3—three items—the numbers of water and juice glasses for the pitcher considered second plus the relative quantity of juice for the other pitcher (in qualitative terms, relative quantity means simply more juice than water or less juice); strategy 4—four items—

Developmental Level	Age of Assession	Type of Item Passed	Global Description of Strategy
1	3–4		Isolated Centration
2	4–5		Unidimensional Comparison
3	7–8		Bidimensional Comparison
4	9–10		Bidimensional Comparison, with Quantification

Figure 2. Sequence of Strategies Observed on Noelting's Juice Problem

the nature and magnitude of the quantity in excess for the first pitcher, e.g., "juice: 2 extra," and the numbers of water and juice glasses for the second pitcher.

Upon reflection, the reader will probably find that strategy 4 seems implausible. For instance, a priori, strategy 4 might seem to require at least six items: the numbers of juice and water glasses and their difference for each pitcher (thus three items for each pitcher). However, the number of items of information is dependent on the strategy used to organize and process that information. Since many strategies can be hypothesized for doing the same job, in order to make some informed choice among alternative strategies, some ways must be found for introducing constraints on the descriptions of strategies. One constraint is to require that each strategy go only one step beyond the previous strategy (recall Siegler's four rules). Another way is to generate or use evidence from cases or tasks not previously considered for which definite and differing predictions can be made from alternative strategies. The data should reduce the number of alternatives. Using both methods, Case determined that the most plausible strategy 4 was the one shown in Table 1.

Observe in Table 1 that each step describes some process, e.g., step 1 involves the process of counting while step 3 involves comparing numbers. The level of analysis useful for this study treated these processes as wholes, as unanalyzed subroutines. A finer grained analysis could be made of counting and comparing. Klahr and Wallace (1976) have done this and their work is highly recommended for those wishing to get at least an intuitive feel for the spirit of task analysis. In the present study, only the outputs of the counting and comparing processes were stored in working memory, for reasons going beyond the confines of this study.

The second column in Table 1 describes the stored items for each step of strategy 4. The third column gives the number of such items. At step 2, e.g., there are two stored items, 2.1 and 2.2. The first of these, 2.1, is the output of step 1, namely the number of orange juice glasses for pitcher A. The subject must store this number while performing step 2 because he will need it later, in step 3. However, once step 3 (comparison) has been performed, the subject need store only its output. He can forget the number of juice glasses for pitcher A (and the number of water glasses, too). We see now why strategy 4 does not require six items as mentioned above.

Steps 5 and 6 require four items held in working memory, the maximum number for this strategy. Thus, the working memory demand of this strategy is 4. Finally, step 7 makes explicit the developmental point of contact with the previous strategy.

Since strategy 3 did not differentiate between cases where there are excesses on both sides and the amounts of excess are the same and cases where they are not, strategy 4 is more differentiated. Since strategy 4 does not differentiate between equal excess cases involving different absolute quantities of juice and water, a fifth more differentiated strategy can be invented.

Table 1

Working Memory Demands for Executing Noelting's Strategies

Strategy	Steps Involved	Items in Working Memory	Memory Demand
Isolated Centration (3-4 years)	Step 1 - Look for orange juice in A, if it is there, say it will taste of orange juice, if it is not there, say it won't taste of orange juice.	(i) orange juice	1
	Step 2 - Look for orange juice in B, if it is there, say it will taste of orange juice, too. If not, say it won't.	(i) orange juice	1
Unidimensional Comparison (5-6 years)	Step 1 - Count the number of orange juice beakers to be dumped into A. (Store)	(i) # of orange juice (A)	1
	Step 2 - Count the number of orange juice beakers to be dumped into B. (Store)	(i) # of orange juice (A) (ii) # of orange juice (B)	2
	Step 3 - Select larger number and predict that the side with that number will taste stronger. If the two numbers are equal, say they will taste the same.	(i) # of orange juice (A) (ii) # of orange juice (B)	2
Bidimensional Comparison (7-8 years)	Step 1 - Count the number of orange juice beakers to be dumped into A. (Store)	(i) # of orange juice (A)	1
	Step 2 - Count the number of water beakers to be dumped into A. (Store)	(i) # of orange juice (A) (ii) # of H_2O (A)	2
	Step 3 - Note whether relative amount of orange juice is more, or less than amount of water. (Store)	(i) orange juice with H_2O (A)	1
	Step 4 - Count number of orange juice beakers to be dumped into B. (Store)	(i) orange juice with H_2O (A) (ii) # orange juice (B)	2
	Step 5 - Count the number of water beakers to be dumped into B. (Store)	(i) # orange juice with H_2O (A) (ii) # orange juice (B)	3
	Step 6 - Note whether amount of orange juice in B is more, less or same as amount of water in B. (Store)	(i) orange juice with H_2O (A)	2
	Step 7 - Pick side with more orange juice than water, as more, or side with less orange juice than water as less. If relative amount on each side is in the same direction, say they have the same	(ii) orange juice with H_2O (B)	
Bidimensional Comparison, with consideration of additional factors: quantitative compensation (9-10 years)	Step 1 - Count orange juice in A. (Store)	(i) # of orange juice (A)	1
	Step 2 - Count water in A. (Store)	(i) # of orange juice (A) (ii) # of H_2O (A)	2
	Step 3 - Note which has more, and how much more. (Store)	(i) orange juice > H_2O (A) (ii) Dif = 5	2
	Step 4 - Count orange juice in B. (Store)	(i) orange juice > H_2O (A) (ii) Dif = # (iii) # of orange juice (B)	3
	Step 5 - Count water in B. (Store)	(i) orange juice > H_2O (A) (ii) Dif = # (iii) # orange juice (B) (iv) # H_2O (B)	4
	Step 6 - Note which has more, and how much more.	(i) orange juice > H_2O (A) (ii) Dif = # (iii) orange juice > H_2O (B)	
	Step 7 - Apply same decision rule as in Strategy III, unless relationship is the same on both sides, in which case choose side with greater excess. If excess is equal, say same.	(iv) Dif = #	4

The most exciting fact concerning this analysis is that completely analogous analyses account for the development of performance on rather different tasks devised by other researchers (Case, 1978b, Table 4). Thus, a theoretical unification has been effected at the level of actual performance.

Measuring Working Memory Capacity

Analyses of the above sort have been carried out for a number of tasks, usually those which are appropriate for younger subjects. Case and Globerson (1974) analyzed the Backward Digit Span (Wechsler, 1958), a task useful for measuring the working memory capacity of younger (pre-adolescent) subjects. Other tasks also useful in this regard are Case's Digit Placement Task, Pascual-Leone's Compound Stimulus Visual Information Task, Parkinson's Serial Ordered Visual Information Task, Buttis' Figural Intersection Task, the Clown Task, and the Missing Color Task (see Case, 1978c for more details).

Some of these tasks present information in the form of spoken words, others present pictures. Some present all the information at once, some present it serially. In spite of these differences, all of these tasks yield approximately the same norms: a working memory capacity of about one item for 3-4 year olds, a capacity of about five for 11-12 year olds, and a rate of increase of about one unit every two years.

Theoretically, any task could be used to measure working memory capacity, but only the simplest are actually used for this purpose. Obviously the simpler the task, the clearer the analysis. Since individual differences due to everyday experience would contribute to data variance, laboratory tasks removed from such experience are preferable for measuring capacity. Moreover, since any task might be solvable by a variety of strategies and since the strategy helps determine the memory demand, some control must be exerted over the strategy. In addition to using simple tasks solvable by only a few strategies at most, it is necessary to train subjects on the use of a particular strategy when more than one is possible. When all subjects are using the same strategy, the working memory demand of the task is under the external control of the experimenter who can now vary task demand by varying the number of items of information to be processed. Other considerations are described by Case (1978b).

Decalage and Working Memory Capacity

Decalage can be understood in part by considering working memory demands. Other sources of performance variability exist besides variable memory demands. When tasks involve the same context and the same strategy, differences in memory demand are the major source of differences in task difficulty. The most famous example of decalage, the conservation sequence: amount-weight-volume, was analyzed in this way by Pascual-Leone (1969). He and his students have repeatedly demonstrated that limitations on memory capacity and strategy repertoire place the principal limitations on task performance. Logical operations per se do not pose limitations, that is, when the memory demands of a task are not too great, young subjects solve tasks embodying the logical operations. Piaget's tasks typically confound memory demands with logical demands. When logical demands are held fixed and memory demands are varied, performance varies accordingly.

When the common correlation with age has been partialled out, conservation ability correlates with capacity (DeAvila and Havassy, 1974). These considerations hold for separation of variables (Case, 1977), combinatorial reasoning (Dale, 1976), and other tasks (Case, 1978b) usually associated with the formal operations stage.

For some tasks, experimental variations in memory demand have altered task difficulty by predicted amounts. Scardamalia did this for separation of variables (1977b) and combinatorials (1977a), Pascual-Leone (1969) for modified versions of Inhelder and Piaget's water level task and Pascual-Leone and Smith (1969) for modified versions of several classification tasks.

Responsiveness to training has also been shown to depend on working memory capacity and this brings to mind Siegler's encoding hypothesis. What one encodes should depend on one's working memory capacity as well as one's encoding strategy. Recall that eight-year-olds were much more responsive to experience than five-year-olds. One likely reason is that their working memory capacity was larger by one unit. In addition to the separation-of-variables training study, to be described below, other studies revealed a strong interaction between the size of the training effect and the learner's working memory capacity (Case, 1978a, 1978b). Besides cognitive developmental tasks, Case and his students have begun to develop pilot curricula for teaching traditional classroom tasks such as telling time, adding fractions, solving algebra equations, solving ratio problems, and finding the missing subtrahend and addend (1978a).

The focus on relatively simple tasks reflects the youth of the research program and not any inherent limitations. Persons interested in task analyses of physics or chemistry problems will not find explicit examples worked out. However, an extremely intriguing by-product of the task analysis approach has been to show that some tasks, once thought of as appropriate for adolescents and beyond the abilities of primary school pupils, are less difficult to master than had once been thought (by Piagetians in particular). An example of this is the separation-of-variables scheme, presumably not spontaneously developed until middle adolescence and recalcitrant to instruction (of younger subjects).

A Training Study

Case (1974) conducted a training study in which the separation of variables procedure was taught to seven and eight-year-olds. Responsiveness to training was related to the match between the attentional demands of the instructional method and the working memory or attentional capacity of the subjects. When demands did not exceed capacity, training was very successful, otherwise it was not.

Working memory capacity can be measured in many ways as mentioned above. One way will be briefly described here, namely the backward digit span (Wechsler, 1958). (Case now recommends other ways, but this need not concern us here.) This task requires the subject to first listen to a string of digits spoken at an even pace (about one per second), then repeat them in reverse order. If this could be done for strings of three digits, but not strings of four, then working memory capacity is three informational units or three schemes, at least as indicated by this task. Other capacity measures exist and correlate well among each other. In research of this kind, usually at least two capacity measures are used. Subjects with a capacity of three would presumably understand or be able to keep track of the line of reasoning in the following instructional sequence.

Subjects were shown two kinds of metal rods, brass and aluminum. The rods were embedded in blocks of wood from which they could be easily removed. First, subjects were asked to determine which rod was heavier. They did this by picking up the blocks with the rods in them. The aluminum rod was judged the heavier. Then they were asked to check their judgment against that of a balance scale, only this time the rods were withdrawn from the blocks and then placed on the scale. The scale showed the brass rod to be heavier: surprise and cognitive conflict. The familiar weighing technique clearly showed there was something wrong, that they had made an error. Although some could figure out on their own how they had been tricked, over half were provided with an explanation.

Before proceeding with the explanation, the attentional demand of the meaning of the trick should be mentioned. If a subject could not grasp how the trick worked, there would be no point in training the correct strategy. Case posited that three informational items or three schemes had to be dealt with at the same time. These were:

(1) Something made the scale tip.
(2) It could have tipped because of the aluminum rod.
(3) It could have tipped because of the block.

Indeed, subjects whose working memory capacity was only two did not follow this line of reasoning and consequently failed to profit from the rest of the instructional procedure.

In a later phase of the training, subjects had to follow this line of reasoning; again requiring a capacity of three:

(1) Something makes the scale tip.
(2) It can be either the rod or the block.
(3) If the blocks are the same, it can't be the block.

This sequence illustrates two key components of Case's method, chunking and saliency. First chunking. Note that scheme 2 combines schemes 2 and 3 of the earlier sequence. When two schemes are simply put together as one, "chunking" has occurred. Chunking is a term derived from Miller's (1956) term "chunk," which was meant to suggest an ill-defined yet discrete informational entity, something less easily measured than the bit. Chunks are not really much better defined than when Miller coined the term, but "chunk" has been replaced by the older term "scheme," probably in tacit recognition of the fact that "scheme," as used by Piaget, Bartlett, and Kant, labeled previous attempts to make one of the points that Miller was trying to make: knowledge is composed of complexes which function as units. In various ways, these units can become richer and one way is via simple association: two units or schemes which occur together, which are attended to at the same time, become associated. Activate one scheme and you activate the other at no additional expense as measured by working memory capacity. Another form of chunking called reciprocal assimilation was described by Piaget (1963). Still another, described by Ausubel (1978), is called differentiation.

Since the second sequence of schemes (above) could exploit the chunking process, there was room for one additional scheme. Thus, the second sequence introduced only one new idea (scheme 3), the others being relatively familiar. In this way, among others, the new idea is rendered salient. In general, "when any new component is introduced, it is always rendered salient by the instructor at first, and then gradually allowed to assume its normal salience as the subject becomes accustomed to taking account of it" (Case, 1978a). Thus, new ideas, new schemes are introduced simply in the context of a familiar background. This serves the double purpose of minimizing attentional demands and highlighting the new schemes.

The General Training Approach

Case's instructional method is not entirely verbal, not by any means, although only that part of it has been presented here. The above sequences are accompanied by manipulations of the experimental materials. The blocks are handled even as the instructor describes how the heavier block pulled down the wrong (aluminum rod) side of the balance. Equal weight blocks are handled and used to repeat the comparison of the two rods, the blocks are weighed separately as are the rods, and so on. Afterwards, subjects practice on analogue problems, just as with Siegler's procedure.

Thus, by deed as well as word, new schemes are made salient as they are fitted, one by one, into a coherent system of actions, perceptions, and statements which is then recycled in analogous situations. This instructional procedure resulted in raising the performance level of 7 and 8-year-olds above that normally attained by untrained 15 and 16-year-olds. Moreover, the performance gains were stable over (at least) two months. Judging by subjects' responses to novel tasks, their performance could only be called insightful. Before describing another study, note the general characteristics of Case's developmental method and how they compare with the work of others.

The tasks to be taught must be carefully analyzed. The learner's initial knowledge state must be carefully assessed. The learning activities must be rationally sequenced to bring the learner from his initial state to the desired state. At every step in the learning process, care is taken to minimize the informational load on working memory. The whole approach is quite similar to Siegler's, differing only in Case's explicit account of working memory capacity. Case's approach differs markedly from that of most Piagetian education researchers. There is no attempt or need to classify learners as concrete or formal. Instead, Case's developmental approach "advocates assessment of the learner's initial state in terms of the strategy which he applies to the criterion task spontaneously."

This differs also from Gagne's approach which in a sense ignores the learner since a Gagne hierarchy analyzes a task performance into logically derived levels of component skills. The component skills are described in vitro, divorced from the functional organization imposed upon them by the learner. In this sense, a Gagne hierarchy is psychologically abstract. Of course, when presented with these abstract components, the learner assimilates them as best he can to his current level of functioning. Either approach might work, but presumably the advantage of the developmental method is that by conforming as much as possible to the learner's state, by initiating as much as possible the learner's own activity during the process of assembling new skills, new ideas, etc., in a word, by conforming to developmental processes, the learner's "capacity for coordinating information is not overtaxed" (Case, 1978a).

The guiding principle appears to be this: In the natural course of development, in natural (untutored) learning situations, the child conceptualizes a task so as to maximize its familiarity and minimize its complexity. If the child has no way of knowing whether he succeeded on the task, then we would expect to see problem deformations of the kind so well documented by Piaget. But when the child has reliable means for evaluating his progress, then he will gradually come to grips with more and more aspects of the task environment. In this way, the child self-regulates his cognitive development.

A final word about Gagne's hierarchies. There may be task environments which bear so little resemblance to the learner's past experience that the learner has too few useful strategies to spontaneously apply. (Left to your own devices, how would you fly the lunar landing module? What would you do first, aside from panicking?) In situations such as these, it may be best not to leave the learner to his own devices, particularly if learning time is short. In some school situations, there may be no point in waiting for the learner to generate some strategy, i.e., to provide Case's developmental approach with an entree. In such situations, then, the principal developmental guideline would be to minimize the memory load of the information to be supplied.

In order to minimize informational demands, one first needs a way to measure them for various instructional methods. The psychological research literature does provide a number of examples (the work of Pascual-Leone is definitive), but the matter is very complex at present. It is one thing for a researcher to pore over and compare the aspects of several instructional methods and it is quite another thing for the classroom teacher to draw up tomorrow's lesson plan. Thus, one can legitimately doubt, at this point, whether these detailed task analysis approaches are worth pursuing. There are good arguments in favor of these pursuits for both researchers and classroom teachers.

The case for researchers is the same as for any application of theory to practice. Such applications are never trivial when the potential benefits are great. The cost of trivilizing theory to ease the way to application is very high: useless research and wasted time and effort. Furthermore, the researcher holds out the promise of usable ideas. Most anyone can drive a car, but few understand the underlying engineering and physical principles. It should be possible, indeed Case would insist it is, to provide teachers and curriculum developers with results and methods based on detailed analyses, but which do not require knowledge of these analyses to be effectively used.

In like fashion, Case reports (1978a) that after teachers have been trained to apply the developmental approach, they use it in general, approximate, and intuitive ways to be sure, but in ways which have been judged successful by these teachers. To use this approach requires a teacher to think very carefully about what a pupil is doing and why, as well as about what a pupil should learn to do. Thinking carefully and analytically about pupil and subject matter has to be beneficial in itself. This has been an attempt to convey the spirit of Case's methods in far too few pages. The reader is urged to go to the primary sources (Siegler and Case, in particular). If science educators are to benefit from developmental psychology, they must stop relying entirely on other science educators for their information.

Other Developments

Recent advances in cognitive psychology extend and refine or supplant Piagetian psychology, itself undergoing slow change. The science education researcher interested in developmental psychology should keep abreast of the gist of current developments. Contemporary cognitive psychology is in many ways clearer than Piagetian psychology, but it is less inclined toward global ideas which, by a wave of the hand, transform into comfortable cure-alls for education. The great value in becoming acquainted with the new theories and empirical methods is that they give a better appreciation of the complex conceptual issues of cognitive development in relation to education than do the more global attempts of Piaget.

Task analysis and memory limitations, discussed in the previous pages, are two of many interesting developments and perhaps the two most accessible and practical. Other developments include conservation training procedures, new theoretical accounts of classification, seriation, and conservation performances, spontaneous inferential processes during reading, comprehension and recall of realistic text as well as word lists, long-term memory organization, basic problem-solving skills and their organization into problem-solving strategies. Some of these developments relate more to the primary school; others, to the secondary levels and beyond. The best understood and most researched domains are the simplest, e.g., transivity and conservation, while problem-solving strategies are the least researched and understood.

If problem-solving does not require much creativity, as when the problem is of a kind met and solved before, then Siegler's work and Case's work take us a long way towards designing effective instructional strategies. If more ambitious problem-solving is wanted, such as is found in courses for science majors, then the work of Simon and Greene should also be consulted although this line of research is still new.

It should not be surprising that the study and analysis of problem-solving has yielded very complex and detailed descriptions of the process even at a gross macroscopic level. Were the process a simple one, I doubt whether good problem-solvers would be so rare. By the same token, we should not even think of <u>the</u> process as if across-task similarities of problem-solving strategies were more informative than strategy differences (Simon, 1976, p. 96). Thus the complexity and uniqueness of different strategies make it unlikely that an all-encompassing theory will be simple and readily forthcoming without sacrificing predictive power (as Piaget's theory did). Nevertheless, perusal of this literature is sugge ted to get an idea of what one is up against. If one has not thought about these matters in detail, then this research can provide a very useful framework within which to proceed (see for example, the seminal work of Newell and Simon, 1972).

At the least, the science education researcher can adopt some of the methods of the cognitive psychologist. One in particular is that of doing single-subject research, that is, of taking a long, hard, and close look at individuals before running off to test hundreds and thousands of students with hastily conceived measures. It does not seem unduly optimistic to suggest that careful single-subject research will yield very useful information for educators.

At the same time, it would probably be helpful if teachers could be shown how and encouraged to organize classes to allow for increased contact with individuals. Piagetian ps ology's dream of scientifically pigeon-holing or sorting students in broad groupings is untenable. The research of the past decade has shown that individual differences are quite important as determinants of performance and thus, to optimize learning outcomes, ways must be found to deal with these differences on an individual basis.

In the following pages, the reader will find a somewhat arbitrary selection of topics chosen from a rather large collection. Those discussed are those currently most meaningful to the writer and the reader is referred to the rest of the literature.

Teaching Strategies and Conservation Training

Although conservation training is apparently still of interest to science educators, Murray (1978) observes that "it would appear that the conservation training preoccupation of developmental psychologists may have ended" (p. 419). Nevertheless, Murray notes that in the conservation training studies of the psychology literature, "a number of precise teaching techniques were created and, more importantly, were evaluated." The more than 140 training studies indicate that "there is nothing in principle to distinguish the traditionally researched conservation concept from any other that might be of interest of the curriculum developer" (p. 419). This conclusion is diametrically opposed to the Genevan hypothesis (Inhelder, Sinclair and Bovet, 1974), but this no longer need concern us.

Theoretical considerations aside, the varieties of training study are interesting in themselves. Although designed for four to seven year olds, nothing restricts the applicability of the methods to this age range. Young children were the primary focus because concrete operations make their earliest appearance at these ages. However, these studies do not provide techniques which can be applied mutatis mutandis to the whole science curriculum. The immediate application is to concepts in the sense of definitions or relations. Murray includes the abstract Platonic concepts of justice, virtue, and goodness, so there need be no restriction to concrete concepts (number, weight, area, etc.). What is not included? Consider proportional reasoning tasks. Being able to recognize that a task involves proportional reasoning depends on having the "concept" in Murray's sense. Being able to set up and carry out the required computation may depend on other skills as well. With this in mind, Murray presents "eight primary varieties of training that have been researched and have direct classroom applicability."

(1) Feedback—can involve measurement procedures which contradict non-conservation.

(2) Cognitive conflict—eliciting conflicting judgments regarding the same object.

(3) Training by analogy—relying on previous analogous concepts.

(4) Cue reduction or shaping—minimizing the effects of misleading variables or cues.

(5) Discrimination training—focuses directly on the independence of the concept and irrelevant variables.

(6) Verbal rule instruction—algorithmic learning.

(7) Theoretical prerequisites training—hierarchical training.

(8) Social interaction, imitation, cognitive dissonance, role playing—conservers and non-conservers sort it out among themselves.

When conservation performance is the sole criterion for evaluating these eight methods, "there is no compelling empirical superiority of any except (i.e., social interaction, etc.). Some of the methods may be preferred for one reason or another. For instance, cognitive conflict, training by analogy, and social interaction are particularly Piagetian (or Herbartian) and are the methods of choice for me, although the judicious use of rule instruction is often desirable. However, no "compelling empirical superiority" for these methods can be cited unless three of my own research studies with Lawson (Wollman and Lawson, 1977, 1978; Lawson and Wollman, 1976) and Chen (Wollman and Chen, 1978) are included. Surely three are not enough.

Murray has also provided evidence in favor of social interaction (Murray, Ames and Botvin, 1972). However, "concerning the use of peers as teachers," Brainerd (1977b) feels "there are too few data available at present on which to base a rational conclusion." Brainerd points out that "learning researchers have been able to produce excellent improvement in conceptual skills...with procedures that are completely non-manipulative," again a diametrically non-Piagetian result.

Brainerd, though in favor of manipulatives, is quite wary of supported Piagetian claims and has written probing methodological and theoretical critiques of Piagetians' most cherished notions concerning stage, structure, and training responsiveness. For instance, Brainerd (1977a) has argued that researchers have used invalid statistical techniques to demonstrate that responsiveness to training is a function of initial Piagetian cognitive level. Murray (1978) emphasizes that Brainerd's conclusion is empirically supported.

Brainerd's writings are strongly recommended in the hope that, in his words, they "might encourage readers who incline in the direction of such [cognitive-developmental] curricula to take a hard look at their premises" (Brainerd, 1977b). Other reviews and articles on conservation training should also be consulted (Berlin, In press; Brainerd, 1973; Brainerd and Allen, 1971).

Memory Development: Metamemory

The memory research literature is worth a review in itself. Indeed, there are several (e.g., Kail and Hagan, 1977; Rohwer, 1973; Brown, 1975; Flavell, 1970; Belmont and Butterfield, 1969). It seems essential that science education research avail itself of the carefully obtained empirical findings in this area. The problem of transfer of learning probably will be solved, if it is solved at all, in the context of memorial research. Transfer is not magical although it may seem so to teachers, curriculum developers, and some researchers (Belmont and Butterfield, 1977, p. 445). What is it about a novel task that reminds the student of previously acquired problem-solving strategies? Put the other way, what is it that a student remembers which allows him to recognize the applicability of previous information to a new setting? Can we identify useful memorial strategies? Can we teach them? Having taught them, will they do the job for which they were intended? These are some of the questions that arise in the memory research literature. Only a few of the many interesting facets of this research will be discussed.

Flavell and his students (Flavell and Wellman, 1977) initiated a research domain called "metamemory," or knowing about remembering. Young school children (five to six years) in particular know surprisingly little, far less than older children. By cueing or otherwise instructing them, researchers have elicited striking gains in children's ability to recall information. Even training the retarded typically yields extremely large effects. For example, a list of words such as carrot, dog, potato, cat, monkey, bread, etc. is easier to recall if you notice that all the words fall into one of two groups, food and animals (vegetarians may have an easier time with this particular list). Young children do not notice the possibility of grouping although they are quite able to use this memorial strategy if only it is pointed out or explained to them.

Flavell (1970) coined the term "production deficiency" to describe situations in which subjects can easily produce a given strategy or activity, but do not do so for some reason. Evidence that they can is obtained by giving simple cues or instructions. In this way, subjects have been shown to have production deficiencies for remarkably simple memorial strategies. Even rehearsal is not spontaneously used by very young children (four to five years).

Not all strategies are equally easy to learn by young children, but all are easy to forget (Flavell, 1970; Rohwer, 1973). On the other hand, in these studies instruction in memorial strategies has been quite brief (a matter of minutes). Lasting effects with very young children might be obtained from more massive training efforts.

Older children (about 12 years) use a memorial strategy called "elaboration." This technique adds material to or elaborates upon the material to be recalled, e.g., a list of words to be recalled would be embedded in a sentence of the student's own invention or, if the words labeled objects, the student would elaborate a mental image incorporating the objects. Rohwer and his colleagues (Rohwer and Dempster, 1977) review and discuss elaboration and other strategies.

Rohwer and Dempster (1977) note that "educators may not appreciate the central importance of memory for attaining intellectual competence." One suspects that educators and science education researchers do appreciate the general importance of memory, but that they have taken memory for granted or assumed it would not pose problems of its own once other pedagogical measures were taken such as the use of manipulatives, graphics, laboratory demonstrations, and other developmentally sanctioned devices. Rohwer and Dempster are thus essentially correct.

Former colleagues in physics have said that when they observe, hear, or read a description of a phenomenon, they, too, elaborate the bare input with what they know of relevance to the phenomenon. This includes running a mental moving picture of the phenomenon unfolding, relating this dynamic imagery to a moving point on a graph, and covert verbalizing of key descriptive words. Perhaps students are unaware of how helpful this kind of elaboration can be. And perhaps this is prejudging the question—this kind of elaboration may only be an unimportant epiphenomenon. However, the question deserves looking into.

Some experimental evidence clearly suggests that elaboration can be controlled to yield positive results. For example, a sentence followed by a paraphrase of the sentence is much more likely to be recalled than the same sentence repeated once (Honeck, 1973). Mental images, graphs, equations, and verbal propositions can each paraphrase the others. But science concepts are not remembered in isolation; they are related to and integrated by other concepts or conceptual frameworks. Elaboration beyond paraphrasing takes place, at least among skilled scientists and teachers of science.

Indeed, elaboration of this kind or "schematic" elaboration is the rule rather than the exception, but only for familiar knowledge built up over long experience. Piaget has written of the "schema" in this way, borrowing the term from Kant or Baldwin. Selz (1922) and Bartlett (1932) did much to popularize the notion which is currently much in vogue not just because it is a useful analytical tool, but primarily because computer programming and artificial intelligence have concretized it and shown how the concept can work (e.g., Norman and Rumelhart, 1975).

The chapter by Minsky (1975) is particularly useful as mentioned before. These works provide a framework for describing the kind of organized knowledge subjects exhibit when you observe them coping with and responding to a variety of tasks and questions on a common theme. In a sense, this type of research is an extension of the Piagetian clinical interview method, now applied to a new goal, namely a dynamic, functional description of knowledge and performance (as opposed to the Piagetian goal of a static, structural account of knowledge alone). Incidentally, Kintsch and van Dijk (1975) obtained evidence specifically suggesting that schemas are culture specific.

Schemas are acquired, it appears, through diverse experience over time. Moreover, the elaborative or constructive processes which incorporate and relate new experiences to schemata apparently go on without conscious intent. However, this is not to say that these processes cannot be intentionally initiated.

Again, recollections of physicists suggest deliberate attempts to "schematize." Perhaps the average student, faced with science material, unsure of his computational ability, becomes passively submissive and wonders "What am I supposed to do?" An analogy might be drawn with the young child who does not even recognize that rehearsal improves recall. Just as the younger child is unaware of simple memorial strategies that we take for granted, the older student may be unaware of simple schematizing activities. To continue the analogy, simple instruction and cueing, carried out frequently and over a long period of time, might result in the stable acquisition and deliberate use of schematizing activities. Even though we are ignorant of the underlying psychological processes, we can still model them at the level of overt behavior. This might produce results as dramatic as those obtained with young children.

Memory Research: Training and Transfer

Other research into the nature of memory has surprisingly raised questions of structure and structural learning constraints reminiscent of Piagetian training literature. The details of this research may not be immediately useful, but its parallels with Piagetian research make it informative for investigators wishing a better understanding of the general developmental framework and its principal conceptual issues. Brown (1975) and Belmont and Butterfield (1969) have contributed important research and masterly reviews of the literature. I suggest the reviews, to begin with.

Memorial research draws a distinction between control processes and structural features. "Structural features are invariant components of the system, akin to the hardware of a computer." Thus, they are analogous to Piaget's logical structures, the concrete and formal operations, which, theoretically, are invariant or stable over long developmental periods. Control processes, however, are seen as optional strategies. As such, the implication is that the use of control processes is trainable. On the other hand, structural features are not trainable, again analogous to the Piagetian hypothesis. Brown et al. do not have Piagetian structures in mind, however, but they do allow for the kind of theoretical distinction which Piagetians claim has been empirically justified.

It is interesting to note that as far as memorial abilities are concerned, "no structural differences...have been demonstrated clearly ...Piagetians have argued that there are cognitive structural differences between the normal and the retarded. Thus, we have another apparent conflict between Piagetian and other developmental research. However, Piagetians have logical structures in mind, whereas neurophysiological structures are the presumed bases of memorial performance invariants: The two kinds of structure play different epistemological roles. Although memorial research has not confirmed the existence of structural differences between retardates and normals, Campione and Brown (1977, p. 396) "would guess that structural differences do exist." However, they go on to discuss some of the difficulties, both theoretical and empirical involved in confirming the existence of structural differences. Piagetians should take note.

Brown and her associates have done much in the area of training memorial strategies, particularly with retarded children. Aside from the considerable humanitarian gains latent in this work, there are also implications for research with normal children and adolescents. By focusing on the less able, one can sometimes discover what passes unnoticed in the able (Freud's strategy).

Although dealing only with memorization tasks, this research illustrates a degree of analysis which may be useful for other kinds of tasks. For instance, separate topics include the subject's performance in "estimating task difficulty, monitoring the use of a strategy, adjusting the strategy to task demands, and making use of implicit and explicit information and feedback" (Campione and Brown, 1977).

It may be instructive to become acquainted with the way some of the problems of developmental research in science education reappear and are dealt with by other investigators in other fields. As regards the acquisition, retention, and transfer of mnemonic strategies, training research has produced substantial acquisition gains and even enhanced retention among retardates, but training "does not appear to influence generalization" (p. 402). Even here there is hope because the research may "not have been carried out in such a way as to maximize generalization." A weakness of the transfer training has been "that the experimenter simply tells the subject what to do and leaves the subject to infer why he should do it." Science education research has similarly neglected to encounter the problem of transfer head-on.

The Instructional Approach to Developmental Research

Belmont and Butterfield (1977) review and evaluate the "instructional approach to developmental cognitive research." The major ideas of their review most relevant for education research include notions about the material to be learned, the training method, the learner, and the effectiveness of training. They identify and substantiate a potentially very useful framework for science education developmentalists.

The goal of this research is not to discover how to teach, but to use responsiveness to instruction as way of answering questions in psychology. This technique is identical in spirit to research in physics where one examines a physical system by empirically producing and observing a rich variety of interactions between the poorly understood system and another relatively well understood one. Then, a theoretical model is invented to account for the interactions. The procedure is recycled until reasonable closure is achieved.

Obvious potential spin-offs from this research are instructural and assessment techniques. However, commenting on a typical procedure, the authors observe that "the instructional ingredients are many and the effective ones are unknown." The situation is typical of education research as well. Replication of results, a procedure common to all scientific enterprise, is difficult since "it would require much effort to report the [instructional] procedures well enough to permit exact replication" (p. 439). In addition, the various factors are "neither quantified nor varied systemically, and so their relative contributions elude us." Nevertheless, "it is precisely because the approach is both problemmatic and promising that we analyze it here." Is this analysis really necessary?

Science education instructional strategies would not need to be analyzed if they achieved their ends. Since they do not, or do not very well, and since there remains an undying faith in developmental methods, the same state of affairs obtains here as in psychology: an approach is both problemmatic and believed promising.

Commenting on instructional approaches, Belmont and Butterfield note that these approaches "now appear to have succeeded for conservation in young children and also for much more heady thinking, such as scientific inference and experimental design in 10-year-olds." "The most notable successes" have come from investigators "who focused on verbal rules or on constituent processes," i.e., "logically implicated subprocesses of cognition" (p. 445).

Task-related activity provided valuable data, of course, but only recently have psychologists tried to measure this directly instead of just inferring it. Direct measurement of task-related activity is now "legitimatized...as the basis for creating instructions to produce quantitative gains in children's information processing." Indeed, "direct measurement is fast becoming the foremost technique of developmental cognitive psychology" (p. 443).

As a result of using instructional techniques, some fairly firm conclusions have emerged. Regarding whether some given material can be taught, try to teach it. When instructions fail, Belmont and Butterfield assert (p. 465), "The only worthy response is to improve the instructional routine until it works according to whatever standard has been adopted." In particular, avoid the "philosophical absurdity of reacting with...structural interpretations [of failure]."

Instead, use careful task analyses of the demands of the learning situation. The authors reiterate a point this reviewer has made concerning those who employ task analysis: the "mediational instructionalists...have enjoyed a measure of success that still largely eludes the Piagetians" (p. 446). This approach has been evaluated in ways motivated by pragmatic and theoretical considerations. Pragmatic considerations are (1) the ease with which individuals adopted instructed strategies; (2) the increase in uniformity of performance across individuals; (3) the increase in recall. Detailed measures include the extent to which complex response patterns are successfully predicted (cf. Siegler's balance beam rules and the response patterns which confirmed them).

Direct measurement of overt task-related activity, e.g., the individual's thinking-aloud verbalizations or pencil-and-paper work, are crucial data and "requires close attention to individual differences in criterion task performance" (p. 445). Complex problem-solving activity would thus require single-subject research rather than group assessment. Evaluating elaboration or schematization training, Belmont and Butterfield state, "The point is that understanding group differences...will come only in a carefully balanced analysis of individual differences...matched off against individual differences in a satisfactory independent measure of study activities" (p. 452).

The science education preoccupation with concrete and formal labeling has moved researchers away from the area of individual differences. The concrete/formal distinction is not a useful "individual difference." For instance, it essentially groups all primary school pupils and most junior high students together. Using the conservation sequence to make finer distinctions still conceals too many differences. Piaget deliberately ignored these differences in the belief that he could still elucidate the essentials of knowing. Educators, necessarily confronted with obvious individual differences of some sort, must look more closely at them than Piagetians do.

Thus, we must look at individuals in detail, just as we must analyze and evaluate tasks and instructions in detail. Regarding Flavell's work on production deficiencies, Belmont and Butterfield interpret it to mean that "one cannot fix an age range within which children make the transition from mediational nonproducers to producers." Moreover, "dichotomizing children as producers and nonproducers is inaccurate and misleading" (p. 444). Again, these remarks apply with equal force to the concrete/formal categories.

By focusing in sharp detail on the individual and the task, it appears plausible that no information processing strategy "can be depended on to have an equal or even unidirectional effect across tasks" (p. 455; see also Simon, 1976). The success to date of instructional methods with young subjects "reaffirms that the important aspects of cognitive structure are laid down early and remain unchanged" (pp. 458-459). (This conclusion must be staggering for Piagetians.)

One criterion in science education for judging training to be successful is transfer. It is the only criterion, in a sense. It is also "by far the most highly debated standard for the instructional approach" (p. 466). "It would be lovely if informed guessing or loose reasoning could provide the task analysis required for tests of transfer" (p. 467). Belmont and Butterfield go on to discuss the task-analytic requirements involved. They are formidable and one can only sympathize with "informed guessers" and "loose reasoners."

The authors add, "We know of no transfer test that has employed two well-analyzed tasks." They raise doubts that detailed theoretical advances are forthcoming in this area.

It would thus appear that researchers who must occupy themselves with this matter can only try to make more informed guesses and tighten their reasoning. One's goals will determine the level of theoretical and empirical detail appropriate for research efforts. For this reason, education need not be quite as detailed in its theory and methods as psychology. However, the present degree of detail is too low. To become better informed, education researchers will need more information about students' task-related activity.

Problem-Solving

Problem-solving research uses the technique of deliberately provoking overt task-related activity by asking the problem-solver to "think aloud." Whether this distorts more than it reveals the processes involved is a question which can only be answered by research. Verbalizations lead to the construction of models of problem-solving which are then tested in subsequent research.

Greeno (1976a, 1976b, 1977a, 1977b, 1977c, 1978) has described some of the kinds of knowledge which appear necessary for problem-solving. His theoretical models are computer programs based on the thinking-aloud problem-solving activities of students. The models "work" in the sense that they include components of knowledge sufficient to solve the tasks for which they were designed (unlike Piaget's logical models). Readers may find his work forbiddingly detailed at first glance. "However, a detailed representation is needed in order to obtain a clear understanding of the nature of the knowledge that students acquire and the way it is used in solving problems" (1978, p. 14).

Greeno provides a concrete example from problem-solving in geometry (1978). He identifies three kinds of knowledge involving visual pattern recognition, propositions for inference, and strategic knowledge or planning. All three are necessary, but the last does not ever receive explicit mention in geometry texts. The first is taken for granted, that is, students are expected to learn without difficulty how to recognize external angles, tangents, right angles, etc. Propositions include the definitions, postulates, and theorems of geometry. Ability to make inferences from propositions is not taken for granted. However, while students can almost always follow the inferential steps of a problem solution, they have great difficulty anticipating or organizing those steps. Strategic knowledge is required here and may be the most crucial of the three kinds in determining successful problem-solving.

Planning activities of different kinds have been studied in various contexts (Sacerdoti, 1975; Fahlman, 1974; Sussman, 1973). Though perhaps requiring substantial specific knowledge, strategic knowledge is very general and one can wonder whether it can be taught.

For example, means-ends analysis is a general heuristic used in organizing solutions (Newell and Simon, 1972). "In means-ends analysis, the problem solver compares the current situation with the goal situation to identify differences between them" (Greeno, 1978, p. 16). Then, attempts are made to set up subgoals in order to reduce the various differences found. Since the means-ends idea is so general, whereas the ways to apply it are so endlessly varied, how could such an idea be usefully taught?

The first point to bear in mind is that strategic knowledge or heuristics can be learned. If they can be learned, there must be a way to enhance that learning. However, Greeno's work does not "provide a basis for recommending any specific instructional practice" (pp. 59-60). Nevertheless, some general recommendations can be made. "A reasonable conjecture is that students are able to learn components of a strategy by a process of induction from example problems that are given in the text and worked by the teacher" (p. 62). Since strategic knowledge is a "form of skill," it is "natural to represent knowledge for skilled performance as a procedure." Moreover, "procedural representations are especially appropriate in the context of problem solving" (p. 62).

If teachers shifted attention, from time to time, away from the usual subject matter to the way it is organized during the course of solving problems, then students would become more aware of strategic components of knowledge. Perhaps some time should be allotted to strategic knowledge as subject matter per se. Polya (1957), an early champion of heuristics, i.e., strategic knowledge, believes heuristics can be learned this way, that is, by induction from specific examples [see also Landa's work (1974, 1976)].

I would like to add some comments based on my own research (Wollman and Lawson, 1978) concerning thinking-aloud and procedural knowledge. Although Piaget might have advised against it, I conceived of a Piagetian (or neo-Piagetian) training procedure based on subjects' ability to physically carry out a procedural solution to a type of proportions problem suggested by Lunzer and Pumfrey (1966). After manipulating objects, subjects were asked to describe what they had done in such a way that a listener could do the same thing by following their directions. This type of verbalization is not exactly what Greeno and others in this field have in mind. But, even at this simpler level, 12-year-old subjects had great difficulty at first describing what happened rather than what will happen and why.

There is no doubt in my mind, after having observed seventh grade math and science classes, that students of this age are almost never called upon to be articulate, to explain themselves clearly. In order to articulate problem-solving strategies, not only must there be something to talk about, but there must also be an ability to express one's thoughts clearly, coherently, and at some length. Our subjects eventually expressed themselves reasonably well, but not well enough to consistently describe their procedures in general terms. This required them to produce sentences such as "Divide the larger number of red blocks by the smaller number and then multiply your answer by the number of brown blocks." Instead they preferred to say, "Divide these (pointing to the larger set of red blocks) by these (pointing to the smaller set), etc." This would have been acceptable but for the fact that they were asked to pretend that they were giving instructions over the phone, in which case pointing does not get very far.

Nevertheless, they developed action solutions, or concrete procedures to solve a class of proportions tasks. These physical procedures, easily remembered, were hypothesized to form a procedural proportions scheme which would assimilate new tasks by way of analogy. In subsequent training sessions, explicit analogies were drawn in this way. The resultant symbolic representation of proportions was presumably the end state of the sequence (1) specific procedural knowledge; (2) specific linguistic representation of (1); (3) generalized linguistic representation of (2); (4) application of (3) to new problems with explicit drawing of analogies; (5) recycling of previous steps; (6) general proportions scheme. (I felt then and still do that this approach is a generally useful one.)

In another paper, Greeno discusses problem solving in arithmetic (1977a), in particular the processes involved in simple word problems. He distinguishes, for the sake of discussion, two kinds of understanding, linguistic and conceptual. Linguistic understanding refers "to processes involved in the cognitive representation of arithmetic expressions." The field of artificial intelligence provides many ways of representing this knowledge (e.g., Newell and Simon, 1972). Conceptual understanding goes "beyond the kinds of achievements...(called) linguistic understanding." The linguistic aspect has received much separate attention recently (e.g., Hayes and Simon, 1974; Simon and Hayes, 1976; Anderson, 1976; Schonk, 1972; Kintsch, 1974), but the focus will be on conceptual understanding since it more closely represents the present concerns of science education.

Consider a word problem such as "Sue had three marbles. Nancy gave her five more. How many does Sue have now?" The imagined action in the simple narrative "involves a semantic model of arithmetic" (just as the real actions performed by my subjects involved a semantic model of proportions). "Thus, we can view the task of solving the word problem as a process of relating the formal language of arithmetic to one of its models" (Greeno, 1977a).

Similarly, arithmetic can be related to a more abstract concept such as commutativity. In this case, addition provides one model of commutativity, just as combining marbles provides one model of adding. A hierarchy is in the making. Each level of the hierarchy represents the models on the next lower level, while at the same time being one of many models for the next higher level [for Koestler's (1964) holons)]. Which models fall under which level of abstraction?

If we are dealing with addition and subtraction, how does a child decide which semantic models map onto addition operations, which onto subtraction? "The decision process...is a relatively complicated one." Greeno (1977a) suggests that "children's understanding of arithmetic would be facilitated if specific instruction were given involving semantic distinctions" of this kind. By the same token, subjects in the proportions study could have used such specific instruction. Instead, they were left on their own to induce the criteria for making these distinctions. Some did, but as for the others, something more was needed.

Greeno speculates that a difficulty with the new math of the 60s was due to a confusion of levels. Understanding commutativity can be expressed either at a procedural level or a more abstract level. At the lower level, the child need only demonstrate by his counting procedures in addition that he understands commutativity. At the higher level, a formal language is required. The problem is that a way is needed to connect procedural knowledge with the formal language. Greeno is optimistic "that recent advances in the development of a theoretical framework for analyzing procedural knowledge will make possible a much sharper statement of what we want children to learn and understand in their study of arithmetic and other subjects" (1977a).

Greeno feels that "a theory of procedures is also needed for the ideas of Piagetian theory to be developed in a clear way." He notes that Piaget's formal structures have been related to tasks "by weak analogies, rather than by a thorough analysis of the processes involved in performance of the tasks." This viewpoint is quite prevalent among cognitivists conversant with both computers and Piaget.

The work of Simon using the computer simulation framework to study problem solving is considered fundamental by those working in this area. The introductory parts of Human Problem Solving (Newell and Simon, 1972) would interest all science educators. Simon has investigated performance on puzzles such as the tower of Hanoi (Simon, 1975; Hayes and Simon, 1976), serial pattern detection (Simon and Kotovsky, 1963; Kotovsky and Simon, 1973), and cryptarithmetic (Newell and Simon, 1972), on chess perception (Simon and Gilmartin, 1973; Chase and Simon, 1973, 1974), algebra word problems (Paige and Simon, 1966), and college thermodynamics (Bhaskar and Simon, 1977).

Although he believes he can identify abilities which recur in these varied domains, he does not think variance in performance data can be reduced by classifying tasks according to whether they require this or that ability. The variable difficulty of different problems depends more on the variable need for problem-specific knowledge, the ways problems are internally represented, memory limitations, and the various ways in which basic abilities need to be organized (Simon, 1976).

In 1976, Simon wrote, "We have just begun to explore these new (computer simulation) techniques, and have hardly begun at all to consider how we may apply them to the practical concerns of teaching, training, and child development" (p. 97). Nevertheless, the work of Simon and his colleagues should be consulted for these interested in the study of problem-solving. It is clear that thinking in the "computer mode" gets you closer to describing and understanding performance than thinking in the Piagetian "logical mode." Moreover, hypothesized programs, such as those devised by Resnick and her colleagues and by Siegler, bring out the limitations of theorizing by allowing one to see what the "program" can and cannot do or where the program lacks sufficient detail.

Recently, Klahr and Wallace (1976) have simulated performance on Piagetian tasks such as conservation and class-inclusion. Once you have worked through some of these programs, it is difficult to continue thinking about tasks in the old Piagetian structural way. Procedural or process descriptions provide a much more natural way to think about performance.

The science educator will be more interested in problem-solving investigation in physics that in cryptarithmetic, although "artificial" problems may provide more useful domains for exploratory work with new theoretical concepts and observation techniques. The number of studies of problem-solving in science is still small (in addition to the above, see Klahr, 1978; Larkin, 1977; Schoenfeld, 1976; Simon and Simon, 1978). The typical goal of these studies is a reasonable computer simulation of the problem solving activities of a small number (a few!) of closely monitored subjects. A second goal is a determination of the differences between the activities of skillful problem-solvers and novices. If clear differences can be found, then perhaps we will know what novices must do to become skillful.

Not only is theorizing about these matters in its infancy, data gathering devices are still being developed; new ones create as much or more stir than the results of their present use (see, e.g., Bhaskar and Simon, 1977). This is to be expected in a new and changing field. The bubble chamber of elementary particle physics burst upon the scene about two decades ago, but its earliest uses were prosaic and soon forgotten. The potential of a measuring instrument can be recognized as significant long before significant measurements are made.

From Word to Deed

The final research area of this kind to be drawn to the attention of science education researchers concerns the meaning of written text as a function of its organization. Mayer (1975) reviewed research by Greeno and his colleagues comparing the acquisition of two kinds of material, internally and externally connected. Internally connected material exhibits strong relations among its elements, but weak connections with concepts the learner already has in memory.

Externally connected material has the opposite qualities: strong connections to memory, weak internal connections. Externally connected material lacks the elegance of format of internally connected material.

Elegance of exposition has been an attractive goal for mathematicians since the days of Gauss. Derivations in physics and chemistry texts also reflect the heritage of Gauss. This elegance, so prized by expositors of mathematics and mathematical science, works against assimilation of the material. Mayer (1978) extended these ideas to different domains and obtained evidence confirming the efficacy of externally connected exposition. In doing so, Mayer rediscovered Ausubel's concepts of the advance organizer and meaningful learning set. The purpose of the latter is to make the learner more receptive to the new material.

Mayer's (1977) research on finding meaningful learning contexts is described by his "assimilation-to-schema" idea (sound familiar?). The Piagetian framework thus lives on with computer simulation of procedural knowledge replacing logical representation of structural knowledge. Other approaches to the problem of how subjects make sense out of text in general and problem instructions in particular are discussed elsewhere (Mayer, 1975). Most of the work is exploratory, but there is a widespread feeling that new psychological theory should be developed for meaningful as opposed to artificial behavior. Thus, Kintsch (1974, 1977) is elaborating a theory of understanding realistic text material, whereas not long ago, psychologists were more interested in the learning of lists of nonsense syllables. Kintsch uses paragraph length material from, say, history books or literature. Clearly, the psychological theory being developed in meaningful contexts will not suffer from the criticism of irrelevance so often applied to research of the past. By relying on well specified models, usually in the form of programs of flow charts, psychologists are devising theories with clearly testable implications. Post-hoc appeal to vague concepts can no longer be used with impunity: the new research is too hard-nosed for that.

Achievement Motivation and Discovery Learning

Motivation is usually considered as something that intervenes before or during learning, if it intervenes at all. But motivation can also be a learning outcome and, like all learning outcomes, it may vary with the learning environment. Two ideas are suggested: (1) motivational variables should be routinely measured along with other learning outcomes, and (2) discovery learning may be uniquely suited for motivating independence, active learning, and critical thinking. This brief discussion is based on the work of Weiner and his colleagues (Weiner, 1974, 1977; Weiner, et al., 1971; Weiner and Kun, 1978).

Weiner and others have observed that in achievement-related situations, individuals most often account for outcomes in terms of one or more of four basic causes: ability, effort, task difficulty, and luck (e.g., Weiner, 1977). For instance, individuals will explain success or failure by referring to their own ability and the effort they expended in relation to the perceived difficulty of the task. If apparently relevant, luck will also be mentioned. The main idea is that success (or failure) interacts with these attributions in different ways with different consequences for subsequent motivation in similar achievement situations. The theory of how attributions affect behavior is called attribution theory.

These four attributions fall into two classes or dimensions, the internal-external dimension and the stable-unstable dimension. Ability and effort are internal while task difficulty and luck are external. Also, ability and task difficulty are relatively stable while effort and luck are unstable. For instance, individuals believe that effort will vary with mood, desire, and circumstances and these are themselves unstable. Ability is perceived as underlying competence or potential, an invariant characteristic. However, individuals' self-perceptions do change with sometimes dramatic consequences. Thus, a way to alter achievement motivation is to alter self-perception. (Think of the women's movement and the black's movement with their accent on the fundamental need to alter debilitating self-perceptions.)

For example, attributing failure to lack of ability or the presumed difficulty of tasks will decrease the expectancy of future success, hence decrease motivation. Attributing failure to a lack of effort or luck would be preferable (Weiner, 1977, p. 182). It would be interesting to see whether teachers could enhance students' motivation in a low-achievement context by playing down or suppressing entirely any references to task difficulty or students' ability while playing up the positive role of effort.

When I teach young children arithmetic or game playing strategies, I always assure them that the task is not really very hard, but that some effort is needed. When the child has difficulty, I overtly attribute it to unstable factors such as tiredness (you've had a big day), bad luck (I made a move that messed up your plans), effort (after lunch you'll feel like trying harder), and task specific knowledge (there's a trick you ought to know). I do not know whether I succeed in enhancing motivation because so many other factors are confounded with these attributions. I do know that it is easy to learn to "say the right thing" and that experimental studies have confirmed that altering self-attributions alters motivation (Steckhausen, 1975; Dweck, 1975).

Teachers may point out that they are always trying to be supportive, that they always reward success no matter how modest. However, and not too surprisingly, even with praise there can be too much of a good thing. Rewards for success on a task perceived as easy may be interpreted to mean that the student has low ability (Gold, 1975). It makes no sense and may be dangerous as well to tell a student "You did it on your own" when it is clear to the student that he had lots of help. If, on the other hand, the student felt that he did do it on his own or that he had only a little help, then not only would praise be in order and appreciated, but the implication of the praise might be the attribution "I have ability." This attribution would be all the more justified if luck and low task difficulty could be ruled out in the mind of the student.

Discovery learning procedures may be well suited for enhancing a student's feeling that he has ability because in discovery learning, the student can reasonably conclude that his success was due in large measure to his own activity. This will depend on the teacher's ability to engage the student in self-activity and in recognizing that an answer or task solution can display intelligence and reasonableness even if it is wrong or inadequate.

Recall Case's conception of teaching as teaching the student to modify his existing strategies. In strategy modification, much is retained and relatively little is changed (so as not to exceed working memory capacity for unfamiliar ideas). The student can be given credit for what is retained and can be assured that even the best scientists start out by making lots of mistakes. In fact, to pursue the analogy, what scientists know best is how to look for mistakes. Most scientific activity consists of correcting old mistakes while, at the same time, making new mistakes (Kuhn, 1962).

Since discovery learning involves much guessing and wrong guessing at that, teachers must find a way to turn this into a positive aspect of the learning process. Attribution theory may prove useful in this regard.

As to whether discovery learning lends itself to enhancement of student self-perception better than receptive learning, this is a question that must be answered empirically rather than dogmatically. It may turn out that the two teaching approaches differ little in their effectiveness at raising students' beliefs about their ability. Recall Resnick's observation that even very young pupils will spontaneously invent strategies more skillful than the ones taught via receptive learning (Resnick, 1976; Resnick and Ford, 1979). These children did it on their own, in a real sense. They might be made to understand this.

It is the nature of the mind to invent. Thus it is up to the teacher to nurture this quality. This may be done either with discovery or reception learning techniques. Again, it is an empirical question whether or not one method is generally superior or even just superior for specific domains. The teacher, in any case, must learn to recognize invention for what it is and reward it accordingly. But that raises anew the issue of how to reward students.

We tell teachers to be supportive, but while this is no doubt necessary, there is an art to it. Teacher training programs would do well to better define the relation of supportive behavior to task difficulty and student effort coupled with degree of achievement. By focusing on achievement alone, the teacher may overlook these important concomitant variables: the student may not. Perhaps grading of students could be differentiated so as to account for the various components of achievement (Weiner, 1977, p. 205).

Teacher behavior may also change intrinsic motivation into extrinsic motivation (Weiner, 1977, p. 205). Activities once initiated for the pleasure of it (intrinsic motivation) may later be initiated for teacher praise (extrinsic motivation). After this transformation has occurred, the individual may not initiate the activity unless cued by the teacher. But this research area is very new. Although intrinsic motivation can be undermined, it is not yet clear how this is done and how it can be avoided.

Another important question is whether individuals acquire a set of self-attributions early in life and then stick to them. Perhaps modification is effective only for the very young. On the other hand, if students do "get into a rut" early in life, much of their subsequent behavior will be unreasonable and they will be old enough to understand that. A student may not make even a minimal effort because he thinks himself incapable. This is generally unreasonable: one should at least try hard before giving up the task as hopeless. This common sense advice can make good sense to older students. It has made excellent good sense to individuals who have long thought, for whatever reasons, that there were some things they just could not do (witness the women's movement and the appearance of "math for women" courses).

These are useful areas for investigation. Proponents of discovery learning in particular should be concerned because they so often talk of the need to foster the development of autonomous, active, critical individuals. Motivation and self-perception seem to me to be necessarily involved and should not be overlooked by science education research.

Some Educational and Research Implications

The key words are task analysis, performance patterns, computer simulations, thinking-aloud data, memory organization and capacity, single-subject research. Research implications have been drawn throughout the preceding pages. In this section the use of task analysis (implied is single-subject research or at least observational methods of much finer detail than now employed by science education research) is emphasized.

Psychological research uses task analysis and computer simulation (real or pseudo-programs) to describe procedural knowledge and the constraints of the human information-processing system. Although this is a recent tradition, its successes in describing and predicting performance have been notable. Moreover, the methods used have proved immeasurably superior to those of previous psychological research, including that of Piaget and his school.

The work of Piaget, Ausubel, Bruner, Dewey, and the rest has been mostly pre-theoretical, providing intuitive and philosophical frameworks within which to construct theories such as Piaget's logical stage theory. Current psychological research is properly theoretical and shares many of the philosophical assumptions of the earlier work. (An example of psychological research which is philosophically different is Skinner's. The difference between Skinner's concepts of knowledge and learning are far greater at a philosophical level than any differences between Piaget, Ausubel, and modern cognitive psychologists using the "computer connection.")

Guy Cellerier, one of Piaget's closest associates and the man Piaget chose to be his successor in Geneva, once remarked that if Piaget had been born a generation later, he would most likely have recognized the potential of the earliest uses of computers, principally the analog computer accomplishments. Inevitably he would have been drawn to the computer as a framework or source of models for describing knowledge. As Cellerier (1972) has observed, the computer program has not yet been written which can capture the functional properties of schemes, assimilation and accommodation. Still, artificial intelligence theorists have been particularly attracted by Piaget's theory and goals as spiritually akin to their own and attempts have been made to simulate development by designing programs which learn and reprogram themselves.

Thus, the principal implication for science education of the psychological research described in these pages is simply stated: adopt the theoretical framework of task analysis with its emphasis on the dynamic, procedural manifestations of knowledge.

In a sense, the static or structural aspects of knowledge, emphasized by Piaget, will take care of themselves. By this I mean that science education is in fact concerned with how students display their knowledge, with what they do. Similarly, educators are concerned with what students and teachers must do in order that students perform well. In a word, science educators and researchers are directly concerned with <u>performance</u>. If educational problems can be solved at the level of performance, then the problem of "providing" students with structures will also have been solved, although the latter problem will be primarily of theoretical interest.

The situation is quite analogous to the current state of affairs in psycholinguistic research. The emphasis has shifted away from the structural competence theories initiated by Chomsky. The current focus is on performance theories. Nevertheless, some individuals still prefer to think about competence. The relation of competence theory to performance theory reflects what was described above as "primarily of theoretical interest." Thus, given a performance theory, a competence theory must only exhibit a parsimonious compatibility with that theory.

In any event, it appears that performance theorists and competence theorists have different interests. Education is concerned with competence only indirectly: competence is <u>inferred</u> from performance. It follows that education must strive to promote the kinds of performance which will allow reasonable inference of an underlying competence. The qualifier "reasonable" is necessary because judgments of competence are always relative to the criteria of the individuals evaluating performance. These criteria vary from individual to individual. Agreement among individuals requires give and take or "reasonable" compromise.

Having suggested that science education researchers adopt the framework of task analysis, it might be suggested for the same reasons that they learn to take into account the limitations of the human information-processing system. The limitations which most often arise are related to memorial capacity, but difficulties with encoding and retrieval of information can be very important too, particularly with younger students.

For those science and mathematics concepts and skills of central value, research should be directed at determining how students of different ages perform on tasks embodying these concepts and skills. The inadequate strategies of novices and younger students should be understood no less that the performance of skilled students.

In the context of Case's (1978a) research, learning results from progressive modification of existing strategies. The teacher should know what these strategies are in order to be able to devise situations in which the student can clearly see for himself that he needs to approach things differently. Thus, learning becomes learning to make and correct useful errors, that is, errors which reveal those points where the web of knowledge needs strengthening. Education becomes self-improvement.

A second reason for focusing on the student's existing repertoire of strategies is that these strategies place smaller demands on memory than unfamiliar strategies. By reducing memory capacity demands, teachers can give students some capacity leeway for incorporating new ideas.

Task analysis research is quite demanding. There is every reason to expect that research into the nature of a system as complex as humans in a learning environment must be demanding research. The system is far more complex than any studied by physicists. We do have an advantage over physicists, however: having been part of this system, we intuitively understand how the system works. Out intuitive knowledge is imperfect, but it provides an excellent zeroth approximation. Nevertheless, the theoretical and practical demands of task analysis research are significantly greater than the demands of Piagetian educational research as science educators have perceived them to be. Is task analysis worth the effort? This is an empirical and practical question. A tentative answer can be given.

Task analysis should be applied to some several key science and science-related mathematics topics. To qualify as a key topic, two conditions should be met: (1) the topic should play a central role in the structure of the subject matter, and (2) the topic should be particularly recalcitrant to current teaching attempts. Key topics should be worth the research effort. Then, depending on how much effort was expended and the results achieved, researchers can decide whether to continue this line of research.

Some psychological sophistication on the part of the science educator will be necessary, even if the research team includes a psychological consultant. A great deal of sophistication is required to grasp and apply Piagetian psychology too, but the predigested, neatly packaged popularizations of Piaget's work led to a simplistic understanding and use of his theory. Task analysis promises no such "quick fix." Still, for all its rigour, approximate versions have been put to on-the-spot practical use by classroom teachers, as Case (1978c) and Scardamalia (1977a) report.

There is not nor cannot be a substitute for careful reading of the research literature. Still, some general ideas on how to perform task analysis are in order (Case, 1978a). For one, do the task yourself, blocking out the procedure. Try to identify subroutines in your procedure. Represent your strategy as a program or flow-diagram. To see whether it works, ask a colleague to solve a problem following your program and only your program. If he or she has difficulty, you may have omitted a step which you, the skilled subject, performed so automatically as to be unaware of it. The student or novice may not be so fluent. Or, you may not have omitted anything. Instead, you may have inadequately described a step. Or your sequencing of steps may need rearranging.

You may be surprised at how easy it is to fail to clearly and correctly describe your own problem-solving activities. You will also be pleasantly surprised at how fast you will learn (to judge by the experiences of some colleagues). Describing the problem-solving activities of others will be more difficult of course.

Very often, a novice will make an error because he is using a strategy appropriate to a problem slightly different from the one you gave him. Try to imagine a reinterpretation of the original problem for which the novice's answer would be correct. This will provide you with an insight into the strategy he is using. Also, try to imagine what task-specific information the novice might be lacking. Listening to junior high school and high school science teachers indicates that these teachers are constantly saying things which could <u>only</u> be understood by someone who already knew the subject matter. Teachers do attempt to break things down into small manageable parts, but problems persist because these parts are often out of context, that is, the <u>analysis</u> which results in the parts is never mentioned and thus students have no framework in which to place these parts.

Of course, a skilled performer can often reconstruct the analysis from a few parts, that is, he can see where another skilled performer is coming from. Thus, teachers like and choose textbooks which <u>they</u> understand. If the book proves too difficult for the students, the teachers choose another book which they, the teachers, understand. The novice gets lost in the process. Hence, the need to understand what the novice does.

When the time has come to empirically validate a task analysis, subjects should be given a variety of tasks which will lead to a variety of performance patterns, depending on the subjects' strategies. Siegler's (1976) work is an excellent model for this technique. The patterns as well as the levels of performance are the principal data. These kinds of data relate to task analysis theory the way physical data relate to physical theory. The patterns of interaction as well as their strength allow physicists to sort out alternate theoretical descriptions.

Finally, as regards discovery versus receptive learning, the work of Resnick has shown that there is always likely to be discovery based on the student's own activity regardless of the teaching approach. Only the degree of discovery may vary with the approach. How much discovery is desirable is at present a question of taste. There is little empirical support for any precise answer to this question, although there is apparently unlimited emotional bias.

If an educational goal is to teach students how to discover, then students must have practice in discovery activities (ask any doctoral student's advisor in physics, mathematics, etc.). If, however, the goal is to pass certain written tests, then the degree of required discovery activity may be much less. Again, if the goal of discovery learning is to build self-confident, independent, critical thinkers, then science educators must find ways of measuring self-confidence, independence, and critical thinking.

Where is the developmentalist's dream of harmonizing the methods and content of instruction with the developmental level of the learner? Consider the aspirations of two proponents of the techniques of task analysis: "If we [task] analyze the performance requirements of various school activities and then analyze the skills that individuals bring to these task environments, we should be able to match the two" (Pellegrino and Glaser, 1977). The dream lives on.

This reviewer's bias is in favor of discovery learning, but his definition of that term does not preclude use of direct instruction. Unless trivial knowledge is being taught, direct instruction will always fail to completely and thoroughly encompass the nature of the subject matter and how to use it. Thus, there will always be room and need for substantial and significant activity on the part of the learner over and above paying close attention to the teacher.

As a developmentalist, I am committed to understanding the dynamics of change. Contemporary cognitive psychology, using the "computer connection" to describe the states and transformation of knowledge-in-action, provides education research with a theoretical framework and models markedly in advance over previous developmental research. It is time for education developmentalists to move ahead.

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