ABSTRACT
            The needs of children have not been major forces in
shaping child development research; the pressures of such influences
as war, vio'ence and poverty on their lives have received little
attention. Changes in the child development sciences have been linked
primarily to changes within the discipline. Educational research has
been no more relevant to social need than scientific research.
Technological effort in the behavioral sciences has not produced much
of a foundation for solution of children's educational needs. Solid
curriculum elements have come from basic as much as from applied
research. Current trends toward anti-scientism and the cut-off of
funds for child development research will further limit our ability
to meet children's needs. Lack of research and training funds now
will affect children decades in the future. The research most needed
is in the area of psychosocial development. Little research has been
conducted concerning the social variables affecting cognition. It is
suggested that the variables of tasks, influence source, influence
procedure, social setting of the classroom, cultural and familial
context, non-social environment and individual differences be
studied. Studies of how social cognition develops are also needed. It
is predicted that the lines cf demarcation between studies of
cognitive and social development will soon disappear, in itself a
step toward better meeting children's needs. References are provided.
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The Needs of Young Children and the Needs of Research:

Psychosocial Development Revisited[1]

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Research Fashions of the Recent Past

Although the past thirty years have visited their peculiar pressures on the lives of children -pressures of war, violence, and poverty--these pressures have been reflected but dimly in child development research. From time to time, studies have been published dealing with such topics as father absence (Hetherington & Deur, 1972), infants without families (Freud & Burlingham, 1944), or children's attitudes about war (Escalona, 1963), but social cataclysms have seldom determined fashions in basic research with children. And yet, the discipline of child psychology has changed its course repeatedly, and at surprisingly regular intervals. If these changes have not been determined by the needs of children, what forces have been responsible?

Let us examine the history of child development research more closely. The decade between 1940 and 1950 was dominated by research on personality development and clinical phenomena even though it was not a period in which the personality of the child had some critical significance. World War II produced numerous horrific "experiments of nature" (e.g., Freud & Dann, 1951), many of which had a bearing upon children's personal motives

and anxieties, but few studies of the effects of war actually appeared.
Thus, war-related pressures were not responsible for the scientific
Zeitgeist of the time. No, the forces responsible for the emphasis on
personality development during the 1940s were internal to psychological
science. By this time, child psychology had discovered Sigmund Freud.

Why was this man's influence so pervasive? The reasons are several.
First, psychoanalytic phenomena were vastly interesting and the theory
was heuristically appealing. Moreover, existing research was of little
value in elucidating these phenomena. Thus, a new theory demanded test
and, fortunately or unfortunately, the researchers were willing. And
withal, the needs of children who had suffered unusual privations of
war or family dissolution had little to do with setting the course of
research during the decade.

Let us examine another period--the years between 1950 and 1960.
During this time, studies of children's learning and motivation dominated
the field. One important line of research consisted of studies of social
learning, and other investigations were mounted which illuminated more
basic phenomena such as reinforcement effects, discrimination and generali-
zation processes, and the role of anxiety and frustration. The period
also spawned what is known as "experimental child psychology."[2] These
developments--an enlarging literature on children's learning and
increasing skill in employing experimental approaches in the study of
children's behavior--were all triggered by events within the science.
Behavioristic theories were ripe for test with child subjects, and a
variety of problems in child psychology were ready for experimental, as

opposed to non-manipulative, attack. Demands of classrooms and demands of parents were distal influences. Once again, the needs of children were not major forces in shaping child development research.

About 1960, child psychology moved into a period of research on "pure" cognition. This movement is often thought to have been a consequence of pressures deriving from the race with the Russians to conquer outer space. Additional agitation, it is assumed, derived from the belated discoveries of widespread poverty amidst American affluence and the failing capacities of the U.S. public schools to meet the needs of disadvantaged children. At the same time, however, internal forces pressed for a change in child development research priorities. Psychologists were chafing under the yoke of behaviorism, largely because of its limitations in accounting for complex phenomena such as language acquisition, perceptual development, and the emergence of reasoning abilities. That child development then became dominated by the study of cognitive development is, I submit, less due to Sputnik than to the atrophy and exhaustion of the older theories. Piaget would have been re-discovered if the Great Society had never been born. Less money would have been available for studying Piagetian phenomena, but child psychology would have become a cognitive psychology even if the social serenity of the 1950s had continued.

Changes in the child development sciences thus appear to be linked primarily to changes within the discipline. External pressures may not be insignificant in determining research priorities, but they have probably supported or extended more basic potentials evolving within the sciences

themselves. Social pressures and internal forces interact in affecting the evolution of research in a manner which is not unlike that in which genetic and environmental factors interact in influencing ontogeny in the behavioral phenotype.

There is the possibility, of course, that external pressures and internal scientific pressures are not as independent of one another in determining research priorities as are genetic and environmental influences in determining behavior. In the first place, the layman frequently consults the scientist about social problems. Second, public decision-makers are not completely disinterested in the opinions of scientists concerning the existence of unsuspected problems. Thus, the research on early childhood programs that was fostered by the Great Society did not spring independently and full-blown from the brain of President Johnson and neither were these research priorities established independently by Washington bureaucrats. On the contrary, "scientific experts" were consulted and these same experts were already moving, within their own Zeitgeist, toward a renewed emphasis on cognitive psychology. Such "experts" would hardly be expected to counsel that vast funding increases should be sought on behalf of personality research. No, the new priority given to studies of "pure" cognition was determined, in large measure, by the professional experts themselves. Researchers have had more clout with decision-makers than they realize.

This state of affairs, in which child development scientists have largely determined their own priorities, may be ending. Important voices within our country are urging changes in the balancing of forces

that generate scientific activity. The charge is made that self-determined decision-making by scientists has produced a corpus of information that is largely trivial and/or irrelevant to the needs of modern society. From both Washington and from our own profession come the specifics of this cry: a) child development research is irrelevant to children's needs; b) the resideratum of millions of dollars spent on this field is trivial; c) in any case, this research is of little consequence in proportion to the social urgencies involved.

Such accusations are accepted as truths in many quarters, both liberal and conservative. They pervade the thinking of the Nixon administration although they did not originate during the past four years. Moreover, these views do not emanate exclusively from Washington. The education professions themselves have been hostile to child development research for many decades. The charge of irrelevance can be heard from every elementary school teacher who ever got a C or a D in a child psychology course and, over the years, the anti-science forces within the education professions have managed to put together a powerful establishment. Note that there is a traditional emphasis in educational research funding on contracts instead of grants, and on product-oriented research activity instead of basic research. Both priorities attest to the anti-scientific attitudes of the educational establishment.

Is this to say that the pressing needs of today's children should not serve as the basis for decision-making about research? Why shouldn't our efforts be bent on a technology of child development rather than upon basic science? Why should I evidence concern about anti-scientism? To

assert that the needs of children should be the primary basis of our national research effort is surely an appealing plea.

My answer (which partly begs these questions) is as follows: There is no proof that technological research in the behavioral or educational sciences has produced greater payoff for children than well-supported basic research activity. In terms of marketable "products" and "packages," the research programs of the Office of Education (the educational establishment) have been no more "relevant" to social need than the research programs of the National Institutes of Health (the scientific establishment).

It must be admitted that, in certain instances, technological activity in child development research has paid off rather well. Probably the most famous of these is the impact made by research passing under the rubric of "behavior modification." This technology has swept the education professions and positively touched the lives of millions of children (Risley & Baer, in press). Whether a pupil is enrolled in a program governed by token economies or whether he is enrolled in an eclectic program of "individual guided instruction," he is a consumer of this technological advance. I do not minimize the impact of this work as related to child development nor do I deplore it. It is, however, no educational panacea. Contingencies are likely never to account for phenomena such as language acquisition even though they may be implicated in numerous aspects of language behavior and development, and the limitations of functional analysis in accounting for social motivation, learning to read, and other aspects of child behavior are readily apparent.

The issue here, however, is not the limitations of behavior modification research. Rather, the issue is that sheer technological effort in the behavioral sciences has not produced very much by way of a solid foundation for the solution of children's educational needs. The really solid elements of curriculum for young children, both the components of effective parental management of child behavior and guidelines for the organization of educational environments, has come from "basic" as much as from "applied" research. Besides, good curriculums do not get "built" or "packaged" so much as they evolve in the minds and hands of individual teachers who know the capacities and motivations of children on the basis of intensive study--study of the data as well as studied experience (Hartup & Smothergill, 1967).

Thus, from the standpoint of children's needs, there is reason to deplore the anti-scientism rampant in the country. With every training grant in child psychology that the present administration allows to lapse we are constraining our ability to meet the needs of children. These constraints will affect our ability to respond to children's needs for decades to come. We were unprepared for the Head Start explosion, not because many hundreds of millions of dollars had been spent wastefully on child development research in the earlier years, but because the child development disciplines had been funded so pitifully during all that time.

It is a simple fact that research on child behavior has never had very high priority in this country, and it certainly did not have this priority during the decades leading up to 1960. For example, the immediate post-war period was a time when the country discovered mental health; research

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on children was largely ignored. (Even mental health appropriations during the post-war years were appallingly low considering the amount that was spent on "catching-up" on the nation's highways.) The National Institute of Child Health and Human Development was not established until 1964. By 1965, when the country suddenly discovered the cause of child development, the Society for Research in Child Development sported less than 800 members. Where had they come from? In twos and threes, over four decades, from a very small number of university centers.

The decelerating priorities of 1972 represent a drifting back to the priorities of the early 1950s. Both research and training funds are shrinking--more than general economic conditions require. It is already too late to do much more, by way of research, that relates to the needs of young children during the 1970s. We have already trained the scientists who must handle these problems. But the social policies of the present, which indicate that both children and science are out of favor, will force children to pay dearly in the more distant future. Not in the 1970s, but in the 1990s.

The Need for Renewed Research on Socialization

The research most needed on behalf of young children lies in the broad area known as psychosocial development. There is now beginning to be a redundancy in the output that passes under the rubric of cognitive development. Stages in cognitive activity have been verified and elaborated (Flavell, 1970), the grammar of the child is better described (Cazden, 1972), and the course of perception is better understood than it was ten years ago (Pick & Pick, 1970). But the processes by which changes in

cognitive skills are brought about remain obscure; the implications for education of the new facts are largely uncertain. So-called training studies, in which a "non-conserving" child is helped to "conserve" (e.g., Gelman, 1969), are really studies in an older child development tradition: they consist of applying discrimination learning principles to the conservation problem. The child is taught which, of the many cues available in a particular conservation problem, are relevant or salient to task solution. Thus, in most cases, the training does not produce new "cognitive" learning but an appropriate application of discrimination learning to the cognitive task.

Particularly unclear is the significance for educators of an enhanced understanding of the stages of cognitive development. There can be little question but that linguistic and cognitive competencies are prerequisites to scholastic success both at the preschool level and the levels of elementary and secondary education. How can a child be expected to survive fifth-grade arithmetic in the absence of concrete operational skills? In the same way, how can a child learn to read without basic linguistic competencies? But much of our curricular work--whether for disadvantaged children or for all children assumes only that, if a child has the necessary skills for successful performance, his chances for academic success are automatically enhanced. Our theory of educational motivation, then, runs as follows: a) increased intellectual competence will produce increased chances of task success; b) increased chances of task success positively affect the child's self-concept and establish motivational conditions conducive to academic learning.

The evidence is mounting, however, that competence may be a necessary but not a sufficient condition for enhancement of children's school success. Success is contingent on both organismic and contextual variables as well as basic competencies. Intellectual performance seems to depend greatly on such factors as the child's savvy in assessing the people present in a situation, his skill in adjusting his behavior to these attributions, and his past history of exposure to values about these attributions. There is mounting evidence that furnishing the child only with basic competencies does not produce the sustained, successful effort that is required for successful coping, either that which is required in classrooms or in other environments.

Psychosocial factors in learning have not been ignored altogether by the new curriculum builders. Most, 'a fact, would argue that their models have taken such factors into account. The original Head Start guidelines confirm that social behavior was one of the priority goals of that program, and numerous other program descriptions (Hodges & Spicker, 1967; Klaus & Gray, ' 68) indicate important concerns for the psychosocial aspects of the curriculum. But most of these same preschool operations have given highest priority to designing effective programs of instruction in language, concept formation, and basic cognitive skills. To meet the needs of today's children, we must produce an outpouring of research of two kinds: a) data showing the manner in which social conditions impinge on cognition; and b) data showing the forces that shape the child's social cognitions. Only an increased understanding of the social-motivational-attributional-cognitive components of school performance will thrust our field forward again.

Social Variables Affecting Cognition

The greatest deficit in social research on children's learning and cognitive development is the absence of information concerning the manner in which experimental-developmental variables interact with the "situation." Nearly every theory of child development, from psychodynamic theories to cognitive theories, asserts that social inputs have a bearing upon the child's learning and performance. Thus, in Piaget's theorizing, commerce with the peer culture is assumed to be critical to the development of concepts about moral justice (Piaget, 1932). But few theories are particularistic with respect to how the child learns to behave in accordance with situational demands--whether he accedes to parent as opposed to peer demands in a cross-pressure situation, whether he conforms to a particular social norm or ignores it, whether he behaves assertively or passively when confronted with a particular conflict, or whether he attends to the salient aspects of a television drama or to more peripheral ones. Virtually everyone acknowledges that the situation accounts for substantial variance in child behavior. Some reference must be made to the fact that children behave differently in various situations, from day to day, and from year to year (Mischel, 1968). But beyond some rather simple-minded applications of discrimination learning principles, our theorizing does not account for the manner in which the situation makes a difference.

If the situation is which learning takes place is so widely acknowledged as an important factor in learning, what situational variables should be studied? To this end, the child's "situation" can be divided into five major components: a) the task which the subject is asked to

perform; b) the identity of the persons included in the situation, and the child's past experience with them; c) the methods being used by these people to influence the child's behavior, and his past experience with these social influence manipulations; d) the nature and organization of the social group in which learning occurs, including the child's past experiences with the various members of the group and his perceptions and attributions about them; and e) the cultural and familial context in which the child matures. Other factors which it is important to study in conjunction with these features of the environment include: a) salient features of the physical ecology, and b) characteristics of the individual children, including their individual capacities, styles, and tempos for learning.

Tasks. The manner in which a particular teaching strategy affects the child's performance is, in part, a function of the task itself. A generally "warm" teaching approach, for example, may enhance the modeling of incidental cues but, when the goal is to teach the child directly a set of conceptual skills, its effects may be less noticeable (Bandura & Huston, 1961). Punishment may increase correct responding when an unpunished response alternative is available but have little effect on tasks when the alternative is simply to respond or not to respond (Willoughby, 1969). Thus, teaching techniques may vary in effectiveness according to the particular demands of the task being employed.

Stevenson (1972) has put the problem this way: "As the performance of a single subject may not be generalized readily across all subjects, performance on one task may be unrepresentative of the influence of our

independent variables on other tasks. When this cccurs, the relevance

of a single study for the theoretical position it was designed to test

may be questioned (pp. 75-76)." If educational treatments interact with

tasks in this same manner (as they must), the relevance of single studies

with respect to the establishment of larger educational principles may also

be questioned.

To correct this, Shirley Moore at Minnesota is conducting a series

of studies involving component interventions that contain selective

reinforcement, corrective feedback, modeling, and other modifying elements.

To date, these strategies have been tried with such varied behaviors as

question-asking (Cooper, 1972), task persistence (Wilson & Moore, 1972),

and several other forms of coping behavior. The use of such a range of

outcome variables helps greatly in establishing the task-defined limits

to the manipulation(s). More frequently, however, investigators choose

a small range of tasks that is constructed to fulfill some particular

need of the laboratory paradigm and which is chosen with reference to

prevailing laboratory lore. As a consequence, both the theoretical deductions

and the incorporation of the task into curricula for young children

require caution. The problem, of course, is not simply that the experi-

mental task may lack ecological verisimilitude; rather, the problem lies

in the constraints a single task places upon our educational theories

and our educational practices.

The influence source. The dominant socializing agent in most

school-type programs for young children consists of a white, 30-year old

female. While such persons predominate in potential teacher pools,

there are those who would argue that other minorities--e.g., racial minorities and men--should form a larger portion of this reservoir, especially for service in certain situations. Thus, Professor McCandless of Emory University has mounted a rather elaborate research program to study the impact on young children's development of the inclusion of male teachers in the classroom. (Pending the outcome of his work, I will simply assert that affirmative action in the field of early childhood education may one day include determined effort to recruit men into the field in preference to further importation of women!) At the moment, however, it is not possible to argue, from other than a civil rights basis, that there is greater value in employing male teachers in preference to female teachers, black teachers in preference to white teachers, young teachers in preference to older teachers, or mothe   ....ssionals in preference to non-mother professionals, in programs for young children. Moreover, we have little data on the value of children themselves, employed as socializing agents, as compared to adult teachers in effects produced on the learning of young children. A few promising studies have been started in which teen-agers are employed as teaching professionals, or in which slightly older children are used for instruction of their juniors, but their effectiveness in relation to the outcomes produced by adult teachers is largely unknown.

The idea is not new that the identity of the socializing agent is an important factor in children's learning. Nor are attempts to carry out demonstration and research projects involving male teachers, interracial pairings of children and teachers, and programs including peer leaders.

Unfortunately, most research and demonstration efforts in this area are poor, research-wise; careful research designs have not been used (frequently because random assignment of children to groups is considered unethical). In these cases, however, poor data are no better than no data. The investigator who is not willing to assign subjects randomly to conditions for purposes of assessing the influence of a particular class of socializing agent might as well save his energy. The problem of accumulating a taxonomy of influence sources must be broached, but it must be understood that personal tendernesses never excuse poor science.

The influence procedure. Laboratory research in child development is replete with schemes for manipulating the actions of the influence source (teacher). So is educational research. Few of these schemes, however, have been systematically studied. As an example: some years ago, I was associated with the invention of a procedure called "nurturance-withdrawal" (Hartup, 1958). This consisted of five minutes during which an experimenter played attentively with the child, followed by five minutes of ignoring him. This paradigm is not unlike sequences of events observable in every nursery school and, indeed, it was constructed because of its similarity to commonly occurring events in natural child rearing. We employed this procedure in several studies, and then a series of variants was used in other investigations (Gewirtz & Baer, 1958; Bandura & Huston, 1961). This use of plural methods was estimable, except that the interface among them was never established by direct experiment. As a result, inconsistent findings have been difficult to interpret, and the implications of our laboratory results for professional practice have been unclear. A

similar problem is presented by current classroom work on teacher variables
such as warmth versus dominance, punitiveness, proportioning of positive
and negative reinforcements (Feshbach, 1973), and the use of social com-
parisons (i.e., comparisons between children, Masters, 1972). Most con-
temporary work showing the limitations of various teaching styles is
buried in evaluation research on large global "models" of early educa-
tion. Badly needed are clear small-scale component studies in which
variations in teaching stance are measured in terms of a carefully
specified range of outcomes.

The social setting of the classroom. Both educational and psycho-
logical research has made little reference to the social setting (group)
as this interacts with educational input. Most laboratory work on
children's learning is accomplished in two-person groups (usually the
child and the agent of influence). Ordinarily, members of this dyad
cannot interact freely with one another. As a consequence, studies are
needed showing the effects on performance of reinforcement schedules
delivered in three-person and larger groups. Also, we need to know more
about the operation of vicarious processes in larger groups and we need to
know something about diffusion and contagious effects occurring within
the group.

For these purposes, we need field experiments of appropriate design,
as well as imaginative selection of new problems. It is incredible that
diffusion effects of individual behavior modification regimes have not been
better studied. A few years ago, Scott, Burton, & Yarrow (1967) found
that peer reactions toward a target child changed as a function of the

altered adult contingencies toward the subject. But these effects were only

casually noted, and such effects on the total functioning of the group

seem seldom to have been observed. Where, in addition, are the studies

of media influences diffused to a group through a single child? Most

studies of media influences involve exposure of all members of an experi-

mental group to the media either one-by-one in individual sessions (Hicks,

1965) or simultaneously (Stein & Friedrich, 1971).

Various parameters of group composition should be examined. The

problem of socioeconomic "mix" has not been studied carefully and thoroughly

heretofore, and yet the implications of such information for social policy

making are vast and the consequences of having no data are severe. Per-

sonally, I have little doubt but that integrating classrooms socio-

economically produces larger developmental changes among children on many

behavioral dimensions than does an unintegrated strategy. And yet, this

point cannot be debated until we have a body of high quality research

dealing with the problem. Moreover, this is not the only parameter of

group composition that requires study. Age-mixes (a hoary issue), mixes

of teachers and pupils, size of groups for component curriculum experiences,

and a host of other problems relating to group components should be examined.

The cultural and familial context. Variables such as "culture" and

"class" are difficult variables to explicate. In psychological terms, they

are "secondary" variables, in the sense that they do not refer to the

conditions that directly affect learning and performance. Such variables

reflect confounded constellations of conditions. Thus, until "race"

and "social class" can be translated into specific components,

they have limited psychological usefulness. They constitute mere demo-
graphic categories, better suited to population research than to psycho-
logical investigation.

Nevertheless, there is agitation among both community leaders and
educators, to augment existing data dealing with the interaction between
cultural factors and learning processes. This agitation usually takes
some polemical form such as: a) middle class values should not serve as
the basis for curriculum development for working class children; or
b) standard English should not be stressed in language programs for
children who do not come from a standard-English speaking subculture.
These arguments, of course, have their origins in longstanding intergroup
conflicts and their resolution involves the realm of social philosophy as
much as social research.

Nevertheless, I wish that educators and community leaders, when
faced with curriculum decisions of this kind, would more frequently ask
the following questions: "What do the data show?" "What is known about
the processes of language acquisition across dialects or across languages?"
"What are the long-term consequences of reinforcing non-standard English
as opposed to standard English?" Educators are inundated with opinions
bearing upon these questions but, unfortunately, few facts float about
on the surface of this contentiousness. Surely, there is everything to
gain and little to lose by endorsing broad-scale research on the cultural
context and family values as factors in children's learning.

Programs such as Project Follow-through were founded on the as-
sumption that family influences do not, in disadvantaged environments,

adequately support and extend the impact of school-type intervention programs. But parental "lack of support" for schooling can range from indifference to antipathy; basic parent-child relations can vary from the desirable to the perverse; and street values may range from the diffident to the delinquent. Thus, family environments, like school environments, can range from excellent to awful on hundreds of dimensions. Without knowledge about the manner in which particular environments affect the learning process we must grope blindly as we attempt to design models of Follow-through, Catch-up, or Home-start.

The non-social environment and children's learning. Several years ago, Shure (1963) published a very interesting ecological study of the Cornell University nursery school. The implications of that study are far-reaching, even though its impact has been minimal. Briefly, she showed that there is a consistent relation between the physical arrangement of school environments and the character of the social interaction occurring there. The "relevance" of the children's behavior was highest in art areas, and "complex social interaction" occurred most often in the doll corner. Boys were more irrelevant in their behavior in the art and doll areas, and girls more often irrelevant in the block area.

Some of the information from Shure's study is probably useful in a normative sense. Certainly, educators who want to construct new learning environments for groups of young children should consult this article. But the main value of the study, it seems to me, is in simply showing that a relation exists between physical space and social behavior. Different relations may exist in different nursery schools in different

regions. But where are other studies of these problems? Other than
this investigation, I can think of only a few researches on like topics,
such as Jersild and Markey's (1935) study concerning the relation between
space and aggression. Somehow, ecological science just does not turn on
psychologists.

· Individual differences. One can list many individual difference
variables which affect the influence of social-contextual factors on
children's learning: IQ, sex, age, cognitive style, role-taking skill,
anxiousness, achievement striving, dependency, and fears of failure.
Of these, chronological age is perhaps the most seriously ignored. This
assertion may seem strange in view of the large number of studies that
appear each year in which age has been included as a variable. The fact
is, however, that in studies of social factors in learning, age has not
been regarded as very important. Most of the research on imitation,
reinforcement effects, vicarious processes, and social motivation that
has appeared during the past decade has not been completed from a
developmental perspective.(Hartup & Coates, 1970; Hartup, in press).

It is inconceivable that social influences operate irrespective of
age level. Patterns of authority, demands for independence, and social
comparisons are used differently by teachers according to the age of the
children in their charge. Since their decisions are largely based on
intuition and educational folklore, we can only speculate about how dif-
ferent our situation would be if we possessed numerous developmental studies
of the social influence effects.

Studies in Social Cognition

There have always been a few psychologists who have been interested in "person perception." They have lived and worked mostly on the fringes of social psychology and, in the developmental literature, one must look long and hard for classical studies of children's cognitions about other people or about social phenomena. Usually, this search ends with studies of race awareness, sex role attitudes, and perceived parent behavior. In each of these problem-areas, however, the interest of the developmental psychologist has ordinarily been focussed on what the child perceives or believes to be true about other persons rather than upon how he acquires these attributions. And yet, even though it is important to know that ethnocentric attitudes are evidenced early in the young child's life, it is perhaps even more significant to know how such cognitions are formed. It may be important to know whether a child perceives his parent as supportive or hostile and whether he regards his teacher as nurturant or controlling. At the same time, however, there is also a need to know something about the origins of these perceptions and the forces which shaped them.

Child psychology has not been ready, until recently, to emphasize research on social-cognitive development. First, considerable knowledge has been required concerning the manner in which the young child interacts with objects, disregarding the nature of the object. This is not to say that the identity of the object (e.g., whether it is the mother or whether it is a plastic mobile) is unimportant in its contributions to the child's mental development. But it has been necessary to develop methods for studying behavior-object interaction, and these methods were probably best

developed first for studies of the child's interaction with non-social objects. After all, the mother is a rather large object, and fathers are often bigger!

Second, it has been necessary to acquire a certain expertise about the manner in which the social context affects children's cognitive development. Some prior understanding has been needed about the ways in which the social environment impinges on cognitive activity in general. Gradually, we are reaching this point. The interface between social development and cognitive development is being studied, and we are now ready to expand directly research dealing with the child's growing ability to attribute thoughts, feelings, ideas, perceptions, motivations, and attitudes to other people.

Flavell (in press) has devised a conceptual model which characterizes the development of social cognitions in terms of four classes of knowledge or ability: a) the child must know that other individuals possess psychological properties (that is, he must recognize the existence of psychological conditions in other people); b) he must evidence awareness that the particular situation in which he finds himself calls for some kind of inferential activity regarding the behavior of others; c) he must be able actually to carry out that inferential activity; and d) he must be capable of using the resulting deductions toward some situationally-appropriate interpersonal end. Each of these classes of ability represents an important area of developmental study because it appears that young children, in contrast to older ones, are relatively inattentive to signs of listener incomprehension in interpersonal situations, are unable to understand all

the signs they attend to, and are unable to see the communicative implications of all the signs they do understand.

Normative-descriptive work is currently going forward on certain of these problems: to discover when children recognize that psychological activity exists in others, and when inferential abilities become evident. However, very little explanatory writing has been done with respect to any of these problems because, by and large, students of cognitive development are not very interested in explaining transitions in cognitive functioning. A few social cognitive training studies have been done with older children, probably the most successful one being Shantz's (1970) effort to produce responsive communicative behavior in second-graders. Frequently, however, the researcher's interest in conducting such a training study is diagnostic; that is, by demonstrating that one can increase communicative responsiveness in second-graders, one demonstrates the prior existence of the basic inferential skills. I contend that research on social cognition requires something more than simple demonstrations of reinforcement effects and their relation to the emergence of social cognitive activity in the child.

Still another type of research that is needed in the area of social cognitive development concerns the role of cognitive processes in group functioning. In a new study from our own laboratory, we are attempting to elucidate the role of cognitive factors in the development of aggression. We have been particularly struck by the increasing importance, as children grow older, of threats to self esteem (i.e., insults) in instigating aggression. Aggression among preschoolers is largely instigated by

territorial imperatives and has a highly instrumental quality to it; the child wants to hang on to his equipment or take it away from someone else. At the same time, derogations and insults (even when employed) are often ignored. Among older children, however, threats to self esteem appear to be the primary elicitors of aggression. Moreover, such threats instigate particularly hostile, person-directed violence.

We have hypothesized that: a) younger children are limited cognitively in the attributions which they can assign to others, and b) this limitation constrains the arousal of frustration-produced affect necessary to the activation of hostile aggression. Establishing these hypotheses empirically is difficult, but a differentiated analysis of aggression development requires a social cognitive approach that contrasts sharply with the social learning approaches of the past.

Such analyses could also re-make our theorizing with respect to such problems as the development of altruism and prosocial activity, interpersonal attachment, achievement striving and independence, and the emergence of moral behavior.

Conclusion

Before long, the lines of demarcation between studies of cognitive development and studies of social development will have been blurred beyond distinction. Students will no longer identify themselves as students of intellectual development or students of social development. It is about time. Moreover, there is little doubt, on my part, that this return to thinking about the child as an integrated organism functioning within a known social milieu is a long step forward toward better meeting the needs of children by means of empirical research.

Footnotes

1. This paper was prepared with assistrnce from Grant No. HD 005027-03
and was first presented as an invited address at the annual conference of
The National Association for the Education of Young Children, Atlanta,
Georgia, 1972.

2. I have never liked this term because it sets apart certain researches
on the basis of a methodology rather than on the basis of topic or problem.

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