MAIN CURRENTS OF CRITICAL PSYCHOLOGY

Vygotskij, Holzkamp, Riegel

M.H. van IJzendoorn
R. van der Veer

in collaboration with:
F.A. Goossens

translated by:
M. Schoen

IRVINGTON PUBLISHERS, INC.
551 FIFTH AVENUE NEW YORK, N.Y. 10017

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Library of Congress Cataloging in Publication Data

IJzendoorn, Marinus H. van, 1952-
Main currents of critical psychology.

Translation of: Kritische psychologie.
Bibliography: p.
Includes indexes.
1. Psychology--Philosophy. 2. Dialectical materialism --Psychological aspects. I. Veer, René van der, 1952- . II. Goossens, Frits, 1949- .
III. Title.
BF38.I4713 1984 150'.1 83-22664
ISBN 0-8290-1517-5
ISBN 0-8290-1518-3 (pbk)

Printed in the United States of America

UNIV. LEIDEN BIBL.

To Jeanet and Marie-José

PREFACE

The expression "critical psychology" can be interpreted in a number of different ways. What unites all the schools of this name, however, is the common conviction that empiricism is an unsatisfactory approach to the problems to be dealt with in psychology; a scientific theory of human behavior should at least take into account the historical and social context of that behavior. This call for the integration of an historical-social and an empirical approach appears, however, to have found little response at all within the behavioral sciences.

It is against the background of what is felt by many to be a crisis in psychology that we shall be presenting three schools of critical psychology, whose theoretical and methodological development have rightly won them a following. The oldest of these schools is the "Cultural-Historical School", with which the name of Vygotskij is associated. It was this school that provided the historical and intellectual basis for the "Berlin School of Critical Psychology", which arose around the figure of Holzkamp. The Cultural-Historical School also dealt with the same problems as "Dialectical Psychology", developed in the United States by Riegel and others. We shall not only be describing these three schools in full, but be investigating their theoretical and methodological tenability as well.

This book is a completely revised edition of a publication which first appeared in Dutch under the title *"Kritische Psychologie"* ("Critical Psychology"), published in 1981 by Ambo. To this original book we have added several paragraphs and a closing chapter, re-writing nearly all passages, in part based upon information recently made available to us. Our colleague Frits A. Goossens had an important share in the Dutch version of the chapter on Dialectical Psychology, but other obligations prevented him from contributing to the revised edition. We are grateful for the material he made available to us in revising this particular chapter. We should like to acknowledge the encouragement and support of the specialist on Soviet psychology, professor Carel F. van Parreren. We should also like to express gratitude to our Dutch-American colleague Marc Schoen, whose thankless task it was to translate the original text into English. We should further like to thank our colleagues at the University of Leiden for providing the stimulating intellectual climate in which to work on this book and Cora Jongsma for typing out the manuscript.

Marinus H. van IJzendoorn
René van der Veer
Leiden, August 1983

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INTRODUCTION

Psychologists seem to be a bit masochistic. At least, this is the impression one gets when studying recent essays on the structure, range and relevance of psychological research. Time and again, psychologists indulge in self-chastisement. Koch (1969), for example, speaks of the "pooled pseudo-knowledge that is much of psychology". Kantor (1979) states that "in spite of all the historical efforts to make psychology a science, and the ambitions of psychologists to convert psychology to a science, this discipline cannot fully qualify as a natural science". But even researchers for whom natural scientific research is not automatically the standard for all scientific research have uttered bitter sentiments as to the "state of the art".

One problem, for example, is the position of theory in psychological research. It is as if many researchers are possessed by one thought only: how can I escape as quickly as possible the stage of theory and hypothesis construction and get on with the "real work", in other words, collecting data and testing hypotheses. This attitude has several unfortunate consequences. At times there seems to be hardly any connection between theory and practical research; the choice of research problems is arbitrary. As a result, not only are problems sometimes solved incorrectly, but

more often the wrong problems are being solved. Knowledge is not being accumulated optimally and experimental research does not always lead to new insights. It is this "empiricism" that has resulted in the view of psychology as a science of trivialities.

A second problem is the validity of the results of psychological research. In many cases, it is unclear what factors are responsible for an effect uncovered in an experiment. Often, the results of research into human or animal behavior are, in fact, elicited by the artificiality of the research situation (Rosenthal, 1976). Outside the laboratory, these results are meaningless. Was Bertrand Russell perhaps right after all when he said: "One may say broadly that all the animals that have been carefully observed have behaved as to confirm the philosophy in which the observer believed before his observation began. Nay, more, they have all displayed the national characteristics of the observer. Animals studied by Americans rush about frantically, with an incredible display of hustle and pep, and at last achieve the desired results by chance. Animals observed by Germans sit still and think, and at last evolve the solution out of their inner consciousness"? (Russell, 1956, p.32-33). Even the seemingly most exact aspect of psychological research, statistics, can find no favour in the eyes of methodologists (Meehl, 1967). They oppose equating Popper's principle of falsification with statistically testing the null hypothesis. There are then no adequate technical safeguards against trivial and invalid research results. It is not at all certain that in the Darwinian struggle of theories, the fittest will survive.

These and other problems have considerably eroded the faith in traditional psychology. So much so that there is talk of a "crisis", although not for the first time in history of psychology (Boring, 1950). This feeling of crisis is only reinforced by economic problems; the drying up of the financial sources for social scientific research has, of course, not contributed much to the mitigation of the internal problems. How did this crisis in psychology come about? We suspect that unless the following three mutually related

problems are taken seriously, psychology will not be able to overcome the crisis within a short period of time.

PROBLEMATIC PSYCHOLOGY

Firstly, there is the problem of unity. That psychology is characterized by so much disjointedness is felt by many researchers to be a considerable shortcoming. Numerous "mini"-theories exist side by side and a more comprehensive theory is lacking altogether. By means of a Kuhnian analysis, some researchers have also demonstrated that there are several different paradigms at work in psychology (see Palermo, 1971, Marková, 1982). The problem is that in psychology, these paradigms exist side by side, each one enjoying the support of a particular group of researchers. Time and again, this unsatisfactory situation results in the call for a "strong" theory or paradigm, which will bring "law and order" into the chaos of psychological research. Some see a redeeming role reserved for cognitive psychology. Research into meta-cognition is very popular and certainly not only in neo-Piagetian circles. Others make a case for holism (Giorgi, 1976; Kitchener, 1982). They suggest that some of the shortcomings of present-day psychology are the result of a one-sided mechanistic and atomistic approach. Still others respond by saying that it is an illusion to think that uniformity in psychology is possible. There is no theory that can explain both the iconic memory and the cause of aggression in large cities. It is in the nature of the human being to be disjointed. Even if there were such a theory, it would be so general as to be meaningless. But perhaps it is possible to construct a sort of middle-range-theory (Merton) which manages to avoid both the Scylla of the "dataism" (Bunge) and the Charybdis of the empty cliché. In doing so, the Cultural-Historical School (see chapter 1) could be a source of inspiration.

Secondly, there is the problem of choosing between alternative strategies for clarifying psychological questions. We refer to the dilemma understanding versus explanation which has existed as long as psychology itself. Wundt had already created a special "Völkerpsychologie" for those problems difficult to clarify experimentally. Recently, Cronbach (1975) made a plea for idiographic case-studies, in addition to already existing experimental and correlational research. This approach is designed to produce a reconciliation of the empirical-analytical with the hermeneutic approach. Habermas (1973a) too, regards such a synthesis as a necessary step in the direction of a critical social science. Cronbach's plea is appealing, but because he appears unable to point to any good examples of a succesful combination of "hard" and "soft" methods, it is not very convincing. In the philosophy of the social sciences, one reads frequently that the postulates of empirical-analytical science are such that they are especially attuned to workings of nature but simply create difficulties in studying human behavior. The behavior of human beings is not only determined by natural factors, but by cultural ones as well. Cultural factors are by nature transitory and evaporate in the light of an empirical approach. Cultural factors are not fixed, causal determinants in the strict sense of the words but the transitory context in which a real understanding of human behavior is ultimately made possible. If people are aware of the operation of cultural "laws", these laws are at once dissoluble. The Berlin School regards it as one of its tasks to bring the results of research into cultural "laws" to the attention of those involved and in doing so, increase their freedom to choose between several options for action (see chapter II).

Of course, human behavior undeniably possesses natural as well as cultural facets, among them, for example, all sorts of psycho-physiological reflexes. Some believe that this "natural" aspect - the first nature - can be explained along social scientific lines. On the other hand, the "cultural" aspect - the second nature - can only be hermeneutically described and understood (see for example, De Boer, 1983). It is

still very much in question, however, whether each aspect of human behavior requires its own means of interpretation. As we shall see in chapter 1, the Cultural-Historical School rejects this approach. It is also in this connection that the distinction between "behavior" and "actions" is at times put forward. Behavior and action are the two extremes of a continuum that runs from natural reflexes (behavior) to intentional, potentially conscious actions. Ultimately, actions become automatisms and habits. The advantage of this is that they can be executed quickly and with little efforts. A disadvantage is that the subject is no longer capable of easily changing those actions. We are then dealing with "petrified regularity" (Habermas) or "fossilized behavior" (Vygotskij) which can only be made intentional again through conscious insight. It is characteristic of behavioral processes (for example psycho-physiological reflexes) that the individual is not usually considered responsible for them. At times, however, it is possible to have the individual gain conscious control of these seemingly autonomous processes, for example through bio-feedback. Throughout the entire continuum, first nature ("behavior") and second nature ("actions") are indissolubly intertwined.

The above is closely related to the third problem, the problem of human capacity for change. Critical psychologists attempt to demonstrate that much seemingly "fossilized" behavior can, in fact, be altered. If individuals are only conscious of their status, such behavior can once again be made intentional. It is as yet unclear how far it is possible to move along the continuum in the direction of (seemingly) biologically determined forms of behavior in exposing them as "cultural actions". The three schools to be discussed in this book are quite optimistic in this regard. And it must be said that in view of the large variations in culture uncovered by cross-cultural research, this optimism has yet to be refuted. The problem, however, is that the above does not necessarily mean that it is easy for a single individual to change. One need only be reminded of the wolf-children to realize how variable, but also irreversible human behavior can be. Both the Cultural-Historical School (chapter I) and the

Berlin School (chapter II) have been intensely occupied with the question of the human "biological and cultural inheritance".

THE INFLUENCE OF CRITICAL PSYCHOLOGY

In the preceding paragraphs, we have mentioned a number of psychology's major problems. We also mentioned a number of the critical-psychological positions on these matters. Remarkably, however, these critical schools have yet to play a significant role in the discussion on the crisis in psychology. As yet, a full account has never been given of the possibilities and the limits of critical psychology in finding a solution to the crisis. The work of Riegel (see chapter III) provides an important initiative in this regard, but the account of this American psychologist of critical schools outside the United States is very brief indeed. In the one reference he makes to Holzkamp, and then in a footnote (1978, p.243), Riegel cites the language barrier as the probable cause for this general neglect, in particular for hardly devoting any attention at all to the representatives of the Cultural-Historical School. One would almost erroneously assume that such European philosophers and psychologists as Sève, Lorenzer, Habermas, Holzkamp and Vygotskij had little to contribute to the combating of the crisis in contemporary psychology. This disregard for the critical approach is, however, partially the result of the relatively esoteric style of the abovementioned researchers. It is also partially based on the caricature people have of critical psychology. It is equated with a primitive form of "vulgar Marxism", in which slogans take the place of any scientific approach. It is presumed that a dialectical psychology would devote exclusive attention to the material foundations of society, reducing the human being to a tool of economic forces. In this book, we hope to show that this is a distorted view of the matter. We believe that despite all the qualifications that can and should be made with respect to the views expressed in critical psychology, there is

no justification for the fact that they have been almost entirely ignored in the discussion on the universally felt need for a reorientation in psychology. Obviously, critical psychology cannot provide any well-tested means of combating the crisis; too few of its ideas have been brought into practice. But the critical schools do attempt to systematically analyze the causes of the present crisis, and their suggestions for solving the crisis merit at least serious attention. However, the idea that progress is only possible through constructive criticism applies to critical psychology as well, and we have made no attempt to conceal such criticism when appropriate.

As we said earlier, there are various critical schools in European psychology. We mentioned Sève's French school. We also referred to Lorenzer's psychoanalytically inspired school and Habermas' Frankfurt School, which more and more has begun to complement its sociological approach with a psychological one. There is more, then, to critical psychology than just a discussion of Vygotskij's Cultural-Historical School, Holzkamp's Berlin School and Riegel's Dialectical Psychology. We chose to discuss these three schools, however, because they have demonstrated what the consequences are of their theoretical points of view in terms of concrete psychological research. In addition, these points of view are expressed in the statistical analysis (see chapter II), and the experimental design (see chapter III) of their studies as well. To what extent these ideas have lasting value is a subject we shall be discussing in the chapters in question. In contrast, the schools taking shape around the work of Sève, Lorenzer and Habermas still appear to be lingering in the stage of purely theoretical reorientation. They have not yet reached the stage of concrete applications into experimental designs or statistical analyses. We therefore believe at this point, a critical analysis of the work of Vygotskij, Holzkamp and Riegel is by far the most interesting for psychologists.

THREE MAIN CURRENTS

In closing this introduction, we shall briefly go into a number of facets of these three schools. It is appropriate to begin a book on critical psychology with a chapter on the oldest and most extensive critical-psychological school, the Cultural-Historical School. This school arose in the Soviet Union as a reaction to the dilemma "understanding" versus "explanation" raging in psychology at the beginning of this century. Dissatisfied with a division of labor in which lower, reflexive behavior was studied by psychologists oriented towards the natural sciences and the higher processes (creativity, thought, problem-solving, etc.) by psychologists with a phenomenological orientation, Vygotskij preached the third approach of a causal, i.e. "natural scientific" analysis of the higher psychological processes as well. This, then, would be a way of mending the rift in psychology. Central to this analysis was the notion that higher psychological processes are developed through culture and that therefore, their origins and development can be followed and analyzed each time anew. Vygotskij's analyses of the origins of higher functions in human history have produced interesting viewpoints on the organization of the brain, early cognitive development, education, intelligence, consciousness etc. In this connection, it is also of interest to investigate, from a methodological point of view, how the Cultural-Historical School conducted their research. In doing so, we can see that in particular, Holzkamp's Critical Psychology has considerably expanded upon the Cultural-Historical School's methodological arsenal.

In the last decade, the Berlin School of Critical Psychology has undergone a tumultuous development. In the United States, Brandt (1979), in particular, has devoted some attention to the early critical and constructivistic work of Klaus Holzkamp. In the seventies, however, Holzkamp's enormous productivity and enthousiasm quickly raised this Berlin School above the level of pure criticism of traditional theories and methodology. Inspired by the Cultural-Historical School, in

particular the work of A.N. Leont'ev, he and his associates have worked on a comprehensive methodology for psychological research, in which the formulation of theories and hypotheses is subjected to methodological criteria. Traditional psychology lacks an inductive logic of the context of discovery, so that in the very decisive, initial stage of the research process, methodology leaves the researcher to his own devices. In research practice, this has resulted in an almost anarchistic "accumulation" of scientific knowledge, which in and of itself may be regarded as one of the most important causes of the present crisis. By means of the functional-historical method, the Berlin School proposes to meet the researcher's demands for a more regimented means of formulating theories and concepts. In addition, this school has also had a great deal to say about the stage in which theories and hypotheses are tested, for which (somewhat adapted) empirical methods as the experiment, action research and statistical techniques play a vital role.

As did Berlin Critical Psychology, Dialectical Psychology also linked up (via Rubinštejn) with the ideas of the Cultural-Historical School. The important initiator of this American variant of Critical Psychology was Klaus Riegel. Based upon the work of Rubinštejn, and in contrast to that of Piaget, he attempted to design a developmental psychology covering every phase of life. Development is interpreted dialectically as the result of conflicts or asynchronisms between the four most important aspects of human existence, i.e. the internal-biophysical, the individual-psychological, the cultural-social and the external-physical aspects. Dialectical Psychology wishes to understand the changing individual in a changing society and, in the area of methodology, calls for a subject-subject relationship between researchers and the participants in their experiments. But in contrast to Berlin Critical Psychology, this does not result in an unequivocal preference for action research. The dialecticians do, however, agree on the importance of a sociology of psychological knowledge, in this case an approach through ideology-critique, in which an investigation is made of the historical and social context of psychological theo-

ries. To our mind, the dialectical ideas are not, in all instances, formulated with equal clarity nor developed in equal detail. But they have drawn such enthousiastic reactions in the United States that a further inspection of these ideas would appear desirable. The most important merit of this school is without doubt that it has resulted in a renewed interest in the study of the philosophical and methodological foundations of psychology.

PART I

HISTORICAL ROOTS OF CRITICAL PSYCHOLOGY

Chapter I: The Cultural-Historical School

INTRODUCTION

Lev Semenovič Vygotskij may be regarded as the founder of the cultural-historical theory. He was of the same generation as the Swiss Jean Piaget, but unlike the latter has yet to achieve a place in most introductory textbooks. There are several reasons for this. Firstly, of course, there is the language problem. Only very few Western psychologists can read Russian and but a fraction of Vygotskij's work has been translated into English. His best known work is, of course, "Language and Thought". Unfortunately, this is a drastically abridged version of the original, in which nearly all references to its philosophical (Marxist) backgrounds have been deleted. The serious student must therefore rely upon the original or other translations (see Vygotskij, 1982b, 1977a). A lot of the literature published in English can be found in Vygotskij (1978). Recently, Wertsch in particular has done much to clarify and propagate Vygotskij's legacy (Wertsch, 1978, 1979, 1980, 1981a, 1981b). A second complication in evaluating Vygotskij is the volume of his collected works. In approximately ten years, Vygotskij wrote some two hundred articles and books on the most diverse subjects. Even in the Soviet Union,

all his work has not yet been published. Only when his collected works have been published in their entirety will it be possible to make a balanced evaluation of Vygotskij's contribution to psychology. For this chapter, only three (Vygotskij 1982a, 1982b, 1983) of a total of six volumes were available to us. Finally, a correct evaluation of Vygotskij's significance to psychology is hindered by the fact that his work is closely interwoven with a philosophical background relatively unknown in the West. Vygotskij was not only a psychologist, but a methodologist and a philosopher as well. He attempted to chart out a new vision of psychology with a foundation in Marxist-Leninist thought. Some knowledge of the work of Hegel, Marx, Engels en Lenin is therefore indispensable when reading Vygotskij's work.

In this chapter, we shall expound upon the main theses of the Cultural-Historical School, based upon the empirical research and ideas of the men who inaugurated this psychological school: Vygotskij, Lurija and Leont'ev. Most attention by far will be devoted to the figure of Vygotskij, being the great theoretician and inspirer of the three. In the 20's and 30's certainly, the merit of Leont'ev and Lurija was more in their conducting competent empirical research than in constructing new theoretical conceptions. Only later did Lurija (see Cole, 1978) and Leont'ev (see Wertsch, 1981b) set out on their own courses and in fact, it is still being investigated how the three researchers influenced one another (Radzichovskij and Chomskaja, 1981). In discussing the Cultural-Historical School, it is our assumption that it is incorrect to separate the psychological ideas of this school from its philosophical background. Although these ideas are quite valuable on their own, many of the epistemological and methodological notions bound up in Vygotskij's work would be missed if their philosophical background were ignored. But we are not attempting to provide a more or less complete description of the socio-economic, ideological and political circumstances under which the Cultural-Historical School developed. Such an externalistic description is beyond both our competence and the scope of this book. For this, the reader may consult, for

example, Bauer (1952), McLeish (1975), Kolakowski (1981) or Joravsky (1961). Our main objective here is to exhibit the viability of the ideas of Vygotskij and his associates. However, for a better understanding of his ideas, we shall devote the first part of this chapter to a rather extensive look at their historical background.

L.S. VYGOTSKIJ. PERIODS IN HIS WORK

It is beyond question that Lev Semenovič Vygotskij made an unforgettable impression upon his contemporaries. Those associated with him in the 20's and 30's speak of him in almost lyrical terms, even decades afterward. Zaporožec, who met Vygotskij as a student and was later associated with him at the Second State University of Moscow and at the Krupskaja Academy for Communist Education, compared Vygotskij to a "fire-spitting smelt-oven", continually emitting new ideas, new conceptions, new hypotheses, new original experimental plans (Zaporožec, 1966, p.180). Lurija is, if possible, even more laudatory and calls Vygotskij a genius: "Through more than five decades in science I never again met a person who even approached his clearness of mind, his ability to lay bare the essential structure of complex problems, his breadth of knowledge in many fields, and his ability to foresee the future development of his science" (Lurija, 1979b, p.38).

Apart from this, it is clear that Vygotskij was an extremely versatile thinker and enormously energetic. Vygotskij was born on November 5th, 1896 at Orsja, a village in White Russia northeast of Minsk. He attended the Gymnasium at Gomel, graduating in 1913 with a gold medallion. After this, he studied law, literature, philosophy and psychology in Moscow. In 1917 he graduated with a specialization in literary science and very quickly began to publish. In order to see Vygotskij's publications in the proper light, it is important to realize that he underwent a pronounced development in which four more or less clearly delineable periods can

be distinguished. In discussing a number of themes from Vygotskij's work in the rest of this chapter, we shall also have to take into account the period in which these themes were dealt with.

The first period, running from approximately 1917 to 1924, is marked by the book "The Psychology of Art" (1971). The second period runs from 1924 to 1927. In this period, Vygotskij attempts to develop an objective psychology. To this end, he analyzes, among others, fundamentally differing psychological schools of that period, resulting in the marvelous essay "The Historical Significance of the Psychological Crisis" (Vygotskij, 1927: in Vygotskij, 1982a). In the third period, which can be estimated from approximately 1927 tot 1930/31, Vygotskij developed his famous cultural-historical theory of the development of the higher psychological functions. And finally, in 1930/31, Vygotskij shifts his attention to the problems surrounding thought and speech and the development of these processes during child development. This research culminated in his 'unfinished', the well-known "Thought and Language" (1962). Only this last book has been extensively studied in Anglo-Saxon countries. It is therefore not surprising that this last, fourth period is regarded as his most important one by Western (but also some Soviet) researchers, also because in "Thought and Language" there are a number of interesting changes in Vygotskij's research. For example, Vygotskij shifted his attention from the cognitive factors in children's thought and speech to the emotional and motivational factors and began to concentrate his research upon the meaning (značenie) of words instead of the words as such.

The third period is also generally acknowledged as being very important, as in this period Vygotskij formulated his famous cultural-historical theory (see below). It is the cultural-historical theory that is still regarded as the most important psychological and educational theory in the Soviet Union. At present, the cultural-historical theory's influence is mostly felt through Leont'ev's theory of activity (dejatel'nostnyj podchod). For some time, there has been a debate on the

differences between the theories and their respective merits (see, among other, Asmolow, 1982; Davydov and Radzichovskij, 1980a+b, 1981; Davydov, Zinčenko and Talyzina, 1982; Radzichovskij, 1982; Luckov and Pevzner, 1981; Wertsch, 1981b). A controversial question, for example, is whether Leont'ev's concept of activity should be regarded as an important complement to and correction of Vygotskij's theory (A.A. Leont'ev) or whether the notion of "activity" is one Vygotskij may never have mentioned but, in essence, had already dealt with (V.V. Davydov). Though it is therefore logical to regard the third and fourth phases of Vygotskij's work as the most important ones, it is now also beginning to be realized that a correct evaluation of his work is impossible without thoroughly investigating the first two phases. We shall therefore now provide some information on these first two periods of Vygotskij's work.

The First Period

Several authors, for example Hydén (1978, 1980), Mecacci (1976), Jaroševskij and Gurgenidze (1977; 1981, 1982) and Leont'ev (1982) place the first phase of Vygotskij's scientific development at about 1917 to January 1924, when he began to work at the Moscow Institute of Psychology under the direction of Kornilov (see Lurija, 1979b for background). At that point, Vygotskij's career begins as a psychologist in the strict sense of the word. Not a great deal is known about this first period. We do know (Leont'ev, 1982; Lurija, 1979b) that Vygotskij studied law and philosophy. He was also interested in theatrical sciences and wrote reviews of theatrical performances. In addition, he taught at a teacher's college at Gomel and spoke at seminars on political economy. He founded the literary magazine "Verask" in which he published his first literary-psychological studies. His main theme was to study the classics of world literature, for example the works of Tolstoy, Dostoyevsky and Shakespeare. In doing so, his primary objective was to uncover the psycholo-

gical laws at the basis of the aesthetic experience a reader has when enjoying a good book. As we know, this study resulted in his "The Psychology of Art" (1925). It would seem that this literary period in Vygotskij's work is not of particularly great importance to psychology and education. Yet this is not the case. The important role language (signs) plays in Vygotskij's cultural-historical theory can, in part, be explained through his literary studies. "All the basic systems of psychological functions are dependent upon the level the child has reached in the development of word meanings", Vygotskij writes in 1932 (in Vygotskij, 1982b, p.415). A book such as "Thought and Language" could not have been written without this literary schooling. Several of the ideas expressed in this book are now traced to the work of the Russian linguist Potebnja. Among other things, this researcher from the Humboldt school had already expressed the idea of the various historical origins of thought and speech (as discussed by Vygotskij in chapter 4 of his "Thought and Language") and expounded the notion that the word or sign should be seen as a tool or instrument of thought (see *Potebnja, 1922 and Piskun and Tkačenko, 1981*). More than once, Wertsch has also pointed to the importance of the semiotic aspect of Vygotskij's work (Wertsch, 1979, 1980, 1981a).

In addition to this linguistic influence, the first period in Vygotskij's work is of particular importance in that he made acquaintance with classical German philosophy and with Marxist writings. We shall later see how this acquaintance influenced his cultural-historical theory. Finally, Vygotskij absorbed himself in this period in the work of Descartes and Spinoza. As we know, he wrote an essay on the significance of the work of these two philosophers for psychology, which will be published in the last volume of the Collected Works. In particular, Vygotskij saw Descartes as the founder of dualism in psychology and education. The division between a descriptive (Verstehende) approach, examining the higher processes of the human psyche, and an objective approach, dealing with the lower processes, has its origin in Descartes' division between the *res cogitans* and the *res extensa* (see

below). Within Soviet psychology, this analysis has also been endorsed by Rubinštejn (see Payne, 1968). On the other hand, Spinoza fascinated Vygotskij especially because of his monistic and deterministic analysis of the psychophysical problem (Vygotskij, 1970; Van der Veer, 1983a).

The Second Period

The second period in Vygotskij's work is characterized by the intensive study of the psychological and educational literature of his age, among others psychoanalysis, the available works of Piaget, Gestalt psychology, philosophical education etc. (see below). In this period, it is striking how reflexological and reactological Vygotskij's ideas still are. This influence is also to be seen in his evaluation of the work of someone as Thorndike, whom Vygotskij extols, and in his notion of what education is. In 1926, for example, he still wrote; "That is why the psychologist is quick to define education as a process of the accumulation and elaboration of *conditional reactions*, a process of the adaptation of inherited behavior to environmental conditions, a process of the creation of new relationships between organism and environment, that is, a process determined at every point" (in: Vygotskij, 1982a, p.192). About 1930, Vygotskij would have very different things to say about education. "Education can be defined as the artificial development of the child. Education influences not only one or another developmental process, it also restructures all behavioral functions in the most fundamental way" (in: Vygotskij, 1982a, p.107). Despite the great differences between the two quotations, it is true that they have as common element the regulatory influence of the environment. And it is this underscoring of the social determination of education that prevented Vygotskij from becoming a complete reactologist, reflexologist or behaviorist. In this second period, he criticizes Thorndike for almost entirely ignoring "the social aspect of education ... . Education is directed from teacher to pupil, and is always extremely individualistic and is reminiscent of

... an educational duet between teacher and pupil" (Vygotskij, 1926, in: Vygotskij, 1982a, p.180).

Finally, this period is concluded with an extremely interesting, approximately one hundred and fifty page essay entitled "The Historical Significance of the Psychological Crisis. A Methodological Study" (Vygotskij, 1927, in: Vygotskij, 1982a). In this study we encounter Vygotskij as an accomplished philosopher of science and methodologist. Implicitly or explicitly such burning questions as the relationship between theory and practice, the role of the experiment and methodology, and the internalism-externalism question with respect to the development of a science are all reviewed. And Vygotskij's treatment of these problems has a surprisingly up-to-date ring to it. His criticism, for example, of the empiricist tendencies in psychology is still defensible. While thousands of miles away, Carnap and others were laying the foundations for logical-positivism, Vygotskij was already undermining a number of its suppositions. He argued, for example, that in principle, an objective, atheoretical registration of pure facts is impossible. Vygotskij: "In a very disguised form, and imperceptibly to researchers themselves, theoretical suppositions entirely determine the way empirical data are interpreted: the interpretation of the facts obtained through observation in accordance with the theory one or another author supports" (Vygotskij, 1927, in: Vygotskij, 1982a, p.342). Vygotskij also contests the notion that such sciences as physics are concerned with a process of objective registration, finished off by a blind induction procedure. "The scientific study of facts distinguishes itself from registration in that it is an accumulation of concepts, transaction of concepts and facts with gains on the concept side" (o.c., p.317) and "In this way, physics studies the very things that are invisible to the eye ... But that is exactly the point, that scientific knowledge and direct observation do not coincide at all" (o.c., p.343). "It is usually said: history interprets traces of the past, but with the help of instruments, physics observes the invisible as directly as the eye. The instruments are the extended organs of the scientist: microscope, telescope, tele-

phone, etc., make the indirect experience a research-object and make the invisible visible: physics does not interpret, it sees. But this notion is erroneous" (o.c., p.345).

Later, he posits that there is no fundamental difference between the use of instruments in physics and interpretation in psychology. The use of a certain instrument means the endorsement of a certain theory of perception, we would say today (see chapter II). As does the eye, the instrument selects certain phenomena "... both are selective organs ... So that scientific knowledge's overstepping the boundaries of the observation is rooted in the psychological essence of obtaining knowledge itself." (Vygotskij, 1982a, p.348). Vygotskij is convinced that a registration-induction procedure can never lead to scientific knowledge, that we are dealing here with a fiction. Firstly, this "registration" is always a selection based upon theoretical notions already available. Secondly, Vygotskij posits that the origin of a certain phenomenon can only be understood through an analysis, a reconstruction of ideas after the fact. Here, he even inclines at times towards a constructivistic notion of science. "How do the sciences act in studying that which is not directly evident to us?", Vygotskij wonders. "Generally speaking, they construct, create the object of study by the method of explaining or interpreting its influence, in other words indirectly" (o.c., p.344). It is also interesting to note the role Vygotskij reserves for the experiment in this (re)construction. Though we know that he was sharply critical of the experiment as it was then conducted (Van der Veer, 1983b), he certainly does not reject it. "It might appear that analysis, like the experiment, distorts reality. Hence the demands of authenticity and naturalness made upon the experiment. If this idea goes farther than the technical requirement of not frightening off what we are investigating, it leads to absurdity. The strength of the analysis lies in the abstraction as the strength of the experiment in artificiality" (o.c., p.406).

In this essay, Vygotskij's notions concerning the role of observation and the instrument in particular have an unmistakably modern ring to them. Many years later, similar statements were made, for example by Lakatos and Feyerabend (see chapter II). Acquaintance with this study, then, is in itself quite interesting. In addition, Vygotskij's methodological thesis has influenced his psychological research for the cultural-historical theory. Later, as well, we see that Vygotskij confines and qualifies the role of observation in psychological research, and emphasizes theory. Readers at all familiar with the work of Marx and Engels will have recognized many of Vygotskij's statements. In their epistemology, there was, for example, also no objective registration of data. Marx states that perception is itself a component of man's practical relationship to the world, so that its object is not simply "given" by indifferent nature (Kolakowski, 1981, p.142). In the paragraphs to follow, the influence of Marx and Engels on Vygotskij's work will be apparent many times. First, however, we shall discuss Vygotskij's relationship to the most important psychologists of his time.

VYGOTSKIJ'S CONTEMPORARIES

"It is probably impossible to assess all the influences on us as we undertook a grand revision of psychology back in early 1925 ..." writes Lurija in his intellectual autobiography (Lurija, 1979b, p.41). An examination of the crisis in psychology to be described later in this chapter indicates that Vygotskij, Lurija and Leont'ev read practically all the important publications in the area of philosophy and psychology. For example, they were, of course, familiar with the ideas of such thinkers from the objective school within psychology as Wundt, Ebbinghaus and Fechner. On the other hand, they were also very conversant with the ideas of James and Dewey and with those of such phenomenological psychologists as Dilthey and Spranger. "I wanted a psychology that would apply to real people as they live their lives, not an intellectual abstraction

in a laboratory ... I looked for alternatives in the books of scholars who were critical of laboratory-based psychology. Here I was influenced by the work of the German neo-Kantians, men like Rickert, Windelband and Dilthey." Lurija was particularly attracted to the ideas of Dilthey concerning a realistic psychology, but influenced by Rickert's criticism, he later realized that a descriptive approach in psychology is insufficient (Lurija, 1979b, p.22 ff).

Freud

One of the most important psychological schools of that time was composed of adherents of the psychoanalytic theory of Freud, Jung and Adler. We know that Vygotskij and Lurija assumed a rather balanced attitude towards psychoanalysis. Originally, Lurija was enthousiastic: "Sigmund Freud's *The Interpretation of Dreams* and several other of his early books had been translated into Russian, and other writings of his, as well as of Alfred Adler and G.C. Jung (including his *Studies of Diagnostic Association*) were available in German. Many of Freud's ideas seemed speculative and somewhat fantastic to me, but the study of emotional conflicts and complexes using the method of associatons seemed promising. Here, I thought, was a scientific approach that combined a strongly deterministic explanation of concrete, individual behavior with an explanation of the origins of complex human needs in terms of natural science. Perhaps psychoanalysis could serve as the basis for a scientific *reale psychologie*, one that would overcome the nomothetic-idiographic distinction" (Lurija, 1979b, p.23). Lurija even established a small psychoanalytic circle and corresponded with Freud himself. In the years that followed, Lurija's interest led to a number of analyses of psychiatric patients with the aid of Jung's method of free association. He also wrote a number of articles on psychoanalysis and started a book that would never be completed (Lurija, 1925, in: Cole, 1978). "But I finally concluded", Lurija wrote, "that it was an error to assume that one

can deduce human behavior from the biographical 'depths' of mind, excluding its social 'heights'." (Lurija, 1979b, p.24).

Cole notes that this reversal was attributed to two factors: firstly, Vygotskij's influence and secondly, the growing conviction that Marxism and psychoanalysis were not compatible (Cole in: Lurija, 1979b, p.203). And indeed, Vygotskij's criticism was scathing. He believed Marxism and psychoanalysis to be incompatible and that enumerating the formal logical similarities of the two approaches, or combining them eclectically was only possible by ignoring the essence of both approaches. In 1927, he makes a sarcastic assault on the fragment of Lurija's book mentioned above. In this study, entitled "Psychoanalysis as a system of monistic psychology", published in a volume edited by Kornilov, Vygotskij accused Lurija of arranging the "coincidence" of the two approaches by means of a series of "extremely naïve reforms." Vygotskij reproaches his associate for not having conducted a more thorough methodological analysis and concluding that psychoanalysis and Marxism coincided "without any analysis of Freud's basic concepts, without a critical weighing or examination of his suppositions and assumptions, without a critical elucidation of the origins of his ideas, even without asking the simple question of how he himself conceived of the philosophical foundations of his system, but by the simple formal logical accumulation of characteristics" (Vygotskij, 1927, in: Vygotskij, 1982a, p.331).

This is not to say that Vygotskij's attitude to psychoanalysis was entirely negative. He found Freud an extremely interesting thinker and endorsed many of Freud's discoveries, although regularly rejecting his interpretations. Concerning the unconscious, for example, Vygotskij writes: "and we therefore do not believe that in psychology it is entirely justified to speak of the psychological conscious and the psychological unconscious: the unconscious is the potentially conscious" (1930, in: 1982a, p.146). In his interpretation of the unconscious, however, Vygotskij tends towards the notion of Watson. As we know, Watson had asserted

that what Freud called the unconscious was essentially non-verbal. We cannot remember occurrences from our early childhood because language was not yet available to us as an efficient method of storing experiences. According to Watson, it is the weakness of psychoanalysis that it attempts to exert influence upon non-verbal processes with the aid of verbal expedients. As speculative a notion as that of the death-wish is also received favorably by Vygotskij. In a preface to one of the approximately six books of Freud's translated into Russian, he defends the necessity of this notion (p.16). "In doing so, I did not declare Freud's solution to this equation to be a main road in science, nor a road everyone could follow, but to be an Alpine path above the abysses for those unaffected by dizziness. I stated that science also needs such books: they do not uncover any truths, but accelerate the search for truths, though as yet none have been found. I also stated resolutely that the significance of this book is not dependent upon the actual inspection of its validity. In principle, the problem is posed correctly. And posing such questions, I said, requires more creativity than the subsequent observation according to the standard method of whatever science." (Vygotskij, 1927, in: Vygotskij, 1982a, p.336). It is clear from these words that it was not only Lurija that had a great appreciation of Freud.

So although both Lurija and Vygotskij were not blind to the value of depth psychology, they were ultimately convinced that the emphasis upon the biological nature of the human being such as found in Freud's work does not do justice to the phenomena in question. Vygotskij, indeed, emphasized the social origin of higher psychological processes. Apart from that, it is remarkable that both Vygotskij and his great opponent in the area of child development, Jean Piaget, were influenced by Freud and that Vygotskij often criticizes Freud's ideas through Piaget.

Piaget

It is apparent that Vygotskij was impressed by the importance of the work of the young Piaget, not only because on several occasions he extols Piaget directly, but also by virtue of the fact that in reality, a large portion of his "Thought and Language" consists of a polemic with this Swiss researcher. Vygotskij had great admiration for the precision with which Piaget uncovered the singular nature of children's thought and for the wealth of factual material this made available to psychology. But he did often interpret this factual information in an entirely different fashion. The main theme of the polemic between Piaget and Vygotskij concerns the role of the biological and social aspects of the child's psychological development. With respect to Piaget's notion of the development of thought, for example, Vygotskij writes as follows: "In this connection, Piaget's conception is utterly simple. Along with the psychoanalysis of S. Freud and that of E. Bleuler associated with him, Piaget assumes that the first stage in the development of child thought is thought guided by the pleasure principle. In other words, the young child thinks with the same objective as lies behind all its other activities, that is to obtain pleasure. In this regard, Piaget, like Freud and Bleuler, presents the thought of the young child as an exclusively biological activity of a partly instinctive nature, directed towards the obtainment of pleasure. But the more the child develops, the more continual are the conflicts with the social environment, demanding adaptation to an adult's way of thinking. The interaction of the child with its surroundings requires it to understand the thought of others, to reply to that thought, to communicate its own thought. From all these types of social interaction develops the process Piaget so expressively called the socialization of children's thought. The process of the socialization of children's thought is reminiscent of the process of the "socialization of private property". Child thought as something belonging to the child, constituting its "private property" as a particular biological individual, is supplanted, replaced by modes of thought thrust upon it

by its surroundings" (Vygotskij, 1932, in: Vygotskij, 1982b, p.403-404).

In a few sentences, Vygotskij here sketches the Piagetian notion he would also contest in "Thought and Language". In his view, the child is not by nature a biological individual later to be socialized under pressure from its environment. In later paragraphs, we shall see that to Vygotskij, the child is far sooner social by nature, developing through interactions with adults. He classifies the stage of a-social, primitive biological development, encountered in Freud, Bleuler and Piaget, as being within the realm of fiction. It should be noted, of course, that Vygotskij's criticism concerns Piaget's early work (Piaget, 1923, 1927, 1933; see also Gulutsan, 1967).

Pavlov

To find a solid objective basis for psychology, Vygotskij, Lurija and Leont'ev turned to Pavlov's research into higher nervous activity. "The basic structural units that produced adaptive adjustments to the environment were then being studied by Pavlov and his co-workers at their experimental station near Leningrad. Pavlovian psychophysiology provided a materialistic underpinning to our study of the mind." (Lurija, 1979b, p.41). From the early writings of Vygotskij and his co-workers, it is quite clear that they were very impressed with Pavlov's research into the conditioned reflex. In 1924, Vygotskij's view is that acquired reactions (conditioned reflexes) develop on the basis of innate reactions (unconditioned reflexes) and are themselves, in essence, also combinations of innate reflexes. The composition of these combinations are entirely dictated by the environment. Vygotskij: "It is the very organization of the environment that also determines the conditions upon which the formation of new associations depends, the foundation of animal behavior. In relation to each of us, the environment plays the role of the laboratory, in which

conditioned reflexes are taught to dogs, and which, by combining and uniting stimuli (meat, light, bread and metronome) in a particular way, again and again superbly organizes the animal's behavior. In this sense, the mechanism of the conditioned reflex is a bridge joining the biological laws concerning the development of hereditary adaptations established by Darwin with the sociological laws established by K. Marx" (1924, in 1982a, p.73). Pavlov's influence on Vygotskij would last several years at least. It expresses itself, for example, in the laudatory words he dedicates in this period to reflexology, reactology and behaviorism. As late as 1926, in an introduction to the Russian translation of Thorndike's "Educational Psychology", he concludes "that behavior should be regarded as the total sum of reactions" (1925, in 1982a, p.189). Gradually, however, Vygotskij and his co-workers recognize more and more limitations in Pavlov's research.

In the late 20's Vygotskij commented rather extensively on the place of Pavlov's physiology in the study of human behavior. He agrees with Pavlov that the most general principles of higher nervous activity are identical for human and higher animals. But as soon as we move from the elementary to the complex forms of higher nervous activity characteristic of human beings, Vygotskij discerns two different methodological approaches to studying the features that distinguish man's complex behavior. The first approach Vygotskij discerns is that of investigating in humans the same principles uncovered in studying animals. As does Pavlov, he believes that this method (comparative psychology) should only be applied with the greatest of caution. There is every likelihood, Vygotskij believes, that the qualitative difference with animal behavior will also be expressed in physiological research.

The second approach is actually psychological research. Here the assumption is that there are specific forms of human behavior. The first specific aspect of human behavior lies in its higher complexity and development, the qualitative and quantitative perfection of the cerebral hemispheres. But according to

Vygotskij, the most important difference lies in the fact that humans develop new ways of adaptation, distinguishing them from any animal. Pavlov believed that the foundation of behavior is signalization. The most general description of cerebral activity is that it is signal activity, in other words, that conditioned reflexes are the basis for the physiology of higher nervous activity. Vygotskij: "But human behavior is distinguished by the very fact that man creates artificial signal stimuli, among which is the 'great signaling capacity' of speech, and thus masters the signal activities of the cerebral hemispheres. The fundamental and most general activity of the cerebral hemispheres in both man and animals is signalization; but the fundamental and most general activity, distinguishing man from animals, psychologically speaking, is signification, i.e. the creation and use of signs" (1977b, p.62). At this point we shall not go farther into the distinction between signification and signalization, as it will be discussed with Vygotskij's cultural-historical theory. It is sufficient at this point to note that in later years, Vygotskij qualifies the value of Pavlov's physiological research, for it cannot explain truly *human* behavior.

Gestalt Psychology

One of the most important psychological schools in the first half of this century was Gestalt psychology, with its representatives, among others, Koffka, Wertheimer, Lewin and Köhler. There was lively interest in the Soviet Union as well and books and articles of Köhler and Koffka were translated into Russian. Vygotskij's most extensive reaction to the ideas of the Gestalt psychologists appeared in 1934 as a preface to the translation of Koffka's "Die Grundlagen der psychischen Entwicklung". He is particularly sympathetic to Gestalt psychologist's attempts to bridge the methodological gap in psychology. In this regard, Köhler, Koffka and others criticize both "mechanistic" and "vitalistic" psychology. Koffka states that the

mechanists conceive of nature as a totality of machines. Human behavior is also reducable to the operation of a living machine. In opposition to the machine-model of humanity and in opposition to a psychology that explains the whole of human behavior by analyzing the parts (elements) it is composed of, Koffka posits the specific character of the totality. The whole is more than the sum of its parts. The existence of so-called emergent characteristics makes it impossible to reduce, as the mechanists do, the complexity of human functions.

From this vantage point, the Gestalt psychologists also criticize the vitalistic, descriptive psychologists, who also accept the mechanistic approach to the physical reality, but do not consider it applicable to human life. Their approach only exists by virtue of an erroneous conception of a physical reality which they accept with regard to nature but reject with regard to human life, Vygotskij writes. He is very sympathetic to this analysis of Gestalt psychology, but believes that the psychologists of the Gestalt school themselves have not eluded the evils of reductionism. Although they reject the atomistic approach of reality and the reduction embodied in the machine-model of humanity, they are guilty of another form of reduction, namely that of reducing human behavior to animal behavior. The Gestalt psychologists saw no fundamental transition between an animal's ability to solve problems and that of human beings. To refute this notion, Vygotskij discusses Köhler's famous experiments designed to determine a chimpanzee's ability to solve complex problems. At some point in working on a problem, the chimpanzees appeared to grasp the problem's inner relationship through insight. Köhler, however, showed that in fact, the animals are the slaves of their visual field. This is a fundamental difference with human beings. When children are presented with Köhler-type problems, it is quickly apparent that language plays a decisive role in their behavior. Vygotskij claims that the child does not operate in a visual field but in a semantic field. "As experiments have shown", Vygotskij writes, "the word liberates the child from the servile dependence Köhler observed in animals" (1934, in: 1982a, p.265).

Vygotskij also reproaches the Gestalt psychologists for not explaining child development. He states that for Koffka, Gestalt structures are as characteristic of the perception of the child as they are of that of the adult. A fundamental gap cannot be observed anywhere. In 1924, thus after his cultural-historical theory had already been developed, Vygotskij on the other hand does see fundamental differences between the perception of the very young child and that of the adult. There is a transition from passive, direct perception to active, mediated perception. The young child, as yet unable to use signs (language), perceives very differently than the adult, whose perception is greatly influenced by linguistic concepts. This same development from direct, passive functioning to mediated, active functioning also occurs in other psychological processes such as memory. According to Vygotskij, the Gestalt principle is thus "anti-historical [for] it is as applicable to the instinct as to mathematical thought. We must seek that which raises the development of the child above the Gestalt principle" (Vygotskij, 1934, in: 1982a, p 282).

Finally, Vygotskij rejects a too broad usage of the notion "Gestalt". This notion had demonstrated its value in the psychology of perception, but should not come to be looked upon as a kind of metaphysical category in reality. Vygotskij: "Finally, having become a world view, Gestalt psychology discovered the Gestalt in physics and chemistry, in physiology and biology, and the Gestalt, withered down to a logical formula, proved to be the foundation of the world; when he created the world, God spoke, let there be Gestalt - an everywhere, there was Gestalt" (Vygotskij, 1927, in 1982a, p.307-308).

Despite these critical notes, Vygotskij had a great appreciation of this school. Another indication of this is the association he developed with Koffka, who, for example, participated in the research project in Uzbekistan (see below). Scheerer (1980) has shown that there was a lively exchange of data between such Gestalt psychologists as Koffka and Lewin, and Vygotskij and Lurija. Only towards the end of the 30's, when

the cultural-historical theory began to assume concrete form, did Vygotskij and his associates succeed in escaping the appeal of Gestalt psychology.

Other Influences

In order to complete our picture of Vygotskij's relationship to the psychology of his time, we shall briefly discuss a number of other psychological schools, after which attention will be paid to a number of influences not strictly of a psychological nature.

What has been said about Vygotskij's attitude towards Gestalt psychology applies in fact to his attitude towards the psychologists from the Wurzburg school as well (Külpe, Ach, Bühler, and others). Often using the research data of Bühler in particular, and just as often rejecting their theoretical interpretation, Vygotskij continues to criticize them at the methodological level. He regards Külpe's adherents as the subjectivistic school in psychology. Nor does Watson's behaviorism escape Vygotskij's criticism. As we know, in about 1925, a number of psychologists in the United States and the Soviet Union attempted to create an "objective" psychology. In particular, they were opposed to the use of introspective reporting on internal experiences. In its place, they wished to initiate a psychology based upon intersubjectively testable data. In this attempt, the objectively measurable reflex gradually received more and more emphasis, while such matters as consciousness, difficult as they are to measure, disappeared from the field. Though Vygotskij was initially greatly influenced by the reflexologist movement (witness, among other things, the notion of consciousness he developed at that time), he quickly recognized its limitations. As early as 1925, he criticizes Bechterev's reflexology and the objective of a "psychology without consciousness". Such a psychology has the following limitations.

By placing consciousness beyond the realm of psychology, important problems of human behavior are lost sight of. In Bechterev's reflexology only the most elementary reactions are investigated. The attempt to construct a psychology without consciousness has also resulted in the study of only those reactions visible to the naked eye. Psychology is then "completely powerless and weak, even in investigating the simplest problems of human behavior" (Vygotskij, 1925, in: 1982a, p.79). Every fundamental difference between human and animal behavior is erased. Human behavior is investigated as that of a mammal, while that which is fundamentally different, brought about by consciousness and the psyche, is ignored. It is clear, Vygotskij says, that a law of animal psychology cannot be automatically extrapolated to human psychology. In excluding consciousness from psychology, the dualism and spiritualism of the earlier subjective psychology remains largely unchanged. It was subjective psychology's objective to study the pure, abstract intellect. In behaviorism, we encounter its counterpart; while the former studied the psyche without behavior, the latter studies behavior without the psyche. The dualism of mind and body remains unaffected (Vygotskij, 1982a, p.80-81). Finally, Vygotskij states that from a biological point of view, those who recognize consciousness, but regard it as an unimportant derivative, are expressing an absurd notion. Why, then, would such a useless phenomenon have survived the course of evolution?

At this point, some words should be devoted to two more diffuse influences every early twentieth century social scientist unavoidably encountered, namely: the collection of ideas usually described as romanticism and the influence upon the social sciences of Darwin's theory of evolution. As regards the theory of evolution, Darwin published in the 1860's his "The Origin of Species", in which he argued that there was an uninterrupted transition between human beings and other animals. Seen from the vantage point of the biologist, there is no fundamental difference between animals and human beings. Accepting the theory of evolution means, in any case, two things. Firstly, one accepts the notion that human beings have not always had the same

structure and characteristics, but have become what they are in the course of evolution. Secondly, one accepts the importance of comparative study of humans and animals. With respect to the former consequence, Vygotskij's position is as follows: It is true that humans have undergone a biological evolution. But that is not what makes them genuinely human. The truly human processes have developed in a social process. Here, he also qualifies the value of comparative psychology (the second consequence). In discussing the cultural-historical theory, we will discuss this theory more extensively and the sharp criticism it has been exposed to (recently in Brušlinsky, 1979). Suffice it to note here that while accepting Darwin, Vygotskij (after Hegel and Engels) still recognizes a fundamental gap between humans and animals.

As we know, romanticism may be regarded as a reaction to Kant's rationalism and opposed to the spirit of the Enlightenment in general. It is characterized by an appeal to human beings' non-intellectual, emotional powers and an opposition to mechanistic explanations of reality. The criticism of natural sciences' conception of causal explanation was to have its influence upon the humanities and the social sciences. From this characterization of romanticism, it will be clear that Vygotskij, who after all advocated the ideal of a strictly causal analysis in the social sciences, felt no affinity with this school. We have brought up this subject, however, for two reasons. Firstly because in his educational theory, Vygotskij (linking up with Hegel) criticized the ideas of Rousseau, generally regarded as the precursor of romanticism (Gay, 1973). Secondly because to a certain extent, romanticism established the conditions without which a well-founded cultural-historical theory could not have developed. We refer here to the great interest in the origins of language, religion and societal forms that developed at this time. This interest also initiated the beginnings of the systematic study of foreign peoples and cultures. At this point we will suffice with mentioning that in grounding his cultural-historical theory of the higher functions, Vygotskij regularly made use of data provided by cultural anthropolo-

gy (in particular the data of Lévy-Bruhl), a science with its origins in romanticism.

Here we end our paragraph on Vygotskij's contemporaries. Obviously, the above is not at all exhaustive with respect to the many influences upon and by Vygotskij and his associates. For example, separate paragraphs could be written about the influence of the philosophies of Feuerbach and Spinoza on Vygotskij's view of the mind-body dilemma (Van der Veer, 1983a), about Vygotskij's association with the famous filmmaker Ejsenštejn (Lurija, 1979b, p.207) etc. In these introductory paragraphs, though, we hope to have presented the most important of these influences and to have sketched a picture of the intellectual setting and the "Zeitgeist" surrounding the work of Vygotskij and his associates. We have consciously excluded a discussion of the dialectical tradition in philosophy. Because it was of such great importance to Vygotskij's ideas and those of Soviet psychology in general, it merits a separate paragraph.

THE INFLUENCE OF HEGEL

In all likelihood, Vygotskij familiarized himself with the ideas of Hegel primarily through the works of Marx, Engels and Lenin. We know that these three interpreted Hegel's work in their own materialistic way (Kolakowski, 1981; Krancberg, 1981). In this paragraph, therefore, we do not pretend to present the only correct interpretation of Hegel's philosophy. We shall only demonstrate how Vygotskij utilized dialectical philosophy (see also Marková, 1982).

For a proper understanding of Vygotskij and Soviet psychology in general, Hegel's notion concerning scientific knowledge is of great importance. Hegel makes a distinction between appearance (Erscheinung) or semblance (Schein) and essence (Wesen). With this he means that in knowing reality, we first encounter the immediately manifest, the appearance, and only by further

analysis can we penetrate the essence of phenomena. It is our task not to dwell on appearances, but to expose the essence. This distinction is related to the historical approach Hegel advocated. He pointed out that the reality we live in is the product of a lengthy historical developmental process. During this lengthy process, humans have as it were moulded reality to their own ends. To Hegel, reality is a created reality. It is not *imposed by the laws of nature, but produced by humans* themselves in a concrete historical process. Hegel therefore concludes that reality can be altered. In Hegel's analysis, what at first seemed to be inevitably dictated by the laws of nature now appears to have been made by human beings in history. It is the task of science to expose the dominion of the so-called "laws of nature" as semblance and reveal their essence (as historically determined). Whatever one thinks of the idea of the distinction between appearance and essence, it is a fact that it has played an important role in the dialectical tradition of Soviet psychology (as we know Marx adopted this distinction). And it is in this distinction that the Berlin School of Critical Psychology (see chapter II) found one of its points of departure.

Another important theme for Soviet psychology in Hegel's doctrine is his notion concerning the relation between the general (the theory) and the particular (the empirical, the individual) in knowing reality. In this connection, he is in opposition to the inductive theory of knowledge of the empiricists and materialists. According to Hegel every induction contains a theoretical-deductive moment (compare also E. Cassirer, 1923, p.237 ff). Unlike in the work of Locke, humans are not a *tabula rasa*, but start with theoretical premises: the general (the theory) always precedes the particular. This criticism of Hegel's especially influenced Vygotskij in his work with respect to concept formation. Recently, the Soviet educationalist V.V. Davydov has once again attempted to formulate an air-tight theory of concept formation, largely using Hegel as an orientation point (Davydov, 1972).

If we are to see Vygotskij's cultural-historical theory in its proper context, we must certainly devote attention to Hegel's anthropology as well. In Hegel's view of humanity, labor plays a central role. With the aid of this concept, he attempted to break down the time-honored division between subject and object. In the rationalism of Descartes, Leibniz and Wolf, the emphasis was placed upon the subject. It was in the subject, in consciousness, that pure knowledge originated. In empiricism, it is the object that is central; the subject is a *tabula rasa* or nothing more than a collection of experiences generated by objects. But according to Hegel, both schools remain trapped in a subject-object dualism. For Hegel, the subject and the object cannot be separated: without the subject, no object and without the object, no subject. He now presents another category which he believes better suited in understanding human beings, the category of "Tätigkeit" (activity). In Tätigkeit, in labor, subject and object are involved with one another. If human beings are to be understood, their daily activity must be the point of departure. According to Hegel, it is in their labor that human beings know and change their world. Here, too, is to be found the human's fundamental distinction from and superiority to the animal. Unlike animals, human beings are capable of altering reality according to a certain plan. Here we remember the famous quotation of Marx: "The spider carries out operations remeniscent of a weaver and the boxes which bees build in the sky could disgrace the work of many architects. But even the worst architect differs from the most able bee from the very outset in that before he builds a box out of boards he has already constructed it in his head." (Vygotskij, 1978).

Unlike the animal, says Hegel, human beings stand above reality, which they can alter according to certain objectives. The world is not a manifest nature, it is man-made. According to Hegel, human beings are not doomed, then, to submit passively to existing social conditions. They can liberate, emancipate themselves through labor. Labor, defined as changing reality according to a specific plan, is for human beings a means of liberating themselves. Hegel calls this the

externalization of the intellect (the plan). We must remember here that Hegel's system does not concern the activity of the individual but that of the world spirit, with the individual as its tool.

In addition to the externalization in the course of history, the individual also experiences an internalization. In their development (ontogenesis), individuals absorb the culture created in history by humanity (phylogenesis) and internalize it. But individuals themselves need not plod the laborious path of induction in order to attain the achievements of culture, repeating phylogenesis in ontogenesis. These are transmitted through language, labor, communication etc. Hegel sees this process as follows: In language is stored information concerning reality. The concepts adults use express something meaningful about the organization of reality and these concepts are passed on to children through rearing and education. In doing so, the adult passes a part of humanity's cultural heritage on to the child. It would be nonsensical to have the child discover everything all over again on its own (and civilization would be impossible). The same holds true for tools. The structure and form of the tool or instrument reveal something about the knowledge the tool-maker has about the material to be processed or to be regarded by the tool. Knowledge concerning production has been precipitated in the tool. According to Hegel, it is in the process of acquiring language, in communication, in labor and in the use of tools ("means" in Hegel's terminology) that human beings acquire the cultural inheritance of their ancestors.

Finally, a few remarks on Hegel's ideas concerning education and rearing. To Hegel, education and rearing are the means by which a child is liberated from the state of its first nature. In this state, the child follows its natural urges and impulses. This situation is undesirable. Through education, the child must be brought to its second nature. Then it can function as an independent human being. But this transition is not made spontaneously through maturation. It requires good rearing and good education. It is in particular this

rejection of the idea that a child develops purely through maturation (*sensu* Rousseau), and the emphasis upon the important role of education and rearing that Vygotskij was later to absorb into his ideas concerning developing education and the proximal zone of development. We see, then, that Vygotskij adopted many of Hegel's ideas, making fruitful use of Hegel's philosophy in his epistemology, his anthropology and in his ideas on rearing and education.

CRISIS IN PSYCHOLOGY

We have just seen what a motley mixture of psychological schools existed in Vygotskij's day. But according to Vygotskij this is not the whole story. In turn, all these schools can be categorized into two main groups, with their own philosophy of science and their own methodology. On the one hand, there is the objective, natural science psychology and on the other the descriptive or phenomenological psychology. According to Vygotskij, objective psychology advocates a natural scientific philosophy of science. Psychological processes should be analyzed in terms of cause and effect relationships. Lower psychological functions lend themselves best to such a causal analysis, for example elementary reflexes and conditioned reactions à la Pavlov. Humans and animals have these lower psychological functions in common. More complicated are the higher psychological functions. Some advocates of objective psychology claim that these higher functions do lend themselves to causal analysis because all complex behavior can easily be reduced to a combination of simple reactions. They are then advocates of a kind of reductionism propagated by Watson in early behaviorism (*compare* Giorgi, 1976). Other advocates of objective psychology claim that higher psychological functions do not lend themselves to causal analysis in the above sense and therefore elude scientific analysis. For that reason, objective psychology has primarily concerned itself with studying lower psychological functions.

On the other hand, descriptive psychology was strongly critical of this state of affairs. Because of its reductionistic ideal of knowledge, objective psychology lost sight of the unique character of higher psychological functions. With higher psychological functions, they meant such matters as verbal memory, volition, voluntary attention, etc. (see below). These are characteristic of human beings. According to Vygotskij, it was acknowledged within descriptive psychology that a causal analysis of psychological processes should take the form of a reduction to simpler processes. Objective and descriptive psychology then agree as regards the analysis of psychological processes. The descriptive psychologists claim that this form of a reductionist analysis (which they, along with the objective school, see as the only possible one) does not do justice to the higher psychological functions. To understand the higher functions, they propose emphathetic understanding. In this way, it becomes impossible to gain an objective understanding of these functions, according to Vygotskij unjustly so. We have seen, then, that a sort of division of labor took place: the objective natural scientific oriented psychology studies primarily the lower psychological functions and descriptive psychology concerns itself with higher processes. At the same time, both schools agree with respect to the essence of causal analysis, i.e. the reduction of complex processes to simple ones. How, then, does this agreement come about? According to Vygotskij, and to someone as Rubinštejn as well, this occurs because both schools go back to Descartes (Rubinštejn, 1977, p.26).

In essence, both schools remain trapped in a Cartesian mind-body dualism. To Descartes' mind, there is a contrast between incorporeal, non-spatial thought (the *res cogitans*) and the independently existing world of spatiality; the corporeal (the *res extensa*). The body belongs to the corporeal and is subject to the laws of mechanics. The mind, however, is only accessible through inward comtemplation and eludes a mechanistic analysis. According to Vygotskij and Rubinštejn, in Descartes' doctrine are to be found the origins of the later division of labor in psychology: on the one hand,

the mind, only to be observed through introspection, on the other the body, to be interpreted through mechanics. Vygotskij believes that the distinction between lower and higher functions is of essential importance to psychology (in this sense agreeing with descriptive psychology), but believes that the higher functions can be analyzed within the context of an objective methodology. To his mind, the lower functions are of a biological or "natural" origin and the higher functions of a social origin. But that does not mean that the latter therefore elude causal analysis. In his cultural-historical theory of the higher functions, Vygotskij explains how the higher psychological functions can be analyzed (see the next paragraph, which is based on Van der Veer and Van IJzendoorn, 1984).

THE CULTURAL-HISTORICAL THEORY OF HIGHER PSYCHOLOGICAL FUNCTIONS

Phylogenesis: Labor

We have already seen that Vygotskij rejects the approach of reflexologists and behaviorists because they wish to reduce psychology to the study of "only the most elementary connections between a living being and the world" (Vygotskij, 1979b, p.5). He agrees with so-called descriptive psychology's criticism that in the behavioristic approach, the higher functions characteristic of human beings are neglected. On the other hand, he rejects descriptive psychology because it abandons the possibility of causal analysis within psychology. According to Vygotskij, it must be possible to practice a psychology that takes the higher psychological functions seriously and, at the same time, wishes to explain them causally.

The distinction between lower and higher psychological processes had been made prior to Vygotskij. Oswald Külpe and Wilhelm Wundt, for example, had already written about this distinction. Wundt distinguished the psychological and the historical methods of research. The latter he reserved for research into the

higher psychological processes, which to him were those processes for which parallel physical processes had not yet been discovered. These were learning, thought, memory, volition, etc. The psychology concerned with studying these processed Wundt called "Völkerpsychologie". To him, it was impossible to study these processes experimentally. They would be studied indirectly, for example by describing the historical development of language, with the morals, habits, and religions of primitive people to be found in it.

Vygotskij's approach is quite different. In the first place, he believed that the higher psychological processes could be studied experimentally. In the second place, he developed a quite original view on the distinction between lower and higher psychological processes, consistent with Marxist classics. In order to understand how humans' higher psychological processes developed, we must consider their phylogenesis. Hegel, Marx and Engels had commented upon it on several occasions. Having integrated these comments, Vygotskij developed the following view on human development: At first, the species developed according to the laws of biological evolution as formulated by Charles Darwin in his "The Origin of Species". All of human's elementary psychological processes, such as non-verbal thinking, and eidetic memory, developed during this evolution. They form the foundation of human behavior. But it is not these processes that make the human being. They are shared by humans and animals alike (Vygotskij, 1977b; 1982a; 1982b). The genuinely human processes, that is the higher psychological processes, cannot be explained by this biological evolution. They are cultural in origin. At a particular point, the biological development turned into an historical development. As did Marx and Engels, Vygotskij sets the beginning of this development at the time when humans began to work cooperatively. Work involving division of labor leads to new forms of behavior that are no longer determined by direct instinctive goals. Lurija (1979a) gives the example of planting seeds of grain. From a point of view of immediate satisfaction of physical needs, this is nonsense. But from the point of view of intentional, systematic labor, it is an extremely useful activity.

Through work, humans control nature and create the conditions for their own development through purposeful, systematic changes. Moreover, this work is "mediated", as opposed to the "unmediated" activity of animals. Vygotskij draws a direct parallel between the use of a tool in labor and the use of a sign in thinking or remembering. Elementary forms of behavior presuppose a direct reaction to the task set before the organism, which can be expressed by the simple S-R formula. Basic to all higher psychological processes, however, is mediation, that is the use of some intervening instrument or tool between stimulus and response (see also the paragraph on research methods). For example, when one ties a knot in a handkerchief as a reminder, one is constructing the process of memorizing by forcing an external object to remind one of something. In elementary forms of memory something is remembered; in the higher forms of memory humans remember something by the use of a sign (see Vygotskij, 1978; 1983). Because humans create their own environment, which in turn determines their development, they are, with regard to their historical development, their own creators. Here we have an example of Vygotskij's, and more generally, Soviet psychology's optimistic view of human potential. It is within this historical development that the higher psychological processes, such as abstract thinking and speech, develop.

Vygotskij saw the following indications of the validity of his theory of the development of higher psychological functions. Firstly, research indicated that the elementary reactions of primitive and civilized peoples are alike (Vygotskij, 1960; Božovič, 1977). Secondly, higher psychological processes, thought in particular, differ markedly between primitive and civilized peoples. How can we explain this difference? It cannot be said that the physiological substratum differs, for the elementary processes are the same. According to Vygotskij, this means then that cultural causes are responsible for the differences in thought.

Phylogenesis: Speech

We have seen that, according to Vygotskij, in phylogenesis, the change from the animal to the human occurred when humans began to work co-operatively and systematically. However, a second factor which was as much to determine the distinction between humans and animals is the development of speech. In the process of division of labor, the necessity to associate with one another, to describe certain work situations, leads to the development of speech. According to Lurija (1979a) we must imagine this process approximately as follows: At first, primitive humans made only undifferentiated sounds closely related to gestures and practical work situations. The meaning of the sounds was very dependent on the situation. Gradually, however, a whole system of differentiated codes developed. The development of these codes (in Vygotskij's terminology: signs) was of great importance to the development of human consciousness. Indeed, at first humans were strongly tied to practical situations and reacted to stimuli from their immediate surroundings. When the code - or sign - systems developed, it was possible to think about situations that were not directly perceptible. The sign (e.g. the word) refers to an occurrence or matter that does not have to be perceptible at the time. Humans could thus go beyond the boundaries of sensory experience. Through this, abstract thought became possible. One could say, without work and language there would be no abstract thought.

If the above is true, then it follows that we must not look for the sources of abstract thought and other higher processes in the individual, but outside humans themselves, namely in the cultural forms of human historical development. Vygotskij: "There is not the slightest bit of hope of finding the origins of purposeful action in the height of the intellect or in the depths of the brain. The idealistic path of the phenomenologists is as hopeless as the positivistic road of the naturalists. To find the origins of purposeful action, one must transcend the limitations of the organism. The source of human consiousness and freedom

should not be sought in the internal world of the intellect, but in the social history of mankind. To find the soul, we must abandon it."

Ontogenesis: Internalization

But how do the higher psychological processes develop during child development? In child development, "natural" and cultural developments coincide and interact in complex ways. On the one hand, certain elementary, biological processes develop during ontogenesis through maturation. On the other hand, the higher psychological processes develop in the child through its social interaction with adults. To Vygotskij, these two lines of development are fundamentally different and can actually be distinguished. It is especially in the first three or four years of life that more or less "natural" processes are to be found. In his study on the development of attention he writes: "We call this entire period in the development of the child the period of natural or primitive development ... because the development of attention in this period is a function of the general organizational development of the child - above all, the structural and functional development of the central nervous system. The development of attention in this period is based purely on the organic processes of growth, maturation, and development of the neurological apparatuses and functions of the child" (Vygotskij, 1979a).

Vygotskij's account of the development of the higher psychological processes is as follows. The child grows up in a society and a culture in which sign system are already available. Children acquire these sign systems through their social interaction with adults and through education. To Vygotskij, the notion of social interaction (občenie) means two things. There is immediate social interaction, which we have with young children. This interaction manifests itself in cuddling, touching, that is, in affective reactions. This form of interaction changes, however to mediate(d)

social interaction as soon as the child is able to use signs. Although this mediate(d) social interaction developed from immediate social interaction, due to the use of signs it has a character all its own. It is within this mediate(d) social interaction that the internalization process takes place. To illustrate this concept, which has been described more thoroughly elsewhere (Vygotskij, 1978; Wertsch, 1979), we will give an example borrowed from Lurija. Imagine that we wish to know how children learn to jump consciously. We cannot direct this process in little children. Now and then the child jumps and that is all. The mother is not yet able to elicit the behavior. But at a particular point, the child becomes able to jump when the mother requests it. The mother says: "jump" and the child jumps. The child then makes use of an external stimulus. A bit later in its development, the child is able to say the word "jump" itself and so to direct its own behavior. Finally, the child only thinks of the word and voluntary, independent behavior begins. In the preceding (somewhat absurd) example, the following occurred according to Vygotskij. First there was a social, interpersonal relationship between mother and child, in which an external stimulus (the word "jump") induced a certain action. From this, the individual, intrapersonal process began, in which the child, as it were, sets itself a task with the help of a word. Children's talking to themselves is derived from interpersonal talking. It can also be put in another way. Besides its communicative function, language also has a guiding, regulatory function. From this regulatory function self-regulation develops, the direction of one's own behavior (see Zivin, 1979).

With this principle of the internalization of social actions, which was to stimulate a great amount of research, Vygotskij joined the dialectical materialistic tradition in the human sciences. The concept of internalization is, indeed, a direct assimilation of Hegel's concept of "Verinnerlichung". Hegel also considered the development of language as a means by which the child internalized the culture of its society. Hegel also spoke of the role of signs (tools) and the importance of social interaction in child development.

In addition, Vygotskij believed to be linking up with the ideas of Marx. To illustrate, he quotes the sixth thesis on Feuerbach and changes it as follows: "Altering Marx's well-known statement, we could say that man's psychological nature is the ensemble of social relations, which have been internalized and become functions of the personality and forms of its structure..." (Vos, 1976). To Vygotskij, the importance of society for the development of individual consciousness had in this way been demonstrated.

A FIRST CRITICISM

Now that we have sketched Vygotskij's theory of the phylogenetic and ontogenetic origins of higher psychological processes, we can discuss the distinction he made between lower and higher psychological processes in more detail. By higher processes, Vygotskij understands, for instance, "logical memory", "creative imagination" and "verbal thinking". As examples of lower processes he mentions "direct perception", "involuntary memory", and "pre-verbal thinking". We have seen that the main distinction between lower and higher psychological processes is that the latter are mediated by signs and social in origin. They are the result of social interaction between child and adult. However, now and then, Vygotskij characterizes the lower psychological processes as "natural" and the higher psychological processes as "cultural". In other words, he seems to imply that the influence of culture on the mental development of the child is brought about only by *social* interaction. Later Soviet researchers have pointed out, however, that the child is also actively interacting with objects and surroundings influenced by culture (Brušlinskij, 1968, 1979; Božovič, 1977; El'konin, 1966; Tichomirov, 1961; Zaporožec and El'konin, 1979). Through this interaction the child acquires knowledge about its environment and this interaction influences the development of psychological processes, which Vygotskij considered "natural".

Particularly important in this connection is the research of the so-called Kharkov school of developmental psychology. This school consisted of a number of psychologists under the direction of A.N. Leont'ev, who tried to develop the sometimes schematic ideas of Vygotskij (other well-known members of the group were Zaporožec, Gal'perin and Zinčenko). The researchers of this school emphasized the active character of the lower, unmediated psychological processes. They stressed the concept of action (dejstvie). All psychological processes are considered to be based on external actions (predmetnoe dejstvie). Viewed at any given time period, the child's mental processes are the results of the history of its interactions with the social and non-social environment. On this basis it evolves a large set of specific adaptations that operationalize relations among objects and people the child encounters (Cole, 1979/80). According to Zaporožec, we have here a new conception of psychology: humans' psychological activity developed on the basis of their practical activity, on the basis of the orientative and regulatory processes connected with this activity (Zaporožec, 1966).

V.A. Asnin, for example, a member of the abovementioned Kharkov school, tried to demonstrate the importance of activity as follows (Asnin, 1979/80). He showed "that a generalization occurs in the process of the subject's concrete activity as the result of transfer of a procedure acquired in performing one task to the condition of a new and different task". Subjects were presented with a series of problems of increasing complexity. It was shown that by working actively through the whole series of problems, children are able to solve even the most difficult ones. On the other hand, if in the initial phase of the experiment the passive child is shown how to solve a particular problem, it can imitate this problem solving method, but cannot generalize it to a more difficult problem. Asnin concludes that neither instruction nor accumulated experience alone leads to generalization; experience must be appropriately organized for a generalization to be formed. "For this, the subject must be *active* relative to the objective reality present under the parti-

cular conditions ... By trying the problem in our series, using similar methods to solve them and then transferring these methods to new problems, making mistakes in the process and correcting them, the subjects arrived at a generalization that enabled them to solve a problem they previously had been unable to solve".

The main feature of this and other experiments carried out by the Kharkov school is the insistence on continuing interaction with the culturally determined environment. Zinčenko, another member of the Kharkov school, demonstrated in a series of experiments that the conception of visual perception as a passive registration of stimuli must also be considered obsolete. Nowadays, Soviet psychologists consider perception to be perceptive *acting* (perceptivnye dejstvija). The reason for this is that a much more important role for the effector components of perception has been demonstrated. As early as in the first months of life, the activity of the child based on orientative reactions is very great. As Zinčenko demonstrated, the lower processes are of an active nature and they actually do change in ontogenesis (Zinčenko and Vergiles, 1969; Zinčenko and Ruzskaja, 1959). Children of different age levels asked to look at a certain object, show different patterns of eye-movements. According to Zaporožec, these findings indicate that in ontogenesis perceptive actions develop. These actions have a specific, unique structure as a result of the child's mastering (usvoenie) society's sensory experience (Zaporožec, 1966, 1969). The processes, which Vygotskij considered "lower" or "natural" (e.g. "direct perception"), are thus not of a passive nature. Zaporožec suggests that Vygotskij proposed his faulty conceptions of lower psychological processes because he himself did not investigate them. In that way, he had to rely on the common notion of lower processes as "natural reactions of the organism", which only change through maturation.

We have thus seen how Vygotskij erroneously restricted the influence of culture to social interaction, that is the association of the child with adults. A

second objection made by Soviet psychologists is that Vygotskij restricted the influence of social interaction to speech. For example, in his study on the development of higher forms of attention in childhood he writes: "Its attention is, as it were, in a state of neglect, it is not directed, it is not captivated and regulated by the speech of adults as in the attention of the normal child. In a word, it is not acculturated" (Vygotskij, 1979a). The consequence of restricting the role of social interaction to the role of speech is, once again, that psychological processes in which no speech factors are involved are considered "natural" or "biological". Recent research (Lewis and Freedle, 1973; Bruner, 1975; Bullowa, 1979) has, however, demonstrated that mother-infant interactions in the pre-verbal phase of life are of fundamental importance for the development of verbal communication. Mother and infant appear to communicate quite well without words and to go through different phases of interaction. The infant is an active participator in this interaction process (Trevarthen, 1977; Newson, 1977; Brazelton, Koslowsky and Main, 1974). These patterns of interaction later become associated with words, possibly in the way described by Bruner (1975). In a still later phase, the words can be used as commands. There thus seem to be several important phases of social interaction between the child and adults, before the process starts which Vygotskij described as internalization. By emphasizing the role of speech, Vygotskij neglected these developmental periods, considering them "passive" and "natural".

It is, therefore, understandable that several Soviet psychologists criticize Vygotskij for having separated too sharply the lower and higher psychological processes. Brušlinskij, in a penetrating study (Brušlinskij, 1967; 1968), even concluded that Vygotskij is guilty of constructing a dualism. Depicting the lower processes as quite passive, biological in nature and stressing the verbal (speech) character of the higher psychological processes, Vygotskij, the untiring opponent of methodological dualism, remained an ontological dualist himself. We must realize, however, that in a truly Vygotskijan account of child development

some principal distinction between lower and higher processes should be retained. He was, after all, trying to develop a dialectical approach in which development is seen as a series of qualitative transformations. In such an approach, the higher psychological processes are not reducible to lower ones, but have a character of their own. With the introduction of the concept of activity, later Soviet researchers working in the tradition of Vygotskij's theory, have tried to overcome this dualism and they proposed a more consistent socio-cultural theory. It is interesting to note that, in that way, the attention switches from speech to that other fundamental factor in the phylogenesis of man: labor. As we have seen, Marxist classics consider speech and labor the two factors that made man really human. Vygotskij emphasized the role of speech. His followers emphasize the role of activity, which they consider to be a form of labor. In doing so, they avoid the idealism for which Vygotskij was reproached in the 1930's. If culture is transmitted from parent to child through language, without the intervention of an objective reality, the origin of the development of the psyche is then seen as the result of the interaction of subject and subject, rather than the result of subject and object. This would run contrary to the anthropology formulated by Marx and Engels (see Rahmani, 1973, p.45).

VYGOTSKIJ ON CONSCIOUSNESS

In the twenties in the Soviet Union, it was rather unusual to support, as did Vygotskij, a psychology in which consciousness played a major role. Pavlov and Bechterev rejected psychology as an independent science and considered human behavior to be a biosocial phenomenon to be studied by biology and sociology (see Bauer, 1952, p. 68 ff.). Subjective factors played no role at all in their view of the human being. Consciousness was an illusion. It is possible, these researchers admitted, to subjectively feel spontaneity, or freedom, but they are just appearances. Concerning

this, Pavlov said that there is an immediate impression, hard to surmount, of some voluntary freedom of action, of some spontaneity. The theory of conditioned reflexes could, however, eventually explain these phenomena (Bauer, 1952, p.68).

So we see, consciousness as an integrating factor in human actions, was not very popular in the Soviet Union at the time. To explain the integrated character of behavior, psychologists turned to the concept of the physiological dominant. Propagated by Uchtomsky after Avenarius, Sherrington and Vvedensky, this concept goes as follows. When different areas of the brain are stimulated, all energy flows to the area that is most stimulated. This is the dominant area. The behavior which has its physiological substratum in the dominant area, now dominates the entire behavior of the animal or human being. Psychologists tried, for example, to explain the phenomenon of attention in this way. The problem with the notion of dominance, however, is that the relative strength of the stimulation is determined *post factum* on the basis of the behavior which is evoked (Bauer, 1952, p. 72). But psychologists continued to seek a notion of human beings, in which their behavior could be considered to be completely pre-determined. This also explains Freud's initial popularity at this time (see above). In Freud's view, "coincidental" slips of the tongue, gestures etc. proved to be determined by subconscious processes.

Vygotskij, Leont'ev and Lurija deserve all the more credit for having pushed to the foreground the role of consciousness in that age of "psychology without consciousness". Speaking to the second All-union congress of psychoneurologists in 1924, Vygotskij stated that psychology "could study no more than the most elementary connections between a living being and the world" (Vygotskij, 1979b, p.1). But what view did Vygotskij and his associates develop with respect to consciousness? How did they believe consciousness developed? We will discuss this briefly. In his analysis of consciousness, Vygotskij is linked to the idea William James had in this regard. In this connection Vygotskij cites James' essay "Does consciousness exist?" and

praises James for his brilliant analysis. "Completely different paths have led (me) to the same view as that presented by James in his speculative analysis" (Vygotskij, 1979b, p. 32). According to Vygotskij, Pavlov had a good explanation for unconscious, automatic combinations of reflexes. These include reflex arcs, unconditioned and conditioned reflexes and reflex chains. But, Vygotskij states, if we consider not only this *system of reflexes*, but *also others and the possibility* of shifting from one system to another, this is also the mechanism of consciousness. "The capacity of our body to serve as a stimulus (through its action) for itself (for new actions) - therein lies the basis of consciousness" (Vygotskij, 1979b, p. 19). Every stimulus humans receive results in a reflex, which in turn is a stimulus for a following reflex etc. To Vygotskij, the more correctly each internal reflex, as a stimulus, calls forth a sequence of other reflexes from other systems, or is transferred to other systems, the more able we are to communicate to ourselves and to others what our experiences are, and the more consciously we experience them. To Vygotskij, communicating something means here transmitting a group of reflexes to another (higher) group of reflexes. What is not conscious is simply that (reflex) which has not been *transmitted* to other (higher) systems. Vygotskij: "There may be an infinite variety of stages of *consciousness, i.e. the interaction of systems* participating in the mechanism of an active reflex. To be conscious of one's own experiences means nothing less than to possess them in object form (stimulus) for other experiences. Consciousness is the experiencing of experiences, just as experience is simply the experience of objects" (Vygotskij, 1979b, p. 20).

Humans gain control of their behavior by learning certain instrumental techniques, for example aids to memory and using them as external stimuli in directing their own behavior. Leont'ev: "The controlling of behavior becomes possible only by the mastering of stimuli" (quoted by Bauer, 1952, p. 74). In this way, the controlled behavior of Vygotskij and his associates develops in an unusually roundabout way. By learning to use certain external stimuli (including the signs of

language), humans can give their own behavior an organized character. Lurija states that a "direct route" is not possible. According to him the consideration of the voluntary act as accomplished by "will power" is a myth. Humans cannot by direct force control their behavior any more than "a shadow can carry stones" (quoted by Bauer, 1962, p. 74). If it seems as if human beings can determine their actions "directly", it is only because through the internalization process, certain processes can no longer be observed after a time. The child no longer says "jump", but thinks it. The feeling of consciousness develops because certain reflexes (for example muscular sensations) become stimuli for certain other reflexes (for example muscular movements). "Consciousness is always an echo, a system of responses". According to Vygotskij, the problem of self-awareness and self-observation can also be approached through this conception. Now it is clear, Vygotskij states, why one person's experience is accessible to him or her only. Only I and I alone can observe and perceive my own secundary responses, for only for me my reflexes do act as new stimuli of the proprioceptive field. With this thought in mind, Vygotskij once again sees the value of the subjective experience his predecessors denegrated. He indeed advocated the necessity of studying these inhibited reflexes. Vygotskij elucidates his theory of consciousness on the basis of three areas of research: into emotions, into cognition and into willful action. We will limit ourselves to a discussion of the research into emotions and will.

With respect to research into emotions, Vygotskij agrees completely with William James' theory of emotions. Vygotskij: "James took the three usual components - A the cause of an emotion; B the emotion itself, and C its corporeal manifestation - and rearranged them in the sequence A - C - B. (...) I should only like to point out that this formulation laid bare:

(1) the reflex nature of an emotion, an emotion as a system of reflexes A and B;
(2) the secondary, derivative nature of the consciousness of an emotion when one's own response serves as a stimulus for a new internal reaction B - C" (Vygotskij, 1979b, p. 26).

With respect to research into the will, Vygotskij says the following. Every movement must first be performed unconsciously. Kinesthesis (that is the secondary action) is then the basis for its entry into consciousness. Up to now, we thought that a willful action took place as follows: I think and then I do. This is erroneous. First there is a primary reaction (doing) and only then is there a secondary reaction (consciousness). Consciousness has a derivative, secondary nature and, genetically speaking, is determined by the environment. Being determines consciousness. Looking at Vygotskij's view of consciousness, what is noticeable (besides the strong influence of James) is how much his early work was still influenced by the largely reflexological thinking of psychology in those days. Later, the role of (the signs of) language would be investigated and stressed more in his research. It would have been very interesting if at that time (about 1933), Vygotskij had once again extensively formulated his thoughts with respect to consciousness.

EDUCATIONAL IMPLICATIONS

The Zone of Proximal Development

Vygotskij has propounded a number of educational ideas in accordance with his cultural historical theory of higher psychological functions. The first problem we will discuss is that of the relationship between child development and education. What is the link between education and development? Are they independent of one another? Can education influence the development of the child? In his later works, particularly in "Thought and Language", Vygotskij attempts to answer these questions. He begins with a critical investigation of the then available psychological and pedagogical literature. In chapter 6 of "Thought and Language", he distinguishes three notions of the relationship between education and development.

He attributes the first notion to Piaget (and Blonskij). In it, education and development are seen as completely independent of one another. Here child development is regarded as a process of natural maturation that cannot be influenced by education. Education can only ascertain that natural development has reached a certain stage and then coincide with it. This means that education always lags behind development. Within this notion, it is also appropriate to have a child take an intelligence test in order to determine the stage of mental development it has reached, and to match the child's education to it. According to Vygotskij, this idea is also to be found in Piaget's work. To Piaget's mind, the child passes through certain necessary stages, independent of whether or not it receives education. This idea is also at the root of Piaget's research into concept formation. In this research, Piaget consciously limits himself to the so-called "spontaneous" concepts. These are concepts a child acquires spontaneously. On the other hand there are the "non-spontaneous" concepts, which are passed on to the child through the educational process. To Vygotskij this is a typical example of the neglect of the role of social interaction and education by those guilty of biological reductionism. In view of his link with Hegel, Vygotskij could, never, of course, agree with this. In chapter III we will see that Riegel criticized Piaget in the same way. There we will try to defend the Piagetian theory against this kind of criticism.

Vygotskij attributes the second notion to James, Thorndike and the reflexological movement. In this notion, no distinction whatsoever is made between education and child development. Child development is based on learning processes of a simple associative nature (conditioned reflexes) and education is concerned with the very same processes.

The third notion is that of the Gestalt psychologist Koffka. He states that child development is the result of both natural maturation and education. Vygotskij considers this notion to be a great improvement upon the first two. His criticism, though, is that the relationship between the factors maturation

and education has not been worked out sufficiently. To Koffka, both maturation and education influence child development. These processes influence one another, but Koffka does not indicate exactly how. Linking up with these ideas of Koffka, Vygotskij now tries to develop a theory on the relationship between education and child development that clarifies these ideas. In this theory, his concepts of the zone of actual development and the zone of proximal development play a central role. Until then, according to Vygotskij, it was assumed that education had to be matched to a certain initial capacity of the child. The question was whether a child was mature enough for a certain level of education. Of course this is true. In order to ascertain this, though, it was unfortunately always the child's independent achievement that was measured. In addition, however, it is also possible to determine what a child can comprehend with assistance. When the former procedure is followed, it is the zone of actual development that is ascertained, forming the lower boundary of achievement. When the latter procedure is followed, the zone of proximal (nearest) development is determined. This establishes a maximum level of what the child can comprehend at that point. Education must bear both boundaries in mind. It is the task of education not to be satisfied with the child's actual, factual level of development, but to work in the area between the highest and lowest level. In that area are to be found those tasks the child cannot complete independently. In this way, education actually stays one step ahead of development, stimulates it and lifts tasks out of the region of potential, dependent capability into the region of actual independent capability. Education is developing education. Basic to this idea is that education calls forth cognitive developments which would not have developed independently, i.e. without this education.

Such a view also implies a positive view of the role of imitation in child development. "Looked at broadly, imitation is the principle form in which education influences development. Teaching language, education at school is largely based on imitation. At school, the child is, after all, not taught what it can

do independently, but what it is as yet unable to do but proves to achieve in collaboration with and under the guidance of the teacher." (Vygotskij, 1982b, p.250). From this viewpoint of Vygotskij, we can see once again how important the role of social interaction and, in particular, education is in his thinking. As does Hegel, Vygotskij believes the child to be dependent upon upbringing and education. The notion of child development as following fixed, unalterable stages, makes a natural law out of a natural course of events. The fact that a child's cognitive development is in part influenced by cultural factors is then ignored.

It is thus clear that Vygotskij's ideas concerning the relationship between education and cognitive development were strongly influenced by Hegel. The emphasis upon the role of social interaction, upon the child's dependence on upbringing and education, and the distinction between a natural law and a natural course of events have all been borrowed directly from Hegel (see below).

Transfer of Scientific Concepts

We have spoken up to now of the importance of social interaction and education, and the role they play in transferring society's cultural inheritance to the child. But we have not yet discussed how exactly this knowledge is transferred. When Vygotskij began to construct his cultural historical theory, he believed this transfer to take place with the help of signs, and in particular with the help of words. Towards the end of his life, he expands on this notion and shifts the emphasis somewhat. Where he had once spoken of words as signs, he now begins to speak of the meaning of words. According to Vygotskij, if a word is to play a significant role in the process of communication, it must not be just a sign, referring to one object, but a concept, referring to several objects at once. Communication can only be meaningful if the words are not all signs (names). It must be possible to communicate concepts.

Imagine that I want to say to someone: "I saw a car". That person will only understand me if a car is a concept, that is a word referring to a collection of identical objects. If this were not the case, the word "car" would be a sign or name, only meaning something to me (unless the person I am speaking to just happened to know what the sign stood for). Due to their generalizing effect, the function of concepts is therefore to make it possible to communicate without having to refer directly to a concrete state of affairs. In that sense they also free us from our senses.

According to Vygotskij, there are two kinds of concepts, namely empirical (everyday, living or spontaneous) concepts and theoretical (scientific) concepts. Piaget had already made a similar distinction (see the preceding paragraph) with his spontaneous and non-spontaneous concepts. But Vygotskij, more in line with Hegels' thinking, places the emphasis differently. What, then, is an empirical concept? In order to understand this, we must examine for a moment the empiricist theory of abstraction, which in the Soviet Union was criticized by Vygotskij and later by V.V. Davydov. According to this theory (the idea of which can be traced to J.S. Mill, Locke and Aristotle), the process of concept formation can be understood as follows.

The human being (the child) is confronted with a great variety of objects and is capable of distinguishing those characteristics which the objects have in common. Objects that have certain characteristics in common are put into specific categories. In essence, the function of the intellect is to compare and distinguish a collection of objects we perceive. By comparing birches, oaks and pines, we automatically come upon the abstract concept of the tree. Together, concepts form a pyramid, with at the top the concept of species, which at the same time is the most devoid of content. Those who subscribe to this theory believe that in principle it is possible to arrange objects into classes on the basis of any characteristic. They do not believe that it is possible to speak of certain *essential* characteristics of an object. This empiricist abstraction theory was criticized by Davydov (1972), as by Hegel, among

other reasons because in it, the role of the senses was overstressed. It is true, Davydov says, that concept formation as described above often occurs, but it is not the only kind, and not the most important. There are also scientific theoretical concepts which cannot develop by means of formal generalization. This is because they transcend directly observable phenomena. Hegel had already made this distinction between empirical concepts and theoretical concepts (Vos, 1976, p. 275). To him, the distinction between "the particular" and "the general" played a role in this. Empirical concepts arise by means of induction, that is from the particular to the general. The subject reconstructs the concept (the general) from the instances of it (the particular) he runs across.

Hegel has little regard for this type of concept formation, for being based upon the objects' external characteristics (Schein), it is incapable of developing a genuine concept of their essence. Only through abstract thought are we able to develop genuinely theoretical concepts that expose the essence of phenomena. An empirical concept (tree) is reconstructed from instances we run across (oak, elm, birch, etc.). This is not the case with a theoretical concept. In this case we begin with conscious insight into what the concept is and compare it with instances of it. At school, for instance, we learn the concept mammal, and with the help of this concept, we can then understand many animals better. The problem we have with Hegel is that it is difficult to understand how exactly we develop these theoretical concepts. In this respect, Davydov resorted to dialectical logic (Davydov, 1972).

Vygotskij adopts Hegel's distinction between two types of concept formation or generalization. However, by adding the idea of social interaction, Vygotskij gives this distinction a different significance. The child learns empirical concepts by continually interacting with material, concrete things, and constructing a concept from this interaction (little pets, with four legs and a tail, that bark but do not mew, are bowwows). Children adopt theoretical concepts from their parents. These concepts get "substance" through daily

life. In short, Vygotskij's idea can be stated as follows: In forming empirical concepts, the child extracts a definition of the concept from a number of examples it runs across. (The word "definition" should not be taken literally here. Not until the very last stage of concept formation will the child be able to put the definition of a concept into words.) With respect to the theoretical concept, the child is given the definition of the concept and it confirms this definition in the examples it runs across. The child acquires general, theoretical concepts ready made and does not have to follow the laborious path of induction to get them. In this connection, Vygotskij regularly draws a parallel to learning a foreign language. It takes us five years or so of toiling at our native language to learn to speak it reasonably well. Only after that, when we go to school, do we get the first principles of grammar. The process of learning a foreign language, however, is quite different. First we learn the grammar, through which we can get a reasonable command of the language.

To Vygotskij, this is also the way the child acts in its social interaction with adults. It acquires certain concrete concepts independently, but it is greatly dependent on interaction with adults to acquire theoretical concepts, particularly at a young age. To Vygotskij the relationship between concrete, empirical concepts and theoretical concepts is very complex. On the one hand, the child will have had to develop some empirical concepts in order to successfully learn a theoretical concept. On the other hand, the presence of theoretical concepts influences the development of empirical ones. Vygotskij, too, believed that reality could be grasped better by means of theoretical concepts than by means of concrete concepts. How the presence of theoretical concepts influences the development of empirical concepts remains somewhat unclear. Vygotskij: "It is quite certain that conscious insight into concepts and the arbitrariness of concepts .... lies within the zone of proximal development. In other words, they become visible and effective in collaboration with adult thought" (Vygotskij, 1982b, p.264). Further on in "Thought and Language", Vygotskij

states that the scientific concepts transform the spontaneous (empirical) concepts and elevate them to a higher level, thereby creating the zone of proximal development. In this way, he connects his notion of the zone of proximal development with the scientific, theoretical concept. If the child were not to come into contact with theoretical concepts through its interaction with adults, it would be doomed to remain in the zone of actual development. The number of theoretical concepts the young child has at its disposal would not increase (Vygotskij, 1982b, p.272). However, Vygotskij's development of this idea is rather meager. In addition, quite a number of objections could be raised to the empirical research that Vygotskij describes in supporting his ideas on the relationship between empirical and theoretical concepts (see Brušlinskij, 1967).

The Disabled Child

In the preceding paragraphs, we saw the importance Vygotskij attached to social interaction, through which the higher psychological processes develop. All the more serious, then, when an individual is in danger of being cut off from this social interaction, as is the case with children with one or another physical handicap, such as blindness or deafness. "Even human perception could not have developed outside the human community. Speech is the very basis and transmitter of such social experience; even when thinking in solitude, the fiction of social interaction is ever present. In other words, without speech there is neither consciousness nor self-consciousness" (Vygotskij, 1924 in: Vygotskij, 1983, p.78).

Because it is so difficult for the deaf child to learn to speak, it is in particular the mental development of the deaf child that Vygotskij believes endangered. But Vygotskij certainly does not regard the situation of the deaf, blind or retarded child as hopeless. On the contrary, he believes these children capable of full social existence, but we must provide

them with the means or tools to develop intellectually. Those means particularly suited for achieving this goal, such as speech and writing, have been adapted to the average person possessing full faculty of hearing and sight. It is now the task of psychologists to uncover new, adapted means of enabling the disabled child to participate in society. If they fail, the child will remain primitive. Vygotskij makes a comparison in this regard to primitive cultures, where they lack the means or the tools, for example a written language, which enable a civilization to flourish. In the case of the child with a particular organic handicap, these tools are simply not adapted to its problems (Vygotskij, 1983, p.25). In both cases, then, cultural development is limited, but Vygotskij sees ways of stimulating that development.

Take, for example, the case of the blind child. Vygotskij does not regard the loss of sight as fatal, for culture is accessible through such cultural instruments as speech, reading and writing. And these cultural instruments are not bound to one particular sense. The blind child does not read with its eyes, but with its fingers. The deaf child does not speak with its mouth, but with its hands. In principle, Vygotskij believes, there is no difference. The culture is just as accessible to the disabled child, only through other means, by using other cultural tools (Vygotskij, 1983, p.171). Indeed, Vygotskij refuses to regard the disabled child as inferior, but again and again emphasized that it is simply different. In 1931 he even wrote: "we assume that even if human beings only had four senses, it would make no essential difference in the knowledge that they could acquire, for fundamentally, thought - the means by which we assimilate experiential data - would remain the same. The picture we develop of the reality around us is not only based upon perceiving it directly, but also upon our rationally assimilating experience. In principle, both the blind and those possessing sight know much more than they imagine; they know much more than they can observe through the five senses... And so for the blind child, thought is the most important means of compensating for its inadequacy in perceiving images" (Vygotskij, 1983, p.211). In this

way, Vygotskij links his view of the disabled child to his epistemological notion that direct perception does not necessarily result in usable knowledge (see above).

To Vygotskij's mind then, the absence or deficiency of a lower function arising in phylogenesis, such as perception, can be compensated by utilizing a cultural instrument. In this regard a "disabled child" does not differ from a "normal" child at all. When for example, the natural, direct memory fails, the normal child will also use language (classifying the objects to be remembered into categories) and all kinds of mnemonic systems. In doing so, the child as it were increases its storage capacity. Vygotskij borrowed this notion that psychological functions can be brought about in different ways from Binet (Vygotskij, 1931 in: Vygotskij, 1983, p.122). For each failure of an organic function, the psychologist must attempt to find a cultural instrument to replace it. In order to do so, a qualitative diagnosis of the disabled child is required. It is necessary to determine exactly which faculty is disturbed, but also which functions have remained intact. Again and again, Vygotskij opposed a purely quantitative diagnosis, determining only how far a child lags behind and what it is incapable of doing. The psychologist needs a qualitative analysis of the child's strengths and weaknesses. The psychological test can perform a supportive role in this diagnosis, but no more than that. Measurement and diagnosis are two different things. "The tape measure sees nothing", writes Vygotskij in 1931 "without subjective assimilation, that is, without thought, without interpretation, [without] decoding the results, [without] evaluating the data, there is no scientific research" (in: Vygotskij, 1983, p.299). A consequence of the quantitative approach in diagnostics is that children are characterized as "mentally retarded". But is that not like telling the patient that he is ill? Only on the basis of a detailed, qualitative analysis is recovery possible.

Vygotskij also makes some concrete suggestions for improving the treatment of disabled children. Most im-

portant is that the child should be able to participate *in social activities and communicate with others.* Social isolation is the greatest danger threatening the deaf, blind or retarded child. The psychologist must attempt to enable the child to attain social interaction at all costs. According to Vygotskij, the most serious consequences of a physical defect are the social ones and compensation should be socially oriented. In this connection, Vygotskij points to the dangers of special schools. An attempt should be made to integrate the special schools into society. The disabled child also has a meaningful role to play in society, but it is then necessary that the well-intentioned, charitable care of these children should be replaced by an active education.

But is this view not a bit too optimistic? After all, these children do suffer from an organic defect. Vygotskij does acknowledge that these are organic deficiencies, which in and of themselves cannot be remedied. But here, too, he says that "constitution is not a fate, but a biological play of forces which, when its internal dynamics are known, can be influenced" (Vygotskij, 1931, in: Vygotskij, 1983, p.294). But by means of the cultural tool, successful reorganization of the personality is also possible. "It is therefore understandable that the biological processes primarily responsible for the first stage in the development of [the] mentally retarded [child] become concealed; they are not eliminated but neutralized (snjatymi) during the process of development..." (Vygotskij, 1931 in: Vygotskij, 1983, p.118). With this explicit reference to Hegel's dialectic, Vygotskij once again expresses his optimistic view of humanity. Human beings can be "almost entirely re-educated" (1928). The higher psychological functions are also attainable for the blind or deaf child, so "nothing can limit their possibilities". Nature is overcome by nurture. "In the psychology of the blind, then, the line of action is aimed at overcoming the defect through its social compensation, through participation in the experiences of those who can see, through speech. The word overcomes the blindness" (Vygotskij, 1983, p.95).

A SECOND CRITICISM

In the preceding paragraphs, we described a few educational implications of Vygotskij's ideas. They represent some of the most interesting aspects of his work. The cultural-historical theory was designed to be a practical theory. Vygotskij and his co-workers tried to formulate a theory that would have practical consequences for, among other things, the education of disabled children, the development of curricula in schools and the treatment of patients suffering from brain damage. We will now discuss some of the above-mentioned implications.

The internalization theory proved to be one of Vygotskij's most fruitful ideas. One need only think of Gal'perin's school, which has produced a wealth of important psychological data (see Gal'perin, 1953, 1957, 1969). The idea of internalization was, however, not new. Both Vygotskij and others, among them Lurija and Gal'perin, have pointed out that the internalization concept also played a role in the ideas of Durkheim's French sociological school and in those of Piaget. In "Thought and Language", Vygotskij compared Piaget's internalization idea with that of the French school. Vygotskij: "In its philosophical essence, it (Piaget's idea) was related to the sociological doctrine of Durkheim and other sociologists, who saw space and time and objective reality as a whole as stemming from man's cultural life" (Vygotskij, 1982b, p.71). Vygotskij cannot agree with this epistemological position, as it runs counter to the tenets of dialectical materialism (see McLeish, 1975).

Gal'perin, who states that the notion of internalization was first posited by the French school, considers their position idealistic. To him, internalization is the process by which the transition from the non-psychological to the psychological is made

(Gal'perin, 1980). According to Gal'perin, Piaget's internalization is a secondary phenomenon with respect to the logical development of thought. To Piaget, it meant the development of a level of ideal, actually logical constructions. The question of the transition from the non-psychological to the psychological is not involved in it at all (Gal'perin, 1966, p.25). On the other hand, Vygotskij does mention this transition. The higher psychological functions develop first as external actions and only then become individual psychological processes through the internalization process. To Gal'perin, the fundamental significance of this is that the gap between psychological and non-psychological functions can be bridged. He believes this idea to be one of Vygotskij's great achievements.

Vygotskij does not always deal with the notion of the zone of proximal development with equal consistency. In most of his work it means "what a child is cognitively capable of doing with the aid of an adult". A crucial point here is the idea that the child is dependent upon its interaction with adults for its cognitive development. Through this social interaction, the zone of proximal development is created. However, Vygotskij is not quite consistent here. In his essay "Play and its role in the psychological development of the child" he states that at play the child transcends its normal cognitive level of activity: "...the child attempts as it were to vault above the level of its customary behavior. We can compare the relationship between play and development to the relationship between education and development. Play expresses changes in needs and changes in consciousness of a more general nature. Play is a source of development and creates the zone of proximal development" (Vygotskij, 1966, p. 74). Here Vygotskij seems to suggest that in play, the child creates its own future possibilities and is therefore not dependent on the interaction with adults for them.

Actually, the concept of the zone of proximal development itself requires a bit of clarification. It assumes that it is possible to classify a series of tasks according to difficulty or the difficulty of the

mental activities (operations) involved. A child might then, for instance, be able to complete the first 60 tasks alone (the actual zone of actual development) and with the help of an adult the first 80 (the zone of proximal development). The same type of example can be imagined for cognitive operations (mental activity). But does this now mean that it is possible to have achieved different zones of actual or proximal development in different cognitive areas, such as language, mathematics and problem solving? Can a process of "décalage" (see chapter III) occur?

Vygotskij himself states that he could determine children's mental ages. This probably means that he gave the children tasks in different areas and then determined the actual and proximal zones of the aggregate. Two children, whose actual achievements each correspond to a mental age of 7 can then achieve, with the help of an adult, a result corresponding to a mental age of 8 and 9 respectively. It would seem to us, however, that a differentiated profile test would provide more insight into the child's potential. From our western "bias" it would not seem entirely impossible to construct an intelligence test in which assistance has been integrated as a strictly standardized factor. In any case, the concept of the zone of proximal development is not without difficulties. This does not alter the fact that Vygotskij's related notion of education as developing education has proved to be of great value.

We will discuss Vygotskij's treatment of concept formation quite briefly. We have already expressed a number of points of criticism. Vygotskij does not make clear how theoretical concepts develop and where their superiority with respect to empirical concepts lies (the latter is the case with other Soviet authors as well). In addition, the way theoretical concepts influence the development of empirical concepts remains unclarified. The empirical research of Vygotskij and his co-workers on concept formation, which we did not discuss above (see Vygotskij, 1977, chapter 5) is described very unclearly and does not seem very substantial. Later authors have demonstrated that the

several stages of concept formation Vygotskij pointed to are related to the type of instruction (see Lompscher, 1972). All in all, it can be said that Vygotskij's research into concept formation leaves a great deal to be desired. But it must not be forgotten that this research was not finished at the time of Vygotskij's premature death.

THE PHYSIOLOGICAL SUBSTRATUM

The question as to the material basis of complex psychological functions is a very old one. Even the Hippocratic School of physicians knew that epilepsy and madness were disturbances of the brain. They believed that not only our pleasure, our joy and our laughter but also our sorrow, pain, grief and tears arise from the brain (Blakemore, 1977). In the middle ages, it was believed that the spirit resided in certain cavities called ventricles. At the beginning of the last century, the phrenology of Gall and Spurzman was highly regarded. Gall believed that on the basis of the shape of the skull, it was possible to draw conclusions about the shape of the brain and, through it, about psychological characteristics. A highly developed bulge in a certain place might, for example, indicate a particular aptitude for higher mathematics.

The more scientific study of the question of localizations in the brain begins with the work of Broca, Wernicke and Jackson. With their discovery of speech centers, the first two researchers cleared the way for a flood of research attempting to localize a certain well-defined place in the brain for complex psychological functions. On the other hand, the English neurologist Jackson claimed that the representation in the brain of psychological functions was dependent upon their complexity. Very simple functions, such as the faculty of cutaneous sensation, were perhaps represented in strictly localized cortical areas. But complex psychological functions, so Jackson believed, must be represented at several different levels. Each function,

then, is represented at a low level (spinal cord, brainstem), a middle level (sensory or motor cortex), and at a high level (in the frontal lobes) (Lurija, 1973, 1979b). Jackson's research resulted in a school of neurology regarding complex functions as a result of activity of the brain as a whole. And so, two opposing schools developed within neurology. On the one hand, there were the strict localization theoretists, who attempted to connect every mental function to a specific cortical area. On the other hand, there were the "holists", who regarded the brain in its entirety as being responsible for psychological functions. From the latter point of view, the degeneration of psychological functions is not so much determined by the location of certain brain damage as by the amount of brain tissue affected. This was the situation in about 1925, when, starting from Jackson's thesis, Vygotsky and his associates attempted to develop a new theory on the cerebral representation of higher functions.

Basing himself upon research with children into the higher psychological functions, Vygotskij claimed that these functions formed complex functional systems with a mediatory character. Because humans have a different historical experience than animals, the cerebral organization of their psychological functions must also differ essentially. Attention processes, for example, have in humans and animals a different cerebral organization because humans use speech in directing their attention. Therefore the cerebral organization of the human attention process will in part be related to language or comprehension centers. Furthermore, a theory of the cerebral organization of higher functions must account for processes, such as those involved in writing, that depend in part on external, historically conditioned mediators (Lurija, 1979b, p.126).

Because the history of writing is but a few thousand years old, while the evolution of the human brain required millions of years, Vygotskij and his associates assume that the cerebral organization of writing rests on a complex co-operation of several cerebral areas. This co-operation arises in child development

and applies in particular for the complex, language-mediated functions. Elementary functions, such as the registration of sensations through the retina, can probably be localized exactly in one particular cell group. The fact that the cerebral organization of complex psychological functions arises during the course of child development (ontogenesis) also means that the cerebral organization of a particular function can vary with age. In his research with children, Vygotskij had demonstrated that complex mental processes initially proceed with the aid of external means. Afterwards, a child's behavior becomes condensed and internalized. It would seem obvious that this varied progress of certain types of behavior should also be reflected in cerebral organization.

On the basis of a number of clinical studies, Vygotskij did, indeed, succeed in making this assumption plausible. Among other things, Vygotskij gives the example of a man suffering from optical agnosia (the inability to attach meaning to sensory impressions; see Lurija, 1973, 1979b). This man was still capable of judging objects he was unable to judge or name on his own. It could then be said that the higher "centers" remained intact (logical judgment), while the lower "centers" (perception) were disturbed. Such a disturbance, however, will never be observed in children, for if a child's cerebral representation of the lower functions (perception) is disturbed, the higher functions (logical judgment) will not develop at all. For a child, such a situation is much more serious too, for it cannot compensate for the lower functions by means of the higher ones. A child would not be able to master language. Agnosia is not found in children. On the basis of such considerations, Vygotskij posits the following general law: "In the early stages of ontogenesis, brain damage results in the underdevelopment of the higher psychological functions. The same brain damage in adults results in a disturbance of the corresponding lower systems, while the higher psychological functions remain intact".

Initially, it was Lurija in particular who in this spirit developed these, in the final analysis, rather

simplistic ideas concerning cerebral organization. Not least of all, this research was made possible by the abundance of subjects produced by the atrocities of the Second World War. Basing himself, as did Vygotskij, on clinical and neurological material, Lurija developed the science that would later be called neuropsychology. An excellent survey of Lurija's research can be found in Cole (1978). The consequences of the above view on the cerebral organization of psychological functions are, among others, as follows:

(1) If it is determined that the development of the complex psychological functions (logical thought, problem solving) proceeds differently in different cultures, these differences can then not be reduced to differences in basal, physiological processes. The lower functions are the same. The differences develop because in ontogenesis, the complex functions are organized differently. Language plays an important role in this organization.

(2) Individuals with the same genetic material, but with a different "historical experience" or personal history, will exhibit differently organized complex psychological processes, but similar achievements in the area of lower psychological functions. In order to test these consequences, Vygotskij, Lurija and Leont'ev devised cultural-psychological research, which we shall describe in the following paragraph.

CULTURAL-HISTORICAL RESEARCH

Uzbekistan

In 1931 and 1932, A.R. Lurija led two expeditions to Uzbekistan to investigate whether the higher psychological functions of the residents of Uzbekistan differed fundamentally from those of the inhabitants of the Western world (Lurija, 1974; 1976a). If this proved to be the case, and the lower functions did not differ,

Vygotskij and his associates would have demonstrated that the complex psychological processes develop in ontogenesis. Until that time, Vygotskij and his co-workers had in part based their cultural-historical theory on the anthropological data of the French scholar Lévy-Bruhl. Lévy-Bruhl was a member of the French sociological movement near the turn of the century which emphasized the role of the collective in the development of psychological characteristics and behavior (Cole and Scribner, 1974, p.19). From this point of view, he attempted to analyze the cognitive functioning of people in several different cultures. In doing so, he arrived at his well-known characterization of the thought of "primitive" peoples as being pre-logical (Berry and Dasen, 1973, p.11). The problem is, however, that Lévy-Bruhl based his theory entirely on rather dubious information, i.e. reports of missionaries, travelers and amateur anthropologists. In their research, for example, both Herbert Spencer and Lévy-Bruhl had used data provided by a researcher who reported on an abruptly ended conversation with certain Indians. From this he concluded that "the mind of the savage appears to rock to and fro out of mere weakness" (Cole and Scribner, 1974, p.20). Lurija, too, admits that the data then available were only of anecdotal value (Lurija, 1979b, p.59). This is also the reason Vygotskij, Lurija and Leont'ev wished to organize their expedition. In order to better understand the place the cultural-historical theory of the higher psychological functions occupies within cultural psychology, we shall first discuss briefly a number of theories regarding the relationship between culture and cognitive development.

Within cultural psychology, two groups of researchers may be distinguished. On the one hand, there is a group of theoretists emphasizing the differences in cognitive functioning between different cultures. On the other hand, there is a group emphasizing certain universal thought processes developing identically in all cultures. The former group of researchers, by and large inspired by Darwin's theory of evolution, draw the evidence for their position from several sciences. Most controversial is their attempt to trace differen-

ces in cognitive functioning to genetic differences. Associated with this group are, among others, the names of Herbert Spencer, and G. Stanley Hall. Herbert Spencer, for example, believed that acquired traits were hereditary and regarded this mechanism to be an explanation for the, to him, established fact that some races occupied a higher rung on the evolutionary ladder than others. Stanley Hall believed that the development of the species (phylogenesis) was recapitulated in child development (ontogenesis) and therefore that primitive people's inferior cognitive achievements were proof of a less advanced evolution. In this form, arguments based on genetics have fallen into discredit, if only on the basis of the following considerations:

(1) Today, it is no longer believed that genetically determined differences can play a major role in cultural differences because (a) human development from hominide to *homo sapiens* took approximately 2 million years; (b) the rapid cultural advance of *homo sapiens* occurred during such a short time span (about 50,000 years; Harris, 1968) that important genetic changes are improbable; (c) radical cultural changes can occur within one generation.

(2) It has proved difficult to define the concept of race in terms of biological traits. This indicates that the concept is probably more sociological than biological in nature.

Recently though, another group of researchers has emerged, led by the American Edward O. Wilson, who believe that genetic differences can play a role. It must be said that Wilson is considerably more cautious in his position than the researchers discussed above, at least in his *opus magnum* "Sociobiology: a new synthesis". Wilson states, for example, that it is part of the conventional wisdom that virtually all cultural variation is phenotypic rather than genetic in origin. He also admits that human populations are not very different from one another genetically (Wilson, 1976, p.550). But Wilson is not prepared to accept what he calls "the extreme orthodox view of environmentalism", which in fact claims that genetic variance plays no

role in the transmission of culture. In this respect, he opposes the view of Dobzhanskij. Though the genes have had to relinquish most of their sovereignty, Wilson writes, they maintain a certain amount of influence, at least in regard to the behavioral characteristics upon which differences between cultures are based. He bases this opinion on research presumably showing that to a relatively high degree, heredity plays a role in such conditions as neuroticism, dominance, depression and schizophrenia (Wilson, 1976, p.550). Wilson: "Even a small portion of this variance invested in population differences might predispose societies toward cultural differences. At the very least, we should try to measure this amount. It is not valid to point to the absence of a behavioral trait in one or a few societies as conclusive evidence that the trait is environmentally induced and has no genetic disposition in man. The very opposite could be true. In short, there is a need for a discipline of anthropological genetics" (Wilson, 1976, p.550).

Other researchers from the group emphasizing the differences in cognitive functioning look for a sociological explanation, attempting to draw conclusions concerning cognitive processes from an analysis of the culture in which the individual participates. Among this group of researchers can be classified, among others, Lévy-Bruhl, whose data Vygotskij repeatedly refers to. This method has been attacked by, among others, Boas, Köhler and Bartlett (Cole and Scribner, 1974, p.19ff). The most fundamental objection raised by these critics is aimed at the method's indirectness. Is it possible to draw conclusions about thought processes based on an analysis of the products of culture?

A third school attempts to reduce cultural differences in cognition to psychological causes. To this school belong, among others, the developmental psychologists Heinz Werner and Jerome Bruner, whose thinking exhibits a great similarity to that of the Cultural-Historical School of Vygotskij and his associates. According to Bruner, intelligence is largely a result of the internalization of the tools provided by a particular culture, tools being used here in the broadest

sense of the word, under which Bruner also classifies symbolic systems (Bruner, Olver and Greenfield, 1966). Cultures differ with respect to the tools they provide the individual with and regarding the social institutions developed to transmit knowledge and skills (Cole and Scribner, 1974, p.24).

A majority of the researchers discussed up to this point were inspired by the notion of evolution. A majority of those to be discussed now can be classified among the structuralists. In this group, there is an inclination to regard observed cultural differences as external phenomena. They are the expression of common underlying cognitive structures. In anthropology, for example, it is regularly claimed that cultural differences in cognition lie in the area of the content of thought processes. They are differences in convictions, notions and beliefs. On the other hand, cognitive processes proceed the same everywhere. In every culture, attempts are made to uncover cause and effect relationships, with thought proceding according to the principles of abstraction and generalization. At present, this view is typified by Claude Lévi-Strauss. To his mind, the intellect works identically in all cultures and has also not changed during the period of *homo sapiens* either. Only the material that is used for thought differs per culture (for example, the attributes used in forming classes). To Lévi-Strauss, this also means that there cannot possibly be different levels of cognitive development in different cultures. In our discussion of Lévy-Bruhl, an objection was already mentioned that is also applicable to Lévi-Strauss' procedure: there is danger in basing conclusions regarding thought processes on an analysis of cultural products and institutions (ways of classification, myths, etc.).

A similar objection can also be raised with respect to psycholinguistic researchers. There is certainly no clear support for the hypothesis that grammatical rules in linguistics correspond to psychological thought processes. This does not alter the fact that linguistic structuralism, with its undisputed leader Noam Chomsky, has had a great influence on psychology. This school

pointed, for example, to the fact that languages cannot be classified according to their difficulty. Regardless of the abovementioned objections to its procedure, structural linguistics has shown that it is impossible to deduce anything about levels of thought on the basis of (supposed) differences in the levels of languages. All languages have certain structural characteristics in common. According to Chomsky (1968) for example, all sentences are generated by means of formational and transformational rules. In addition to claiming that the basic structures of all languages are identical, he also assumes that all children possess a certain innate aptitude enabling them to learn a language (see Cole and Bruner: in Berry and Dasen, 1973).

All in all, it can be said that through the influence of the structural school in linguistics, cultural differences in cognitive functioning are seen more as exterior phenomena than as differences caused by differences in basal cognitive processes. The question then arises as to what position the cultural-historical theory of Vygotskij and his associated occupies in this "mixed bag" of theories. Well, Vygotskij arrives at a sort of synthesis of the ideas described above, which states that people from different cultures have both common psychological processes and differences in cognition. The lower psychological functions are universal, being by and large psychophysical in nature and strongly determined by the basal processes arising through evolution (sensations, elementary forms of attention and memory). Being mediated by language, however, the higher psychological functions differ according to culture. This hypothesis would appear easy to test (assuming the distinction between higher and lower functions is workable, see above), but up to now, this has never occurred systematically. To our mind, the principal conclusion that can be drawn from Vygotskij's theory is that the origin of any differences ascertained between cultures and peoples should not be sought in certain stable (genetically determined) characteristics of individuals but in their personal histories. It is in the contact between individuals and the culture that their higher psychological functions develop.

It was with this idea in mind that Lurija went to Uzbekistan in 1931 and 1932. In particular, he very much wished to disprove the thesis of Koffka and other Gestalt psychologists that visual illusions occur in every culture in the same way. Koffka himself participated in the second expedition, but left after a time due to illness. Details on his participation and the relationship between Koffka and Vygotskij can be found in Mecacci (1981), Scheerer (1980a) and Koffka (1981). Imagine Lurija's enthousiasm when it appeared that the residents of Uzbekistan, indeed, showed hardly a trace of the classical illusions. Cole mentions that Lurija immediately wired these results to Vygotskij (Cole, in: Lurija, 1979b, p.213). At that time, agriculture in Uzbekistan was just being collectivized, which meant that farmers were forced to join the kolkhoz (collective farms). Lurija wisely refrains from mentioning to what extent this was accompanied by violence. We also do not know if Lurija whole-heartedly agreed with these measures, simply tried to save his own skin or had a blind spot for the ethical aspects of scientific research. The fact is, however, that more than once, he participated in somewhat dubious research (Lurija, 1979b, p.202/3). Be that as it may, the collectivization measures resulted in a transitional situation. On the one hand, there were the "traditional" farmers, who could neither read nor write and never left their native village, and on the other the "revolutionary" farmers, who joined the kolkhoz and/or received a number of years of schooling. Due to the considerable obstacles their Islamic religion presented to their participation in public life, the women formed a separate group. Lurija's basic design was to form two, three or four groups and present them with tests concerning perception, classification, logical inference, etc. From the difference in achievement between the groups, he then deduced how the social structure, i.e. (non) membership in the kolkhoz or (non)attendance of schooling, determined the structure of the higher psychological functions.

From a technical point of view, this is an example of an ex-post-facto design with all its drawbacks (Campbell and Stanley, 1966). In addition, insight into

the study's value is hampered by Lurija's providing very little information on the composition of the experimental groups. These groups differed in their "educational level". Even assuming that Lurija would have been capable of distinguishing two or more distinct classes of "educational level", a number of problems still arise. He tells us nothing about whether or not the subjects were matched for sex, age, etc. We also do not know if during the interview, the conductor of the experiment was unaware of the "educational level" of the subject (the double-blind procedure). As is customary with Soviet psychological research, there is also no statistical analysis of the results. We shall therefore suffice with a description of a single section of Lurija's extensive study, that concerning the classification of objects.

The subjects were presented with 19 geometrical figures drawn on cards. The first task was to name the figures. In this, Lurija distinguishes abstract and concrete identification. He speaks of an abstract identification if the subject identifies the stimulus as an example of a geometrical category (for example: triangle, square, etc.). An identification is concrete if it demonstrates that the subject regards the stimulus as an object. The results are shown in Table I.1.

TABLE I.1
Number of concrete identifications of geometrical figures in relation to "educational level" (in percentages)

Type of subject	Number of subjects	Concrete identification
Women without any education	18	100
Women participants in courses	35	85,3
Participants in kolkhoz	24	59
Pupils teachers polytech	12	15,2

It can be seen here that while the concrete identification of geometrical figures still completely dominates the answers of women with no formal education, this identification has strongly declined with the pupils of the teachers polytech (a course at secondary school level). This is accompanied by a commensurate increase in abstract identifications. In Table I.2, are shown a number of examples of concrete identifications of objects.

TABLE I.2

Concrete descriptions of the objects

○	"moon"	●	"coin"
∪	"bracelet"	□	"bucket"
△	"mountain"	▭	"mirror"
⁘	"clock"	⁙	"bird-cage"

In all likelihood, the identification of objects also influences their classification. To investigate this Lurija asked the participants to divide the figures into groups. They were free to classify the figures as they pleased. The results are shown in Table I.3.

Lurija interprets the results of the Tables I.1, I.2 and I.3 as follows: The daily activity of, in particular, the groups of women lacking formal education is strongly related to concretely observable matters. This is also reflected in the way they identify objects. Once these figures have been identified concretely (that is, as being objects), this plays a role in classification. It is simply absurd to place the moon, a bracelet and a coin together in one group for in daily life, these objects have nothing to do with one another. And so subjects either give up clas-

TABLE I.3

The classification of geometrical figures in relation to "educational level" (in percentages)

Type of subject	Number of subjects	Refusal to classify	Classification		
			as object	according to individual graphic features	according to geometrical categories
Women without any education	18	21.8	20.4	57.8	-
Women participants in courses	35	18.3	8.4	55	18.3
Participants in kolkhoz	24	12.8	11.6	30.8	44.8
Pupils teachers polytech	10	-	-	-	100

sification altogether (they see no relation between the objects whatsoever) or they classify them as objects according to individual graphic features. A "roof" (Δ) and a "house" (□), for example, are then classified together, for in daily life they are also always associated with one another. Only after the subjects begin to work together with others and attend schooling do they become capable of more abstract classification principles. Lurija concludes this section of the study with this remark: "We must therefore conclude that to a significant extent, even relatively simple perceptual processes, such as the perception of [...] simple geometrical figures, is dependent upon the nature of the subjects' practical experience and their cultural level".

We should only like to add here that demonstrating the influence of daily experience on thought processes is one thing, but what interests psychologists is how and in what areas that experience exerts such influence. Is it training at (primary) school that raises the thought processes to a formal level? And more particularly: what specifically defined operations were Lu-

rija's uneducated subjects incapable of? True, using such terms as "social determination" or even "the influence of the school" does point to the important aspect of the social influence upon the structure of cognitive processes, but fails to indicate concretely how the cognitive structures then change.

Twins

Early in the nineteen thirties, Lurija and his co-workers had the opportunity of collaborating with the Medico-Genetic Institute in Moscow, where research was done, among other things, into the early development of mono- and dizygotic twins. Although the Lysenko affair had not yet broken upon the scene at this time, this research had heavily political overtones. In the Sovjet Union, as we know, great emphasis was placed upon the influence of the (system of) society in moulding the individual, and any opposition by obstinant genes was met with only the most unfavourable of commentary (see, however, Eysenck, 1982). This was also the reason that a large percentage of the research material Lurija collected was lost, leaving us only with a number of rather general descriptions (see Lurija, 1935, 1936, 1963, 1971, 1979b; Vygotskij, 1983).

Lurija's approach to the nature-nurture problem was drawn entirely from the ideas of Vygotskij. Although he and his associates were familiar with the research of, for example, Cyril Burt, they chose to follow their own course. One of Lurija's objections was that these researchers made use of the I.Q.-tests, which to him was not a suitable means of measuring the workings of the intellect. "These test, which were developed on a purely pragmatic basis to predict school performance, seemed to us then, as now, to be a hopelessly atheoretical and opaque means of observing the structure of higher psychological functions" (Lurija, 1979b, p.82). Lurija and his co-workers therefore decided to approach the matter differently. They de-

signed a series of tests in the area of higher psychological functions with a view to investigating their development. In the area of memory, for example, they used three memory tests devised by Leont'ev. But before describing these tests, we shall briefly discuss the difference between higher and lower psychological functions in the area of memory.

As did Vygotskij, Lurija believed it actually possible to distinguish so-called "natural" lower memory processes from so-called "mediated" higher memory processes. He assumed, for example, that through a particular memory test, for example having to repeat a series of numbers, the "natural memory" could be measured with a fair degree of accuracy. From interviews with subjects, Lurija deduced whether or not he was dealing with an actual case of memory based upon natural processes. If the interviews indicated that subjects had in no way structured or recoded the material, Lurija determined that they had relied upon their natural memory. If, however, the subject appeared to have looked for and found certain verbal or logical relationships in the material, the subject's test results were presumed to be based upon culturally mediated higher memory processes. From this point of view, abstract stimulus material provides a more accurate estimate of the "natural" memory than of the "higher" memory. During child development, however, changes occur in both the natural and the culturally mediated memory, both of which expand. According to Lurija, earlier research had indicated that in solving memory problems, children of 5 to 7 years of age still use a primarily "natural" approach, while children of 11 to 13 start with a predominantly culturally mediated approach. Lurija therefore sees the relationship between lower psychological memory processes (being genetically determined) and the higher ones as follows. In young children, lower memory processes dominate (passive impression, unmediated by speech or symbols). As the child grows older, the higher memory processes begin to predominate. Both processes remain, however, distinguishably intact, the memory capacity in both cases increasing.

Lurija's idea was now to test the above hypothesis by means of research into twins. His reasoning is that in tests calling for the use of the primarily genetically determined "natural" memory, the test results of monozygotic, identical twins would be approximately the same, even if they were reared in different environments. But memory tests calling for the use of culturally mediated processes will be dealt with almost identically even by dizygotic, non-identical twins, assuming of course they have been exposed to the same cultural influences (see Vygotskij, 1983, p.131-132). Table I.4 shows the distribution of the 100 sets of twins Lurija used in his experiment.

TABLE I.4

Distribution of 100 sets of twins according to type and age

type of twins	age	
	5 - 7	11 - 13
monozygotic	25	25
dizygotic	25	25

As we have already stated, Lurija used in his study three memory tests designed by Leont'ev. In view of the fact that in his analysis, Lurija only utilized the results of two of them, we shall confine our discussion to those two tests. In the first test, after having been shown 9 geometrical figures, the children had to try to re-identify them when shown in a collection of 34 such figures. In a second test, the children were shown 15 words accompanied by pictures bearing no obvious relation to them. The children were repeatedly shown these words until they knew the entire series. If they required support from the pictures in remembering the words, they had to draw a relationship between the word and the picture itself. Lurija regarded this task

as a suitable means of measuring culturally mediated memory. The child can use the pictures as a mnemonic, and therefore cultural device, thereby increasing the capacity of the natural memory.

After presenting the children with both these tests, the following results were obtained (see Table I.5).

TABLE I.5

The relationship between age and type of memory for identical and non-identical twins

age	type of test					
	"natural" test			"mediated" test		
	D_d		D_m	D_d		D_m
5 - 7	18	>	5.4	4.4	>	1.9
11 - 13	14	>	5.6	1.2		1.5

D_d = the difference in test scores between dizygotic (non-identical) twins;
D_m = the difference in test scores between monozygotic (identical) twins;
> = larger than (not tested statistically).

In a quantitative sense, these results bear out Lurija's expectations. For tests in which the natural, direct memory dominates, the results at both age levels for identical twins bear much greater similarity than those of non-identical twins (in both cases, $D_d > D_m$). In the test drawing upon the mediated memory, the same pattern can be seen for the young children (4.4 is larger than 1.9), but for the older children, D_d and D_m are nearly identical (1.2 is approximately equal to 1.5). This is exactly what Lurija had expected, for in this second test, genetically determined, natural

memory processes no longer play a role: the children use culturally mediated forms of memorization.

Lurija's assumptions were also confirmed by the qualitative analysis. Interviews with the children indicated that for the "natural" test, both older and younger children memorize in an elementary fashion. Hardly any of the children used indirect or logical processes in remembering the geometrical figures (Lurija, 1979b, p.85). For the second test, though, considerable differences emerged between younger and older children. Most of the 5 to 7 year-olds memorized the words in the second test in the same direct fashion as they memorized the geometrical figures in the first; they were incapable of utilizing the pictures, of making a logical connection between them and the words. "The children were completely unable to say anything about the relationship between the words they had remembered and the pictures they had seen. When we asked the children about how they remembered the words, they usually responded: I just remembered them. I do not believe that their answers reflect a lack of introspection. Their memory was simply not mediated by the pictures" (Lurija, 1979b, p.85-86).

Several objections can be made with respect to this study. Let us first try to discern from Lurija's incomplete description what tests the children were actually presented with. In the first test, the children were presented with 9 geometric figures which they then had to identify among a collection of 34 figures. This, then, was a visual recognition test. We assume that the other 25 figures were of the same type (triangles, squares, circles, etc.) as the first 9. But it is unclear how similar the 34 figures were and if the children received any information in this regard. And yet this is of immediate importance to the memory process. If all the figures were very similar, an extensive internal coding process would be needed. If there is little similarity, simply having the children examine the figures is probably sufficient; the 9 figures in question would then be identified automatically. It is also unknown how much time elapsed between the presentation of the 9 figures to be remembered and

the presentation of the entire collection. And yet this, too, may determine a particular memorization strategy.

In the second test, 15 word-picture combinations were presented. From the description, we infer that in the first instance, the children were asked to memorize the series of fifteen words. This, then, is a recollection test. Actually, we do not know if the words were presented visually or auditorially. It is unclear how the simultaneous presentation of pictures can facilitate the memorization of the word-series and whether or not the children knew that after the memorization stage, they would be asked to name the words solely on the basis of seeing the accompanying pictures. It would appear to us that two different memorization processes are at work here; one involving learning 15 words in a particular order (possibly constructing a relationship between one word and the next) and the other involving learning 15 word-picture combinations (perhaps attempting to describe the word and the picture in one sentence). In both cases, it is possible to tackle the problem with either "brutal force", i.e. simply "drumming" the series of words or word-picture combinations into one's head, or with a more subtle method, for example by constructing a running story on the basis of the pictures or the words. To what extent the stimuli allowed for this possibility is unknown.

So in both tests, it is unclear what the children were actually assigned to do. Both allow for different memorization strategies ("drilling" versus extensive verbal description or structuring). Because it was less abstract, the material from the second, mediated test probably lends itself better to verbal recoding. But to our mind, this does not automatically indicate a reliance upon natural, genetically determined memory processes in the "natural" first test. It is even questionable whether such processes exist. From perceptual psychology we know that learning to recognize and differentiate between simple, abstract figures is also greatly dependent upon experience. We are therefore inclined to see the difference between the first and second memory test not so much as one between a "natural", genetically

determined test and a "culturally mediated" test, but far sooner as a difference in the extent to which the material allowed for verbal description and structuring. In addition, a recollection test probably requires a more active, structuring role from a subject than an recognition test.

In brief, to our mind, two entirely different tests are being compared here, but the difference has nothing to do with the nature-culture problem. It was naïve of Vygotskij and Lurija to believe that it was still possible to encounter almost entirely genetically determined processes in 5 to 7 year-olds. Here again, the earlier criticized dichotomy between natural, passive lower functions and cultural, higher ones takes its toll. On top of that there are many deficiencies in the design and description of the study. This makes it difficult to draw any definite conclusions from it. It would first appear necessary to replicate the study, this time meeting the above mentioned objectives. Perhaps such a replication would result in a confirmation of Vygotskij's optimistic assertion that "the results of experimental studies demonstrate that the higher psychological processes can be educated more easily, for the source of the development of their structures is the collective education of the child" (Vygotskij, 1931, in: Vygotskij, 1983, p.132).

VYGOTSKIJ ON RESEARCH METHODS

In one of the first paragraphs of this chapter we mentioned some of Vygotskij's epistemological points of view. In the preceding paragraphs, we discussed two cultural-historical research projects. We clearly saw how Vygotskij's theoretical approach determined the experimental designs. We will now deal with his view on research methods more explicitly. To Vygotskij's mind, the cultural-historical theory of higher psychological functions also gives rise to a new research strategy. In the final analysis, he remarks, research methods are always a reflection of the way fundamental problems in

psychology are viewed and resolved. But in order to see the research strategy Vygotskij advocates in its proper context, his view of the methods used until that time should first be considered.

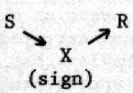
The Stimulus-Response Model

According to Vygotskij, psychology up to then had been dominated by the so-called stimulus-response model. That is to say, the experimenter presents the subject with a stimulus and then monitors the subject's response to it. This applied to both the objectivistic school in psychology, with which the names of Watson and Bechterev are associated (and in the borderline area between physiology and psychology, of course, the name of Pavlov), and to the subjective psychology of the Würzburg school and Wundt. Vygotskij believed that although these two schools do differ in their theoretical interpretation of data, they both embrace the same methodological approach. This also applies to Wundt's psychophysics, which ultimately regarded the essence of the psychological method as the systematic variation of stimuli that result in a change in the processes they are associated with (Vygotskij, 1978, p.59). Vygotskij does, by the way, believe there are good sides to the stimulus-response model. To his mind, it brought psychology closer to the methodology and spirit of the natural sciences and paved the way for an objective psychological approach (Vygotskij, 1978, p.59). But with this approach it is only possible to study lower psychological functions, representing the passive aspect of human behavior. Wundt, too, had said as much, and for that reason had excluded the higher psychological functions from the experimental approach (see above).

The stimulus-response model, however, does not do justice to those active components so characteristic of human behavior. This can be demonstrated as follows: It had already been stated by Pavlov that both humans and animals had one fundamental type of behavior in common,

that of signalization. In other words, both humans and animals are capable of learning to make a connection between two stimuli such that one stimulus begins to function as a signal for the other. Pavlov also regarded words as being such stimuli (the second signal system). According to Vygotskij, however, humans possess an extra faculty separating them from every animal, that of signification. "... human behavior is distinguished by the very fact that man creates artificial signal stimuli, among which is the "great signaling capacity" of speech, and thus masters the signal activities of the cerebral hemispheres ... the fundamental and most general activity distinguishing man from animals, psychologically speaking, is signification, i.e. the creation and use of signs" (Vygotskij, 1977b, p.62).

It is with the help of these self-created stimuli (the signs) that humans become capable of directing their own behavior. Most of the signs are acquired by the human child through its contact with adults. The child assimilates these signs through its contact with adults, in particular the signs of language (words and concepts). But humans also construct non-lingual signs. This is illustrated by the so-called rudimentary psychological functions. These functions are remnants of the historical development of the human species. An example of a rudimentary function is the memory aid. Imagine, for example, that I get instructions (stimuli) to perform a certain act (response) later in the day. I can now attempt to "drill" the connection between stimulus and response (with or without intervening links) into my head. This is what occurs with adults in a vast majority of cases. In that case, the creation of the artificial links between stimulus and response is no longer visible. But I can also tie a string around my finger. In that way, I myself create an artificial external stimulus (a sign) with a view to influencing my own behavior (Vygotskij, 1982a, p.103ff). Instead of the model S → R, we get the mediated model



Vygotskij calls memory aids rudimentary psychological functions because he assumes that all psychological behavior has its origin in external social behavior (the principle of internalization). For most functions, this process is no longer visible. Sometimes, however, we are fortunate enough to have artificially created stimuli that are not yet internalized. In addition, it is possible to present the subject with signs, words or concepts and in so doing provoke an internalization process. This provides us with the opportunity of observing the internalization process *in vivo*.

The rudimentary psychological functions therefore demonstrate that humans do not react passively, as in the stimulus-response model, but create stimuli themselves, which in turn influence their own behavior. Vygotskij: "Man's active alteration of nature is the central characteristic of human adaptation. The alteration of nature by man is the foundation of all human history. By necessity it also presupposes the active alteration of human behavior. Influencing the natural environment and changing it through his activity, writes K. Marx, man simultaneously changes his own nature" (Vygotskij, 1977b, p.62/63).

A New Approach

For Vygotskij then, the stimulus-response approach has little to recommend for itself. In line and intermingled with his cultural-historical theory, he therefore attempted to find a new methodology for psychological research. In doing so, his departure point is the distinction Engels makes between naturalistic and dialectical approaches to human history. According to Engels, the substance of the naturalistic approach to history was the assumption that nature alone influences human beings and that historical development is only determined by natural conditions. On the other hand, while the dialectical approach admits of nature's influence upon human beings, it also posits that in turn, human beings influence nature. Through these changes in

nature, they create new natural conditions for their existence. It is this picture that Vygotskij also believes applicable to the situation in psychology. All the stimulus-response methods share the shortcomings Engels ascribed to the naturalist approach to history (Vygotskij, 1978, p.61). They see the relationship between human behavior and nature as reactive one-way traffic.

Vygotskij elucidates his research strategy, in which he attempts to avoid the error described above, on the basis of three concept-pairs, namely (1) object-process, (2) explanation-description, and (3) fossil-genetic. Each of the concept-pairs expresses an aspect of his method. Regarding the first concept-pair, he remarks that all the psychologists of his day regarded the psychological functions as objects (the Gestalt psychologist Koffka had said this as well) requiring analysis as to their constituent components. It is, of course, a valid approach, but it should be complemented with the genetic approach. For every psychological process is continually changing, and in order to acquire a full understanding of a higher psychological process, these changes too, must be studied (Vygotskij, 1978, p.61). This can be done by observing the process of change, but also by eliciting this change. This is why Vygotskij, like Werner, calls this strategy the experimental-genetic approach. By means of this approach, it is possible to reconstruct all the stages in the process' development.

Vygotskij illustrates this by means of the two other concept-pairs. He states that a psychological process can be analyzed in two ways: in its present manifestation, as it is now, i.e. phenotypically, or according to its development or origin, i.e. genotypically (Vygotskij adopted this distinction from the Gestalt psychologist K. Lewin). This is for him an important distinction, for two psychological processes may very well be phenotypically identical, but at the same time be genotypically (or as Vygotskij at times expresses it, causal-dynamically) radically different. Vygotskij gives the example of the language of a child of one and a half to two years old, greatly resembling

the language of adults. But from a genetic or genotypical point of view, the language of a two year old and that of an adult are not related at all. On the other hand, egocentric speech and internal speech do differ phenotypically, while genotypically they are strongly related. Vygotskij relates this, to him, important distinction to Marx's observation that if "the source of things were to correspond with the form of their external manifestation, every science would be superfluous". He might also have referred to the distinction Erscheinung-Wesen (appearance-essence), which we pointed out in our paragraph on Hegel.

Vygotskij classifies those who suffice with an analysis of the external manifestation of psychological processes as the descriptive psychologists. Those who in addition also conduct genotypical analyses earn the title of objective psychologist (see Vygotskij, 1978, p.63). To Vygotskij, exclusively phenotypical analysis is so often inadequate because existing behavior is often fossil behavior. With this he means that many psychological processes have already undergone their development. So far as their development is concerned, they have, as it were, already died and are analyzed posthumously. This applies in particular to automatisms. The special characteristics of fossil behavior can only be understood by means of an analysis that takes account of its developmental history. As did Blonskij, Vygotskij posits that psychology should not study the product of a long development, but the developmental process: behavior can only be understood as the history of behavior (Vygotskij, 1978, p.65).

Some Techniques

Until now, we have actually done nothing more than to present a number of theoretical considerations underlying Vygotskij's research strategy and arising from his cultural-historical method. But Vygotskij also used a number of devices and methods to uncover the origins of behavior, which we shall now briefly discuss.

In a number of problem-solving situations, Vygotskij firstly often introduces obstacles or difficulties interfering with routine solution processes. For example, in a study on children's communication and the function of egocentric speech, Vygotskij gave children the task of working together with children that spoke another language or were deaf (Vygotskij, 1978, p.12). Secondly, Vygotskij often presented the children with external aids that could be used in finding a solution in a problem-solving situation. In this case, he was interested in seeing how the children used these aids (signs) in reaching a solution and to what extent this process was related to age. This was connected with his view of higher psychological processes as processes involving the use of instruments or tools (see above). Thirdly, Vygotskij confronted children with a task that was actually beyond their knowledge and skills. The intention was to discover what rudimentary capacities and skills the child had already acquired (the zone of proximal development, see above).

It can be easily seen here that what the three techniques have in common is that they are designed to expose behavioral development. In addition, no attempt is made with these techniques to limit behavior or to observe a particular aspect of behavior through systematic variation of a stimulus. On the contrary, in Vygotskij's experiments, the child is given the room to exhibit all kinds of behavior. It is, after all, the way the child analyzes its behavior that interests Vygotskij.

It is said of psychology that interesting conclusions are rarely valid and valid conclusions rarely interesting. This dilemma is also applicable to Vygotskij and his associates. Many of his findings are extremely interesting. But are they also valid? We are not really in a position to judge that here, simply because the reporting of the research is so incomplete. The experiments are discussed by Vygotskij and his associates only in outline form, without providing the raw data and neglecting to subject them to any statistical operations. Often, the research design is also only described in the briefest of terms. The suspicion

arises that many of the studies with which Vygotskij illustrates his brilliant structures are not, in fact, very significant at all. Cole, too, has expressed this criticism (in: Vygotskij, 1978, p.11). In his intellectual biography, in fact, Lurija also fully acknowledges this criticism. "The individual studies that we carried out at this time ... must be considered banal in and of themselves. Today we would consider them nothing more than student projects. And this is exactly what they were." (Lurija, 1979b, p.51). It would appear that more than being an empirical researcher, Vygotskij was in essence a theorist who succeeded in elegantly applying and refining the dialectic-materialistic classics in psychology (Radzichovskij, 1979, 1982; Jaroševskij and Gurgenidze, 1977). Though he valued the importance of the experiment, Vygotskij himself was a system-builder, who left the laborious work of systematic experimentation to his co-workers Lurija, Leont'ev and others. In this regard, it is interesting to compare Vygotskij with his contemporary Piaget. Piaget, too, was by and large a theorist whose experiments were more an illustration of an all-embracing theory than a test of its validity. It is perhaps possible to trace this similarity between these great researchers to their familiarity with the ideas of Freud. It is, after all, known that Piaget borrowed his clinical method from psychoanalysis and in any case, we know that throughout his entire life, Lurija regarded the clinical session as a valuable research technique. If Vygotskij was also influenced by Freud in this regard, it becomes clearer why his research is actually more diagnostic than experimental (see also chapter II and III for recent developments regarding the role of the experiment in psychological research).

Finally, as did Cole, we mention a number of consequences of a more general nature that Vygotskij's research approach has for contemporary psychology (Cole, in: Vygotskij, 1978, p.14). Firstly, in scientific research, both qualitative and quantitative data should be of importance. Secondly, we can conclude from his work that to Vygotskij, pure laboratory research does not occupy a prominent place in research practice as a whole. It is just as good, if not better, to

conduct experiments or observations at school or in play situations. Thirdly, with Vygotskij, the strict boundaries between the different sciences concerned with humans and their culture become blurred. If we are to build the foundations of a cultural-historical theory, we will have to make use of data from developmental psychology, pedagogy and neurology, from sociology and anthropology.

CONCLUSIONS

In the preceding paragraphs, we have discussed some of the most important ideas of Vygotskij and his co-workers. Much, however has had to be left undiscussed. Even now, after 50 years, we are still impressed by the abundance of ideas Vygotskij formulated in less than a decade. Lurija has even written that by the end of the twenties, the course of his own further career was already established: he would devote his remaining years to developing Vygotskij's ideas (Lurija, 1979b, p.56). It is therefore difficult to pass a final judgement on the Cultural-Historical School.

Vygotskij, Lurija and Leont'ev began their reconstruction of psychology with no small pretentions. Broadly, their research was as follows. Firstly, they wished to trace the development of the higher psychological functions from the natural, biological functions. This was the developmental branch of their research. Secondly, the three researchers and their associates wished to study disturbances of the higher psychological functions resulting from brain damage. This was the pathological branch of their research (see the paragraph on the physiological substratum).

Their developmental research comprised three themes, each touching upon a different aspect of the biological-cultural relationship in human cognitive development. The first theme was to investigate the determinants of the structure that higher psychological functions assume in child development through the

culture in which the individual grows up. To this end, a sort of cultural-anthropological study was conducted (see the paragraph on Uzbekistan). The second research theme included the longitudinal study of identical (monozygote) and non-identical (dizygote) twins. A comparison of mono- and dizygote twins offered the opportunity of unraveling the influence of nature and culture. The research approach used, however, differed from the approach customary in the West (see the paragraph on twins). The third research theme concerned a comparative study of the intellectual development of normal and several types of "disabled" children (see the paragraph on the disabled child).

The combination of the developmental and pathological branches should then elucidate the relationship between the biological and cultural aspects of the development of the higher psychological functions. But this research plan was only concerned with human ontogenesis. In addition, Vygotskij and his colleagues claimed to have provided an air-tight theory of human phylogenesis. Of course, the history of human development does not lend itself to stringent experimental testing. The data in this area are, indeed, of a rather speculative nature, and in Vygotskij's day this was even more the case. Some researchers, for example, regard tool-making and hunting as the start of an accelerated evolution resulting in the *homo sapiens*. Tool-making requires a more complex neurological organization and tools increase the chance of survival. But for the time being, there is painfully little evidence to support such speculations (see Leakey, 1981; Klix, 1980). In addition, the category labor as used in the dialectical tradition is not easy to operationalize. In any case, Vygotskij does not regard the use of tools as labor. Indeed, Köhler had already demonstrated that chimpanzees can also make and use tools. But Marx and Engels had written that labor played a decisive role in human development. In the preface to one of Köhler's books translated into Russian, Vygotskij attempts to resolve this seeming contradiction (Vygotskij, 1930, in: Vygotskij, 1982a, pp.210-220). Though he acknowledges that there is a gradual transition between the use of tools by animals and that by humans, he still

sees a qualitative difference. According to Vygotskij, tools played no role whatsoever in the chimpanzee's struggle for existence or adaptation to the environment. It can therefore not be said that the use of tools was characteristic of the ape. But as Marx and Engels had already argued, this was the case for human beings.

There is also great uncertainty concerning the time at which speech developed. Wilson, for example, wonders if speech originated more than two million years ago with the Australopithecus-anthropoids or much more recently with the *homo sapiens* (approximately 100.000 years ago). On the basis of the shape of the palate, for example, researchers investigating this problem attempt to determine whether or not a particular type of human was capable of speech. Not much is clear in this area, although some researchers are inclined to place the origin of speech very late in the stage of *homo sapiens* (Wilson, 1976, p.559; Leakey, 1981). All in all, it is not really possible to estimate the value of the theses postulated by the anthropology of the Cultural-Historical School. But the fragmentary archeological data we do have do not appear to run contrary to this historical materialistic theory of the role of the labor and speech in the history of human development (see Klix, 1980).

In the preceding paragraphs, we have more than once criticized the view of Vygotskij and his associates on the development of the higher psychological functions. We shall therefore not repeat that criticism here. It is clear that many of their notions require clarification. Thorough research will have to demonstrate, for example, whether the views of the Cultural-Historical School can play a fruitful role in the nature-nurture debate. Many of the cultural-historical ideas, however, have already demonstrated their fruitfulness. We need mention only the internalization theory, developed in a particular direction especially by Gal'perin, and the ideas concerning the representation of the higher psychological functions in the brain, further developed by Lurija. In addition, the Cultural-Historical School also played an important

role in the resistance to an S-R approach to human behavior. Finally, Vygotskij and his co-workers defended the combination of quantitative and qualitative methods of research.

If we compare the material of the preceding paragraphs with the earlier formulated research plan, we can do little but note that this plan has yet to be fulfilled. Its design was too ambitious. But as a research program, it is still very capable of generating much fruitful study. Only through such study will we know if the cultural-historical theory can stand the wear and tear of time. "We know very well that in taking the first step, we cannot avoid making many, and perhaps serious mistakes. But the whole point is that the first step should be taken in the right direction. The rest will take care of itself. What is invalid will be eliminated. What is missing will be supplied" (Vygotskij, 1983, p.342).

INTERMEZZO

In the second part of this book, we shall be discussing Holzkamp's Berlin School and Riegel's Dialectical Psychology. Their origins, like those of the Cultural-Historical School, are to be found in the Soviet Union of the nineteen-twenties. The Berlin School bases its work on that of Leont'ev, Vygotskij's associate from the very beginning, while Dialectical Psychology drew its inspiration in particular from the work of Vygotskij's contemporary, Rubinštejn. Vygotskij, Leont'ev and Rubinštejn all tried to develop a dialectical psychology, based upon the work of Hegel, Marx, Engels and Lenin. It is therefore not surprising that there are so many parallels in the research and theoretical approach of the three abovementioned schools.

The parallels between the Cultural-Historical School and the Berlin School are particularly clear. The latter is, in fact, a further development of the former; not surprising in view of the fact that, as we mentioned earlier, the Berlin School makes ample use of Leont'ev's work. In its initial stages in particular, Leont'ev's work was entirely dominated by Vygotskij's cultural-historical theory. His first experimental research, into mediated memory, was in fact a direct demonstration of Vygotskij's ideas on the role of the

sign in psychological development. But even later, he continued to be greatly influenced by Vygotskij. In discussing the six most important features of Leont'ev's theory of activity, Wertsch (1981b) provides an illustration of this influence; no less than four of them prove to be adapted directly from Vygotskij's work. This also applies to the functional-historical method the Berlin School adapted from Leont'ev. This method is a particularization of the developmental or genetic explanation. To Vygotskij's mind as well, genetic analysis can be as concerned with phylogenetic and historical data as it is with ontogenetic data (see Wertsch, 1981b, p.27). The anthropological model used widely within the Berlin School is also strongly related to Vygotskij's viewpoint. Characteristic in both schools is that the biological aspect of any psychological process is assigned a minor, prefatory role, gradually overshadowed by cultural aspects.

From about 1940, Leont'ev begins to detach himself somewhat from his mentor, emphasizing in particular the concept of activity. According to Leont'ev, psychological processes represent a particular form of activity and are derived from people's involvement with external objects (see Rahmani, 1973, p.47). He criticized Vygotskij's emphasis on the role of social interaction with adults in a child's psychological development. These developments within the Cultural-Historical School have already been described in the first part of this book. In his criticism of Vygotskij, Leont'ev was in all likelihood influenced by Rubinštejn (see Wertsch, 1981b, p.13). In fact, both Leont'ev and Rubinštejn were seeking a better connection with Engels' "Dialectics of Nature", which appeared in translation in 1925. According to Engels, the opposition between nature and spirit is not that of two different substances in a particular genetic relationship. Consciousness is not a thing in itself, but an attribute of material objects organized in a certain way, or a process which takes place within them (see Kolakowski, 1981, p.379).

This monistic view, too, is clearly to be found in Leont'ev and Rubinštejn, and it is also this view that Riegel finds so appealing in Rubinštejn's work. He

states that Rubinštejn's synthesis tries to overcome the mind-body dualism which has remained unbridgeable in Western thought and has led to the development of several different 'psychologies'. According to Riegel, the mental introspectionists such as Wundt, Titchener and the psychologists at Würzburg focused exclusively upon consciousness. Their opponents, the behaviorists, focused upon the overt stimulation and overt reactions of the organism (Riegel, 1979, p.29-30).

Rubinštejn emphasizes the unity of consciousness and behavior. As did Leont'ev, he puts forward a third category from which to observe psychological development: human activity. Rubinštejn's conception of human activity is that of a dialectic of subject and object. In this conception, he tended to emphasize the role of independent exploration and interaction with the objective world as the source of our reflection on it (see Wertsch, 1981b, p.13). As does Wertsch, we believe that the principle difference between Vygotskij and Rubinštejn is the importance the former ascribes to speech and language. It was this emphasis upon speech and language that inclined Vygotskij to regard child development as a result, in particular, of child-adult or subject-subject interaction.

Another central idea in the work of Vygotskij, Leont'ev and Rubinštejn is that all development is a dialectical process. In Riegel's words: "Development does not merely consist in the continuous accumulation of information and goods but progresses in dialectical leaps. Modifications are brought about by structural changes created by the organism. But as these structures change, the functions change. The invention of some tools and linguistic forms changes the expression and significance of men's work and communication" (Riegel, 1979, p.30). These words of Riegel's were occasioned by Rubinštejn's work, but of course, dialectics are also ever-present within the Cultural-Historical School. Vygotskij, for example, wrote in regard to child development that it proceeds "dialectically and through conflicts". Elsewhere, he speaks of "the drama of child development" (Vygotskij, 1983, p.161, 272). His work is filled with such statements, which

are obviously incompatible with a conception of development as continuous accumulation.

But that is not all. The entire theory of lower and higher psychological processes is a further development of Vygotskij's view of child development. By utilizing cultural tools, humans are capable of transforming their biological development into a cultural development. As Riegel wrote: "At the beginning, the development of an organism is mainly determined by biological evolution. With the historical growth of psychic activities, however, development becomes co-determined by cultural-sociological evolution" (Riegel, 1978, p.12). Riegel's suggestion that the invention of certain tools and linguistic forms changes the expression and significance of human labor and communication is also developed by Vygotskij. Actually, the tenor of Vygotskij's work is much more radical. Through the use of tools and signs (linguistic forms), the course of all human development is altered. And in fact, Riegel (1979) is giving a fair description of Vygotskij's cultural historical theory when he says that a basic assumption of Dialectical Psychology is that people create language but, at the same time, are created by it.

That Rubinštejn hardly knew of Vygotskij's work is unfortunate, for we believe it would have appealed to him greatly. Riegel, too, might have been assisted in making his ideas more concrete by a familiarity with Vygotskij's psychological work. More than Rubinštejn, it was Vygotskij's strength that he succeeded in applying dialectical materialism to concrete psychological research. This is in part the reason why we agree with Wertsch (1981b, p.14) that Vygotskij's work was of more significance to psychology than that of Rubinštejn. But it is not our point here to pass final judgement on the significance to psychology of the work of Rubinštejn, Vygotskij or Leont'ev. It is more important to realize that these figures grappled with the same problems and, by and large, came up with the same solutions. And it is this fact that explains the great affinity between the Cultural-Historical School, the Berlin School and Dialectical Psychology.

PART II: RECENT DEVELOPMENTS IN CRITICAL PSYCHOLOGY

Chapter II:
The Berlin School Of Critical Psychology

INTRODUCTION

Some Biographical Notes

According to Holzkamp's autobiographical notes (Holzkamp, 1972, p. 207 ff), his first confrontation with a critical approach to traditional psychology consisted of his dispute with a student - Irmgard Staeuble - about her master's thesis. This was in 1964. This thesis was concerned with a problem in the area of research into prejudices and was written from the viewpoint of the critical theory of the Frankfurt School. It was the representatives of this school - Adorno and Habermas - who had begun to challenge the representatives of the traditional "positivistic" approach, namely Popper and Albert (see Adorno *et al.*, 1978). These discussions about the foundations of traditional scientific research, primarily restricted to sociology, were to go down in history as the "positivism conflict". Two important themes in this debate are the problem of research into the interrelation between individual phenomena and the problem of values in scientific research. With respect to the first theme, there was the question of the possibility and

necessity of the "pretence of totality", a notion held particularly in (neo-)marxist circles. Does scientific study consist of a series of detailed studies from which insight into historical and social interrelations emerges, as it were, by coincidence? Or should one's attention in each detailed study remain focused upon the historical and social context in which the detail being examined is situated, ensuring the development of a meaningful over-all view? The debate on values concerned the question of the role of science in determining the goals for which scientific knowledge should be employed. In positivistic ranks, the position of value-free science was defended, in particular with references to the "naturalistic fallacy" (Hume). Science is only capable of saying something meaningful about what is; what should be, however, could never be logically deduced from purely factual premises. Science should refrain from statements about the desirability of particular norms, values and goals. Alternatively, the critical camp pointed to the inevitability of supporting or opposing certain goals for which knowledge is employed. And because of this inevitability, it seemed preferrable that science should responsibly, that is rationally, consider these goals, rather than cling to a semblance of neutrality.

Staeuble confronted her advisor with the necessity of conducting this discussion among psychologists as well. From his positivistic point of view, Holzkamp had initially made a good number of critical notes with respect to the master's thesis mentioned above. But in the course of the discussion, he was more and more inclined to take the view that psychology should not be allowed to escape the criticism of the Frankfurt School. It did, indeed, appear that psychological research restricted itself to tiny details of behavior which were observed under very artificial experimental conditions. To the extent that it was verified according to the then current methodological criteria, psychological knowledge was fragmented knowledge. The all-embracing theories of such classical psychologists as Freud had fallen from grace long before, at least from the point of view of the dominating "positivistic" thinkers. The clinical "holism" had in general made way

for a scientifically oriented analytical approach. Alternatively, the question rose, particularly in post-war Germany, to what extent psychology had surrendered itself, by virtue of its demands of a value-free science, to the values and norms of dominant groups in society? In Germany, recent history had clearly demonstrated that a system of values can become totally perverted when science, as in the "positivistic" conception, functions uncritically. The idea of value-free science was therefore difficult to defend. Critical students in particular raised the question whether psychology should not protect itself from such catastrophic instances of 'value-breakdown'. Should it not take the initiative and determine in a more rational way which values and norms should guide scientific research and the application of scientific knowledge? (Note: in this and the following paragraphs we used material from Van IJzendoorn and Van der Veer, 1983.)

The Development of the Berlin School

At the time Holzkamp was confronted with this sort of questions, he could not have conceived that 15 years later, people would speak of Critical Psychology as an influential "school" with a clearly recognizable identity (note: where the C and P of Critical Psychology are capitalized this refers to the Berlin School). The first international congress, held in Marburg in 1977, drew more than 3000 participants, from both within Germany and abroad. A clearer illustration of the resonance of a movement with a relatively short history is hardly imaginable. This school, which Holzkamp preferred not to regard as a separate psychological school but as a necessary addition to traditional psychology, was to move away from its original source of inspiration - the critical theory of the Frankfurt School - and return in particular to the more orthodox Cultural-Historical School of Vygotskij, Lurija, Leont'ev and others (see Part I).

From the very beginning, Critical Psychology was a phenomenon to be found only in Berlin. Though many people outside Berlin and even outside the German Federal Republic were responsive to the results of this school (especially in Denmark and the Netherlands), the developers of Critical Psychological theory have nearly all worked at the Psychological Institute of the Free University in Berlin (Holzkamp, 1978). Some critics even defend the proposition that the Berlin School has become a kind of family enterprise and might better be called the "Holzkamp School" (Geuter, 1977; Huber, 1977). The personality cult such a name implies, and which is unmistakably present in the work of Holzkamp's students, does not appear to be entirely unfounded. One could, in fact, defend the proposition that without the personality of Holzkamp, (historical materialistic) criticism of psychology might have developed but not a Berlin School of Critical Psychology. Holzkamp was indeed the very person to develop detailed, and for many people convincing, immanent criticism of aspects of then current psychology, through his intensive practical experience with traditional psychological research and through his original identification with a variant within the "positivistic" approach, constructivism (Holzkamp, 1968).

He wrote this criticism in 5 articles, published in the period 1968-1970, and published again in the collection "Kritische Psychologie" (Critical Psychology), in which they were provided with an introduction. This collection was rightly subtitled "Vorbereitende Arbeiten" (Preliminary Work), for it only resulted in an attempt, discussed briefly and with little foundation, at an alternative approach to psychological questions (see Brandt, 1979). Emphasis was still laid entirely upon reconstruction and criticism of traditional scientific research. For example, he criticized the lack of social relevance in most psychological research results and exposed the "organismic" view of the human being behind current experimental practices (see the following sections). In addition, he further developed his constructivistic variant of a positivistic methodology, following here Dingler's example, in order to find the necessary areas for immanent criti-

cism. Finally, in a self-critical, nearly autobiographical final paragraph, he somewhat abruptly embraces the historical materialistic theory, which as a sort of *deus ex machina* would definitively correct all the shortcomings of traditional psychology. For example, the solutions to the problems of relevance and values would be implied in the results of research of good quality: in other words, the scientific nature of research itself would guarantee a high level of social and ethical relevance, independent of the subjective choices of the individual researcher or of the specific area of research.

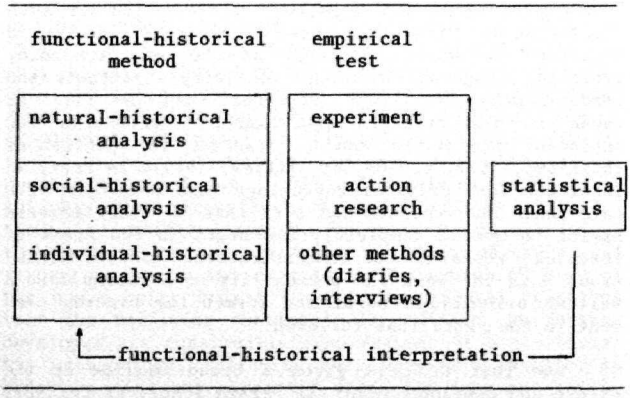
Alternative research strategies were, however, hardly developed. Later, Holzkamp and his associates would try to realize these programmatic wishes and expectations. At the start of the seventies, Holzkamp had a unique opportunity to create the conditions, also in the areas of organization and personnel, under which Critical Psychology could be developed into a school with its own research program. After he had made the necessary theoretical preparations for such a development, a schism, partially brought about by students, developed at the institute at which he was a professor (see Buss, 1979a, p.89ff for a theoretical interpretation of the influence of students on the development of a new paradigm). The departure of a number of more "conservative" assistants and professors led to a situation in which Holzkamp, as the only remaining professor, together with the remaining assistants and predominantly "critical" students, had to fill 12 vacancies for research assistants, 5 vacancies for assistant professors and 4 vacancies for professors (Holzkamp, 1972, p. 269; cf. Mattes, 1979). In fact, an Institute for "Critical Psychology" had developed with more than 500 students and more than 70 staff members having to make a completely new start in the areas of teaching, research and administration. In this situation, both the work and personality of Holzkamp were a welcome orientation point and formed the binding element in the years that followed.

Now that we have given a broad outline of the origin and development of the Berlin School of Critical

Psychology, we would like to devote particular attention in the following paragraphs to the epistemological and methodological aspects of this school. Due to its often devastating criticism of current psychological research, this school has taken on the responsibility of proving that an alternative "critical methodology" is indeed possible. We will devote some attention to this critical methodology. This does not mean, of course, that we can ignore the critical remarks Critical Psychology had made concerning the various methods of psychological research. It will be clear that despite all the work done in the seventies, the "critical methodology" Holzkamp resolutely introduced, still primarily consists of criticism of traditional methodology. In fact, one cannot yet speak of a flourishing "critical" research practice. The structure of this chapter is determined by Holzkamp's view of the general structure of the critical psychological research process (see Table II.1).

TABLE II.1

General Structure of the Critical Psychological Research Process



The so-called "functional-historical method" provides adequate concepts and insight into the fundamental structure of the research subject. Based on - but not logically deduced from - results of functional-historical analysis, hypotheses are formulated which are tested through empirical research ("aktual-empirische Forschung"). At this stage, several different research techniques are acceptable as long as they are suited to the research object (the so-called "object-adequacy"). Following an introduction to the functional-historical method, we will devote particular attention to the experiment and the methodology of action research. In recent years, both have played a central role in methodological thinking within the Berlin School of Critical Psychology. Finally, both statistics and formal logics are also enjoying renewed interest. In the last paragraph of this chapter we shall attempt to discover to what extent the Berlin School has indeed succeeded in "developing alternative concepts in the central range of methodology" (Leiser, 1978a, p.X). Important contributions of Critical Psychology in the areas of perceptual (Holzkamp, 1973), motivational (Holzkamp-Osterkamp, 1975, 1976) and cognitive theory (Seidel, 1976) will only be dealt with in passing, if at all. We feel, however, that we should restrict ourselves; an exhaustive evaluation of these contributions would require a more specialized knowledge of these areas. In addition, in this introduction to the critical schools within psychology, we should like to focus particular attention on their general methodological background. It may be added that after the completion of the work for this book, Klaus Holzkamp published a new, extensive work (1983). By and large, this book is based upon Berlin work already published, but he attempts to raise the material to a higher, i.e. more differentiated and integrated conceptual level. We were not able to incorporate this *magnum opus* in its entirety. But as we will attempt to show at a number of points, the analyses that follow do not appear to be essentially effected by this publication.

THE FUNCTIONAL-HISTORICAL METHOD

The Context of Discovery

In Popper's critical-rationalism, a sharp distinction is made between the context of discovery of new concepts, a new theory or a new hypothesis and the context of justification of propositions. In the first context, intuition and creativity may be used freely in formulating bold speculations which later are subjected to strict tests through the falsification principle. Where a scientist gets his ideas and theories from is no concern of a methodology aimed at falsifying propositions. To Popper, heuristics, e.g. finding hypotheses for research, is an area that should be studied by psychologists instead of methodologists. The only decisive test for the truth of a proposition is to deduce an observable prediction and confront it with such observations. If the predicted situation does not occur, we speak of definite falsification (refutation) of the proposition. The origin of the proposition is of no consequence at all, as this implies an inductive process that can never be strictly logically described. Indeed, Popper did not develop a "logic of scientific discovery", as was the English title of his most important book, but a "logic of scientific falsification". To Popper, there is no logic involved in the discovery process (Van der Veer, Miedema and Van IJzendoorn, 1983).

When Holzkamp views the current state of psychology, he concludes that this behavioral science must have followed Popper's recipe most literally. In no other discipline do so many unco-ordinated, bold conjectures appear to have been made in creating hypotheses and *ad hoc* theories than in psychology! And these in fact, brought forth a hodge-podge of facts with which fundamental theoretical developments did not keep apace. Instead of the integration of available factual material into one general theoretical perspective from which hypotheses could systematically be deduced, a jungle of occasional theories and hypotheses developed. In practice, Popper's dictum that the weeds of incorrect propositions would be uprooted by falsifications

proved to have fatal consequences. The unbridled fantasy and creativity of many a psychological researcher led all too often to tinkering at details which were neither theoretically nor socially relevant. It is Holzkamp's firm conviction that the context of discovery should not be left to its own devices; on the contrary, it should be an integral part of research practice. The question is how.

It is, of course, true that within the "positivistic" tradition, attempts have been made to develop a logic concerning the context of discovery. It might even be said that recently, there has been a revival of interest in the context of discovery. Wartovsky (1980), for example, calls it a "scandal" that the contemporary philosophy of science has hardly investigated the problems of the context of discovery and calls upon researchers to intensify studies in this area. More and more, the classical distinction between the contexts of discovery and justification is criticized and amended. Laudan (1980), for example, complains about all the "nonsense" that in the course of time has been proclaimed about it. He wishes to see a stage of "pursuit" inserted between the discovery and justification of theories (Laudan, 1977) in which there is a preliminary process of weighing a theory on solid, although not strictly logical grounds. As we know, Feyerabend does not believe in context distinctions at all. He believes the procedures followed in "discovering" a theory to be at least as important for scientific development as the idealized justification methodology (Feyerabend, 1975). In this way, Popper's view of the distinction of the two contexts and his assertion that the context of discovery is simply food for psychologists, sociologists etc., was eroded from several angles. In the following paragraph, we wish to briefly discuss a number of attempts within the "positivistic" ranks to provide the context of discovery with a rational basis. Against this background, the contours of Holzkamp's functional-historical method begin to emerge. Unlike modern "positivistic" heuristics, we shall show that Holzkamp's method does, indeed, provide guidelines for the initial stage of the discovery process, the collection of relevant data.

Recent Developments in Heuristics

Two representatives of an important school within the group of advocates of intensive study of scientific discovery are Nickles and Laudan, who believe that, in principle, this process is indistinguishable from other processes of problem solving. Just as there are, in general, good problem solvers and powerful heuristics, so this must be the case for scientific thought as well. This implies that a normative evaluation of the research process can also include a discovery stage. In principle, this also means that the philosopher (methodologist) may give advices; in other words, the discovery process can, to a certain extent, be learned. More or less in accordance with recent developments in the philosophy of science, such a position has been strongly advocated for years by the Nobel prize winner Herbert Simon and others of his persuasion (among others Curd, Schaffner, Tweney, Doherty and Mynatt). An important difference, however, between Simon and these philosophers is that he has the pretention to claim success in the theoretical discussion on the discovery process on the basis of empirical findings (computer simulations). More than anyone else, Simon has contributed to the decreased popularity of the mystical notion of creativity in general, and of the formulation of hypotheses in particular. He is thoroughly convinced that there is no need for a special theory to explain the mechanisms of scientific discovery. To him, they are simply a special case of more general problem-solving (Simon, Langley and Bradshaw, 1981, p.1). Of course, Simon, too, acknowledges that there are differences between the problem solving processes found in a psychological laboratory and those found in other scientific pursuit. However, someone will then have to demonstrate that these differences are of essential importance. Simon himself mentions two important differences. Firstly, he points out that scientific research is a social process, sometimes demanding the participation of several researchers for a long period of time. Secondly, he postulates that scientific research differs from many other types of problem solving

in that the goal is not clearly defined (Simon, Langley and Bradshaw, 1981, p.1). To his mind, however, neither difference affects his position. The latter remark has to do with the distinction between "well-structured" and "ill-structured" problems, a distinction he has qualified in several publications (among others Simon, 1977, p.304 ff). The former remark does not alter the fact that the same basic processes (may) lie at the foundation of both problem solving processes in a broader sense and scientific problem solving. If one agrees with Simon that scientific problem solving does not differ principally from problem solving in general, a normative evaluation of the discovery process is near at hand. After all, for a great number of problems, the most efficient method of solving them is already known. On the basis of these methods, a researcher's activities could be evaluated. There are then two possibilities.

Firstly, an attempt can be made to make a taxonomy of the problems and for each type to establish an optimal problem solving strategy. Among others, Tweney, Doherty and Mynatt (1981) prefer this possibility. With the computer, these researchers simulate "scientific" problems and present them to lay people and experienced researchers. Such research shows, for example, that different types of problems require their own method of solution. This applies not only to the generation and preliminary evaluation of hypotheses. In the justification context, too, there is no simple Popperian falsification recipe. Research into certain artificial problem situations shows that falsification is not always the most efficient goal. They therefore call for a taxonomy of problems with for each of them their own heuristic and testing method, that is, for several "logics of discovery" and "logics of justification".

The second possibility for studying the context of discovery implies looking for problem solving methods so general that they are applicable to every kind of problem. In this case, it is not a taxonomy of problems (e.g. hypotheses with accompanying solution strategies) that is sought, on the contrary: it is strategies with as broad a scope as is possible. This is Simon's ap-

proach. It does mean a choice for rather "weak methods", in other words very general, heuristic principles, unrelated to any specific type of problem. Simon points out that when entering an unknown area, problem solvers will of necessity use such weak methods, which do not call for specific knowledge. In areas that are familiar, however, they will use "strong" (algorithmic) methods that are specifically suited to the problem in question. Put in Kuhnian terms, this means that in the period of normal science, strong methods are probably used, while in the revolutionary phase, very general heuristic principles are "fallen back" upon (Kuhn, 1970).

Certainly in his earlier publications, Simon regarded a parsimonious description of a collection of data as one of the most important goals. It is the *researcher's responsibility to uncover a pattern or a* rule which forms the base of that collection. It is to Simon's credit that he demonstrated that a computer program can efficiently solve a series of problems with a number of heuristic principles, such as "means-end analysis" (a comparison of an existing state with the desired state and the selection of an operation that can reduce this difference), "factorizing" (dividing a problem into parts) etc. In his provocative "Does scientific discovery have a logic?" (Simon, 1979, p.326ff), for example, he showed that in combination with the primitive notions of "same" and "next", these principles could uncover the pattern in the following series of letters: ABMCDMEFMGHMIJMKLMMNM.....

Some Criticism of Simon's Heuristics

If, however, parsimoniously describing collections of data is regarded as one of the most important goals, does that not necessarily result in an inductivistic notion of science? In not taking the step towards generalizing his findings to new collections of data, Simon believes he avoids this hazard. Such a step is not taken until the testing stage (Simon, 1977, p.331).

Elsewhere, he makes this same distinction between describing a collection of data (law-discovery) and positing that this description applies to every collection of data. The latter would be a case of primitive Baconian induction, the former not (Simon, 1977, p.41). This distinction enables him to work on a normative evaluation of discovery processes without continually getting entangled in the induction problem.

For a number of decades now, Herbert Simon and his associates have been occupied with constructing computer programs that can solve problems of varying complexity (see in particular Newell and Simon, 1972). These programs are always constructed to work according to heuristic principles, not only because this provides a better simulation of human thinking, but also because in theory or in practice, many problems cannot be solved by "brute force". As we have seen, heuristic principles have the extra advantage of being relatively neutral as regards content and are therefore generally applicable. The achievements of programs constructed in this fashion appear impressive. In a recent article, for example, Simon describes the program BACON (Simon, Langley and Bradshaw, 1981), which among other things proved capable of uncovering Ohm's law, Archimedes' principle and several laws of chemistry. He also mentions the program AM, which proved capable of rediscovering many matters in the area of number theory. It would be inappropriate here to extensively describe and analyse this and other programs. We would, however, like to point out some objections that can be raised in the case of a computer program performing a task seemingly requiring some intelligence. In those cases in which Simon dealt with such objections, we shall also present the arguments he uses in doing so.

This criticism is as follows. The programs written by Simon and other researchers in the area of artificial intelligence do not really simulate the process of scientific discovery because: (a) they presuppose a clearly defined collection of data; (b) they introduce the collection of possible solutions in advance; (c) they are only capable of uncovering descriptive generalizations; (d) they are only applicable to "well-

structured" problems; (e) they utilize primitive notions that in themselves require an explanation. This short list provides some of the most important criticisms. There are more, as for example the reply that creativity can never be bound to rules (read: heuristic principles), but we shall not deal with such objections here.

ad (a) This is a classical objection often raised in connection with the induction problem. Among others, Hempel, Nagel and Popper have argued eloquently that data are always collected with a particular view or hypothesis in mind. Perhaps a distinction could be made here between being simply confronted with certain data, and regarding (a partial collection of these) data as relevant. There can be no doubt that within the context of the current information "explosion", running across data in a particular area of knowledge is in part dependent upon chance factors. Data concerning a particular problem are perhaps regarded as relevant when differences and similarities between several collections of data are ascertained, and on the basis of reasoning with analogies and models. As do Popper's hypotheses, Simon's data seem to appear out of the blue.

ad (b) This objection is mentioned by, among others, Maull (1980), and Simon himself (1977). In a reaction to an article by Schaffner (1980) on a program for medical diagnoses, Maull provides the following arguments. She points out that if all known syndromes are introduced into the program, then making a diagnosis is ultimately nothing more than a process of classification. This has very little to do with a creative scientific process. No new illness is uncovered and as such, the process of acquiring knowledge would appear to be reduced to a kind of Platonic anamnesis. Other programs, however, such as AM, do not appear susceptible to such criticism. Simon (1977, p.334) believes that this objection is based upon the distinction between "well-structured" problems, which can be analyzed, and "ill-structured" problems, which are presumed to be the exclusive domain of creativity. Simon regards this as a distinction of degree (see further under (d)).

ad (c) Programs are only capable of uncovering descriptive generalizations. Simon does not acknowledge this objection because he regards the distinction between describing and explaining as one of degree (Simon, Langley and Bradshaw, 1981, p.8). Apart from the fact that Simon regards this distinction to be of only relative value, he also claims that programs are not limited to uncovering descriptive generalizations. The program BACON regularly introduces theoretical terms and repeatedly posits "intrinsic properties" (o.c., p.20).

ad (d) A "well-structured" problem, has only one correct solution; it is the researcher's task to find it. An example of this is uncovering the series of moves leading to mate in a chess problem. For an "ill-structured" problem, there is no one correct solution. In principle, a whole collection of solutions may be correct. An example of this is a task such as: "Think of ten ways to use a match box". This second type of problem is considered characteristic of creativity and the objection is that programs cannot solve this kind of problem. Simon does not acknowledge this objection either. To his mind, this, too, is only a question of difference in degree (Simon, 1977). He illustrates this with the example of an architect designing a house. At first sight, it would appear that there are limitless possibilities here and that the architect is subject to few restrictions. However, it soon appears that such a problem becomes "well-structured", in particular due to the nature of the building materials and due to the fact that it concerns a serial process. Once a particular type of foundation has been chosen, the architect will most certainly have to bear its quality in mind when designing the rest of the construction. On the basis of knowledge and certain consecutive choices, an "ill-structured" problem in this way becomes a "well-structured" problem. To Simon, there is no fundamental distinction here. Apart from that, it is quite fashionable nowadays to regard the scientific discovery process as a problem-solving process in which the solution must meet several restrictions (see among others Nickles, 1981).

ad (e) In many of the programs written by Simon and his associates, the primitive notions "same" and "next" are utilized. The problem is, however, that these seemingly simple notions prove to be extraordinarily complex. Much research, for example, has been done into discerning differences and similarities between stimuli without the result being unequivocal (see for example Millspaugh, 1978; Nickerson, 1972; Rosch and Mervis, 1975; Rosch *et al.*, 1976; Rosch and Lloyd, 1978; Silverman and Goldberg, 1975; De Swart, 1981). Tweney, Doherty and Mynatt (1981, p.282) also point out the paramount importance of these basic operations. Even "same" and "next" observations are theory-laden and not as primitive as Simon would have us believe. In the concept formation literature, which concerns itself with these problems, one once again runs head on into the induction problem (see Van der Veer, 1983c). On the one hand, there is a group of researchers who believe that people develop a notion of something by comparing perceptual characteristics of objects. On the other hand, there are psychologists and philosophers that point out that stimuli can only be compared from a particular point of view. It would be inappropriate here to discuss this literature. We should only like to point out that if we continue to regard a notion such as "same" as primitive, we are still far removed from a real explanation of the process of scientific discovery.

Limitations of Traditional Heuristics

As we have seen, the most important drawback in Simon's procedure is that no rules for collecting data can be derived from it. The question of what kind of empirical material is of interest in a particular stage of the development of a discipline remains unanswered. The choice of variables to be measured is presupposed (i.e. cannot be justified theoretically), so that it is simply a question of looking for a model that in all probability most adequately reflects the relationships between those variables (in this case the data collec-

ted). In developing a model, questions as these all play a role: is the problem best represented by a linear model, or should one allow for curvilinearity and interactions? And what weight should be assigned to the elements in the function? However, the point of departure is a data matrix already presupposing the selection of a number of variables from among a collection of possibly relevant variables. It is this selection in particular with which the Berlin School of Critical Psychology has concerned itself most intensively, and for which the functional-historical method was developed. The notions lying at the root of this can best be illustrated with an example.

Imagine that research is conducted into the motivation of children to learn certain material. The research subject - motivation - would seem at first glance to be determined by a whole series of variables as sex, age, socio-economic background, self-image, the nature and structure of the material, the type of teacher, the system of rewards and punishment, the sociometric structure of the class, etc. Indeed, an enormously complex reality. Traditional researchers will try to reduce this complexity by limiting their experiment, for example, to two variables, in order to keep the problem methodologically and technically manageable. Their "positivistic" methodology does not limit them in their choice of these ("independent") variables. Strictly speaking, they need only make a bold conjecture, and subject it to rigorous testing. The researcher takes as it were a slice of a complex reality and examines it, without having to worry about its context. A second researcher having a go at this problem has the same fundamental freedom in choosing a limited number of variables to investigate. In turn, she or he will personally fish out two (most likely) other very promising independent variables and thoroughly investigate their correlation with motivation by means of a carefully arranged and laborious test procedure. It would come as no surprise at all if both researchers found support for their hypotheses concerning the determinants of the research subject. Two *ad hoc* theories of motivation, T_1 and T_2, would be formulated, both of which would be equally "true" according

to "positivistic" criteria, at least they could not be falsified. T_1 would be based on the independent variables (in this case determinants) V_1 and V_2, T_2 on V_3 and V_4. The expectation is then that reality could be reconstructed, as it were, as a mosaic, simply by integrating T_1 and T_2.

But will combining two partial theories guarantee the creation of a theory that describes and explains the essence of the subject? Holzkamp does not think so. He argues that by means of theoretical research, a distinction between essential and non-essential determinants should be made in advance. For imagine that V_1 and V_3 are non-essential variables, influencing the research subject only in particular situations and at particular times. First of all it is unlikely that the results of the partial investigations having led to the theories can be attained again under somewhat different historical and geographical circumstances. Secondly, the integrated final result will be subject to the same limitations. In this case as well, the ideal of universally applicable laws seems to be unattainable, and what appeared general turns out to be but one of many fragmented bits of theory.

The question is, of course, why the natural sciences run up against this kind of fragmentation so much less frequently, despite the fact that a "positivistic" approach is used there as well. Holzkamp believes that in physics and chemistry in particular, the research subject itself assumed more and more an integrative function (that is, influenced the pattern of theory development). This is because its basic dimensions, together forming an integrated system of fundamental properties (such as mass, force and acceleration in classical dynamics) became increasingly apparent. These basic dimensions then lead to a conceptual framework which helped to determine the interpretation of research into particular aspects of a problem. In the social sciences, however, there is no consensus on the basic structure of reality, partially because there is no method at our disposal to analyse existing material as to its basic dimensions. Because of this, the "anarchistic" cancer of social scientific theories and

hypotheses continues to spread. "Positivistic" methodology does in fact contain formal criteria for validity. Though it is true that despite the falsification principle and the verisimilitude thesis, the ideal of truth remains unattainable, we can nonetheless get closer and closer to it. But "positivistic" methodology does not have any criteria for relevance. Such criteria would enable us to distinguish between essential and non-essential dimensions of a research subject. Empirical testing is only a necessary, but certainly not a sufficient condition for determining the scientific value of a proposition about the relationship between variables. Testing according to relevancy criteria, i.e. answering the question whether a proposition relates to the basic dimensions of the research subject, is just as necessary. We would then be able to avoid attributing the same importance to every successfully tested proposition. If we are not in a position to do so, choosing a theory would become a question of taste and fashion.

It would seem that an obvious solution to the problem of relevancy lies within the research subject itself. As we have seen, Holzkamp believed that in the natural sciences the research subject itself established the basic categories for research hypotheses. In the case of the self-reflecting subject of the social sciences, "the human being", it would seem obvious that the often implicit theories and systems of concepts with which participants in experiments describe and interpret their world should be made the basis for pre-structuring the research subject into essential and non-essential dimensions. Use of such strategies has, in fact, been supported by Winch (1970). He stated that the social sciences were not completely free in choosing a system of concepts by which human behavior can be described. Indeed, how behavior in a social context is interpreted by the "actor" and his public greatly depends upon the system of rules adhered to in that context. The interpretation of certain behavior, or better still, a certain activity as, say, sport or suicide, can differ according to the cultural context and "language community". Often, a sharp distinction between suicide and a very dangerous sport cannot simply

be drawn from ethology: describing as objectively as possible the externally perceptible elements of a system of behavior resulting in a voluntary death one inflicts upon oneself. The social sciences must therefore derive the structure of the research subject from the framework of interpretations of the "participant".

There is, however, a fundamental problem with this and similar approaches (e.g. Glaser and Strauss, 1967). This is the phenomenon of rationalization and of unconscious or subconscious motives, which can play an important role in the self-interpretation of a participant in an experiment. The approach assumes, in fact, an idealistic notion of people, in which emancipated human beings are the complete masters of themselves and the world. In reality, however, common sense interpretations free of ideological distortions are nearly unimaginable (Holzkamp, 1978). From the point of view of the world of the participant, reality and appearance flow into one another, and science cannot blindly rely upon the perceptual ability of the individual.

Heuristics and Anthropology of the Berlin School

Holzkamp believes that the solution to this undoubtedly extremely fundamental problem of developing an adequate conceptual framework, can be found in the Cultural-Historical School (see Chapter 1). Leont'ev (1967) in particular, demonstrated that a subject's essential traits and structures can only be discerned by an analysis of its historical, social and phylogenetic (i.e. concerning biological origins) background. The behavior of participants within the framework of contemporary psychological research is not only the result of their individual development. It is also a result of the development of the (biological) species and of the social structures in which the species seeks assurances for its survival and reproduction. The basic dimensions of the research subject emerge when the question arises what the function of particular beha-

vior is for the material process of production and the reproduction of life of the individual and the species. At the same time, the question at which level of social development this process occurs must also be investigated (Jäger, 1977). Leont'ev distinguishes three methodological steps in the functional-historical method, all of which have been adopted by the Berlin School:

(1) Analysis of the natural historical (phylogenetic) development seen as the (re)production of human beings as biological organisms.
(2) Analysis of the social historical development seen as the (re)production of society, that is, the life of the individual within the group.
(3) Analysis of the individual (ontogenetical) development in a particular social context and class, seen as the (re)production of the individual.

Three presuppositions lie at the root of the functional-historical method. First, that the historical development of a phenomenon has left a mark on its fundamental structure. Secondly, that the necessity of (re)production of the individual and the species determines the basic dimensions of a phenomenon. And thirdly, that this (re)production is maximalized in a social context, that is in the group. In short, human behavior is characterized by its historical, material and social nature. The assumption here is, of course, that while the question of the function of behavior will be answered differently at each stage of development, each stage leaves its mark on the next stage. The three fundamental stages of development: (1) the stage of biological phylogenesis; (2) the stage of the origin and development of life in society; (3) the stage of life in a specific (capitalistic) type of society,
do not replace each other entirely, but are bound up in each other. In this view, the actions and thought of a contemporary individual are not only determined by the necessity to reproduce him- or herself and contribute to the reproduction of the species in a specifically capitalistic society. They are also determined by residues of the two preceding stages, that is the stage of (re)production of the biological organism and the stage of maximalizing this (re)production in the

context of the group. The motivation to learn a particular amount of material is therefore not only determined by variables arising in the actual situation (structure of the material, kind of teacher, relationships in the class, etc.). It is also determined by "residues" - paradoxically enough of essential importance - of times past; in particular from the time that the biological organism had to display a certain amount of exploratory behavior in order to adjust as well as possible to an ever-changing environment. Holzkamp-Osterkamp (1975; 1976) in particular has dealt extensively with this biological aspect and in doing so probably broken down the "progressive" taboo on studying behavioral genetics.

The importance of the residues of natural historical phylogenesis, stored in the "biological inheritance", should not, however, be overestimated. Critical Psychology attributes the "leap" from phylogenesis to social development to the uniquely human capacity of conscious construction and use of tools. This led to the development of a sort of social memory - the "cultural inheritance" in which solutions to problems which confronted preceding generations are stored (see chapter I). This "cultural inheritance" covers as it were the "biological inheritance" without neutralizing completely the influence of the latter. The plough, for example, is then the objectification of one of the solutions man has discovered in the course of evolution for the problem of increasing yield in agriculture. By "Aneignung", that is actively acquiring its "cultural inheritance", every generation is enabled to climb on the shoulders of the preceding generations and, in doing so, achieve a higher level of adjustment to the environment, of production and reproduction of collective and individual life (see also Sève, 1975, p.50). At this level of social development, the "social specificity level", the individual is dependent on the group for maximum adjustment to the environment, and his contribution to collective production and reproduction is simultaneously a contribution to his own (re)production as an individual. In this view, the age of saying: *homo homini lupus est*, the bitter struggle for (individual) existence is replaced by an almost idyllic commu-

nal life of which each member is an indispensible part. This is all the more surprising, for we are not concerned here with a traditional "adjustment ideology" of the individual but with an "anthropology" based on historical materialistic principles.

But this notion is concerned with the general "social specificity level" and not with specifying it in the form of an "antagonistic class society". This view therefore has a "contrafactual" character, that is, it is possible but not always - and in the case of capitalism hardly ever - a reality. Furthermore, this idea is concerned with the average individual and not with the unique individual. Holzkamp described this average individual as follows, based upon the results of functional-historical analysis: Of all living things, only human beings have the species-specific biological capability of changing nature through cultural activities. They are capable of participating individually in the determination of the conditions of human life by actively acquiring objective, historically accumulated experience. At the same time, they participate in creating and improving the conditions of their personal life (Holzkamp, 1978, p. 155). The essential characteristics of this critical "anthropology" of the average individual are therefore: (a) a series of unspecified biological capabilities ("biological inheritance") for co-operatively changing nature with a view to preserving and enriching the conditions of life of the species, (b) whereby individuals actively acquire culturally collected experiences ("cultural inheritance"), stored in products of manual and cognitive work, enabling them to attain that level of development, (c) that in maximalizing the collective condition of life, the individual conditions are maximalized as well.

What is essential from a functional-historical point of view, then, is "functional" knowledge and ability, those attitudes and needs of individuals that, firstly, enable them to stay alive and develop, and thereby contribute to the (re)production process society demands. Secondly, such knowledge and ability must also enable them to gain control of their own life

conditions by participating in society's control of reality. Thirdly, they must also enable them, by actively acquiring society's "cultural inheritance", to approach a developmental level which the developments in society as a whole, make possible.

Of course, the central question arising from this functional-historical deduction of a theoretic "model" of the human being as subject of psychological research is: to what extent have the presuppositions of the method determined its results? We have seen that the historical materialistic roots of the method consisted of principles of the historical, material and social nature of human behavior. These principles determined the questions concerning the function of behavior. The principle of the historical nature of behavior makes it worthwhile to dig deep into the history and phylogenesis of human beings when confronted with the question of the structure of the present day subject. It is therefore no surprise that in critical "anthropology", the biological determination of human behavior is mentioned (without further delineating what the exact nature of this determination is). This applies to the principles of the material and social nature of behavior as well. These principles seem to be found in "anthropology", only in another form.

And yet, when functional-historical analyses have been worked out concretely, they result in drastic changes in existing conceptual frameworks. With the aid of functional-historical analysis, Holzkamp-Osterkamp (1975; 1976), for example, develops a totally different motivational needs structure than, for example, Maslow or Freud. On the basis of ethological and biological material in particular, Holzkamp-Osterkamp attempts to show that two kinds of fundamental needs exist. The first are the so-called "productive" needs, that is needs directed towards mastering the environment, in this case the relevant, collective and individual conditions of life. The second are the so-called "sensual-vital" needs, directed towards immediate satisfaction of individual needs, for example, nourishment, sexuality, etc. The desire of human beings to master their environment is expressed in the "produc-

tive" need, among other things by anticipating possible future needs. For this, a collective effort is most suitable. For instance, protection against natural catastrophies such as floods is achieved most efficiently by collective efforts in connection with building dykes. On the other hand, "sensual-vital" needs are directed towards the present, towards the consumption of social products, in part for the reproduction of the individual (Holzkamp-Osterkamp, 1976, p. 23 ff.). While Maslow places the "productive" need to explore the environment relatively low in his hierarchy, just above the satisfaction of physical needs, and Freud "sensual-vital" needs, such as sexual needs, at the top, Holzkamp-Osterkamp stresses the "productive" needs (see Elbers, 1984). We will not expand on the background of and differences between the psychological theories mentioned above. But it is clear that a functional-historical analysis may have a great effect on psychological research on motivation.

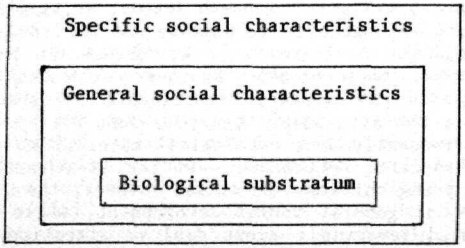
Functional-Historical Method and Biopsychology

The functional-historical method once again draws attention to the biological contribution (not determination) to human behavior, and in doing so, introduces a "biopsychological" perspective into psychology. Of course, this perspective must be placed in a specific context. Functional-historical analysis makes clear that there are limits to human action, arising from an organism's phylogenetic adaptation to the environment and directed to increasing the chance of survival ("functional reflection"). Another well-known theory allowing for phylogenetic determinants of present behavior is the attachment theory of John Bowlby (1971). Bowlby frequently uses ethological material to uncover species-specific determinants in the attachment behavior of young children. He omits, however, the stage of analysis of general social development, while on the other hand devoting a great deal of attention to the last stage in the three-step approach ("Dreischritt"). As does Bowlby, Holzkamp also assumes that only in the

light of "biological inheritance", which evolution has given each member of the species throughout the ages, can we understand the individual's present behavior. The same applies to the stage of the species' more active and collective adaptation to ever changing environmental conditions. In this stage, in which actively acquiring ("Aneignung") one's "cultural inheritance" becomes more important than acquiring one's "biological inheritance", humans acquired specific adaptation strategies also influential in present situations. According to functional-historical analysis, the concrete individual forming the subject of psychological research is built around a phylogenetically determined core or basic structure, around which traits and characteristics develop as a result of the socialization and collectivization of life. Finally, a second layer of characteristics developed that enables (re)production of the species and the individual in a specific capitalistic society. These final characteristics are not universal in nature but limited to a specific historical social context. The basic structure and general social characteristics, however, are universal and therefore essential to a Critical Psychology maintaining the (nomothetic) ideal of discovering universal laws. In Table II.2, the layered structure of the individual is shown schematically.

TABLE II.2

A Functional-Historical Model of the Human Being



The objective of functional-historical analysis is to analyze and structure the apparently inextricable mixture of "biological and cultural inheritance" and characteristics bound to a specific kind of society by means of the "Dreischritt". The goal is to determine which dimensions of behavior are essential and which are less or not essential. The natural step of historical analysis provides the characteristics of the research subject that can be looked upon as essential general biological traits of the human organism. At this stage, a distinction must be made between these general biological characteristics and those that are an expression of the socialization of the average individual, and therefore universal in nature. After the most general and essential biological and social traits have been distinguished in this way, a residue remains of what is specific to life in capitalist society. In the light of this kind of advance structuring of the research subject, systematic hypotheses can be developed and tested in empirical research (see the following sections). Unlike Simon's heuristics, the choice of relevant variables is not presupposed, but can be deduced from the functional- historical method. At the same time, research results can be adequately interpreted in the light of a conceptual framework derived in this manner. In other words, an interpretation preserving the distinction between essential and non-essential dimensions of human functioning.

THE PROBLEMS OF "CONTAMINATION", "INTERSUBJECTIVITY" AND "FUNCTION"

The material the functional-historical method is concerned with is the "Gesamtwissenschaft" (Jäger, 1977, p. 125), that is, the information regarding the research subject to be found in all relevant disciplines, such as biology, ethology, economy and sociology. Holzkamp-Osterkamp's functional historical analysis of the motivation concept is based largely on ethological and biological material (see also Schurig, 1976). Past results of scientific research should form

the basis of such analysis so that pioneer research becomes possible.

A problem remains, though, that Critical Psychology does not have criteria at its disposal to test the validity of the material functional-historical analyses are concerned with. The validity of the results of the analyses themselves are then thrown into doubt. As we have stated, and will explain in more detail below, the Berlin School criticized traditional research rather vehemently. Because of the structure of the research situation, among other things, traditional research could not provide valid, reliable, objective and relevant results. This was the opinion that Holzkamp in particular so articulately defended in his volume of 1972. But these same research results, though partially from different but still "positivistic" disciplines as ethology, and biology, are the basis for functional-historical analysis in constructing a conceptual framework. The question then, of course, is whether the shortcomings of this research material directly or indirectly influence the functional-historical results. The method itself does not have the means of preventing this from happening. Critical as it is of traditional scientific study, Critical Psychology should actually presuppose the inevitability of this kind of negative influence. Of course, available research material is not accepted in functional-historical analysis at face value. Apparently though, what is and is not accepted is a relatively subjective and arbitrary business. For instance, it is not clear on the basis of what criteria Holzkamp-Osterkamp evaluates the work of the biologist-ethologist Lorenz, contested even in traditional scientific circles. This very Lorenz, with his wild speculations concerning the biological determination of human agression, has repeatedly hazarded unfounded generalizations from results of ethological research into human behavior, an area as yet hardly explored by ethology. Even prominent colleagues are vehemently opposed to such generalizations (Hinde, 1978). And yet it is the very same Lorenz whom Holzkamp-Osterkamp consulted extensively in constructing her motivation theory. She has the pretention to put Lorenz' theory in its proper place, but it is not at all clear how she wishes to accomplish this.

Aside from the problem of "contamination" mentioned above, a second important problem with the functional-historical method, is that of "intersubjectivity". This problem concerns the vagueness of the method itself, which is only outlined by a very general three-step procedure and a few presuppositions. It would be naïve to believe that any other well-intentioned researcher could actually work with such a description. Recently, Holzkamp (1983) somewhat refined the "Dreischritt" by indicating that in the phylogenetic process, five steps of analysis must be followed to trace the origin of specifically human characteristics. We shall not expand on this notion in this context. But this refinement also makes a rather abstract impression, and it is questionable whether it sufficiently solves the problem of intersubjectivity. A method pretending intersubjectivity will at least have to make explicit what kind of filter is used in evaluating the usefulness of results available from the "Gesamtwissenschaft". As things stand now, the functional-historical method is not "learnable" and the results are neither testable nor replicable, simply because crucial criteria have not been made explicit. For the time being, the method must therefore be considered elitist and esoteric, useful only to a small number of insiders. In this regard, Simon's computer-directed heuristics are more "democratic", for his programs determine the discovery procedure step by step. Of course, heuristics in the form of a computer program are an extreme example of an intersubjective, replicable procedure. But still, a Critical-Psychological procedure will also have to contain more opportunities for intersubjectivity and replication than is now the case with the "Dreischritt".

A third problem encountered in connection with the functional-historical method concerns the functional view of behavior (see for example Elbers, 1984). In the functional-historical method, material from all relevant sciences is examined from one point of view: to what extent is the behavior encountered functional to the emancipation of the human species and the individual from unnecessary external determination. The point

is to determine the function of behavior with a view to a shift along the continuum between self-determination and external determination. Behavior is evaluated as to its function in (re)producing the human species and the individual. In doing so, emancipation from the whims of nature and social repression are the guiding principles. Rightly, the question has been raised whether in this sense, the concept of function possesses sufficient descriptive and explanatory value (Verbij, 1981).

The question is, in light of the belief in progress implicit in the concept of function, whether all kinds of tendencies pointing in the opposite direction are not, to a certain extent, lost sight of. And of course, it is incontrovertible that throughout history, the balance between external determination and self-determination seems to shift first in one, then in the other direction with seeming randomness. And this not only applies to emancipation from social repression but even to emancipation from nature. Nowadays, we no longer take it for granted to the extent Marx did that emancipation from the whims of nature has followed a linear progression. What at first appeared to be an increasing domination of nature appears now, at times, to be accompanied by increasing environmental pollution, depletion of natural resources, etc. Yet to our mind, it would be incorrect to abandon the emancipatory concept of function simply because it cannot explain every development with equal accuracy. Indeed, it is in the very critical-emancipatory nature of psychology to repeatedly pose the question to what extent certain types of behavior have contributed or can contribute to a shift along the continuum in the direction of self-determination. A psychology lacking an ideal of progress can no longer ask itself questions, the answers to which contribute to human emancipation. And in the final analysis, that is the *ultima ratio* of a critical psychology.

In summary, we may conclude that with the aid of the functional-historical method, the Berlin School attempted to help find a solution to a fundamental problem in traditional scientific research: the swell

of *ad hoc* theories and hypotheses resulting from the methodologically sanctioned anarchy in the context of discovery. On the basis of this contribution, it is understandable why Holzkamp did not wish to call his theory a separate school among the traditional schools within psychology. Has Critical Psychology succeeded in filling the undeniable gap in theory and concept development within traditional psychology, and thereby made the functional-historical method indispensable to every psychological researcher? That question can only be answered when the problems of "contamination" and "intersubjectivity" have been solved.

EXPERIMENTAL RESEARCH

The Principle of Exhaustion

The experiment - and in particular criticism of experimental research - plays a vital role in the history of the Berlin School. In the volume "Critical Psychology" (1972), research is put on a par with experimental research, and in particular laboratory research. The criticism of this research was so severe that the question arose if the experiment could ever regain status within the Critical Psychological methodology. Holzkamp criticized the experiment on immanent grounds, i.e. he showed that this method of testing hypotheses is very often not up to the traditional standards of research. He pointed out, for example, that experimental research offered endless possibilities for circumventing the most extreme consequence of the falsification principle, rejecting a hypothesis on the basis of contradictory experimental data. More often than not, the methods of experimental research make it possible to explain away such a "falsifying" outcome by pointing to the influence of until then unknown intervening factors, errors in the experimental set-up, conditions limiting generalization of the original theory, etc. Holzkamp referred to this defensive research strategy with the term "Exhaustion" (coined by Dingler), and though it was in this very area that he

was sharply criticized by German representatives of the "positivistic" ranks, recent developments in the history and philosophy of science seem to prove him right.

The principle of exhaustion has to do with the relationship between theory and fact. Roughly speaking, the last fifty years have seen three periods in the consideration of this relationship: verificationism, falsificationism and paradigm theories. In these three periods, fundamentally different answers were given to the question of how to envision this confrontation between theory and facts and what role experience plays in justifying theoretical propositions. We wish to briefly discuss these developments in the philosophy of science, not only because they place Holzkamp's principle of exhaustion in proper perspective, but also because they demonstrate the validity of, in particular, critical psychology's emphasis upon theory development. Not only the Cultural-Historical School, but also the Berlin School and the American Dialectical Psychology are characterized by their criticism of the atheoretical empiricism of traditional psychology. They are also characterized by their goal of developing well-grounded theories which to their mind should be a prerequisite, and not a consequence, of meaningful empirical research (see chapters I and III). This "reversal" of the theory-fact relationship in favor of the theory will surprise many traditional psychologists. Yet here, critical psychology is simply drawing logical conclusions from modern philosophy of science, in which empirical research occupies no more than a modest position.

Verificationism and Falsificationism

Seen broadly, verificationism as advocated by the Wiener Kreis assumes that theoretical propositions may be founded upon inductive reasoning. Let us take for example the proposition, based upon Bowlby's (1971) attachment theory, that at a young age, children's separation experiences may result in an anxious attach-

ment relationship between parent and child. This proposition might also have been formulated as follows: all young children who have experienced separation are anxiously attached to their parents. At first sight, it would seem relatively easy to verify this proposition. A researcher simply seeks out as many children as possible with separation experiences (due, for example, to a stay in the hospital) and then investigates whether or not these children have an anxious attachment relationship with their parents. If this is the case, the general proposition would seem to be confirmed. Of course, the observations upon which the general proposition is based must meet a number of requirements. For example, the researcher must have observed a large number of children in a wide variety of circumstances. These observations must also be a direct reflection of sensory experiences. Ideally, an observation should never be filtered through possible subjective interpretations but should be an almost photographic reproduction of what has been observed. In the same circumstances, any arbitrary, normal observer, should be able to make the same observations.

Observations in and of themselves, however, are non-conveyable impressions. Not until they are conveyable, that is transformed into speech, can they play a role in the scientific process. Within logical empiricism, a great deal of thought has been devoted to the form in which observations should be communicated. The problem is how to make use of language while avoiding interpretations that transcend that which is observed. The so-called "sense-data"-statements are presumed to be a direct representation of observations which can be repeated by anyone, are immediately comprehensible and unquestionable. Neurath (1979) has taken up a somewhat more sceptical position regarding the evident truth of "sense-data"-statements and introduced the term "protocol sentence", which describes observations of which the veracity should be determined in relation to all the available relevant protocol sentences. An example: "At time t and place p, researcher X saw a child k that had been in the hospital for some time and did not seek the proximity of its parents when a stranger entered." Based upon a large number of such "elementary" obser-

vations, it should ultimately be possible to confirm Bowlby's general proposition, or at least to achieve a high degree of probability.

It is to Popper's credit that he has layed his fingers on the weak spots of this verificationistic program. His most important criticism was of a logical nature: it is impossible to arrive at a confirmation of a general proposition, which itself implies an infinite number of cases, on the basis of a finite number of observations. Induction would then lose its logical-analytical character, as its conclusion contains more information than is contained in its premises. One can only extrapolate from a limited number of observations in the past and present to an unlimited number in the future based upon a (metaphysical) belief in the fundamental immutability of the world. And such a belief can only be based upon induction, that is upon the repeatedly - but finite number of times - confirmed expectation that no great rifts exist between the past, the present and the future. Popper's falsificationism (see also above) is based upon the logical asymmetry between observations and general propositions: observations (or put more accurately, statements about observations) can never confirm general propositions but can refute or falsify them. From a strictly logical point of view, one single observation of a child with separation experiences and at the same time a warm, secure attachment relationship with its parent is sufficient to refute the general proposition concerning the relationship between separation experiences and anxious attachment. Seen logically, this is a case of the so-called *modus tollens*:

$$\frac{t \rightarrow p \qquad \neg p,}{\neg t,}$$

in which t is a theory from which a prediction p concerning a state in reality can be deduced. Upon further investigation, this prediction does not prove correct, in which case the theory from which the prediction had been logically deduced must be regarded as falsified

(Popper, 1980, p.7). Paradoxically enough, though, according to the falsificationist Popper, the truly strong theories are characterized by bold deductions which could not be falsified, whereby in the end, a verificationistic element is smuggled in through the back door. If we were to keep strictly to Popper's adage: make bold conjectures and persistently try to refute them (Popper, 1972b), success could be pre-programmed with the most unlikely conjecture. The point is to make predictions concerning new "facts" or contra-intuitive phenomena based upon a theory, and to see these predictions remain unrefuted even after severe testing. On the basis of the theory on the relationship between separation experiences and attachment, for example, the prediction could be made that in societies in which children experience an extreme number of separations, only anxious attachment relationships can develop. If after numerous cultural anthropological studies among alien societies in several different parts of the world this prediction proves irrefutable, this then signifies a reinforcement of the theory (its corroboration level increases). But the truth of a theory can never be proved in this way.

Also in this notion concerning the confrontation between theory and experience, the problem of the status of empirical observations plays an important role. Ideally, observations should form the irrefutably solid basis for the (in)correctness of a prediction, and therefore a theory. Popper himself, however has pointed out that observations can only perform their function in a "linguistic" form. With the use of language to describe observations, all kinds of universal concepts are slipped in that unavoidably confer the status of theory upon any statement about an observation. Statements about observations have a theoretical aura and are not at all the irrefutable verities they are more or less still assumed to be in logical empiricism. Measuring temperature with the aid of a thermometer presupposes a theory on the relationship between temperature and the volume of mercury; observing the planets with the aid of a telescope presupposes an optical theory; observing the attachment relationship between mother and child presupposes a theory in which

it is made plausible that in certain situations, some of children's non-verbal behavior is an indication of that relationship (Van IJzendoorn *et al.*, 1983). Buss (1979a, p.13ff) shows that the notion of the theory-laden observation was already present in Marx and his Hegelian interpreter Lukacs. But according to Buss, due to its orientation to the natural sciences, logical empiricism remained deaf to these early critical notes (see also chapter I).

Where theory is concerned, there is also potential refutability. Observation theories can also never be (inductively) confirmed or held as true. We then get the paradoxical situation that theoretical propositions are tested with the aid of statements about observations that are very likely inaccurate themselves. The confrontation between "soft" theory and "hard" facts appears to have changed into a confrontation between "soft" theory and "soft" observation statements. Popper's remedy for this seemingly hopeless circle is that statements about observations may only be based upon extremely strong, thoroughly tested and generally accepted theories. By means of an agreement between the researchers in question, the doubts concerning observation statements are temporarily set aside. In other words, statements about observations are solidified by means of conventional agreements, the convention being in part based upon an estimation of the corroboration level of the observational theory in question.

Falsification or Exhaustion

Yet this artificial, conventional solidifying of observation statements is the Achilles heel of falsificationism. Conventions are a poor substitute for what was once called hard facts. In practice, if a dearly beloved theory is threatened by a falsification, the temptation for researchers and theorists is very great indeed to forget convention and to doubt the correctness of the observation theory. Galileo's colleagues, for example, doubted (according to Feyerabend justifi-

ably so as well) the correctness of the optical theory upon which the construction of the telescope is based, resulting in Galileo's bearing the necessary burden of proof. Galileo's contemporaries rightly wondered why they should doubt simple observations of the naked eye and not the observations with such a new and untested instrument as the telescope (see chapter I). And yet it was this very demand that was made upon them. In the social sciences, this dilemma repeats itself almost daily. By pointing to the deficiency of the experimental design or method of data collection, disagreeable research results are "reasoned away". This is the essence of Holzkamp's principle of exhaustion. A good example of this is Bell and Ainsworth's study (1972) on the development of children's crying behavior in their first year. They found that reacting sensitively to crying resulted in its diminishing. On the basis of behavioristic reinforcement theory, it should be predicted that in that case, crying should actually increase, because sensitive reactions should function as a reward, and therefore a reinforcement of the behavior preceding it. And indeed, advocates of the reinforcement theory have done everything to expose the so-called falsifying research results as an artefact, by means of methodological criticism (Gewirtz and Boyd, 1977).

From the point of view of logic then, the problem of the theory-laden observation does require an adjustment of the previously described *modus tollens*. In terms of logic, the dilemma of a choice between "soft" theory and "soft" statements about observations can be described as follows:

$$\frac{\begin{array}{r}(t \wedge q) \rightarrow p \\ \neg p\end{array}}{\neg (t \wedge q)}$$

in which q concerns the background theory from which observations are made. This is a (singular) interpretation of the Duhem-Quine thesis, which states that a simple hypothesis can never be detached from theory and tested separately, but that a theory network (t ∧ q) is

always in question (see Braun and Radermacher, 1978). Such an interpretation results in a considerable weakening of empirical arguments in theoretical discourse. From a practical and logical point of view, it always proves possible to undermine falsifications by placing doubts upon the observation theory on which they are based, rather than to reject the explanatory theory itself.

Though such a manoeuvre is always possible from a practical point of view, it is imaginable that a normative research methodology might strictly forbid it. This would particularly be so in the case of *ad hoc* criticism of the observation theory only after it has produced disagreeable research results, criticism that weakens the content of the theory itself. And indeed, Popper, for example, has hit out fiercely at these so-called conventionalistic stratagems designed to save theories from falsification *ad hoc*. And not only Popper, but many other scholars as well do not appreciate the validity of the Duhem-Quine thesis, as Mitroff (1979) empirically established. Curiously enough, however, recent historical research into the (in many respects so succesful) natural sciences has shown that the greatest successes (for example the discovery of planets or chemical elements) owe their very achievement to the consistent application of protective strategies against falsifications. In that case, Holzkamp's principle of exhaustion has become a methodological necessity.

Paradigm Theories

In Kuhn's paradigm theory, the methodological necessity of protection against falsification plays a central role. If, indeed, a theory can protect its core of general assumptions and insights from theoretical and empirical criticism, it may look forward to a fruitful period of "normal" science. In that case, there is a disciplinary matrix of more or less interrelated basic convictions of a group of researchers. An

important component of such a matrix are the paradigms in a narrower sense, that is solutions to problems in the form of models with which a prospective researcher is introduced to a particular area of research. These models of solutions to problems, viz. research designs and methods including their observation theory, function as an implicit and undiscussed framework of reference in solving new problems. In this way, conducting research becomes a question of solving variations of one and the same puzzle along well-known lines and with well-known devices. No costly time or effort is lost in reflections on and criticism of fundamental assumptions and well-established observation theories. Concentrating all available energy into specific problems and subdivided areas result in a rapid progression. More and more puzzles of limited scope get solved and more and more ingenious technological applications get uncovered.

If potential falsifications are throttled in the protective belt surrounding the hard core of basic convictions, and only applications are at issue, the question then arises how fundamental changes in theoretical orientation can occur according to Kuhn's paradigm theory. On the basis of numerous examples from scientific history, Kuhn describes how, in the long run, a paradigm can dig its own grave. The thorough detail-studies during the period of normal science necessarily result in the discovery of "facts" that in no way can be squeezed into the moulds of the existing theory. In and of themselves, such recalcitrant anomalies are not capable of destroying prevailing paradigms. What is required is that firstly, even after prolonged and intensive attempts, these facts can still not be fitted into the paradigm. Secondly, there must be an alternative paradigm in the make that is capable of sufficiently explaining the anomalies. And thirdly, this new paradigm must offer promising puzzles heralding a new period of normal science. Arguments for abandoning the old paradigm for the new one can, however, never be sufficiently rational. After all, the new paradigm also leaves many questions unanswered and is confronted with innumerable "falsifications". Accepting a new paradigm therefore in many ways resembles

a religious conversion. There is no solid external criterion (for example "hard facts") by which to test and compare the old and new paradigm.

Of course, rescue operations have been undertaken to save the standard view of science. Such a long cherished myth is not easily abandoned. Lakatos in particular has proved a strong advocate of a rationalization of the choice of paradigm or theory, although he, too, must acknowledge that it is impossible to definitely reject theories purely on empirical grounds. "It is not that we propose a theory and Nature may shout no; rather, we propose a maze of theories and Nature may shout inconsistent." (Lakatos, 1980, p.45). Lakatos refers here to the collision between an explanatory theory and an interpretive or observation theory which produces the so-called facts. Collisions between competing explanatory theories (paradigms or research programs) as well cannot be automatically be resolved by relying upon solid facts as *tertium comparationis*. But the researcher is still compelled to choose between the inconsistent theories. Lakatos attempts to rationalize this selection process by distinguishing progressive and degenerative research programs, differing in their ability to uncover new facts and to develop new applications. Unfortunately, he is unable to indicate definite criteria based upon which this distinction can be made at the moment the inconsistency comes to light. He, too, is aware of the historical fact that after decades of stagnation, seemingly degenerative research programs can experience a revival and surpass their seemingly progressive competitors.

One conclusion, at any rate, remains clear: theories are not verified or falsified but superseded by competing theories that, for whatever reasons, are capable of acquiring more authority. In the course of methodological developments, the relationship between theory and fact has undergone a number of accentual shifts. While initially facts seemed to strongly dominate theoretical discourse, a radical shift in the direction of a dominance of theory above empirical facts has taken place. In part due to the principle of exhaustion, facts have lost their right of veto and at

most - filtered through theories - only appear to be capable of pointing out inconsistencies in the theoretical arguments.

Ecological Validity

Actually, Holzkamp hardly bothered with the question of how scientific progress would be possible if the principle of exhaustion were to become common practice. He only wished to show that there was a gap between common experimental practices and the traditional methodological prescripts contained in the falsification principle. To him, the question of the possibility of scientific progress is not an empirical question, but a theoretical one. The functional-historical method described earlier guarantees that theories are constructed that transcend "scientific inheritance" and make pioneer research possible. Along with this immanent criticism, Holzkamp also made a number of critical observations with respect to the premises of traditional research. He believed that too little effort was made to create experimental situations structurally resembling every-day reality. This is comparable to Bronfenbrenner's problem of ecological validity (Bronfenbrenner, 1979). Holzkamp's impression was that experimental research was more concerned with having the results meet the requirements of statistical analysis than with their meeting the requirement that such results should have something to say about reality. All too often, the experiment constructs its own artificial reality. Statistical analysis of results of an experiment requires that a number of conditions, i.e. independent variables, can be determined as accurately as possible with respect to their effect on the dependent variable. According to Holzkamp, this leads to:

(1) Fragmentation ("Parzellierung") of these conditions. A complex cluster of independent variables is cut up into as many tiny pieces as possible, in order to determine the effect of exactly that

component which is described in the hypothesis. Possible effects of other related components are disregarded.

(2) Reduction of the entire complex of conditions. Possible interference with the experimental set-up must be eliminated in order to determine as accurately as possible the effect of the independent variable upon the dependent variable. If, for example, the capacity of the memory is determined by the speed with which a participant in an experiment learns a series of words by heart, the interference of the participant's own background is eliminated as much as possible by using nonsense words instead of existing words. The idea is that participants probably associate existing words with various experiences that can interfere with the memory in various ways (Ebbinghaus).

(3) De-stabilization of the stimulus situation. Often it is impossible to completely eliminate interference from certain factors. What can then be tried is keeping the participant as much in the dark as possible about the essential characteristics of the research situation and providing as little support as possible for suppositions concerning the objective of the experiment. When the situation is so de-stabilized that the participants have become totally unsure of the expectations the researcher has with respect to their behavior, the experiment is started. Participants will then clutch to the stimulus as their only point of orientation, leading to unusually strong responses. Later, this results not only in the elimination of potential interference but also in a confirmation of a hypothesis based upon completely abnormal responses.

In the construction of this experimental reality, the researcher often has an at times unconscious, abstract image of the participant. This participant is as fixated as possible upon the here and now and just reacts to stimuli manipulated by the researcher. What makes this participant a unique personality, her or his social and historical nature, is placed, as it were,

off-screen. Assuming the marginal conditions are the same as those for fellow participants, she or he will, as a purely passive-reactive organism, react the same way as well. Putting it into terms of analysis of variance, the variance within the group is minimalized while the variance between groups is maximalized. Actually, participants should not be in the position to scan research stimuli for meaning through their past experiences, for each participant's scanning could result in different meanings and therefore different reactions. Therefore, the participant is reduced to an organism, which is why Holzkamp refers to a "hidden organismic anthropology" in traditional psychology. Psychology has taken on the task of expanding our knowledge of human thought, experience and activity and yet in doing so, has paradoxically reduced the subject to a bloodless organism. Holzkamp does, by the way, believe that this organismic anthropology has some real value and reflects some aspects of reality, in particular, the reality of the working classes in capitalistic society. There is some similarity between a "Taylorized" work situation and an experimental laboratory situation. As we know, Taylor stands for the introduction and perfectioning of the assembly line system. Work is subdivided into meaningless parts which the laborer can perform mechanically. In traditional experimental research, only a particular manifestation of human activity is described and accounted for, while more essential and universal dimensions are disregarded.

Pre-scientific Practice

So in fact, traditional psychological research does not meet the requirements of its own methodological criteria of unimpeded operation of the falsification principle. Nor does it satisfy criteria which should be set for the "object-adequacy" of the experimental situation, that is, from the point of view of a critical functional-historical analysis of the research subject - the historical and social human being. But is

there still "hope" for the experiment, after Holzkamp's categorical criticism? Recently, the Berlin School has once again been devoting a great deal of attention to the experiment as research method (Maschewsky, 1977a; 1979a, b). The general impression one gets after studying this material concerning the (im)possibility of the experiment is not entirely negative. Of course, the marginal notes are largely critical *(nomen est omen)*, and in most cases sharper and more detailed than in the past. At the same time, however, more room is created for advanced experimental designs within Critical Psychology. In an adapted form, the experiment should certainly be able to play a role in the stage of empirical testing of hypotheses deduced from functional-historical analysis. This is because experimenting falls in a direct line with pre-scientific practice, in which actions are tried out systematically to see if the changes they bring about are the expected and desired ones. The experiment can definitely not be labelled a pernicious invention of capitalism, as experimentation, and even reflection at a methodological level about experimentation, was taking place long before capitalism existed. Bacon's *Novum Organum Scientiarum*, for example, was published in 1620. In ancient times, the physician Galen was one of the first to establish a series of methodological requirements for the experiment, containing both the principles of systematic variation and control of intervening factors (see for example Klaus and Buhr, 1975, p.397). From a historical materialistic analysis of science, Bayertz (1980), for example, also remarks that the significance of the experiment can only be estimated accurately if regarded not as a general activity, but as a particular case of human labor (in other words, as a particular form of metabolism between man and nature). Also the idea of "conditions analysis" upon which the experiment is based is an everyday phenomenon and therefore not to be brushed aside as reductionistic.

If, for example, a farmer is confronted with the problem that the grass will not grow, he too will then conduct a so-called "conditions analysis". In other words, he will divide the situation into a number of

separate components such as, for instance, the dampness of the soil, the presence of essential nutrients or harmful insects etc. He will then develop a hypothesis as to the most likely causal factor within the complex of possible conditions. By keeping the other factors constant and varying the suspected causal factor - using artificial fertilizer for example - he will test to see if his presumption is correct. If his test procedure with respect to separate components does not have the desired result, he will then probably even think in terms of possible interaction between, for instance, the variable dampness and the presence of essential nutrients. He will see if using artificial fertilizer in combination with the necessary supply of water will result in the desired growth. It is exactly this everyday form of conditions analysis and variation of conditions that can be found in every experiment.

"Conditions analysis" is based on the assumption that reality cannot be "known" by continuously scanning its totality, its unreduced complexity of phenomena, in the hope that an "Aha-Erlebnis" will strike the proverbial match in the darkness. As a model for reducing a complex reality into a series of independent, dependent and intervening factors, "conditions analysis" is based on the idea that reality can only be known in stages, and in particular by manipulating and changing its elements. Of course, a thorough theoretical analysis of the research subject is then needed. In fact, Holzkamp's criticism of reductionism was concerned with blind, atheoretical reductionism. It makes indeed no sense to go fishing in the sea of possible relevant variables in the hope that some connection will become clear *post hoc* (see above). "Conditions analysis" will have to be accompanied by theory, which is based on a functional-historical analysis. This analysis will have to bring out the essential elements of the research subject and make clear which of all conditions will have a negligible, temporary or geographically limited impact. This is a substantial difference between traditional model construction and that which is acceptable from a Critical Psychological point of view. Traditional "conditions analysis" would implicitly start from a model of a reality influenced by a series of variables

that cannot be arranged hierarchically. Critical Psychology, on the other hand, believes on theoretical grounds that reality is adequately reflected in a model in which the influence of a limited number of essential conditions far exceeds that of a series of non-essential conditions.

If it could be proved that the Critical-Psychological method was correct, and if the functional-historical method was capable of separating essential conditions from non-essential ones, this would indeed be a step in the direction of more fruitful scientific research. Indeed, the greatest problem with traditional experimentation is how to keep possible intervening variables under control. It is clear that any type of control in a traditional model must be futile (assuming the ineffectiveness of the randomization technique, see below), as the number of variables that could greatly interfere with experiments is incalculable. Because of personal, material and statistical problems, there will always be uncontrolable factors influencing the correlation between independent and dependent variables, so that both generalization and replication will be greatly limited. For example, it is very possible that the independent variable investigated (in the example, dampness of the soil) only correlates with the dependent variable (growth rate of the grass) if a certain amount of artificial fertilizer is present. If this third variable is not considered, the experiment could yield an entirely different interaction effect in another situation in which another amount of artificial fertilizer is held as constant intervening factor. There seems very little possibility of replicating the experiment with respect to increasing the level of dampness. Imagine, though, that a functional-historical analysis could prove that only the level of dampness and the amount of nutrients were essential and that the effect of the remaining intervening variables (if any) - the presence of insects, the structure of the soil etc., was negligible. First of all, it would be possible to anticipate possible interaction effects by constructing a relatively simple model. Secondly, it would be possible to have more faith in generalizations of the results to other situations in which only the

non-essential factors somewhat vary. In that case, the age-old dilemma of increasing internal validity versus expanding external validity would cease to exist (Campbell and Stanley, 1966).

The Priority of Theory above Fact

The central point of the Berlin School is therefore that the present crisis in experimental psychology, in which generalization, replication and theoretical relevance of results are difficult to achieve, is first and foremost a theoretical problem, with the unnecessary shortcomings of techniques in second place. Maschewsky, for example, does not expect that refinements in techniques will bring us any closer to a solution to the problem. He points to the progress that has been made in this area in the last decades, for instance the success of multivariate analysis techniques and the development of complicated, quasi-experi*mental designs. But if the theory does not keep apace* with these innovations, the problems of controlling intervening variables, and therefore of generalization and replication, will continue to exist. The method of control of, for example, matching - looking for comparable pairs of participants for experiments - is insufficient if the variables that could greatly interfere are unknown. To find sufficient numbers of comparable participants, they can only be matched for a small number of variables. Without theoretical insight into the structure of the research subject, doubt as to the effectiveness of this control remains therefore justified. The same applies to such statistical techniques as covariance analysis and partialization. These methods can also be very effective in limiting intervening factors if the variables relevant in that connection are known. This assumes that a model of the research subject exists which contains not only the possible correlations between the independent and dependent variables being investigated, but also the connection between these and possible essential intervening variables. The construction of such a model is a

theoretical matter; on the basis of prior research and insights obtained in other ways, a functional-historical "conditions analysis" of the subject should be made. The result of this analysis should consist of an hierarchical arrangement of essential and non-essential intervening variables, related to time and place.

The usual solution of the traditional methodology is "randomization". By randomly selecting participants for the experimental and the control groups, all "unknown" interfering variables will also have been controlled. The assumption is that the negative and positive effects of these interfering variables neutralize one another, so that *a priori* the two groups can be considered comparable. But the assumption is then also that the interfering variables do not have a high degree of correlation to one another and can be combined in a linear additive model. Interactions and curvilinear relations can greatly imbalance the neutralization of the positive and negative effects described above. It is then impossible to assume that two groups are, in principle, identical (Maschewsky, 1979a, p. 149). The assumption that through random selection, interfering factors compensate for one another should be made at least theoretically plausible in each individual research project. After all, in our social reality it seems more often rule than exception that a series of variables effect one another and other variables in more complex ways. An important thesis of Bronfenbrenner's (1979, p.38), for example, is that in ecological research the principle main effects are likely to be interactions. Therefore, a theoretical test of this assumption for an effective use of the randomization method points once again in the direction of the functional-historical method.

Cook and Campbell (1979), by the way, mention still other limitations of the randomization method which make it unemployable in certain situations, such as the necessity of using existing groups (for example a class or school) instead of individuals. Because of the limited willingness to participate in experiments, it is usually impossible to randomly divide schools or classes into experimental and control groups. And even

if it would be possible to arbitrarily divide a number of schools into an experimental and a control group, it would still be impossible to prevent schools in the control group (which, for example, do not get an extra teacher or attractive new curriculum material) from behaving differently for that very reason. They perceive an undeserved inequality in treatment and will try to make up the difference by making necessary improvements (internal reorganization, etc.) at their own costs. On the other hand, they may become demoralized and apathetic. It is clear that randomization is not the panacee it had been held to be until recently (Cook and Campbell, 1979, p. 55 ff) and that other methods of control are needed. It is also clear that the effectiveness of these methods is very dependent on theoretical insight into the structure of the research subject, insight that Critical Psychology believes can only be obtained through the functional-historical method.

Not only the problem of controlling intervening variables threatens to discredit the results of experimental research. There is also the artefact problem. With undeniable pleasure, Critical Psychology refers to traditional psychology's sharp self-criticism with respect to the results, for instance, of Rosenthal's artefact research (Rosenthal, 1976). The participant in an experiment had usually been seen as a "black box" that only reacts to manipulated experimental stimuli. But artefact research and the "social psychology of the experiment" - the "meta-discipline" based upon it - have made clear that it would be advisable to consider participants as persons with their own interpretations and experiences, who actively assimilate the stimuli, and respond to them. More and more within traditional psychological study, the insight is beginning to grow that the experiment's natural sciences origin will inevitably lead us to equate the social scientific research "participant" with the lifeless, unreflective "subject" of the natural sciences. In that sense, there is be a "cognitive reorientation" in psychology, in which the question of the human's ability to attach meanings to things plays a central role in psychological research. Artefact research has indeed demon-

strated that "even" in an experimental situation, a participant cannot help reacting like a "human being". It appears, for instance, that most participants have an idea of what is desired to help science along. With respect to experimental research, science still has the aura of dealing with genuinely important matters. On the basis of this authority, most participants feel obliged to contribute to scientific progress. On the authority of the researcher, participants are capable of accepting the most peculiar instructions and executing them - as far as they are concerned - as well as possible. Even the (fictitious) murder of a stranger is not excluded, as the Milgram experiments have demonstrated (Milgram, 1974) . Participants develop an idea of what the experimenter and science expect of them. Sometimes these expectations ("demand characteristics") with respect to the requirements of the experimental situation seem to have a greater influence on the research results than the experimental manipulation of the independent variable. Maschewsky, for example, described an experiment by Orne and Scheibe in which they investigated if the usual reaction to sensory deprivation - hallucinations, depressions, fear, etc. - could be elicited from participants who had been deluded by the experimenter into believing that they would undergo sensory deprivation. And indeed, the (experimental) groups, convinced they were involved in a dangerous deprivation study, appeared to react as such, even though they were treated exactly as were the control groups (Maschewsky, 1977a, p. 170 ff).

Of course, here, too, the fundamental paradox of all artefact research is clear. One could, indeed justifiably argue that the participant does not stop being a human being in "meta-research", and that the results of artefact research are themselves influenced by what is investigated or by other artefacts. This is the so-called "Münchhausen-effect" (Hoogstraten, 1979). It is even more paradoxical when Critical Psychologists believe they can base their criticism of the traditional experiment on the results of artefact research. Indeed, this research is not only subjected to the artefact problem itself. It is also subject to Critical Psychology's fundamental criticism of the experiment;

it cannot be generalized nor replicated and lacks relevancy because it also lacks a substantial theoretical foundation, developed by means of the functional-historical method. As is the case in traditional *experimentation, meta-research is based upon bold conjectures*, the "positivistic" surrogate for systematic theory construction. Maschewsky nevertheless concludes that traditional psychology has also been unable to solve the artefact problem, despite all the methodological and technical progress, and that this state of affairs itself should occasion a "Critical Psychological reorientation".

To conclude this paragraph, we will deal with the question of the position of the experiment in the Berlin School. Most of the Critical Psychological statements about the experiment are concerned with its shortcomings; inability to generalize, replicate or make relevant experimental results. But the crux of the experiment, i.e. conditions analysis, the reduction of *reality to a model and variation of conditions*, is tied neither to a specific ("positivistic") methodology nor to a specific ("capitalistic") society. As opposed to the more holistic schools in psychology (for example, Giorgi, 1976), Critical Psychology does not distance itself from analysis, reduction and model construction, which are the conditions for experimentally testing hypotheses. Maschewsky even warns us of the danger of the opinions of those who only wish to be concerned with the "totality", the connections between individual phenomena, out of distaste for analytic studies. These schools, also not unknown to Marxism, must necessarily slip into speculative reflection as "pure Hegelei". In other words, Critical Psychology endorses the essential principles of the experimental method. But it rejects the way in which these principles are put into practice for research, particularly based on the experience that this practice has too much a trial and error character and too little conscious theoretical underpinning. From this criticism, Critical Psychology does not, therefore, strive to abolish the experiment as a method of gaining knowledge, but to achieve an "Aufhebung" of it, that is to perfect it by integrating its most advanced variants with the func-

tional-historical method. Contrary to Maschewsky, however, Holzkamp (1983) seems to regard the experiment to be especially suitable for areas which, for whatever reason (physical or societal) remain outside the sphere of influence of its subject, i.e. outside the sphere of the potential "self-determination". Further, this method may only be used in research situations in which participants and researchers in principle have a subject-subject relation with one another. If this criterion is not met, Holzkamp believes, the experimental method is not "object-adequate", devaluating the participant to an externally determined organism (see below).

However that may be, we have yet to find examples in Critical Psychological literature of a successful integration of the experiment as an empirical method with a functional-historical analysis. Up to now there have been very few attempts in that direction. The more than 4000 pages that have been published in the past 15 years within the framework of the Berlin School have by and large been filled with theoretical criticism of traditional psychology and with the results of functional-historical research. It would, therefore, be a bit of an exaggeration to consider, as does Maschewsky (1977a, p.217, 225), the achievements of Critical Psychology as proof of the postulate that it is possible to achieve more valid and more reliable up-to-date empirical research results through functional-historical analysis. For the time being, the question of the possibility and effectiveness of integrating the functional-historical method and the experimental method must therefore remain unanswered.

ACTION RESEARCH AS A CRITICAL PSYCHOLOGICAL METHOD

In the preceding paragraphs, we saw that Critical Psychology has made a critical issue of, in particular, the role of the participant in the traditional experiment. Due to the methodological necessity of controlling possible intrusive factors, ardent attempts are

made to reduce the participant to a passively reacting, a-historical and a-social organism. Functional-historical analysis has, however, shown that as subjects of psychological research, humans retain their essential characteristics as historical and social beings and as active assimilators of the stimuli presented. It has also been suggested that such attempts at a-theoretical reduction for the sake of control of intrusive, subjective influences are doomed to failure. Only by pairing the experimental method to functional-historical analysis and structuring the research subject is it possible to guarantee control of relevant, essential and intervening variables while maintaining the "human", i.e. historical and social character of the participants in experiments (see Holzkamp, 1983). We have seen that from the Critical Psychological point of view, the crystallization of this "Aufhebung" of the experimental method is still part of the future *agenda* of this new school.

Features of "Traditional" Action Research

One research method that presumes to radically break down the traditional roles of the researcher and the participant and to guarantee the subjectivity of all the participants in the research design is action research ("Handlungsforschung"). This method, based on American action research (Lewin), has the following features:

(1) Action research regards the meaning participants themselves attach to their reality as a framework of reference for formulating and answering the questions posed by research (Heinze *et al.*, 1975, p.42). The action researcher assumes that only by prolonged participation in the reality of those involved is it possible to bring to light the meaning they attach to that reality. With a view to this, the method of "participant observation" (McCall and Simmons, 1969) has an important place within action research.

(2) As opposed to traditional research, in which situational and subjective aspects of reality are regarded as factors interfering with tracking down the relationships between independent and dependent variables, action research accentuates such aspects. The action researcher does not deny the existence of such relationships, of conditioned, regularly recurring behavioral sequences, but aims particularly at expanding the subjective latitude within the participant's reality.

(3) Action research is aimed at an open discussion - also called "Diskurs" (Moser, 1975) - with participants on the research results. Confronting participants with the notion researchers glean of their reality by means of participant observation and other methods could result in a broadening of the subjective latitude. To the extent action research runs up against "law-like" relationships, this open dialogue will be a means of investigating to what extent these are "actual" nomological propositions and to what extent these are ideologically "frozen" regularities owing their existence to the "ignorance" of those concerned. This is based on Habermas' famous statement that a critical consciousness of existing "law-like" relationships cannot lead to invalidation of the law, but can imply the suspension of its effects (Habermas, 1970, p.154).

It is no surprise that a research method that would appear to distinguish itself so radically from traditional methods with respect to the relationship between researcher and research praxis should receive the necessary attention from the ranks of Critical Psychology. Understandably, the question has arisen whether action research, that in West Germany created such a furore especially within the field of education, is capable of bringing us closer to a solution to the problems associated with empirical research in Critical Psychology. As we saw in the previous paragraph, the discussion on the value of the experiment has yet to result in the crystallization of workable empirical methods, let alone in research practices drawn from this discussion. The 1970's have seen the start of a

number of promising action research projects in the areas of educational science. It is therefore to be expected that Critical Psychology would link up with this initiative to overcome the limitations of traditional methodology.

Subjectivism and Dilettantism

Although action research was brought to the fore, in particular by the critical student movement of the sixties, to demonstrate that is was possible to draw practical consequences from the Frankfurt School's polemics against positivism, Critical Psychology has by no means adopted this research method uncritically. On the contrary, on a number of essential points, the theory and practice of action research are sharply criticized. Schneider, for example, mentions in this regard the objective of action research to achieve, together with the participants, a change in their living conditions and an expansion of the individual freedom of choice. But to her mind, a Critical-Psychological "view of humanity" (Schneider, 1979, p.206), for which the tension between self-determination ("subjektiver Bestimmung") and external determination ("objektiver Bestimmtheit") is characteristic, is not its point of departure. On the one hand, individuals are interwoven into an historical-social fabric, which strongly determines their behavior ("objektive Bestimmtheit"). On the other hand, however, they are capable of actively moulding their own lives and thereby moulding social developments as well ("subjektive Bestimmung"). In action research, individuals should rearrange their lives, that is, utilise the possibility of "subjektiver Bestimmung"; but by and large, the objectives and conditions for this change, which should be based upon a (functional-historical) analysis of their "objektiver Bestimmtheit", are left open. In doing so, action research overemphasizes individual behavioral latitude to the neglect of the external determination of human existence. Some action researchers, for example, have the notion that the field should

be entered without any "prejudices", hypotheses etc. at all, in order to give the participants the opportunity of exerting influence upon the formulation of and responses to the research questions. This, however, results in the danger of a more impersonal description of reality being replaced by subjective experience and values colored by ideologies (compare the foregoing paragraphs). Neglect of the "objektiver Bestimmtheit", that is the historical-social context that puts limits upon the individual's freedom of choice, is often accompanied by an overestimation of that individual's capacity for change. Action researchers working in one school or in one clubhouse, believe that through their research they can instigate permanent change, without seeking contact with such social interest groups as labor unions or political parties. Action research then becomes a substitute for political action, the action researcher a political dilettante.

Action research is also often not aimed at testing theories with a scope broader than the practical theories of the participants, but solely at instigating changes, the objective of which is neither explicit nor well-founded. As a result, the scientific relevance and generalization potential, in this case transferability, of the "research" results are dubious. And according to Schneider, the practice of action research demonstrates the validity of this criticism: even when the action researcher takes the trouble to write a report on the progress and results of the project, its usefulness under other research conditions is, to say the least, unclear. Due to the absence of a theoretical background, we gain no insight into the essential prerequisites for succesfully applying the strategy followed. And because there is no clear objective, an evaluation of the results is usually dispensed with. As it did in its criticism of the experiment, here, too, Critical Psychology makes the point that empirical research can only be meaningful if it is conducted within the context of thorough functional-historical analysis of the research subject. Without such prior structuring, the experiment is reduced to an arbitrary selection and manipulation of variables, while action research only results in activism without theory or objective.

Criteria for Critical Psychological Action Research

Nonetheless, Schneider believes that action research could occupy a meaningful position in Critical Psychology, but then the research method must meet the following criteria. Firstly, the research process must be strongly goal-oriented. We have already mentioned how negligent most action researchers are, particularly in the area of making explicit the objectives of research and change. This usually results in the impossibility of an evaluation, in the form of a confrontation of goal and effect, based upon those objectives. As a result, the theoretical and practical value of the projects is, to say the least, dubious. With a view to the transferability of the results, action research should accommodate itself to the conventional demands placed upon every innovation-evaluation study. In this regard, objectives cannot be solely of a general nature, such as "promoting democratic processes" and "emancipation", but must also be cast into forms of intermediate and preliminary goals that can be operationalized. Because action research purports to be a unity of research and action, lucid research objectives should also be formulated which should include at least an analysis of the prerequisites for change, a determination of all the stages of the strategy for change, and finally an evaluation of the effects of that strategy. Secondly, action research should maintain the necessary flexibility and openness with respect to the participants. A detailed research plan is actually incompatible with a great degree of flexibility with respect to the group being studied, which often only in the course of the research project can make clear what they expect from the research and the changes. Action research is characterized, on the one hand, by researchers participating in the processes they are studying and on the other by participants acting as co-researchers in giving direction to the study. A too rigid and extremely goal-oriented planning could endanger the participation of this latter group in the research process. For that reason, action research must shuttle back and forth between the poles of systematic planning

and flexibility. Thirdly, action research should be not only goal-oriented but also process-oriented. Only a detailed description of the process can lead to insight into the dynamics of the change and do justice to the complexity of its occurrence. The transferability of action research results to other situations is dependent upon such a description of the process. From this, we should be able to extract a model in which the interactions between essential prerequisites, induced changes and (preliminary) effects and side-effects are symbolized. Fourthly, action research should contain a continual interplay between "fact", theory and praxis. All too often, action is accentuated too one-sidedly. An important place shall have to be reserved for theory, in the form of results of functional-historical analysis, in both the initial phase of the study and in the interpretation of the results (Schneider, 1980, p.208 ff).

All the criteria action research must meet in order to be useful within a Critical Psychological framework appear to be aimed at increasing its scientific value. It is only possible to understand why such obvious demands are made upon the transparency, goal-orientation and theoretical foundations of action research if seen against the background of its often activist research practice. For some, action research was nothing more than a disguise for the marketing of changes they felt desired. It is quite easy to imagine that a target group would be more likely to accept strategies for change when advocated in the guise of scientific research than when researchers present themselves as professional revolutionaries. We have already seen that under the authority of "science", "participants" are capable of the most peculiar behavior (see Milgram, 1974). Critical Psychology definitely does not wish to support such activist tendencies in action research. For this school, action research does not have a primarily instrumental function -for example, propagating the correct class consciousness - but a theoretical one, that is, testing theories and models developed through functional-historical analysis. Rightly, Critical Psychology wishes to squeeze vaguely structured action into the tight mould of

methodological rules, which can guarantee the theoretical and practical relevance and transferability of its results, while simultaneously maintaining the necessary openness and flexibility with respect to the group being studied.

At this juncture, it is impossible to determine to what extent such a prerequisite can be met. At odds here is the "idealistic" nature of the criteria described above, arising not from a succesful Critical Psychological research praxis, but imposed upon action research in a traditional, idealistic fashion. In this respect, the procedure of Critical Psychology differs little from critical-rationalistic methodology: certainly, neither eschew formulating research rules on an exclusively normative basis without testing them in practice for their feasibility. The "idealistic" origin of the criteria action research must meet are reflected in the formalism of the model that should serve as a guide in this respect. In this model, based upon the work of Schäuble, Fiedler and Eickels, four stages are represented, the third of which is divided into a number of steps. In the first stage, making contact with the field, an attempt is made to stimulate interest in the action research project and to arrive at a very general formulation of the research problem. In the second stage, innovation planning, the project must be protected from possible outside interference. In this stage, for example, the necessary financial and legal guarantees for the continuation of the project must be obtained, while contact with such kindred interest groups as the labor unions should be sought. In addition, accurate and detailed organization and planning provide internal guarantees for the project's success. In the third stage, realization of innovation, an attempt is made to create a spiral movement between problem formulation, goal determination, instigation of change, its evaluation, repeated problem formulation, etc. The recurrent opportunities for feedback in this process ensure a high degree of openness and flexibility with respect to the group being studied. In the last stage, summative evaluation, a goal-effect analysis is made and a detailed description of the research process is provided. In this stage, legitimizing the project

for the "outside world", and in particular for the providers of funds, also plays an important role.

The Problematical Relationship with the Functional-Historical Method

Oddly enough, this model contains no room for the functional-historical analysis of the research subject, once regarded as so important. The stage of making contact with the field, for example, should be preceded by an analysis in which the choice for the specific area "requiring" action research is motivated. But *functional-historical analysis is pre-eminently a form* of theoretical research in which the "lay person" can hardly be involved. As we saw above it requires considerable knowledge to survey the results of the "scientific inheritance" with respect to a particular area and to analyze them by means of a vaguely structured method. With the functional-historical method, a traditional division of roles between the active researcher and the passive research subject would seem unavoidable, certainly so long as the problem of "intersubjectivity" remains unsolved. In order to be conveyable to colleague-researchers, but also to participants wishing to function as co-researchers, a research method must have a clear, transparent structure. In other words, the functional-historical method is a necessary ingredient for any action research, and simultaneously, a "Fremdkörper" (foreign body) by definition, for it does *not conform to any of action research's basic assumptions*. However, Critical Psychology does not make an explicit theme of this paradox. The question of the relationship between the results of a functional-historical analysis and the image the group being studied has of itself and its situation is thereby avoided. So is the question of how discrepancies between goals logically arising from the results of such an analysis and those regarded as most important from the point of view of the experiential world of the group being studied can be resolved without reverting to the traditional relationship between researcher and subjects.

Recently, Holzkamp (1983) has devoted some attention to this problem. Action research proves for him to be a paradigmatic Critical Psychological research method and more important than the traditional experiment. This is, however, conditional upon the participants, as co-researchers, being aware of the conceptual results of the functional-historical method, capable then of communicating at a "meta-subjective" level with the researcher. That seems to us a great deal to ask of both the average participant and the intersubjectivity of the functional-historical method. We certainly cannot require participants to follow an intensive course in Berlin Critical Psychology before being allowed to participate in action research!

Due to the absence of a separate stage for theory construction preceding the actual action research, it is furthermore unclear what scientific value can be attached to the results of the summative evaluation in Schneider's conception. After all, the central question here is: has the innovation strategy achieved its goal or are there effects that conflict with that goal? Reference to theoretical assumptions and expectations is not possible, so that the results of a functional-historical analysis are not "tested" in this area. As a result, the place of action research in the Critical Psychological research process becomes questionable. Due to the absence of a theoretical pole, the so essential tension between theory, "facts" and praxis disappears, despite Schneider's warnings about action research's degeneration into activism.

Methods of Collecting Data

Until now, the question of how data for the analytical and evaluatory stages in the research cycle are to be collected has been overlooked in this discussion of action research. We have discussed the method of participant observation, which guarantees that justice is done to the participants experiential world. In the literature on action research, hosts of other methods

are also mentioned, not precluding such traditional approaches as the interview, the survey, projective tests, group discussions, the diary method, etc. (Heinze *et al.*, 1975, p.140; Haag *et al.*, 1972, p.68; Moser, 1977). In choosing these traditional methods, the assumption is that they are relatively independent of the methodological context in which they are applied. When interwoven into the strategy of action research, such strongly criticized methods as the survey and the interview are presumed to lose their negative connotation. An important critical position of Berger's (1974) with respect to the interview, for example, was that this form of data collection reflects the traditional relationship between overlord and subject. Interviewers are in the position of questioner who usually knows the "correct" answer in advance. As isolated individuals, the ones interviewed are expected only to respond, even though they have no insight into the background or meaningfulness of the questions posed. The process of the exchange of information is by and large one-way traffic. In the action research cycle, however, this one-way traffic is of a temporary nature. The usual process of continual exchange and feedback between researchers and participants is as it were momentarily suspended on behalf of a short-lived stage of systematic data collection.

However, Critical Psychology is somewhat ambivalent towards such methodological eclecticism, in which it is denied that methods can be "contaminated" with the methodological context in which they are applied. On the one hand, Moser, for example, is strongly criticized for his call for the use of traditional methods (Schneider, 1980, p.181). On the other hand, the use of such methods is not discouraged so long as they meet the following criteria:

(a) transparency, that is research goal, hypotheses and generalizations must be comprehensible for the participants;
(b) communication and co-operation, that is continual feedback of information to the participants and a great openness for commentary and criticism, also concerning the technical aspects of the study;

(c) stimulatory function, that is that data collection and analysis must not only be aimed at the researcher's obtaining as much information as possible, but also at stimulating participants' thinking about their own situation;
(d) attention devoted to both subjective and objective factors in the research situation, that is, the collection of "subjective" data such as attitudes and meanings against the background of objective situational data;
(e) involvement of theory, in other words, that in developing new methods, the theoretical presuppositions of critical action research must be borne in mind;
(f) conveyability, in other words, that the methods and techniques applied must provide intersubjectively acceptable data comprehensible also to others outside the circle of those directly involved;
(g) process-orientation, that is, that in particular, methods should also do justice to the process character of every strategy for change, so that conclusions may be drawn concerning the dynamics behind those changes (Schneider, 1980, p.213 ff; see chapter I). Schneider does not mention any concrete methods meeting these rather vague criteria. Developing such methods is a task for the future.

Generalization of the Results

Of course, the above criteria only apply to the first stage of action research in its narrow sense. There is a second stage attached to the first - namely the implementation and evaluation in a broader social context of the strategy for change that has been developed. Curiously enough, no particular demands are made upon the research methods used in this stage (Schneider, 1980, p.209; 212). The purpose of this second stage is to establish the extent to which the strategy can be generalized to other situations, that is to what extent the effectiveness of the program can

then be maintained. Although Schneider does not expand upon this so important stage for the emanation of action research results, one gets the impression that it concerns a poorly founded device for testing the generalization potential of strategies that are the result of a singular process in a specific practical situation. In close relation to the practical situation, and after many cycles of formulating objectives, and inducing and evaluating changes, the strategy for change has developed with intention of not surprising the participants with a ready-made package of "desired" changes. It would seem obvious that in the course of such open and flexible cycles, resistance to change would be broken down, for participants then have the feeling that they themselves are responsible for the changes. For the very reason that so much time is taken in formulating the definitive program, it could then be assumed that the strategy for change would be effective. In other words, an important ingredient of any innovation resulting from action research is the construction stage itself, during which resistance to changes is gradually broken down. If the final results are then made into a complete package to be implemented in other situations, it should come as no surprise that its effects will differ considerably from the results originally obtained. It also runs counter to the principles of Critical Psychology that in this second stage, participants are once again regarded in their traditional role of passive consumer of a package of changes constructed elsewhere, only to serve as data suppliers during its evaluation. For indeed, the traditional, large-scale, extensive data collection methods recommended for this phase allow no room for open communication.

In brief, then, in view of the fact that an essential ingredient, namely the construction of a test for the generalization potential of a strategy for change, has been left out of the stages for action research, we firstly doubt whether it can achieve the aspired goal. Secondly, we doubt whether this second stage does, in fact, meet the criteria Critical Psychology itself has formulated with respect to action research, and, in particular, with respect to the role of the partici-

pants. As was the case with the discussion of the experiment (see the foregoing paragraphs), here, too, we discover that Critical Psychology is as sharp in its criticism of existing methodologies and methods as it is vague in formulating an alternative. The problems of a concrete methodological and technical supplementation of the action research methodology, of the attachment of the functional-historical method to action research in a narrow sense and of the transferability of strategies for change, remain unsolved. As yet, no relevant experience with critical action research has been acquired. On the basis of this paragraph, we can therefore establish that while Critical Psychology has obtained a clear identity in the area of criticism, it still has a long way to go to construct a truly useful alternative paradigm. It is to the credit of Critical Psychology that in this process, it sees action research as just one of its sources of inspiration. In West-European circles of progressive social scientists, there is too often a tendency to regard action research as a panacea for all ills. Rightly, Critical Psychology limits the scope of action research to questions concerning the development, introduction and evaluation of change. In view of the danger of atheoretical and directionless activism, it can only be regarded favorably that Critical Psychology attempts to pin it to a series of methodological requirements designed to guarantee its scientific character.

THE BERLIN SCHOOL AND STATISTICS

When Marx wrote his "Kapital", only eight percent of the population of England and Wales were industrial laborers, about the same percentage were domestic servants (Haug, 1978). And yet he developed a theory in which the industrial laborer was given an extremely important role in changing society. This theory would not have been confirmed by a statistical analysis of the data from a survey of the occupational stratification and the accompanying division of power among the different population groups. But the industrial revolu-

tion carried on everywhere and the power of the working class grew visibly. Universal sufferage was attained, the labor movement and labor parties became a political-economic factor of great importance and the industrial laborer became a trend-setter as regards the improvement of working conditions, increasing income from labor and expanding the package of welfare services. And although a number of important predictions did not become reality - for example, the material "Verelendung" (impoverishment) of the working class, at least in the western world - it remains one of the theory's remarkable achievements to have recognized at such an early stage the importance of this statistical "outlier".

Adorno's Numerical Mystique

On the basis of such illustrations of the seeming superiority of theoretical above statistical argumentation, Adorno concluded that statistics can only emphasize the importance of the average, of the largest common denominator. The seeds of new developments decisive for genuine social progress are degraded to the status of a marginal phenomenon. Statistics are, according to Adorno, incapable of discerning the special quality of quantitatively insignificant phenomena. In a rather impressionistic article, interspersed with emotive language, Adorno attributes the rise of statistics in the social sciences to a general tendency in bourgeois society to attach more importance to quantity, in the form of an object's exchange value, than to quality, in the form of an object's usefulness with respect to satisfying needs (in Adorno *et al.*, 1978). The researcher attaching great value to the results of statistical analysis is therefore the victim of a social ideology in which the power of numbers is greater than the quality of the product or opinion.

Here, the role of statistics in the social sciences is too easily equated with the role of money in economic exchange: both form the extremely abstract

basis upon which incomparable entities are equated and compared. Such eloquently worded attacks directed at statistics have not made their use very popular in research circles viewing themselves as progressive and critical. Of course, the current practice of assigning numbers to phenomena has itself also contributed to the maintenance of the abovementioned numerical mystique. But all too often in the social sciences, a numerical scale is mindlessly chosen as a model for reality, in which figuratively speaking, apples are multiplied by eggs. Without any theoretical argumentation, what should be a nominal scale for equivalent entities is elevated to an ordinal scale of hierarchically arranged characteristics. This kind of "operationalistic" practices often results in a breach in the relationship between reality and numerical model resulting in the model itself being elevated to the status of reality (Kreppner, 1975). For want of theory, for example, it is necessary to declare an operationalization of intelligence, the IQ-test, to be the phenomenon itself. In that case, "Intelligence is what an IQ-test measures" becomes the operationalistic solution to the problem of no longer knowing at all what is being measured (Vroon, 1982). It is such excesses of what, in principle, is a completely logical representation of reality in model form that are grist for the mills of opponents of statistics.

Formal and Dialectical Logic

In addition, the statistics that built upon formal logical insights and therefore endorsed the principle of contradiction - something cannot be true and false at the same time (Copi, 1978, p.306) - appeared to collide with the dialectical logic as foundation of a critical theory of society. The idea that every phenomenon reflects fundamental social contradictions, and is therefore in itself contradictory - is and is not itself - appeared incompatible to a statistical approach that could apply but one number to that phenomenon and therefore appears to deny immanent contra-

diction. In this regard as well, then, statistics is presumed to be incapable of getting to the core of the matter. It seems only capable of providing materials for a view of reality in which undialectical appearances are taken to be the dialectically structured essence (see also chapter III).

In the meantime, however, it has been recognized that this is a fallacy which for many years has been an obstacle to seriously and critically studying an important attainment of scientific pursuit, namely its arsenal of statistical methods. With the succession of Adorno by Habermas within the Frankfurt School, for example, the tide turned in favor of the use of statistical methods, interwoven into an ideology-critical and self-reflective framework. In the so-called socialist countries, as well, there would appear to be less and less hesitation with respect to statistics. The indeed very simplistic frequency tabulation, which for many years represented the only form of quantification in the East German and Soviet Russian social sciences, is - in theory at least - more and more being exchanged for more advanced statistical analyses, generally speaking borrowed directly from "bourgeois" scientific practice. Critical Psychology, too, has finally hazarded an extensive discussion on sense and nonsense in statistics and in the meantime, the first textbook on statistics written from a Critical-Psychological point of view has been published (Leiser, 1978a).

The road to the rehabilitation of statistics within the critical social sciences (we say rehabilitation because Marx, for example, was certainly not averse to mathematical-statistical methods and even regretted not having had a better grasp of the material for studying economic processes) was opened among other things by the criticism on Hegelian notions on the relationship between formal and dialectical logic. Hegel regarded formal logic as a form of metaphysical thought (*sic*) and sharply challenged its assumptions. In its denial that something can be itself and something else at the same time, Hegel believed formal logic also denied the principle of change and progress in reality (compare Riegel's notion of this, chapter

III). After all, the essence of this reality was the dialectical progress through the struggle between contradictory tendencies found in every phenomenon, reflecting the fundamental historical-social dialectic. The East German logician G. Klaus (1972) has discussed the "illogical" nature of this reasoning extensively. He acknowledges that contradiction is not permitted in formal logic; if this were the case, every assertion would be true. In dialectical logic, on the other hand, it is contradiction that forms the "moving principle" of reality. Rightly, however, he remarks that although in Hegelian word games the use of the notion of contradiction in both contexts gives the appearance of equivalence, the terms have entirely different meanings. Contradiction in formal logic concerns attributing and not attributing one feature to a phenomenon at the same time and in the same context. In dialectical logic, contradiction refers to the fact that in the course of time and in another context, a phenomenon "turns into its opposite". A dialectical statement, for example, might be as follows: the tripartition of the powers (the *trias politica*) is at once progressive and conservative. But if this statement is unravelled and supplied with the necessary contextual information, it might read: "In the 18th century, the tripartition of power was progressive, but not in the 20th century", at least not to Klaus' mind (1972, p.55ff). Dialectical contradiction lives on, the seeming formal-logical contradiction has been eliminated. In short, dialectical contradictions can be described in formal-logical terms, so that dialectical and formal logic, and therefore social science and statistics, are not at all incompatible (see chapter III).

Statistical Model-Building

Critical Psychology, by the way, does not completely share this opinion. In his analysis of the reflective nature of logic and mathematics Leiser (1978b) believes that while it is true that formal logic cannot be replaced by dialectical logic, it should be assimi-

lated ("aufgehoben") into it. Contrary to formal logic and mathematical models, dialectical logic is capable of doing justice to the nature of reality as a process. In connection with the artificially "frozen" moments this process is made up of, formal logic and mathematics are important analytical instruments within the dialectical description of this process. Leiser illustrates this thesis with an example concerning the transformation of the pre-capitalistic production of goods in a capitalistic system of production (Leiser, 1978b, p.160 ff). He demonstrates that it is very possible to mathematically represent both systems of production. But the mathematical models do not show that this qualitative change had far-reaching consequences for the working class. The formal, numerical representation of the two moments disregards the transformation process.

It is, however, questionable here whether Leiser has correctly described and illustrated the supposed shortcomings of logic and mathematics. In fact, an important feature of more recent developments in statistics is that more attention is paid to adequately describing dynamic processes in terms of models. As an example, we mention here the method of the time-series analysis (Kratochwill, 1978). Due to the fact that with this method it is possible to make a relatively "static-free" and very detailed representation of complicated economic processes, it has made quite a furore, in particular in the field of econometrics. We might also mention the method of the path analysis (Blalock, 1972[2]), first developed to describe complicated biological processes, with which causal relationships can be tested for their plausibility. Assuming a sufficient number of points of measurement, both of these methods, now also introduced into the social sciences, are without question capable of minutely representing dynamic processes in terms of models. What Leiser more likely means with his "assimilation" ("Aufhebung") of quantitative models into dialectical logic is that every formal logical or statistical model abstracts from features of reality; they are only formal representatives requiring some content. Leiser states for example that quantitative methods are "superficially"

related to the actual process, for these same models can be applied to the growth of cells, nuclear reactions, economic processes etc. But this says nothing more, or less, than that every model should be interpreted at a theoretical level, so that it can be known which reality is behind any formal description. A model is in and of itself nothing, in the most literal sense of the word, however complicated its fundamental calculations are. The final word is therefore its theoretical interpretation, so that we might refer to an assimilation of quantitative methods in the theory. That Leiser translates theoretical interpretation into a dialectical explanation alters nothing in the above argumentation. By using the term dialectical logic, Leiser takes on the burden of proof for his position that every theoretical interpretation should have a "dialectical" character. He is also responsible for showing what steps must be taken in constructing dialectical reasoning, that is for solving the problem of "intersubjectivity" in dialectical logic.

A Constructive Approach to Statistics

Despite its criticism of formal logical or mathematical-statistical models, Critical Psychology assigns an important role to formal-logical argumentations and quantitative methods. Naïve pretentions to totality have made way for more realistic analytical approaches: unravelling a phenomenon into individual aspects and determinants ("conditions analysis"), studying a process through analysis of its parts at several different points in time etc. Criticism of the Hegelian notions of dialectical logic and of the Frankfurt School's holistic pretentions to totality formed the necessary prerequisites for the abovementioned rehabilitation of statistics in the critical social sciences.

Now, if on the basis of this Critical Psychological analysis of "traditional" statistics there were any expectations concerning the development of an alternative conception, these will be disappointed. The most

important difference between a Critical-Psychological introduction to statistics (Leiser, 1978a) and the more common introductions are the former's illustrations, preferably demonstrating the limited educational opportunities for working-class children, while in traditional introductions random samples from populations of either functioning or malfunctioning light-bulbs are used for these purposes. In addition, it is continually and rightfully emphasized that the gap between empirical material and theory can never be bridged by statistical means alone. There is always a limited number of models compatible with the "facts"; a choice between them can only be made on the basis of theoretical insights. But this, too, has slowly but surely become an obvious point of departure in "traditional" statistics. Within these statistical approaches, for example, there is the conviction that in order to avoid completely arbitrary and unreproducible results, multivariate techniques such as multiple regression require that beforehand there are theoretically specified models of the relationship between relevant variables (see Kerlinger and Pedhazur, 1973). Apart from this, *Critical Psychology has just begun with a critical* analysis of statistical methods and more publications in this area may be expected in the future (e.g. Jäger and Leiser, 1979).

CONCLUSIONS

In the preceding chapter, we discussed the most important elements of a Critical Psychological methodology. The necessary pivot of Critical Psychological research proved to be the functional-historical method. This method, based upon the Cultural-Historical School (see chapter I), is a contribution to solving the problem of heuristics; e.g. the systematic construction of a conceptual framework for developing meaningful hypotheses. We ascertained that the functional-historical method must still deal with two unsolved problems, namely the problem of contamination and the problem of intersubjectivity. The former problem refers

to the negative influence historical material (the "Gesamtwissenschaft") might have upon the "truth" of functional-historical results. The second problem refers to the elitist nature of the method, which would only appear useful to a small number of "insiders".

And yet the results of the functional-historical method are decisive for the stage of empirical testing of critical-psychological theory with the aid of such methods as the experiment, action research, and statistical analysis. Critical Psychology makes unnumerable critical remarks on current experimental practices in psychology, but nonetheless regards the most advanced experimental designs as employable in Critical Psychological research, on condition that the necessary preparatory work is done with the functional-historical method. The same applies to the methodology of action research, which Critical Psychology squeezed into a tight mould of rules and stages. Action research has degenerated into atheoretical and purposeless activism, and must be forced to prove its scientific value. In the meantime, Critical Psychology has also overcome its aversion to statistics and advocates its use in the empirical stage of research. Also in statistical analysis, the functional-historical method will have to continue to provide guidance in guaranteeing the validity of the mathematical-statistical construction of models.

In all its criticism of traditional approaches to research, Critical Psychology is therefore by no means averse to the application of current methods and methodologies. It is in particular critical of the gap that has developed between advanced methods of data collection and analysis, and the archaïc way theories, concepts and hypotheses are arrived at. With the help of the functional-historical method, Critical Psychology attempts to bridge this gap. At this juncture, it is difficult to determine whether or not this attempt will be succesful. Critical Psychology has had too little practical research experience to provide a definitive proof of the success of its approach. This does not mean that the "validity" of functional-historical analyses is determined by the results of empirical

research. Holzkamp (1983) has made clear that these analyses can only be tested by means of historical material. This does mean, however, that the "fruitfulness" of the Berlin approach must as yet be demonstrated by succesful integration of the functional-historical and empirical levels of analysis.

Chapter III: Dialectical Psychology

INTRODUCTION

Basic Tenets

A critical-psychological school that developed in the wake of the Cultural-Historical school, but largely independent of the Berlin school, is American Dialectical Psychology. Dialectical Psychology has two basic tenets. Firstly, change, and not stability, is regarded as the most important characteristic of human development. Traditional psychological schools usually postulate one goal for development, that of a stable personality in harmony with its environment. An example of this we shall later discuss in more detail is the cognitive-developmental theory of Piaget and Kohlberg. In that theory a relatively stable equilibrium is postulated at the level of formal-operational thought and postconventional moral judgement. While it is true that development in the quantitative sense, i.e. the accumulation of knowledge and experience, is still possible in adulthood, it is difficult to imagine a further qualitative step to another structure of cognitive and moral thought (see below). Dialectical Psychology, on the other hand, assumes that change is an essential characteristic of every stage in human

development and regards stability as undesirable rigidity, as the artificial paralysis of a natural development. As a result, entirely different research questions are posed: not stable equilibrium, but continual change in behavioral patterns must be described and clarified. And as we shall see, this also results in a preference for less common research designs.

Dialectical Psychology's second basic tenet is that human development can only be explained in relationship to its historical-social context. Often due to their research designs alone, traditional schools of psychology are compelled to study behavior independent of its historical background and under entirely artificial circumstances. Probably without wishing to do so, traditional psychological research creates a picture of the human as an a-historical and a-social being. One pretends to be able to study individual behaviour without taking interactions with fellow human beings into consideration. During research and in test situations, there are often no opportunities for interaction at all, not even with the conductor of the experiment. Ties with the past are also severed intentionally, to prevent experimental effects from being drowned out by high levels of static and unverifiable influences. A superb example of this is the method of the nonsense syllable, introduced by Ebbinghaus. Meaningless letter combinations make it impossible for participants to rely on their own pasts in solving a memory problem (see also chapter II).

In a broader sense, segregating the historical-social context also means denying the influence of social forces and ideologies upon psychological theory development. Psychologists are often not conscious of their own ties to society, for example, in developing a human ideal as the finished product of development. In addition, society is rather influential in determining the pace of development in psychology, for example, by making more or less funds available for particular types of research. It is known, that during the two world wars in particular, certain branches of psychology (e.g. psychometrics) experienced an accelerated development (Samelson, 1979). Yet such facts hardly

penetrate the consciousness of the traditional researcher. Dialectical Psychology wishes to attempt to take account of the historical- social context of the behavior under study, for example by emphasizing longitudinal designs. In addition, dialecticians wish to demonstrate that seemingly free- floating psychological theories are bound on all sides to society and are influenced by social forces in many areas. Such attempts are designated by Dialectical Psychology in particular with the term "sociology of psychological knowledge" (Buss, 1979b).

The structure of this chapter will be as follows: we will start by clarifying the different definitions and schools within Dialectical Psychology, introduce Riegel as one of its main propagators and trace some of the more important ideas. After describing the concrete content of Dialectical Psychology, we will go into more methodological and meta-theoretical problems tackled by the dialecticians. The chapter will be concluded by evaluating in some detail the pros and cons of Dialectical Psychology.

Schools within Dialectical Psychology

American Dialectical Psychology is not one homogeneous school, but is itself characterized by differences of opinion concerning the significance of dialectics in studying psychological phenomena. In critical psychology, the meaning of the concept "dialectics" is neither unequivocal nor fixed; interpreted dialectically, multiplicity is, in fact, a necessary feature of any definition of dialectics, as Rychlak postulates (1976). But still, a number of its more important definitions should be described in greater detail.

Buss (1979a) refers to three aspects of dialectics. Firstly, dialectics can be regarded as a description of the processes of change in society or nature, with special emphasis upon the driving force of contradictions and conflicts. In this definition, dialectics

are almost an ontology, that is, a description of the essence of reality. Secondly, dialectics at times also imply the idea of the continual interaction between a changing individual and a changing society, in which the two "parties" can be both the cause and effect of one another's changes. At times, this notion is extended and the existence of separate entities - individual and society - is rejected by using the concept of "transaction" instead of "interaction". The objective here is to underscore the interdependence of the individual and society (compare this with Norbert Elias' *concept of figuration*, 1980; *see also the Intermezzo*). Thirdly, the concept of dialectics occasionally refers to a *method of acquiring knowledge*. Rychlak (1976) described these dialectics as a "valid organon". Dialectics then refer to a form of "logical" reasoning in which the conflicts between opposing views and opinions can be resolved at a higher level of analysis. The triad: thesis, antithesis and synthesis, is often associated with this form of dialectical reasoning and a higher status is assigned to it than to formal logic.

Dialectics, then, have at least three definitions: dialectics as ontology, i.e. a description of the essential features of reality; dialectics as a metaphysical postulate emphasizing change; and finally dialectics as a method of acquiring knowledge. Now, it is not always equally apparent which interpretation of the concept "dialectics" is maintained by the different dialectical psychologists. Roughly speaking, two schools can be distinguished. The first school postulates that all of the interpretations should be used simultaneously, the second that dialectics only refer to a point of reference or postulate for viewing people and society. The first school could be classified as "dialectical methodology", with which the names of Riegel, Lawler, Van den Daele, Buss and others can be associated. To them, dialectics are a research method emanating directly from the dialectical structure of reality. A valid research method should be dialectical. The second school could be called "dialectical heuristics", with which the names of Youniss, Baltes, Cornelius, Meacham, Lerner and others are associated. The dialectical heuristicians assume that dialectics pro-

vide us with a world view from which meaningful research questions can be deduced, which can then be tested by means of more or less traditional research. Dialectics as ontology result in an entirely different view of human development, for example by regarding change as a focal point and drawing attention to the conflict between opposing tendencies as the "cause" or motor of the dynamics of development. This results in original research hypotheses and theories shedding light upon aspects of reality hitherto neglected. As such, dialectics are a rich source of new ideas concerning psychological research, but are not themselves an alternative research method within the context of justification (see chapter II). In addition to these two schools within dialectical psychology, another less influential, eclectic school can be distinguished, which regards dialectics as an important, but certainly not the only relevant approach. One of its leading representatives is Gergen, and we shall later devote attention to his idea of the "vivification" of a theory by means of empirical research.

It would be going too far to see Dialectical Psychology as the Klaus Riegel school, regarding the work of other dialecticians as simply a series of footnotes to his extensive writings. But just as was the case with the figure of Holzkamp in the Berlin Critical School, both Riegel's work and personality were an enormously important crystallization point for the development of a dialectical school in America. We shall therefore focus this chapter on this inspiring social scientist and, after a short sketch of his life, discuss his ideas concerning the history of psychology in relation to social change. This will be followed by a discussion of his critical remarks on the established psychological theories, in which we shall look at his attempts to construct an alternative dialectical paradigm, examining more fully his methodological notions and the consequences of his ideas on the subject-object relationship in research. Of course, this approach of focusing upon one person certainly does not mean that other representatives of Dialectical Psychology will remain in the shadow of the great inspirator. We shall devote the necessary attention to social scientists

from whom Riegel drew inspiration, in particular such Soviet psychologists as Rubinštejn (see Chapter I), and to social scientists who were stimulated by Riegel but went their own ways, i.e., the school of dialectical heuristics.

KLAUS F. RIEGEL: SOME BIOGRAPHICAL NOTES

Klaus Riegel was born in 1925 in Berlin, where he spent his youth. After the war, he started out working as a maintenance mechanic, but soon began studies in mathematics and physics. Two years later he left for America, acquiring his M.A. at the University of Minnesota (1955). After returning to Germany, he started on a thesis on the intellectual faculties of the elderly (Riegel, 1957). Shortly after its completion, he returned to the United States to continue his research in that area together with his wife. Among other things, he conducted longitudinal research into the effect of aging on intelligence. He began to publish regularly, primarily in gerontological journals, and now and then on psycholinguistics. At first glance, this would appear nothing out of the ordinary, simply the scientific career of a researcher interested in his subject and, entirely within scientific tradition, reporting regularly on his work. However, this impression is only partially accurate. During this time, Riegel must have read and thought an enormous amount about the foundations of his discipline, for after 1965, an essential change can be observed in the content of his articles. In 1965, he published an article on the relationship between social status and language use and in 1966, the first theoretical contributions to the journal Human Development appeared (Riegel, 1966). This is the beginning of a series of theoretical articles ultimately leading to an attempt to formulate a dialectical theory of human development. At the same time, Riegel was active in the area of methodology; he not only wanted to include the changing individual, but also the changing society in his designs (Riegel, Riegel and Meyer,

1967). Other methodologists, notably Baltes and Schaie, shared his dissatisfaction with longitudinal and cross-sectional designs. Riegel was also occupied with the problem of the a-historical character of (developmental) psychology. He himself sought a solution to the problem of linking up with the historical sciences, which could profit from the methods of developmental psychology and vice versa (Riegel, 1967).

In 1970 he joined the editorial staff of the magazine in which he had so often published: Human Development (previously: Vita Humana). Riegel now had a forum at his disposal for venting his ideas and highlighting the work of people thinking in similar directions. His own ideas concerning the social influence on psychological development crystallized in part through the ideas of Rubinštejn, made accessible to English readers by the work of Payne (1968). In a now classic article (1972), he wrote on the relationships between science (and in particular developmental psychology) and society. At his instigation, annual conferences were held on dialectics. A "Network for Dialectical Psychologists" was established and a Dialectical Psychology Newsletter appeared. This was the birth of the American variant of critical psychology. Riegel collected his essays in Psychology of History and Development (1976), but died a year later at the height of his career. Two books were published posthumously: Psychology, Mon Amour: a Countertext (1978) and Foundations of Dialectical Psychology (1979).

HISTORY OF PSYCHOLOGY

"A spectre is haunting Western psychology; the spectre of scientific dialectics. The scaffold of the academic world is shaking; the time for its transformation is near" (Riegel, 1979, p.14). With these ominous words, Riegel sounds the death-knell upon two, to him, failing traditions within developmental psychology, the "capitalistic" tradition and the "mercantile" tradition. What then is the core of Riegel's objections to

these still current tendencies in theory and research in developmental psychology?

For many years, Anglo-Saxon scientific study was dominated by the "capitalistic" orientation of Hobbes, Locke, Galton, Hall and Gesell. This orientation crystallized into a social-Darwinistic interpretation of onto- and phylogenetic development. According to Riegel, such slogans as *bellum omnium contra omnes*, and "struggle for life, survival of the fittest" capture succinctly the essence of this interpretation. For psychology, for example, this meant that in studying individual differences, the young white male adult, involved in business or industry, was to be regarded as the most succesful "survivor", to be elevated to the criterion by which all other individuals would be measured. If individuals or groups did not meet this standard, they were simply classified as backward, deviant etc. Children were thus characterized as imperfect adults, differing not in the qualitative but only in the quantitative sense from the ideal adult. Through research attempts were made to describe trends and create standards by which individuals could be measured. Within this conception, development is a gradually increasing accumulation of knowledge and skills. That this orientation was able to dominate developmental psychology, especially in Anglo- Saxon countries, is in Riegel's view attributable to specific external, i.e. economic and cultural circumstances. In this connection Riegel mentions the wide-spread myth in these countries of progress through unfettered competition, and the typical English predilection for hunting and breeding, paving the way for a capitalistic perspective in developmental psychology.

Buss (1979a, p.27ff) refines this analysis somewhat by pointing out that in particular, Galton's ideas were stimulated by the following three social tendencies: (1) British capitalism had resulted in a strong specialization of positions in society and their accompanying qualifications. This was legitimized by a theory in which individual differences in capabilities were traced to genetical differences. (2) Measuring, i.e. quantifying differences in intellectual skills was

common practice in a capitalistic society in which goods were not judged according to their usefulness but according to their formal exchange value (see chapter II). And this applied not only to objects but to subjects as well; for example, to laborers, whose efforts were assessed according to their exchange value, expressed in terms of money; (3) The combination of a hierarchically structured class society, and respect for individual freedom and opportunity can only be durable if differences in status are a reflection of innate differences in skill and developmental potential. With his genetic theory, Galton provided this legitimacy. But this same liberal capitalism impeded the development of an eugenic movement, which Galton had hoped to initiate with his utopia "Kantsaywhere". The totalitarian measures of a Platonic state, necessary to put eugenic ideas into practice, for example prohibiting reproduction among certain groups, were incompatible with the liberal foundations of British society. This makes apparent that society is not only capable of stimulating psychological theory development, but also of setting limits on eccentric theoretical tendencies.

Origins of the Mercantile Tradition

As opposed to England's colonial and capitalistic tradition, Europe's most important countries had an especially strong mercantile tradition. There a new middle class arose, not as privileged as the wealthy landed aristocracy, but considerable better off than the working class. Riegel believed that the discrepancy between the social interests of this middle class and their privileges ultimately led to the French Revolution. Competition was permitted especially within the classes, but not between (the hierarchically arranged) classes. According to Riegel, this gave rise to a "mercantile" tradition in which differences in background could be valued. Children were no longer regarded as inferior adults but appreciated for themselves and regarded in the context of their peers. By nature,

human beings were good; it was society that gave rise to differences. The most important exponent of these ideas was Jean-Jacques Rousseau. Fröbel and Montessori followed in his footsteps, introducing a more child-oriented approach in education. Riegel believes that Spranger, too, was influential through his plea for an understanding attitude towards the adolescent sub-culture. Piaget was the last link in this chain of liberal scientific thinkers. Throughout, development is seen as gradual, discontinuous progress through a series of stages which cannot be compared to one another. According to Riegel, competition within stages is conceivable, but it is not of great importance and is rejected as a driving force across stages (Riegel, 1972, p.134). Riegel thus suggests a correspondence between the sociological concept of class and the psychological concept of stage. Just as mobility from the lower to the upper classes was rarely possible, and the representatives of each class were regarded entirely on an individual basis, each stage of mental activity also had an entirely individual character. Because of the emphasis Piaget laid upon the young child's "egocentric character", he is regarded by Riegel as one of the "mercantile" scientific thinkers. On the other hand, Vygotskij would belong to the "socialist" scientific thinkers because of his emphasis upon the social aspects of development (see chapter I).

A Dialectical Synthesis

Riegel regards the "mercantile" approach as a step in the right direction and does, indeed, welcome the growing interest among American researchers for this school. On the other hand, he is not an advocate of a wholesale adoption of this approach, because science can advance only if the divergent viewpoints are integrated at higher levels (o.c., p. 135). Elevating both viewpoints to a higher level, Dialectical Psychology can provide a solution. Development is neither an accumulation of information by an, in essence, passive organism, nor the spontaneous production of new ways of

thinking. Characteristic of Riegel's synthetic concept is the notion that the individual should be regarded as an actively changing organism in a continually changing world and that both the individual and the environment should be the object of study and research. Following upon the Marxist anthropological notion that man changes the world through his labor and, in turn, is himself influenced by this changing world, the dialectic between the individual and the environment should become a major theme in the dialectical school (see chapter I). Riegel summarizes his concept in a diagram in which subject (individual) and object (environment) have either an active or a passive character. In doing so, he arrives at a classification into four schools, and it should be clear from the above that he has an affinity to the active subject in an active environment.

TABLE III.1

Classification of Trends in Psychology

		Individual	
		passive	active
Environment	passive	Locke/Hume Ebbinghaus	Leibniz Piaget/Chomsky
	active	Mannheim Vygotskij/ Skinner	Hegel/Marx Rubinštejn

Some Criticism

In his attempt to uncover a number of trends in the history of psychology and to anchor them in social developments, Riegel tries to describe this history from an externalistic point of view rather than through

the traditional internalistic "Ideengeschichte" (history of ideas). He is not, however, entirely succesful. Riegel's description is actually only in part externalistic, for the lines he draws from Darwin to Galton, Stanley Hall, Terman and Gesell, and from Rousseau to Fröbel, Montessori, Spranger and Piaget can be interpreted much more easily from an internalistic vantage point, that is as the assimilation of ideas of one scientific thinker (or group of them) by others. In addition, Riegel's classification of the researchers mentioned seems a bit contrived. Stanley Hall, for example, was strongly influenced by the theory of evolution, but he certainly did not endorse the notion that development is a quantitative, cumulative process. On the contrary, he also distinguished stages in child development, namely infancy, childhood and adolescence. It would seem more instructive to view the controversies Riegel sketched in the light of the law of biogenetics. As we know, advocates of this law believed that in ontogenesis, children must repeat certain stages from the history of humanity. Firstly, this resulted in an emphasis upon the nature of these stages themselves, and secondly, in a passive attitude towards education (this was exactly Stanley Hall's attitude). However, this combination of the theory of evolution and thinking in terms of stages does upset Riegel's simplistic diagram.

Striking is also that Riegel devotes no attention at all to behaviorism, a school which at that time was on the rise and which was based on entirely different notions of human behavior, namely on the influence of the environment. Riegel suggests that Darwin's ideas were received warmly by the aristocratic upper classes of the population, while it was the middle classes who were attracted to the notions of Rousseau. The question then arises why it was the Anglo-Saxon ideas that gained solid ground in America, despite the absence of a traditional aristocracy. It is not unlikely that the dissemination of ideas in America was particularly the result of increasing immigration from England, which suggests that is was not the aristocracy (who did not, in fact, emigrate), but others who were responsible for this dissemination. In that sense, Riegel's analysis

would appear to have some serious shortcomings, failing to transcend the level of general impressions.

Perhaps Buss' interpretation (1979b) provides an alternative explanation here. Buss states that due to the immigration problem, genetic ideas concerning such psychological skills as intelligence were welcomed with open arms in the United States, despite the largely behavioristic tradition. The doctrine of inherited intelligence, and the intelligence tests associated with it, provided legitimacy to discriminatory immigration quotas restricting immigration from some European countries early in the twentieth century. Unskilled (agricultural) workers from "backward" European regions, and certain minority groups in particular were the victims of these quotas.

It is also curious that Riegel did not mention the philosopher Locke in connection with the opposing views of the Anglo-Saxon countries and the continent. And yet it was Locke who even before Rousseau claimed that children should be regarded as a separate category, which was not to be judged by standards deduced from adult behavior. Riegel's theses on the influence of social factors on the development of psychology are quite appealing but not very convincing.

The epistemological diagram classifying the four tendencies in psychology also deserves critical attention. Piaget's classification in the upper right-hand compartment of the diagram, in which an active role is assumed for the individual but a passive one for the environment, is not entirely correct. In his early work, Piaget was very much interested in social conditions, apparent from his discussion of Durkheim's ideas on the social context of moral convictions (see e.g. Youniss, 1981; Van IJzendoorn, 1980). In his later work, however, the image of a self-directed monadic individual becomes increasingly more prevalent (see Harten, 1977; Broughton, 1981c). Elsewhere we have also shown that Vygotskij cannot be accused of neglecting the active role of the individual (see chapter I). It is clear that Riegel's epistemological diagram is a Procrustean bed in which a number of important psycho-

logists, from whom Riegel himself drew inspiration, are in danger of being distorted almost beyond recognition. The diagram does, however, contain the path he followed in developing Dialectical Psychology: from a critical reconstruction of Piaget's ideas to the fundamental dialectical foundations Rubinštejn provided psychology with. In the next paragraph we shall follow his path.

DIALECTICAL OPERATIONS

The relationship between Riegel and Piaget deserves particular attention. Piaget's theories are an extremely important link to Riegel in the development of a Dialectical Psychology, illustrated by the fact that Piaget is the author most cited in "Foundations". In principle, Riegel was not unreceptive to Piaget's theory of the senso-motoric stage. In this first stage of cognitive development, the dialectic between accommodation and assimilation is still very clearly present. On the one hand, the child adapts objects from the outside world to fit into available schemata (assimilation). At one point, for example, the child clutches at every object whether the object lends itself to that purpose or not. The child quickly realizes that some objects are either too heavy, cumbersome, awkward or whatever. The result is an adaptation of these schemata to experiences (accommodation). For a long time, for example, children are convinced that the amount of water in a tall, narrow glass is greater than that same amount poured into a short, wide glass. They persist in that opinion even when they see the water being poured from one to the other. When that same amount of water is then poured into a third glass, similar to the first, they are suddenly convinced that it is the same amount of water after all. The contradiction prevails until the concrete-operational stage, when the child acquires a schema for the transformation process and its reversibility. Piaget also devotes a noticeable amount of attention to the presence of dialectical thinking in the second, pre-operational stage. Children do not appear to be concerned about

contradictions in their appraisals of reality - according to Riegel with good (creative) results. When shown, for example, twenty wooden beads, of which fifteen are painted brown and five white, a child might conclude that there are more brown beads than wooden ones. This is because it is incapable of simultaneously taking account of all the different dimensions by which beads can be classified.

Piaget's error is assumed to be that he did not define the child's tolerance for contradictions as an example of creative dialectical thinking. Piaget sees it as a (fortunately) passing phase which, in the transition to a higher stage, disappears through the experience of conflicts between behavior and judgement (and between schemata: "décalage"). To Riegel, this meant that Piaget analyzed children's thinking as being more and more alienated, non-creative and non-dialectical. In the pre-operational phase, a child still understands that an object may possess a certain quality and not possess it at the same time. It will not, for instance, have difficulty in constructing a creative explanation for the fact that some small objects float in water while others sink to the bottom. In a later phase, however, the child will regard this as an incongruous contradiction and start to look for unimaginative rationalizations. One of these might be to determine what the relationship is between the object's size and weight in comparison to the same relationship between a volume of water and its weight (the notion of "specific gravity").

In Defence of Piaget

Riegel continually advocates the retention of dialectical aspects of children's thinking. Unlike Piaget, he does not welcome this "alienation of thinking" towards a rigid formal-operational reasoning in which there is no place for contradictions. This can be attributed to the Hegelian notion that dialectical logic is superior to formal logic and as such should

replace, or at least complement it. But while formal logic concerns the formal characteristics of language, we believe dialectical logic should be seen as a working hypothesis for investigating developments in the real world. According to this hypothesis, aspects of reality follow a dialectical course from thesis via antithesis towards synthesis. Dialectics are therefore concerned with the world of objects, while formal logic relates to the meta-level of propositions about reality. It is therefore erroneous of Riegel to believe that a child is thinking dialectically when it remains unconcerned about the contradictions contained, for example, in the statement that a small elephant is both large and small or that an express train travels both quickly and slowly. Indeed, making explicit the frames of reference implicitly contained within these statements results in an absolutely logical argument. We might then "translate" the statement concerning the elephant as follows: in comparison to a big elephant, a small elephant is small, but compared to a large mouse, on the other hand, a small elephant is big. In short, Riegel wrongly places dialectics and formal logic on the same plane: that of the analysis of propositions. Dialectical logic, however, is concerned with the world of objects and is as such unquestionably consistent with formal logic. It is therefore no problem at all to analyze, by means of formal logic, propositions, which attempt to describe the dialectical course of reality (Klaus, 1972; Tolman, 1981; see also chapter II).

That Piaget's theory is not dialectical in the psychological sense is for that matter also contested by a number of French researchers. Goldmann, a co-worker of Piaget in the late forties, has pointed to a number of interesting parallels between Piaget's work and the dialectical epistemology of Hegel and Marx (Goldmann, 1959). The similarities he points to in particular are the genetic approach and the unity of thought and action. As we know, Piaget was particularly concerned with the development of intelligence in ontogenesis. This same emphasis upon behavioral development is to be found in the Russian dialectical psychology of Blonskij and Vygotskij. These authors were inspired by the ideas of Hegel, Engels and Marx

concerning the historical origins of human behavior (see chapter I).

According to Goldmann, Piaget's emphasis upon the fundamental unity of thought and action is in complete agreement with Marx' ideas, in particular with respect to his cautioning against the notion that thought is something abstract, detached from practical activity ("Tätigkeit"). Finally, Goldmann points out that in describing intelligence as the result of accommodation and assimilation, Piaget is in essence presenting the same portrait of human activity as did Marx. Marx, indeed, also wrote that man changes nature (assimilation) and through it, himself (accommodation). This analysis of Piaget's theory, recently endorsed by Garcia (Garcia, 1980), shows that while not a Marxist, Piaget displays some striking similarities to the thinkers of the dialectical tradition (see Broughton, 1981e, p.406).

A Fifth Stage

And yet Riegel felt obliged to neutralize the so-called anti-dialectical character of Piaget's theory by adding a fifth, dialectical stage to Piaget's classical stages, suggesting that the transition through one or more of the "traditional" stages is a condition for reaching that stage. In other words, the highest level of dialectical thinking can be reached both through the senso-motoric stage and through the pre-operational, concrete-operational or formal-operational stages. This stage, then, is an acknowledgement of the fact that the highest level of thought can be attained by various, but equivalent means. Via formal operations, Riegel believes, a scientist must enter the stage of dialectical operations in order to conduct creative scientific work. The manual laborer might achieve that dialectical stage by mastering concrete operations. The artist could learn to master the dialectical skills necessary for his or her activities even through the pre-operational level. Finally, Riegel

believed that the lover might suffice with a dialectical intelligence acquired through the senso-motoric stage. Unfortunately, Riegel fails to make his theory more convincing by not conducting a further, more concrete analysis of the qualification requirements for the different professions and activities. There is no empirical basis for this theory; here, too, we see the strongly impressionistic nature of his work.

Riegel also remarks that through his amendment of Piaget's theory, intra-individual variations in intellectual functioning are more easily explained. Indeed, the different stages do not exclude one another; in principle, each individual can and should be able to function at all levels (the idea of the all-round personality). To do household chores, a scientist needs concrete-dialectical operations, to make love senso-motoric dialectical intelligence. However, Riegel errs in assuming that intra-individual differences cannot be explained within the framework of Piaget's theory. In this connection, he neglects to point out Piaget's important notion of "décalage", i.e. intra-individual differences in level of development depending upon experience. This phenomenon of "décalage" has an important place in Piaget's theory of the dynamics of development, his so-called equilibrium theory. It has also acquired a key position in didactic theories based on Piaget's theory. Riegel, however, repeatedly remarks that it is impossible to draw didactic conclusions from this theory. The child is either in an earlier stage, so that due to assimilative tendencies stimulation is ineffective, or in a later stage where stimulation is superfluous.

In doing so, however, Riegel loses sight of the frequent occurrence of a person's functioning at a higher level in one area, and at a lower one in another unfamiliar area. This "décalage" results in a state of tension and implies therefore opportunities for development that didactics can take advantage of (Piaget, 1976). This also applies to inter-individual "décalage". Kohlberg's didactics for moral education, for example, could in certain respects be characterized as an arsenal of methods for eliminating inter- and intra-

individual horizontal "décalage" (Van IJzendoorn, 1980; see also Vygotskij's related notion of the zone of proximal development, chapter I). Finally, it should be mentioned that Riegel's description of Piaget's didactic paradox assumes only the presence of assimilative tendencies while ignoring the accommodating tendencies so essential in this respect.

In short, Riegel criticizes Piaget for his neglect of the dialectical character of creative and mature thought and adds that Piaget allows too little room in his theory for parallel inter- and intra-individual differences. With respect to the first criticism, we have pointed to Riegel's inaccurate interpretation of dialectical logic. Riegel's second objection is related to his lack of knowledge of the phenomenon of "décalage", an entry noticeably absent from the indexes of his books.

Alternative Dialectical Interpretations of Piaget

In American Dialectical Psychology, the work of Piaget is still the subject of lively discussion, and by far, not every dialectician endorses Riegel's interpretation discussed earlier. Some take a more extreme standpoint than Riegel's, others have a greater appreciation of a number of dialectical elements in Piaget's genetic epistemology. A representative of the former school is Buck-Morss (1975), who accuses Piaget of a socio-economic bias. Buck-Morss believes Piaget's theory of child development to be a direct reflection of the capitalistic society in which it originated. Repairs *sensu* Riegel do not help a great deal in this regard. Buck-Morss' most important argument is that Piaget describes child development as the development and perfectioning of the ability to distinguish form from content. In the course of development, ever better cognitive judgments are made ever more frequently purely on the basis of environmental form-features and less and less on the basis of features relating to

content. One of the first cognitive milestones in a child's life, object-permanence, is already an act of alienation, in that it implies a division between form and content. Objects are no longer evaluated according to their unique, individual utility, but according to their external appearance. Whether it concerns an amount of water or the same amount of clay, only the equivalent quantities are important, whatever is done with the water or clay. In such an evaluation, an object is regarded as a measurable quantity, abstractly and independent of its origins. Buck-Morss regards this as characteristic of a capitalist society, in which the formal exchange value of goods, and not their utility, is of paramount importance (see also Adorno *et al.*, 1978). The cognitive ability to distinguish form from content, and in particular to evaluate according to formal characteristics, is the very skill a capitalist society demands (cf. Lawler, 1975).

The problem is now that by far, not every group in a capitalist society learns to master this cognitive skill perfectly. Research has shown that not everyone is capable of formal-operational thought, and Buck-Morss attempts to explain this with the different positions these groups occupy within society. Only those groups (classes) actively participating in and helping to shape society learn to think completely formally - and these are especially the higher socio-economic groups. The lower socio-economic groups do not have the opportunity of escaping the concrete reality of daily life and of influencing the course society is taking. Indeed, they have no use for formal operations. The question is, however, how this analysis can explain the fact that in the so-called "socialist" countries, formal-operational thought also exists and is very likely mastered to different degrees by different groups. If formal-operational thought were only a reflection of a capitalist society structure, this form of thought would not be present in the so-called "socialist" countries. This seems not to be the case, so the analysis of Buck-Morss must be inadequate.

Habermas on Rational Communication

Another interpretation of the phenomenon of formal-operational thought is suggested by the Frankfurt School. Habermas in particular assumes that independent of the type of society, there is a universal tendency towards a more and more perfect mastery of nature. Formal operations embody the so-called instrumental rationality required for this task. Formal-operational thought enables people in both capitalist and socialist countries to discover the technical means of achieving postulated objectives (cf. also Buss, 1979a, p.115ff). According to Habermas, Piaget has therefore described one of the necessary skills in modern man's intellectual "arsenal". But he adds that in connection with mastering the environment, this skill must be complemented with the ability to formulate the desired objectives together with all others concerned. He calls this skill "communicative behavior", and only where this is mastered is there rational management of the interactions of people with one another and with their natural environment. The skill of communicative behavior incorporates the conditions as shown in Table III.2.

Habermas' model shows that for communicative behavior, not only formal-operational thought is required, but also post-conventional moral judgment, as described by Kohlberg (see Van IJzendoorn, 1983) and a mature identity in the sense of G.H. Mead. Only when these conditions are met is an optimal interaction between people and their natural and social environments possible. Moral development creates the conditions for rational, practical discourse on the objectives society feels justified in aspiring to, and cognitive development creates the conditions for the rational search for the means of realizing those objectives. An optimal identity development ensures that all individuals will be capable of articulating and doing justice to their most essential needs. This corresponds with Meacham's idea of compromises between moral principles and the requirements for optimal self-esteem (1975).

TABLE III.2

Habermas' Model of Rational Communication

Level of Interaction	Level of Behavior	Identity	Moral Development	Cognitive Development
incomplete interaction	concrete behavior and its consequences	natural identity	pre-conventional level	pre-operational thought
complete interaction	roles and norm-systems	role identity	conventional level	concrete-operational thought
communicative behavior	principles	ego-identity	post-conventional level	formal-operational thought
		(Mead)	(Kohlberg)	(Piaget)

(adapted according to Habermas, 1975, diagrams 3 and 4; and Habermas, 1976, p.78 and 83).

Contrary to Buck-Morss then, Habermas does not regard formal operations as a demonic invention of capitalism, but as a necessary condition for an ever greater mastery of nature. Unlike Riegel, Habermas does not complement formal-operational thought with dialectical operations, though he also believes that formal operations are inadequate in making rational the interaction between people and their environment. But Habermas, on the other hand, complements cognitive development with the development of a number of other skills, such as the ability to make post-conventional moral judgments. In doing so, he stays closer (for some too close: Broughton, 1981e) to the achievements of developmental psychology than Riegel, who rather gratuitously adds an empirically unfounded fifth stage to cognitive development. For this reason, we regard the model of communicative behavior, despite its shortcomings, as a psychologically better grounded, and theoretically more tenable alternative for Riegel's amendment. It integrates instrumental and practical rationality without making concessions with respect to the logical and empirical quality of the theory underlying it. Of course, this preference is also related to our rejecting the notion that dialectical logic is a higher form of formal logic. In the interpretation of Piaget as well, dialectical heuristics lead to a different conclusion than dialectical methodology (cf. Youniss, 1978).

THE DOUBLE DIALECTIC AND THE DYNAMICS OF HUMAN DEVELOPMENT

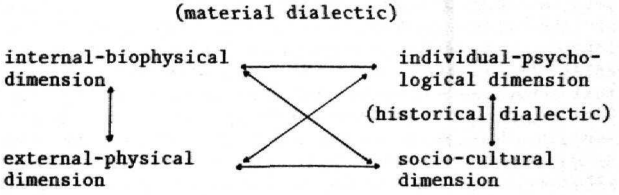
The critical reconstruction of Piaget's theory did provide Riegel with a dialectical picture of the internal dynamics of human development, but it has not resulted in explicit themes with respect to the influence of the social context in Dialectical Psychology. But as we saw earlier, Riegel still sought descriptions of and explanations for the changing individual in a changing society. He now developed his alternative in this direction by consulting Rubinštejn.

The assumption of Rubinštejn's theory is that at birth, the individual's development is primarily determined by bio-physical processes, but that in the course of the chronological growth of intellectual activities, cultural and social processes play an ever greater directive role. In addition to these interactions between the individual and society, there are also interactions between biophysical and individual intellectual activities. The former interaction system represents the historical dialectic, the latter the material dialectic. Riegel believed that an individual's thoughts, actions and emotions could transform those of others living contemporaneously or subsequently, but with respect to contemporaries, the reverse is also possible. With the dynamic interaction of the internal and external dialectic, humans not only transform the external world in which they live; they in turn are also transformed by the world they have created (see chapter I and II). A dialectical theory should be concerned with simultaneous development along four interwoven dimensions: the internal- biophysical, the individual-psychological, the socio- cultural, and the external-physical dimensions. As opposed to Rubinštejn, Riegel believed that environmental influences should be divided into two aspects: in addition to the socio-cultural aspect, behavior could also be affected by natural disasters, the geographical location and the climate (see Table III.3).

Riegel now sees development as the co-ordination or synchronization of any combination of two dimensions and ultimately of the accumulation of progressions along each dimension. However, co-ordination and synchronization are not always possible. When synchronism is absent, the result is a crisis or a conflict. But such a conflict should not be viewed negatively. A crisis is a constructive confrontation in which contradiction or lack of harmony are the source of new changes, both in the individual and in society. We shall illustrate the above by discussing a number of his examples.

TABLE III.3

The Double Dialectic



At the internal-biophysical level, the heart and lungs can function synchronically under normal conditions, but when the person in question is fatigued, they might conflict with one another. But because this concerns a conflict within one dimension, there is no question here of development. An individual may be biologically mature enough for marriage, but because the right partner has not yet been found, lack maturity at the individual-psychological level. It is also possible that the individual-psychological and biological levels are synchronized, but that the housing market is tight so that socio-cultural circumstances are at odds. If a natural disaster occurs, the external-physical circumstances might spoil the plan. Riegel claims that everything is continually changing and rarely in perfect harmony.

From a psychological point of view, dealing constructively with conflicts means progress. But in Riegel's sketchy approach, it remains obscure what he exactly means by "dealing constructively with conflicts", and what conditions must be met in order not to experience and deal with asynchronisms in a destructive fashion. Probably in part due to Riegel's desire not to tie himself to a (closed) vision of humans and society, even the criteria for judging whether or not a constructive solution for an asynchronism has been found remain implicit. In the above example of asyn-

chronism, for instance, it indeed makes some difference whether the solution is sought in the area of social action or in the private domain. But at this point, Riegel refrains from making any judgment. What in any case is commendable in Riegel's theory of double dialectics is its implicit call upon developmental psychology to replace its one-sided emphasis upon, in particular, the interaction between internal-biophysical and individual-psychological aspects with an interdisciplinary approach devoting equal attention to all four aspects. Here we leave the subject of development from a dialectical perspective to go on to an appraisal of other issues, i.e. ideology-critique and methodological questions.

THE SOCIOLOGY OF PSYCHOLOGICAL KNOWLEDGE

On the one hand, Dialectical Psychology is theoretical criticism of the dominant psychological theories, on the other a constructive contribution to psychological knowledge through empirical research. The theoretical criticism utilizes the method of ideology-critique, a branch of the sociology of knowledge. An example of this was seen in the discussion of the dialectical criticism of Piaget's theory. The empirical research in Dialectical Psychology utilizes, among other things, the mixed-longitudinal design and aspires to a subject-subject relationship between the researcher and the "participants". These approaches will be discussed in the paragraphs that follow. Here we shall discuss in greater detail the problem of ideology-critique.

Ideology-Critique in Dialectal Psychology

The sociology of psychological knowledge has especially been advocated by Alan Buss (1979b), who himself is responsible for several examples of such re-

search. In his view, there are two reasons for the necessity of devoting attention to the social influences upon psychological theories. Firstly, the subjects of these theories - humans - change under the influence of society, and this leads to changes at the level of theory development. The sociology of knowledge is designed to trace these changes and if necessary, to identify obsolete theories as being such. Buss calls this the individual-society dialectic. Secondly, the sciences are also exposed to social influences during the construction and adjustment of a theory, and it is self-evident that to a greater or lesser extent, the theory will reflect that society. At times, for example, a theory may bear traces of the scientist's special social position within capitalism. Buss calls this the scientist-society dialectic. If psychology really wishes to be conscious of its ties to society and to operative values, a sociology of psychological knowledge must be capable of describing this dialectic; such a consciousness is a prerequisite for a truly critical psychology.

In and of itself, this is an entirely acceptable line of thinking, and it is no surprise that for many years, critical psychology in all its different variants has produced ideology-critique of established psychology schools and theories. But there are a number of problems connected with Buss' notions, emerging, in particular with the application of his ideas to concrete theories. The problems are related to the absence of a clear frame of reference for criticism. With what right does an ideology-critic place himself above the theories he studies, and what external "validity criteria" are used in doing so? An ideology-critic is also subject to the influences of society and cannot pretend to be alone in knowing the path to a-historical truth. In addition, the ideology-critique as described by Buss contains the danger that traditional scientists will be rather tastelessly "caught" with their supposed social biases and ties, and rather destructively discredited, without this producing any indication of the valuable elements of the theory being criticized. Buck-Morss' analysis discussed earlier is an example of destructive ideology-critique. It can no longer be called dialec-

tical in the sense of being a synthesis of valuable elements at a higher level. To her mind, Piaget's theory is not a necessary step in the direction of a dialectical theory of human development, but more an aberration of alienated capitalism.

Another example is Furby's criticism (1979) of Rotter's theory of the locus of control. According to Furby, this theory contains the bias that an internal locus of control is always *more* valuable than an external one, and according to him that is no coincidence. An internalized locus of control does, after all, serve the interest of capitalism's ruling classes, for with it, faults in the system are attributed to the people themselves rather than to the structure of capitalism. Blaming oneself for unemployment is no real problem for society. Only when the unemployed blame their fate on external circumstances, evidence of an external locus of control, does unemployment become a hotbed of social unrest and conflicts. And because American psychologists belong to the privileged classes in their society, they have a great deal to lose with the development of an external locus of control. Furby's method here is to make a theory suspect by pointing to the privileges of psychologists as a group, while from a methodological point of view, the maker of the theory should not be a significant factor in its evaluation. In addition, there is not a trace of a dialectical "Aufhebung" of Rotter's theory at a higher level. The question then arises whether the theory really does not contain any valuable elements.

Some Definitions of "Ideology"

Perhaps the problem of ideology-critique can be somewhat clarified by producing a number of variants of the definition of the concept "ideology" as used since Marx and Engels (cf. Klafki, 1976). Firstly, "ideology" can be defined as a socio-economically determined, necessarily "false" consciousness. In this sense, ideology performs an important function in maintaining

unjust power relationships. Often the ideology is taken to be true by the underprivileged classes themselves. Ideology-critique is then the scientific description of the circumstances under which a theory could develop and its role in legitimizing social abuses. Secondly, "ideology" can also be used as a designation of the totality of notions, ideas and theories on social reality, that is, the entire "Überbau" (superstructure) of society. In a bourgeois, capitalist society, this "Überbau" is necessarily impregnated with erroneous ideas and justifications. In a so-called socialist society, the official Marxist-Leninist "ideology" is right because it is presumed to reflect a just society. Thirdly, Mannheim's concept of ideology ties in well with the notion of ideology in the so-called socialist countries. It labels all products of thought ideological, due to their relationship to society. In fact, this concept of ideology was the foundation for the discipline of the sociology of knowledge. To Mannheim, the objective of ideology-critique was to distinguish knowledge as a reflection of current social conditions from knowledge as a reflection of earlier, out-of-date conditions. Mannheim believes the latter to be invalid knowledge. A fourth definition of "ideology" is operative in critical-rationalism, indicating quasi-theories that are a mixture of prescriptive and descriptive statements but still claiming a scientific status. Marxism, for example, is an ideology in this sense, and ideology-critique should be directed towards purging its theories from normative prescriptions.

Confusions in Dialectical Ideology-Critique

American Dialectical Psychology's ideology-critique appears designed for the first and third definitions. On the one hand, it concerns demonstrating what legitimizing function psychological theories perform in capitalism. On the other hand, ideology-critique is seen as a means of preventing psychology from lagging behind social changes. Buck-Morss, for example, demonstrates that Piaget's theory of formal operations

legitimizes capitalism's fixation upon the formal-abstract features of goods and persons, while Buss shows that this criticism reflects an obsolete and superseded type of society, that of industrial capitalism.

Such problems can be avoided by developing a theory of ideology-critique and devoting more attention to methodological aspects. Recently, Broughton started on this by formulating six guidelines for ideology-critique at the conclusion of his extensive criticism of Piaget (1981e, p.394ff). Firstly, the level of analysis must be clear: the target of ideology-critique should be theories and not the scientists who formulate them. Secondly, it must be clear what the nature and the direction of the influences are: undoubtedly, science is influenced by social power relationships, but it is not completely determined by them. On the other hand, it may be assumed that science also influences society. Ideology-critique is often one-sidedly and simplistically aimed at disclosing theories' social determinants. Thirdly, ideology-critique should as much as possible rest on evidence from a series of disciplines and not operate by means of simple parallels and metaphors. Greater accuracy is a must. Fourthly, ideology-critique should avoid eclecticism in choosing its targets. There are examples of ideology-critique criticizing such entirely different theories as that of Freud and Piaget with the same method and the same evidence. Fifthly, it must be established unequivocally which ideology is being attacked: Piaget cannot be criticized for his instrumentalism and for his affinity to the values of democratic liberalism, both at the same time. Sixthly, the epistemological position of the ideology-critic must be clear: ideology-critique has no credibility if it does not make its own premises explicit. In this connection, relativism should be avoided. Ideology-critique is not effective if in attempting to remain relativistic, it does not recognize any universal criteria for distinguishing between truth and falsehood. In particular, this last aspect points to an epistemology of ideology-critique still absent in Dialectical Psychology. Neither Riegel, Buss nor Broughton have yet developed a well-grounded and non-relativistic ideology theory, oriented upon an unequivocal

concept of ideology. At present, eclectic approaches dominate, attempting to establish the ideological face value of a theory in a relatively unsystematic fashion. This does not, however, alter the fact that a number of original and fruitful analyses (see for example Buss 1979a, p.149ff), have been conducted.

As stated, ideology-critique is part of the meta-theoretical research of Dialectical Psychology. In addition, Dialectical Psychology also implies a theory of human development, based on Piagetian and Soviet Russian approaches, that can be tested empirically. In this connection, Dialectical Psychologists have discussed the merits of the mixed-longitudinal design, which will be dealt with in the next paragraph.

TOWARDS A DIALECTICAL METHODOLOGY

Riegel's call upon developmental psychology to devote more attention to the double dialectic, finally making it possible to actually describe the changing individual in a changing environment, would have had little cogency were it not for the methodological realization of his dialectical theory. Such traditional research designs as the longitudinal or cross-sectional designs are inadequate. The dialectical alternative integrating these two well-used designs at a higher level is the so-called mixed-longitudinal design. Riegel derived this design largely from the pioneering work of Baltes (1968) and Schaie (1965). It dovetails perfectly with Riegel's attempts in particular to draw into the research design the social context in which certain developments occur. We shall now attempt to illustrate the advantages of the mixed-longitudinal design over prevalent designs.

Imagine we want to follow the development of a certain type of behavior (for example language use). Let us look at Table III.4. On this basis, several comparisons are possible. A cross-sectional design is characterized by a comparison of two cells in the same

TABLE III.4

Different Sources of Variance in Developmental Psychological Research

	Age	
	20 years old	70 years old
year of measurement	year of birth	
1920	1900	1850
1970	1950	1900

row. In 1920, then, you might compare twenty year-olds and seventy year-olds, people born in 1900 and 1850 respectively. In 1970, you could do the same with people born in 1950 and 1900 respectively. By keeping the time of measurement constant (namely 1920 or 1970), the comparison provides both age differences and generation differences (also called "cohort differences"). In other words: cross-sectional designs take both age differences and generation differences into account. The two cannot, however, be separated. A time-lag design consists of a comparison, for example, of the language use of twenty year-olds in 1920 and in 1970 or of seventy year-olds in 1920 and 1970, a comparison then of two cells in the same column. This comparison is contaminated by differences in time of measurement and generation. The generation that was 20 year old in 1970 was born in 1950, while the generation of twenty year-olds subjected to the test in 1920 was born in 1900. The same applies for the seventy year-olds. In other words: time-lag designs furnish results in which the variance is determined by differences in the time of measurement and generation. The role that the two types of differences play individually cannot, however, be distinguished.

A longitudinal design boils down to a comparison of the same group at two different points in time (in the diagram, the participants born in 1900; they are subjected to the test in 1920 and in 1970 at twenty and

seventy years old respectively: the diagonal from upper left to lower right). A longitudinal research design thus furnishes results contaminated by both age and time of measurement differences. Thus none of these approaches provides automatically an unadulterated estimate of age, generation or time of measurement differences. A combination of the three designs results in three equations with three unknowns, the effects of age, time of measurement and generation differences; these effects can therefore be determined exactly. We then have a so-called "mixed-longitudinal design". Psychology can then describe individual developments, sociology can sight the cohort differences and the historical sciences can chart the developments in the course of (chronological) time.

A concrete example: the feasible but fictitious results of a cross-sectional intelligence-study could be represented with a curve showing a decrease in intelligence with the increase of age. This curve could easily be interpreted as a continual decline of intellectual functions corresponding with advancing age. Quite apart from the relative value of IQ-tests, it should be noted that the results could very well be an artefact of the selected design. Such a cross-sectional study does not, indeed, take account of generation differences. Tuddenheim, for example, determined that the median of test scores of recruits in the Second World War corresponded with the 84th percentile of the distribution of the same scores drawn from research among recruits in the First World War. These differences can, however, be attributed to historical-social changes, for example in education, prosperity, communication media, etc. (Riegel, 1978, p.4). In cross-sectional research, however, regression resulting from historical changes is, as it were, built into the design. An "a-historical" and "a-social" developmental psychology appears to be fertile soil for such efficient designs, but from a scientific point of view, they are hardly practicable. On the other hand, the mixed- longitudinal design does allow for conclusions concerning, for example, the influence of historical-social and external-physical changes on individual-psychological and internal-biophysical development, and in

so doing is a good basis for the interdisciplinary dialectical research Riegel was such an enthusiastic proponent of. And indeed, this "dialectical" design has found acceptance, both in the Unites States and in Europe (Elder and Rockwell, 1979).

The Mixed-Longitudinal Design in Discussion

It should, however, be noted that a discussion has recently sprung up about the statistical merits of the mixed-longitudinal design (Adam, 1978) among the ranks of Dialectical Psychology, and that disagreement has also arisen concerning its validity (Buss, 1979a, p.127ff). Among other things, Buss wonders whether the Schaie-Baltes model is a good operationalization of the dialectic between individual and social development. The data collected by means of the mixed- longitudinal design are usually interpreted by means of analysis of variance. Among other things, analysis of variance looks at the interactive effects of a number of independent variables on the dependent variable, an aspect of individual development. For example, these independent, causal variables might be age and the time of measurement. According to Buss, however, the interaction-effects uncovered in this fashion are entirely different from what Riegel meant with the individual-society dialectic. This dialectic assumes reciprocal influence, simultaneous functioning as cause and effect, whereas in analysis of variance, individual development can never operate as dependent and independent, causal factor at the same time. Buss implies that he was able to convince Riegel of the validity of this argument, but in this connection, can only name "personal communication" as his source.

In addition, Baltes and Cornelius (1977) have clearly distanced themselves from Riegel in his efforts to find an exclusive dialectical method for his Dialectical Psychology. They support the view of dialectical heuristics that the context of discovery can greatly profit from an application of dialectical principles to

a certain area of research but that in the context of justification, the time-honored and dependable research methods are suitable for testing the dialectically generated hypotheses and theories. From the point of view of an obsolete, logical-empirical concept of theories (see chapter II), they sketch three levels of theory development: (a) general laws and premises, (b) specific laws, deducable from the premises mentioned in (a), and (c) definitions linking theoretical concepts to observational statements. This last level is the domain of current empirical methodology, in principle suitable for any filling-in of level (a). Ultimately, theoretical deductions will have to be confronted with experience according to the traditional methodological rules, whether (a) is determined by a mechanistic, an organismic or a dialectical paradigm: there is no exclusive relationship between (a) and (c) Put more strongly: at the concrete level of confronting theory with experience, even methodological pluralism is more fruitful than fixation upon one type of method, for example the mixed-longitudinal design.

Restrictions to a Pluralistic Methodology

Apart from an obsolete theory-concept, which it would have been preferable to replace with a Lakatosian notion of the construction of a research program (Lakatos, 1980), we doubt whether this extremely eclectic position is tenable. Of course, any critical-psychological school should be conscious of a number of universal criteria a method should meet in order to be called scientific. These criteria might best be summed up with the term "intersubjective replicability". Research methods should lead to results via a route that in principle can also be followed by others. It is then possible to check whether the results obtained were not merely the products of the individual researcher's imagination. But this does not alter the fact that a metatheoretical orientation such as Dialectical Psychology can result in a certain preference for a particular type of method better suited to the research sub-

ject. This subject is determined in part by the meta-theoretical assumptions of the context of discovery. It would be ridiculous to expect dialectical researchers to rate the utility of cross- sectional survey research as high as that of mixed-longitudinal research when they have defined their subject as a changing individual in a changing society. The meta-theoretical orientation places limits upon methodological pluralism because it determines in part the research subject and because methods may be subjected to the criterion of "object-adequacy" (see chapter II). But Baltes and Cornelius are right in seeking the strength of Dialectical Psychology in the heuristic value of dialectics. We also saw in the previous chapter on the Berlin School of Critical Psychology that within the context of discovery, a new approach is most needed in combatting the existing paucity of ideas and in strengthening the position of the theory within psychology (cf. Lerner, Skinner and Sorell, 1980).

ACTION RESEARCH AS A DIALECTICAL PSYCHOLOGICAL METHOD

The mixed-longitudinal design is intended to trace the influence of a changing environment upon the development of individuals. In and of itself, however, this design makes no demands upon the relationship between researchers and the participants in their experiments. But as does the Berlin School of Critical Psychology, Dialectical Psychology has well-defined ideas as to how this relationship should take shape. In many cases, obviously, dialectical psychologists do not regard Wundt's principles of psychological research as useful. According to Wundt, regarded by many as the founder of experimental psychology, a good experiment should enable the researcher: (1) to properly determine when the events being studies have started; (2) to follow the process conscientiously; (3) to replicate the experiment under similar conditions, and (4) to systematically vary the conditions under which the experiment occurs (see the concept of "conditions analysis", chapter II). The behaviorist conditioning experiment is

paradigmatic for such an approach. To increase the replicability of the experiment, the gap between the researcher and the participant is widened as much as possible and participants are robbed as much as possible of their subjectivity. Earlier, we mentioned Ebbinghaus' experiments with nonsense syllables, in which the meaninglessness of the learning test corresponds perfectly with the degradation of the human subject to a memory machine. As does Holzkamp (chapter II), Riegel draws here a parallel to the way an industrial laborer is treated: in a Taylorized production process, assembly-line workers also perform meaningless sections of a task and are also expected to act like robots and not think for themselves (Riegel, 1978, p.36). In chapter II, we already presented and commented upon Holzkamp's criticism of such artificial research conditions.

Dialectical Research as Dialogue

Riegel's criticism is in the same vein, and in particular concerns the absence of ecological validity: according to Riegel, participants will achieve less when they are forced into an unnatural role than might otherwise have been the case under more favorable conditions (1975b). From a dialectical point of view, however, participants must succeed in maintaining their subjectivity. Ideally, the research situation should consist of a dialogue between the researcher and the participant, in which both parties make their contribution (thesis and antithesis) and develop knowledge together (synthesis). The interrogation is a degenerated dialogue providing the participant with no opportunity to contribute fully to the drawing of the conclusion. And yet it is this very interrogation, and not the symmetrical dialogue, that is characteristic of traditional empirical research (Meacham, 1980). Unfortunately, Meacham's call for research in the form of a dialogue does not result in concrete proposals for alternative designs. As a representative of what we have called dialectical heuristics, he seems satisfied with a more important role for the participant in the

stage of formulating hypotheses. But in the work of Riegel, too, we find no rigorous consideration of the methodological consequences of a subject-subject relationship between the researcher and the participant. The examples of good research he gives are by and large hardly at variance with what also occurs in "traditional" psychology. For example, he described a Piagetian conservation experiment in which conservers and non-conservers are given the opportunity of influencing one another. It then appears that rather often, non-conservers acquire the conservation concept, while the conservers certainly do not lose it. Such experiments, though, are examples of classical Piagetian research, in which an attempt is made to investigate the effects of an interindividual "décalage" on cognitive development. In the Cultural-Historical tradition as well (see chapter I), such a research design is used regularly to investigate, under optimal learning conditions, at what point children reach the height of their achievement-potential.

Another of Riegel's examples is a study on mentally retarded children in which the diagnosis of the retardation in development was conducted by the children's mothers instead of by the researchers themselves. The researcher had chosen this design for the study in order to increase the mothers' willingness to co-operate with therapy. The mothers would then see with their own eyes what problems their children had rather than having to trustingly rely upon the information of the researcher. This is unquestionably an example of a more symmetrical relationship between researcher and participant, though it should be mentioned that this did not apply for the children themselves.

The Dialectical Psychologist as Change Agent

More importantly, however, this study shows the scientist not only in the role of diligent "fact collector" but also as an agent of change. Data are collected on the children's development with the intention of

applying these data in a therapy designed to compensate as much as possible for the retardation in their development. This is moving in the direction of action research, where the process of acquiring knowledge serves the purpose of influencing praxis. It is curious that Riegel hardly concerned himself with the question of the application of psychological knowledge. He was, though, extremely interested in an adequate description and explanation of the changes in the behavior of individuals in a changing environment. But in general, intentionally influencing behavior on the basis, among other things, of the knowledge acquired, is a theme he or Dialectical Psychology in general, in fact rarely touched upon. Yet it would only seem possible to guarantee an equivalent status for participants in research if they can learn as much from the researcher as the researcher from them. And this is often only the case when, on the basis of knowledge and insight acquired about participants, the researcher succeeds in providing them with means of optimalizing their behavior. To our mind, these are the radical consequences of the dialectical requirement of regarding the research process as a dialogue between subjects.

Iconic and Enactive Criteria for Truth

Now Riegel did not disregard this problem altogether either. Based on Bruner (1964), he introduced into Dialectical Psychology the distinction between iconic and enactive representations. It is presumed that Western thought's criteria for evaluating knowledge are characteristically sensorial-perceptual in nature, that is, iconic. The "truth" of a scientific *proposition is determined according to the extent* to which this proposition can be established with sensorial-perceptual data. Truth, then, is the agreement between theory and observed facts. In chapter II, we showed that such a notion has a number of logical shortcomings.

An alternative is representing situations by suitable motor schemata. For example, it would be difficult to give an adequate description of the floor of our living room and yet we can find our way more or less with our eyes closed. This is an example of an enactive representation. The question in an enactive representation is whether knowledge acquired can help us in our interaction with the world. According to Riegel, the error of Western thought is that it has neglected the practical and social aspects of its contributions. To his mind, changing the world should even have priority above pure description and interpretation, this following Marx' famous dictum that philosophers have spent enough time interpreting the world; it is now time to change it. According to Riegel, social relevance as a criterion for the validity of a scientific theory should not be overshadowed by the requirement of methodological rigor (1978, p.189).

In stating as much, he has, it is true, posed the question of the practical applicability of Dialectical Psychology at the epistemological level, but provided no answer at all at the methodological level. And that applies as well for those who have dealt with this material after him. Gergen (1980), for example, emphasizes the necessity of a "reciprocal pursuit of knowledge" (p.261). From this he draws the conclusion that the researcher will have to act as a change agent, but he does not develop that role any further.

It should be clear that it is not our intention to call for action research as the dialectical research method *par excellence*. In the previous chapter, we made a large number of critical notes with respect to Critical Psychology's notion of action research. In addition, in this chapter we have hinted at our sympathy for the school of dialectical heuristics, i.e. for the notion that dialectics themselves only have a heuristic function and are not a research method or an unequivocal reference to a particular method. A dialectical bias does, however, limit somewhat the number of methods suitable for dialectical problems. Against this background, it would seem to us of importance that Dialectical Psychology should not bog down in the discus-

sion on the advantages and disadvantages of experimental and longitudinal research, but should also be inspired by the Western European polemics on action research in order to do more justice to the ideal of enactive knowledge. In this regard, Dialectical Psychology can learn a great deal from the experience of such German action-researchers as Klafki, Moser and Schneider, attempting to avoid their errors. One of these errors, for example, was the initial belief that action research should be subject to less stringent methodological criteria than "traditional" empirical research, thereby opening the door to atheoretical activism. We are convinced, however, that in the long run, a Dialectical Psychology focusing upon the question of a changing and changeable human being cannot escape the question of the intentional influences upon the changes being investigated. In view of Dialectical Psychology's aspiring to a subject-subject relationship between researcher and participant, it would seem logical to expand its arsenal of methods with a form of action research.

CONCLUSIONS

In this closing paragraph, we should like to review some of the main themes of Dialectical Psychology and summarize the critical notes. We shall also point out the merits present in the work of Riegel and other dialectical psychologists. Despite the multiplicity of the concept "dialectics", Dialectical Psychology may be regarded as a school focusing on change rather than stability and focusing attention upon the historical-social context of psychological (theory) development. We have distinguished two dialectical schools, "dialectical heuristics" and "dialectical methodology". The former regards dialectics as a heuristic for developing new theories and hypotheses in the context of discovery. The latter school also believes dialectics to be a "valid organon", a means of acquiring knowledge. And because it is more sensitive to subtle contradictions and conflicts, this means is presumed to be superior to methods modelled on formal-logical patterns.

We have focused our exposition around the person of Klaus F. Riegel, who with Rychlak has the honor of going down in history as the founding father of Dialectical Psychology in the United States. First, Riegel concerned himself with the historical roots of his discipline and traced the development of two competing schools in psychology, the mercantile and the capitalist traditions. He attempted to make clear that the origins of these traditions were in part determined by the specific social relationships present in Europe and in the Anglo-Saxon countries in the 19th century. We concluded, however, that Riegel's analysis was rather sketchy and based in particular on metaphors. Nevertheless, his historical research has been a source of inspiration for a new discipline within Dialectical Psychology, the "sociology of psychological knowledge", pioneered by Buss. Dialecticians like Buss (1979a) and Danziger (1979) were able to provide a number of Riegel's analyses with a more detailed foundation. The dialectical assumption that the historical-social context influences the rise and fall of theories remained uncontested and was provided with a more solid structure.

These historical analyses were then Riegel's guide in constructing a dialectical developmental psychology. Via Piaget and the Soviet psychologist Rubinštejn, a psychology could be developed with the interaction between an active subject and an active, ever-changing environment as its central principle. In Piaget's theory, great emphasis is placed upon the importance of unbalancing factors for a proper understanding of the dynamics of human development. Riegel goes a step farther by almost completely denying the existence of states of equilibrium and reducing all developments to conflicts. We have shielded Piaget's theory from the criticism of such dialectical psychologists as Riegel and Buck-Morss, because in their criticism we sense the misconception that Piaget's theory is a maturation theory.

The idea that dialectical logic is somehow superior to formal logic also seemed to play a role in the dialectical criticism of Piaget. A developmental theory

regarding the mastery of formal operations as a (tentative) terminus of ontogenesis therefore remained unsatisfactory. For that reason, Riegel found it necessary to add a fifth dialectical stage to the four stages of Piaget. And for that reason, Buck-Morss regarded formal operations as a typical product of a capitalist society. We have attempted to defend the notion that dialectics most assuredly cannot be regarded as a competitor of or an alternative to formal logic, as they are situated at different levels. In addition, we have pointed to the interpretation of the modern Frankfurt School with regard to Piaget's work. Habermas in particular has demonstrated that formal-operational thought, though a necessary condition for communicative behavior, is certainly not sufficient on its own. He complements this cognitive ability with post-conventional moral judgment and a balanced ego-identity, and believes that this combination of abilities and characteristics can lead to an optimal interaction between human beings and their environment. American Dialectical Psychology and the German Frankfurt School agree with one another completely concerning the great importance of Piaget's theory for a critical psychology, but differ in the way they believe that theory should be amended. In that respect, Habermas' amendment would appear to have the advantage of a more solid empirical foundation.

In its resistance to formal logic as a terminal stage of human thought, dialectical psychology shows a striking resemblance to the work of V.V. Davydov (1972; see chapter I). In other respects as well there is a great affinity between Dialectical Psychology and the Soviet Cultural-Historical School of Vygotskij, and it may be assumed that the only reason Riegel did not refer to Vygotskij's work was that there were no translations of his methodological and epistemological work available. Riegel - and with him Dialectical Psychology - did derive a great deal from Rubinštejn's theory, available through a translation by Payne (see the Intermezzo). From this theory he derived three fundamental aspects of development, i.e. the internal biophysical substratum, the individual-psychological aspect and the socio-cultural aspect. To these three aspects

he himself added a fourth, the external physical aspect, and argued that the dynamics of human development should, in particular, be sought in the asynchronisms between these different aspects. Unlike Rubinštejn, who emphasized the material substratum of every development, Riegel did not believe it opportune to attribute a special role to any of the four aspects. From this, it could be concluded that Riegel is placing Dialectical Psychology outside the dialectical-materialistic tradition. But in view of the state of knowledge with respect to the determinants of ontogenesis, the question remains as to what extent opting for the material substratum can be justified on other than *a priori* grounds.

In addition to a view of human development contrasting with that of traditional theories, Dialectical Psychology has also concerned itself with the problem of the methodological consequences of dialectical principles. As we saw, there are rather great internal differences of opinion on this matter within Dialectical Psychology. For many years, dialecticians unanimously opted for a mixed-longitudinal design in their research into the changing individual in a changing society. In the meantime, however, there has been criticism from several different quarters. Some believe it absurd to seek an exclusively dialectical design (e.g. Baltes and Cornelius), others have raised critical statistical and theoretical questions (Buss). This discussion has yet to be concluded. More and more, the question also arises within Dialectical Psychology whether research results should not also be evaluated with respect to their practical utility. Riegel called this the enactive truth-criterion, but he himself did not descend from the abstract epistemological level to the area of concrete research methods. In their reflections on research as a dialogue, and the researcher as an agent of change, Meacham and Gergen got a bit closer. But a discussion on the merits of action research as a practice-oriented research method *par excellence* has yet to be initiated. As we saw in chapter II, this discussion has developed in the Berlin School, and we suggested that under certain conditions, the idea of

action research could very likely be a good complement to Dialectical Psychology's arsenal of research methods.

Up to now, Dialectical Psychology has not proved really capable of converting its fundamental philosophical and methodological assumptions into cogent research practice. Riegel can boast of a long career as an empirical researcher, but none of his studies appears very well related to his dialectical principles. But Dialectical Psychology has been amply succesful in raising a number of very critical questions with respect to the "fact-finding" tradition. It has also been partly responsible for theory gaining more ground within scientific work and for popularizing Soviet psychology in the United States. In any case, Dialectical Psychology has considerably enlivened the discussion on the context of discovery and stimulated the development of new hypotheses and theories.

Afterword

CRITICAL PSYCHOLOGY AS TAUTOLOGY

At first glance, the term "critical psychology" would seem to be a tautology. After all, has not science always been "critical" in its nature, set against dominant modes of thought based on myths and preconceptions? A good example of classical critical science was the unmasking of the anthropocentric notion that the sun revolved around the earth, presumed at the time to be the center of the universe. With scientific methods, Copernicus and Galileo attempted to prove the (for that time) heretical theory that the earth revolved around the sun and its own axis. It has been said that Galileo was condemned by the inquisition for his critical scientific work because he refused to confess that his theory represented nothing more than an efficient model for calculating the motion of the planets. In this case, science is critical because it unmasked a deep-seated myth. A second example is Darwin's theory of evolution. In it, he demonstrated that the principles of the "struggle for life" and the "survival of the fittest", variation and selection of the most suitable forms, were the foundation of the evolution of flora and fauna on earth. In this theory, the human being is presumed to be the final product of a long evolutionary process. In view of the dominance of the biblical interpretation of creation at that time, evolution was

considered a truly critical theory. An example of classical critical social science is the work of Marx. He demonstrated that contrary to being lord and master of their own society, people were actually dominated by the forces of institutions, which are partly determined by material economic conditions. Marx thereby unmasked the myth of individual freedom and equality, one which was maintained as much as possible in the area of politics and law, but could hardly be maintained in economic matters. Finally, an example of classical critical psychology is provided by Freud, who demonstrated that as creatures with physical and psychological drives, people were not only not masters of their social environment, but not of themselves either. According to Marx, people were the willing victims of their social conditions; according to Freud, they did not even have control over their own psychological nature.

Science, then, appears to have completely exploded an age-old, mythical world view. The earth is no longer the center of the universe, humans are not completely different from animals; they can hardly be regarded as masters of their social environment and certainly not of their own nature. Regarded as such, critical science is a tautology: in a world dominated by myths and prejudices, the unbiased search for truth is in and of itself critical in its effects. The position of science in the twentieth century, however, has changed radically. Science's self-evident function of exploding myths has dwindled. More and more, science is becoming the tool of existing society and, as Habermas (1970) argued, one misused to stifle criticism of existing power relationships. He refers to science as an ideology, an instrument for masking social conflicts and injustices. In particular, he points out that science is regularly misused to depoliticize politically explosive questions. An example of this is the discussion on nuclear energy, which is depoliticized by asserting that because lay people do not understand the technical details, they can and should not participate in the discussion. In discussing the usefulness and danger of nuclear energy, the lay person is also put off with the almost unverifiable promise that science will succeed

in finding a technical solution to the problem of nuclear wastes. A political question is defused by transforming it into a technical scientific problem. Science, then, is no longer automatically critical, but allows itself to be misused for the purpose of depoliticizing public discussion. And after having laboriously liberated themselves from a dependency upon myth and tradition, people are once again in danger of becoming dependent, but this time on science.

The Frankfurt School calls for critical reflection on the goals and functions of research activity becoming an integral part of scientific pursuit. Science should establish its own priorities and not play lackey to every group that happens to occupy a dominant position in society. As expressed in Kant's age-old ideal of enlightenment, science should direct itself to emancipating people from the superfluous powerlessness and dependence they have forced upon themselves. The unbiased but uncritical search for truth should be replaced by an interest in the pursuit of emancipatory knowledge, its central objective being emancipation from the whims of nature and excessive social and psychological coercion. This ideal of emancipation cannot be achieved by tossing "traditional" science overboard and approaching the accumulation of knowledge from an entirely alternative angle. On the contrary, the search for laws of human behavior must continue unabated, because only by knowing these laws can we liberate ourselves from them. Habermas' dictum, which states that while behavioral laws cannot be nullified, they can be made inoperative, still holds. But a critical science must supplement this kind of research with ideology-critical research and should continually examine behavior in its historical-social context.

TRADITIONAL AND CRITICAL PSYCHOLOGY

The main currents of critical psychology discussed in this book should, in fact, be seen in this light. Critical psychology is not so much an alternative for traditional schools as it is a call to take the study

of the changing and changeable person in a changing society seriously. The main currents of critical psychology interpret themselves either as a synthesis of competing traditional schools (the Cultural-Historical School and Dialectical Psychology) or as a supplement to current psychology (the Berlin School). None of these schools have the pretention of casting the achievements of current psychology aside and starting again with a clean slate. None of the critical schools question, for example, operative research methods. While they believe that some methods and designs are better *suited to critical psychological hypotheses than others*, there is absolutely no question of radically rejecting the traditional methodology. This would also be illogical, in view of the fact that such methods as the experiment, the survey, systematic observation etc., are not the exclusive property of one psychological paradigm. These methods of collecting data, for example, *were not invented by "positivism" in psychology* itself. On the contrary, it is more an *a posteriori* theoretical legitimization of the use of these strategies in science. Ultimately, the methods themselves were adopted from pre-scientific practice.

The critical schools do, however, radically dissociate themselves from the current "fact-finding" tradition and the cult of facts within psychology. Blinded by the technical possibilities, many a psychologist has become obsessed with collecting data without knowing exactly what theoretical premise and what social objectives they were meant to serve. As we mentioned earlier, this has resulted in a mound of (mini)facts which provide us with no further understanding of human development whatsoever. The methods of the natural sciences, at least the caricature of them that penetrated the social sciences, were imitated mindlessly. Because this created the illusion of exactitude, the making of a-theoretical measurements and empirical generalizations became highly respected activities. And these activities continued, even long after the students of the history of science (Kuhn, Lakatos) had demonstrated that the natural sciences followed an entirely different course. This research demonstrated that the basis of scientific progress lay

not in the accumulation of as many and as "hard" facts as possible, but in the competition between theories and research programs. In fact, "hard" facts proved to have a lower status within the natural sciences than within a psychology calling itself "positivistic". It is to the credit of the different critical schools that they have pointed out this weakness. It is these very schools that have continually urged for a radical upgrading of theory in psychological research. In doing so, paradoxically enough, these critical schools are following the actual developments in the natural sciences better than the "traditional" schools, which as yet have remained unsuccesful in shaking off their caricature of such developments.

Practically speaking, the upgrading of theory means that critical psychology goes to great lengths to arrive at a satisfactory definition of the research subject: the changing and changeable human being in a changing environment. Following the ideas of the Cultural-Historical School, the Berlin School developed hypotheses relating not to the arbitrary notion of an individual researcher, but directly to phylogenesis and social history. Despite the universally held notion that the context of discovery precedes the use of any scientific approach, critical psychology attempts, in fact, to make this context rational. Critical psychology can therefore be looked upon as a persistent attempt to have the scientific approach also apply to the context of discovery and, in so doing, to demand greater attention for theory development as the first step in the research process. In that way, unconscious and ill-considered influences of social interest groups can be avoided. It should no longer be a matter of course that researchers allow themselves to be used by the industrial and political powers that be; they should be able to direct their efforts at the very emancipation of underprivileged groups. An attempt should be made to expand the application of the knowledge obtained in this process by an orientation towards such new methods as action research (see chapters II and III) and by developing eduational research (see chapter I).

VIVIFICATION

But critical psychology does not only lead to a different view of the context of discovery, but also to a different function for empirical research in the context of justification. As we mentioned earlier, critical psychology in all its variants continues to make use of the existing arsenal of research methods. The results of this research, however, do not have the functions of testing a theory by verification and falsification. Unlike traditional psychology, critical psychology is extremely sceptical about the determinative value of the results of empirical research. In the Berlin Critical Psychology, this scepticism is expressed in the principle of exhaustion (see chapter II) and as often as not, American Dialectical Psychology appears to ascribe no more than an illustrative value to empirical research. Gergen called this the "vivification" of a theory through research, and we believe that in doing so, he has drawn the proper conclusions from the recent methodological discussions on the relationship between theory and facts (see chapter II). To avoid any misunderstandings, we emphasize here that critical psychology does not join the advocates of methodological dadaism or anarchy. Feyerabend's motto, "anything goes", finds no support in critical psychology. The latter's desire to, indeed, maintain a number of universal criteria for the value of knowledge can be described with the term "intersubjective replicability". The line between journalism and political propaganda on the one hand, and scientific pursuit on the other is drawn by the (im)possibility for an "outsider" to trace and principally replicate the process resulting in a particular conclusion or theory. Only intersubjective replicability can guarantee a meaningful critical discussion of research results. Therefore, qualifying the determinative power of research results does not at all result in anti-empiricism; in all the critical schools, empirical research plays an indispensable role in reviving and solidifying theoretical discourse.

"Vivification" of a theory through empirical research points to the fact that during the research

process, theories gain in complexity and detail. Theorists are forced to so articulate their theoretical points of departure that it should be possible to satisfactorily operationalize the concepts they utilize. At the same time, they are forced to reconsider thoroughly the consistency and completeness of their theoretical model. Indeed, empirical research nearly always demands accounting for possible intervening variables, and a prior and thorough consideration of the relative value of all these variables is imperative (see chapter II). In addition, the results of research may compel the researcher to adjust and supplement the original theoretical model; it is possible that the connections between phenomena are stronger or weaker than was originally indicated. If in such a case the arrangement of the theoretical arguments is not logically compelling - and obviously this is usually not the case - there is no reason not to further delineate the theoretical model on the basis of empirical experiences. The results of empirical research may also lead to the qualification of theoretical conclusions, for example due to it becoming clear that a theory only applies to a limited field or a limited population. In this way, instead of being simply an instrument testing the (in)validity of a conclusion or proposition, empirical research becomes an extremely important means of enriching theory development. In this view, the question of validity will be the subject of a theoretical discussion between scientists aspiring to a (tentative) consensus concerning the "validity" of a theoretical interpretation (supported by empirical arguments) of a particular state of reality (cf. Habermas, 1973b).

CRITICISM OF CRITICAL PSYCHOLOGY

To our mind, critical psychology has numerous merits and is a necessary supplement to traditional schools, in particular because it is modelled along more modern methodological lines than "positivistic" psychology. And yet in the preceding chapters, we have been very critical of a variety of aspects of the

several different variants of critical psychology, convinced as we are that these schools, too, can only progress if they are criticized constructively. Among other things, critical psychology is affected by the following problems and shortcomings. Firstly, it severely lacks conveyable and intersubjectively replicable heuristic and ideology-critical methods. We are forced to conclude that while the Berlin School did offer a pretentious alternative for modern and computerized heuristics, they proved unable to provide assurances for the conveyability of the functional-historical method. The same applies in fact to Dialectical Psychology's ideology-critique. In this school, an equivocal ideology-concept corresponds to a method confusing divergent levels of analysis with divergent analytical perspectives. As a result, dialectical ideology-critique makes an arbitrary and irreplicable impression. This does not alter the fact that like the functional-historical approach, this method has produced a number of original suggestions. But the question is whether their application should of necessity be limited to a scientific elite possessing the "intuition" and "sensitivity" to make responsible use of the method. This cannot possibly be the standard of a psychology whose intention it was to promote democracy through science.

Secondly, critical psychology must still bridge the large gap between its pretentions and its research practices. To our mind, despite all the criticism of traditional empirical research, there remains a glaring absence of critical-psychological research projects capable of illustrating the range of its theoretical points of departure. With the exception of the elegant but extremely simple empirical studies contained within the Cultural-Historical tradition, critical psychology has actually had hardly any experience with empirical research. In addition, the examples cited as good alternatives by Dialectical Psychology are not particularly convincing. In general, even at first glance, they do not meet a number of essential criteria critical psychology itself has established. There is an urgent need for critical research which puts the idea of vivification into practice. This, in turn, is also related to the conveyability of critical psychology: it

must be possible to introduce prospective researchers to this material by means of "exemplars" (Kuhn) of succesful research projects. If these are not available, such training will continue to take place at an abstract level. A third problem the critical schools within psychology, and in particular the Berlin School and Dialectical Psychology, have to contend with is a lack of social relevance, despite their pretenses to practicability. As yet, these schools have been less succesful than their Soviet Russian equivalent, the Cultural-Historical School, in serving the interests of education. In the Berlin School and in Dialectical Psychology, there is the danger that their re-emphasis of theory will run off in a counterproductive direction. Both Holzkamp and Riegel seem to be mesmerized by the theoretical problems connected with studying human behavior in an historical-social context, with the commensurate danger of losing sight of the very objective of critical psychology, i.e. serving the goal of human emancipation. These schools are, of course, of a relatively recent vintage, and the numbers of researchers contributing to them are limited. Still, it would be appropriate that more attention should be paid to the problem of bridging the gap between theory and practice. A better consideration of the method of action research might very well provide an impetus to do so.

But despite all its shortcomings and drawbacks, we still regard critical psychology as an appealing and unorthodox attempt to emphasize the more human, and at the same time more social side of the study of human development. Within the philosophy of the natural sciences, the position of "positivism" has long been rendered out of date and the time has come for the behavioral sciences to change their course as well. As a source of inspiration, critical psychology can play an important role in this shift away from "positivism". In this regard, its attempts at providing a more rational basis for the context of discovery, its re-emphasis of theory and its vivificational view of the function of empirical research are of particular interest. For these very reasons, the main currents of critical psychology, more than ever before, merit attention and appreciation within mainstream psychology.

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ABOUT THE AUTHORS

Marinus H. van IJzendoorn (1952) received his Ph.D. in education from the Free University of Berlin and is currently professor of theory and history of education at the University of Leiden, The Netherlands. His interests lie in the field of the philosophy and methodology of the behavioral sciences. His main area of research is on child-caregiver interactions. He is (co)author of *Moralität und politisches Bewusstsein* (Beltz, 1980), *Kritische psychologie. Drie stromingen* (Ambo, 1981), and *Opvoeden in geborgenheid. Een kritische analyse van Bowlby's attachment theorie* (Van Loghum Slaterus, 1982).

René van der Veer (1952) graduated in psychology at the Free University of Amsterdam and is currently associate professor at the University of Leiden. His fields of interest are Soviet educational and psychological theory and philosophy of science. He is co-author of *Kritische psychologie. Drie stromingen* (Ambo, 1981) and translator and editor of Lurija's *Grondslagen van de neuropsychologie* (Van Loghum Slaterus, 1982).