FIFTY YEARS OF THE RESEARCH AND THEORY OF R.S. LAZARUS

An Analysis of Historical and Perennial Issues

RICHARD S. LAZARUS

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I dedicate this book to
my lovely wife, Bernice,
whose patience and loving support
made it possible to complete this book,
and many others before it.

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PREFACE

At my stage of life, about 75 years old when the completed book makes its debut, after roughly 50 years of research and theory building, it is wonderful—an unusual privilege—to have an opportunity to publish this selective anthology of my work. I have used that work as a basis for a historical commentary on the work's origins, its ideas, the provocative things that happened when it appeared, and its fate as psychology advanced toward it's present state. One theme of those comments is that, despite changing fads and fashions, truly important issues never die, but emerge again and again later in different form, with a new perspective, and with new methods of tackling them.

Inasmuch as the rationale for this book is described in the Prologue, in this preface I need only express appreciation to various people for their encouragement and help as I pursued this venture, which I think is rather unusual. I am grateful to Lawrence Erlbaum Associates—especially Larry Erlbaum, and Vice President and Editor, Robert Kidd, who monitored the publishing—for taking the publishing risks and producing a high quality product.

I thank my good friend, Paul Ekman, for suggesting the idea to me in the first place, though it has been transformed considerably from our original conversations in December 1995. He and Joe Campos, another friend and colleague, read parts of it at an early stage and gave me their informed and honest opinions.

A longtime friend and valuable research colleague at Berkeley many years ago, Jim Averill, was particularly generous in providing two reviews; first of the plan of the book for interested publishers and second, of the near-final text, which facilitated some useful changes that greatly improved the final revision. As always, I must take ultimate responsibility for limitations and defects in the book.

I have tried to write the text commentary as clearly as possible so that any reasonably intelligent and informed person can understand the complex issues that inhere in social science, and in the research and theory that is reprinted. I hope graduate students in several disciplines, undergraduates who hope to do graduate work, professional psychologists in a variety of types of practice, and academics in the social sciences will find the book interesting as a history lesson, valuable to read, and clear.

PROLOGUE

When I joined the American Psychological Association in 1945, there were only a few thousand members. Most influential psychologists were well known and their work was highly visible. They published in standard APA journals and books, which were read, or at least skimmed, by all. What they wrote had a sequential and programmatic quality, each piece extending beyond the substance of the last. There was very little repetition.

Today, conditions are entirely different. Journals have proliferated, as have edited books filled with chapters written by diverse celebrities, and there is too much for any one person to skim, much less read. What is published is read by only a small proportion of psychologists. Publication sources are so spread out that research scholars repeat what they have said many times in separate articles and book chapters in order to reach diverse portions of the psychological public, who seldom read beyond their favorite topics and sources. And, regretfully, researchers have ceased to go back more than five or so years to examine what has previously been written on their subject.

This book of selected writings is an effort to bring to the attention of interested colleagues and students articles I have written that may or may not have been widely noticed. Leaving aside any self-serving motives, the most important reason for doing this is that the articles have both historical significance and contemporary relevance for psychology. Some have been shortened to avoid excessive duplication.

These articles cover 50 years in which enormous change has taken place in our field in the fads and fashions of research and theory and the ways we think about issues. They remain relevant because the psychological issues addressed have not disappeared—important ones rarely do—but have been transformed and handled differently in modern times.

Issues to be Covered

As I was pulling this set of writings together, I began to realize that several divisive epistemological and metatheoretical issues are present in most of my papers, and they continue to be major sources of controversy as we move into the next century. These issues contribute to the fractionation of our discipline. Because they remain unresolved, and divide psychologists into dichotomous schools of thought about the nature of our discipline, articulating them clearly in this book could provide a useful perspective about the past, present, and perhaps even the future of our contentious discipline. The ways I have responded to the issues also provide some of the glue that makes my lifetime of research come together.

I discuss three issues and take positions on them, thereby supplementing the original writings in this anthology with a set of integrating, historically focused comments. I hope the reader sees this as having captured a bit of the recent history of important segments of psychology, or, I should say, one version of this history.

Before we begin, I digress briefly to point out that I think of myself as a philosophical primitive; I would like to be a Grandma Moses, so to speak, not of painting, but of the philosophy lying behind much of psychological research. I say *primitive* because I have almost no formal education in the philosophy of science, and it might seem arrogant of me to pontificate about such matters. I have, however, the temerity to suggest that in some respects some of this primitivity could be an asset.

For example, the prescriptions and proscriptions philosophers have made about the science of psychology have changed radically during my professional life. Therefore, it might be wise not to accept too readily their authority (cf. Kaplan, 1964). Besides, I have extensive scholarly and research experience that could help me think through a number of issues that have long divided psychologists.

For example, I have emphasized a transactional view of the science of psychology, which centers on what I have been calling *relational meaning*. When I arrived at this position, I had not yet read John Dewey, whom I cite later in this connection, and was genuinely surprised to discover that what he said about such matters was in accord with how I saw things, though he said it much earlier and in much greater depth.

Anyway, the first of three epistemological/metatheoretical issues in my commentaries about the articles reprinted in this book has to do with the way one defines the task of science. To frame the issue, a contrast must be drawn between *reductive analysis* and *synthesis* (Dewey and Bentley, 1949).

Analysis Versus Synthesis

Science defines itself as a search for causal variables in an effort to explain the phenomena of interest. The objective is to create laws or principles that can be evaluated by empirical research. The method is a form of reductive analysis in which sources of effect variance are identified and tested while the basic causal building blocks, or "atoms" of psychology are sought. The essence of traditional science is to value simplification, a search for universal mechanisms, and an effort to break complex phenomena into smaller causal elements or units.

Ultimately, if one proceeds top-down through all the scientific disciplines—that is, from the macro to micro levels—reduction finally arrives at the biogenetic level of DNA molecules, where we can still link up with the macro field of psychology, and perhaps beyond to atoms and subatomic particles, where the link to psychology becomes obscure. Such a wide range of levels of scientific analysis creates a major, as yet unrealized, challenge to generate a unified field theory for all science, if one is disposed to seek it.

A common correlate of this approach to knowledge is the effort to reduce all psychology to brain mechanisms, thereby overemphasizing one of Aristotle's four types of causation—namely, the material (as Illustrated by the views of Panksepp, 1993, to which I responded in Lazarus, 1993a). I don't believe that psychological processes can be explained by reference to the brain, which is not to say that relating behavior and physiology is not valuable in its own right.

There is, of course, no doubt that mind and behavior depend on the brain. That is not the issue. I think it is a delusion to believe we need to understand the brain to have a viable science of psychology. Actually, the truth is, I believe, the other way around. Because neurophysiology has to do with the functions of the brain rather than its anatomy, sophistication in that discipline depends on a thorough knowledge of psychology, that is, mind and behavior.

To return to analysis and synthesis, many psychologists, including me, believe that simpler is often simplistic, and that in causal analysis, the component parts, especially if treated as separate and independent, cannot provide a full account of the psychological phenomena in which we are interested. A further step of synthesis, which consists of putting the component parts back together, is essential if we are to attain adequate understanding. Unfortunately, however, this step, which is largely metatheoretical and theoretical, is rarely taken. For those emphasizing this position, holism, systems theory, an appreciation of process (in addition to structure), and contextualism (or an emphasis on situated action) are fundamental to a proper view of psychological science.

Radical Behaviorism Versus Cognitive Mediation

The second issue, which might be considered metatheoretical or ontological as well as epistemological, centers on contrasts between radical behaviorism, which is a stimulus–response psychology that emphasizes classical and operant conditioning, and cognitive mediation, which is a form of phenomenology or subjectivism wherein evaluative thought is said to stand between environmental stimuli and a person's response. The dominant version of cognitive mediation is value-expectancy theory. This theory combines evaluative thought and individual values and goal hierarchies, which are said to shape a person's emotions and actions, and is by far the most influential outlook today in much of social science.

From the 1920s to the 1960s, academic psychology in the United States did not speak of mind, but defined its subject matter as the science of behavior, which was the position adopted in almost all of our textbooks. Leaving aside very early writers, such as Stern (1930) who in 1900 created an individual psychology, strong dissident voices began to emerge in the 1930s and 1940s. The trenchant writings of a distinguished collection of mostly personality and social psychologists provided an advance guard for a major ideological movement away from the doctrine of behaviorism. These writers shared a largely subjective view of human behavior and looked to personal goals and belief systems as sources of human individuality.

My list, which is bound to be incomplete, of these maverick psychologists who could be thought of as father figures of cognitive mediation, includes, Asch (1952), Harlow (1953), Heider (1958), Kelly (1955), McClelland (1951), Murphy (1947), Rotter (1954), and White (1959), to which should be added some grandfather figures such as Allport (1937), Lewin (1935), Murray (1938), and Tolman (1932). Notice that most of the older generation of grandfathers wrote in the 1930s and their offspring did their most important work in the 1950s.

Before leaving the mavericks, I should mention Joseph Rychlak, who for decades has also been critical of traditional psychological epistemologies and has offered his own deviant brand, centering on a frank form of teleology (see, for example, Rychlak, 1994). I was recently reminded of his approach by a useful article in the History of Psychology Newsletter (Rychlak, 1996). There he points out that psychology's identification with

Newtonian science has led our discipline to reject all teleological approaches, which emphasize non-mechanistic purposes, just as my view of the mind and behavior is inherently individual and subjective.

Much of what Rychlak says is compatible with my view, especially when he decries the stranglehold that has been exerted over alternative ways of thinking. We both believe there should be room in psychology for both teleologists and mechanists, and those who think in terms of cognitive mediation.

Lest we think that intellectual deviancy is strictly North American, the list should also include influential writers from a number of European maverick traditions, such as the gestalt psychology movement, existentialism and phenomenology, ethology, social theories critical of existing socioeconomic and political systems, and an outlook referred to as *verstehende* ("understanding psychology"). No such list would be satisfactory without including the large number of European—and later American—psychoanalytic writers who followed and deviated from the views of Sigmund Freud, especially the ego psychologists who gave substantial attention to cognitive mediation and the regulation of emotion and action.

I assume that the intellectual transition begun by these creative thinkers gave birth to the New Look movement, which flowered in the late 1940s and 1950s and was highly critical of the traditional approach to perception. The research that emerged from this movement showed that, to a considerable extent, we perceive what we want and don't want, rather than merely what is present in the stimulus display. My own research in the 1950s, and much of it thereafter, was heavily influenced by this movement, as well as by the earlier mavericks. I say no more about it here, because I discuss it in considerable detail in Part II.

Bolles (1974) explained what happened in the transition from behaviorism to an earlier cognitivism with great clarity. He wrote:

> Originally, before psychology became an autonomous discipline [when it split off from philosophy departments in the university], cognitive views of man prevailed. The early philosophers as well as the man of letters and the thoughtful layman all stressed man's rationality and explained his behavior in terms of ideas, perceptions and other intellectual activities. Then psychologists suffered that curious passion to be scientific. Thinking was merely a physical process going on in the brain; perception was merely the result of certain neural inputs; man was reduced to a mass of S–R connections; and behavior was explained by a vast matrix containing nothing but S–R units. This was an appealingly simple system but it was soon found to be inadequate even for the explanation of animal behavior. (p. 14)

With the advent of behaviorism, the basic building block of behavior was said to be the reflex arc (cf. Dewey, 1896). A focus on learning dominated psychology when I went to graduate school, and this continued until the advent of cognitive psychology. Stimulus and response were regarded as the separate building blocks of behavior, and learning was the means by which the two concepts became connected, making it possible for animals to adapt to the circumstances of their lives. The same outlook was common in those years at the University of Pittsburgh where I had just received my doctorate.

In 1948, when I was just starting out in the Psychology Department of Johns Hopkins University, much was being made by physiological and learning psychologists of the 19th century neurophysiological discovery by Bell and Magendie that afferent (sensory) and efferent (motor) nerves had separate neural pathways. In retrospect, what excited these psychology professors was that the Bell and Magendie law, as it was called, seemed to provide the neurophysiological basis for the separateness and independence of stimulus and response.

The idea that learned connections between inputs and outputs made successful adaptation possible led radical behaviorists to view *mind* as an unnecessary concept. Adaptation rested on the bedrock of acquired stimulus and response connections or habits. This was, of course, a grand illusion, especially for those who saw human behavior as highly complex, variable, influenced by the context in which it occurred, and cognitively mediated.

Interaction Versus Transaction

The third issue concerns the contrast between those who understand behavior in terms of the interaction between a person and the environment and those who emphasize transaction. In *interaction*—which is also a statistical concept in which two variables influence a third—A influences B, and B influences A. In a behavioristic, stimulus-response psychology, however, the person in this interchange is a passive creature, reacting to an environment that stimulates him or her, and that person's influence on the environment is ignored or underemphasized.

I do not believe anyone today would argue against the overarching interactional principle that the environment influences the person and, vice versa, the person influences the environment, which is the most modern version of interaction. Interaction is a fundamental way of thinking in modern psychology and cannot be gainsaid.

However, as I see it, this is not sufficient as a doctrine of mind. What is called for is *transaction*, which is much more than interaction. *Transaction*, which is Dewey's term, brings the causal variables together at a higher level of abstraction; namely, the *relational meaning* constructed by the individual who is confronted by (or selects) a particular environment with its own special demands, constraints, and opportunities (Lazarus, 1991).

As I said in more than one of the articles in this anthology, threat is an example of transaction and relational meaning. It is caused by neither the environment alone nor by the person alone. Threat requires that both these sets of variables, person and environment, be juxtaposed, and the meaning of this juxtaposition evaluated by that person as being under threat.

Consider, for example, two people who have an intimate relationship, whose emotions are influenced mutually, but who react distinctively to the same transaction. Person A is made angry by something person B said or did, and is mostly concerned with repairing a personal wound. The threat here has become a harm or loss, with damage having been done to A's sense of social- and/or self- esteem. A attacks B in retaliation for the offense, and A's anger escalates into a frenzy of vile accusations.

Person B, on the other hand, is now also angry, but even more anxious than angry and, therefore, is mainly concerned with preserving the relationship that now seems threatened. As a result of the anxiety, which is the prime emotion associated with threat, B inhibits expression of the anger in an effort to prevent irreparable interpersonal harm. In effect, what is most important to each of these individuals is not the same, and we can speak of it as a difference in motivation or goal hierarchy, and sometimes a situational intention.

The bottom line is that although both A and B participate in the same event, we can see that the relational meaning of what is happening is quite different for each. The threat has been appraised differently which, in turn, leads each individual to choose different coping strategies and react with a different emotional pattern.

I have long believed that psychology needs to expand its concerns from an exclusively causal epistemology of interaction to one that is focused on transaction—that is, the person–environment relationship, and the evaluative meanings constructed about it by that person. We need to develop a language of relational meaning instead of one based on separate psychological elements or parts, such as events in the environment or in the mind. Each of these events function in a larger field—as parts of a whole, both in the mind and between the mind and the surrounding environment. The parts are important, but they cannot be properly understood without taking into account the larger field or system in which they operate.

Information, which is a basic theme of modern cognitive psychology, has no meaning until its significance is evaluated by a person. Relational meaning is the key to our humanity, and shapes our emotions and actions. Because of the subjectivity of the concept of meaning, however, and the difficulties of addressing it in empirical research, this way of thinking troubles many psychologists. Ironically, despite the fact that psychology constantly shies away from it, relational meaning, rather than information, is mainly what we mean by mind, and I believe it cannot be avoided if psychology is to advance.

These three fundamental epistemic and metatheoretical issues do not exhaust the divergent philosophical outlooks of psychologists. We constantly argue with each other about these issues, and others, in a futile circle, often without recognizing their epistemological and ontological bases, or their connection with the research we do.

A recent book about the cognitive unconscious by Reber (1993) pointed to several other contentious issues, such as nativism versus empiricism (I prefer the terms environmentalism or experientialism). We could also add functionalism versus structuralism, and rationalism versus arationalism to the list of such issues. They are sources of disagreement that, over and above substantive theories, remain bases of contention that constantly plague our discipline.

Because I want the reader to remember the three issues, and I need a handy way to refer to them in my comments, I offer a convenient label for each. Henceforth I refer to the first issue as *analysis versus synthesis*, a la Dewey; the second as *behaviorism versus cognitive mediation*; and the third as *interaction versus transaction*.

The Structure of the Book

All this is grist for the mill of this book. In the remainder of the prologue, I give a brief review of how the book is organized, so that the reader can see where it is heading and what is covered.

The articles I have chosen are organized into five main parts, each with a number of subsections dealing with different but related psychological questions. Each subsection begins with a brief introduction that examines the intellectual history of the ideas or research, and the professional and cultural influences that have led to the positions I have taken.

In the case of my early work in this anthology, I have written a comment following each of the reprinted article or articles, which discusses what happened in the ensuing years, what I learned about the science of psychology from this work, and how the work informed modern research and theory and changed with time. With respect to my recent work, there is little need for any added commentary, because the future has not yet made itself known.

In *Part I, Starting Out With A Bang*, I discuss my dissertation, published in 1948, which was a method of difference experiment that evaluated the Rorschach theory of color shock. I compared reactions to the standard color version of this projective test with a non-color version. The issue is no longer important, but my experience with the research is instructive about potential problems of graduate students, and about controversies in our field.

Part II, Why Psychologists Argue: Perennial Epistemological Issues, has two subsections:

Section A, *The Revolt of the Late 1940s and 1950s: Individual Differences in Motivation and Defense Influence Perception* takes up the New Look movement and discusses it as a rebellion against previous intellectual traditions about perception. Five short research articles are presented, each dealing with a different New Look theme.

Section B is an essay presented as a commentary on this work, which suggests that the New Look movement was actually a forerunner of modern interest in the relationships among cognition, motivation, and emotion. The essay is entitled *The Ancient Greeks Started It: Relations Among Cognition, Motivation, and Emotion.* Adopting this ancient tradition has left us with the unresolved problem of specifying the relationships among these three aspects or functions of the mind, which still fosters debate. An illustrative 1974 article is also reprinted, which explores my ideas and research on cognitive and coping processes in emotion.

In Section C, *The Unconscious*, Robert McCleary and I tried to study discrimination without awareness experimentally during the early 1950s. The phenomenon reported in the article, which we referred to as *subception*, dealt with an unconscious process at a time when academic psychology was very suspicious of the concept, and lead to major controversy. The unconscious is, today, a hot topic, suggesting that the subject matter of psychological research comes and goes, often changing in the way it is viewed, yet does not disappear if that subject matter involves fundamental issues.

Part III covers my *Transition from the Laboratory to Field Research.* It has three subsections:

Section A deals with *Motion Picture Films as Stressors*. The major feature of my progression toward field research, a conservative first step, was my attempt to use movies as a more natural way of arousing stress while remaining in the laboratory to control the stressor conditions and measure stress reactions.

Two research articles are presented in which films were employed. One offered an extensive overview of the early research using a film of a primitive rite of passage, *subincision*, to generate stress. The hallmark of this research was the use of sound tracks and orientation passages, predicated on the theory of ego defense, and played before the film was presented to influence how subjects appraised the emotional content of the film.

The second article was an experiment, identical to one of the sound-track studies with the subincision film we did in Berkeley, but conducted with Japanese subjects in Tokyo to compare stress and coping processes cross-culturally. We found a striking difference, consistent with what we knew about social relationships in the two cultures, American and Japanese.

In Section B, *An Early Field Study*, research is presented comparing a dispositional and process measure of coping to illustrate an early step in my eventual shift away from the experimental laboratory to field research.

Section C, which is entitled *Stress and Coping Theory and Research,*- contains two articles. The first is a systematic overview of my stress and coping theory. The second, published in a European journal that is not widely read in the United States, presented a review of the ten productive years of empirical research by the *Berkeley Stress and Coping Project*, roughly between 1977 and 1987.

Part IV has five subsections and examines the practical applications of stress and coping.

Section A, *Focus On Denial*, offers two discourses. The first deals with the costs and benefits of denial. Contrary to a traditional psychoanalytic premise, I argue that denial may have positive value in adaptation, but it is important to examine the conditions that make it either harmful or beneficial.

The second article, entitled *The Trivialization of Distress*, was written because I became concerned about the extent to which denial became overvalued in the effort, including my own, to rehabilitate the concept from the traditional psychoanalytic position: Because denial involves a distortion of reality, it was originally regarded as a psychotic-like process. I wanted to point out a particularly damaging consequence— that sick or dying patients are commonly pressured by their loved ones, friends, and doctors to think positively and be upbeat, to deny the personal disaster, at least to others who are unable to deal with it— which tends to distress and alienate these patients.

Section B, *Focus on Daily Hassles*, explores the role of garden-variety daily stress in health and well being, in contrast to the large catastrophes of life. It also examines the numerous ways in which seemingly minor stresses of daily life, in contrast with major life events, could affect our emotions and how we live.

Section C, *Focus on Psychotherapy*, presents two book chapters from the late 1980s. One compared my approach to stress and the emotions with that of Ellis and his "Rational–Emotive Therapy." The other, which appeared in a major handbook of psychotherapy, made the case that reasonable harmony among the mental functions of cognition, motivation, and emotion is an essential feature of mental health.

In Section D, *Focus on Job Stress*, I am critical of the traditional, normative approach to stress at work. The article makes a case for individual differences in the sources of work stress, and examines how such stress should be tackled in research and practice.

Section E, *Focus on War and Peace*, looks at the psychological dilemma of Israel, a nation that has for a long time struggled with the *stress of recurrent wars* with hostile neighbors. In 1975, I presented a discourse about the relevance of stress and coping theory and research for this struggle at an international conference in Israel. The published article, slightly expanded and updated in 1986, can be regarded as an application of stress and coping theory to a clinical and sociopolitical problem that has major psychological implications.

Part V presents a point of view about the relations between stress and emotion, expressed in the title, *Expanding Stress and Coping to the Emotions*. It has two subsections:

In Section A, *The Utility of the Emotions in the Study of Adaptation*, my 1993 prefatory chapter, which appeared in the *Annual Review of Psychology*, is reprinted. In it I proposed that we would gain by expanding our research and applied interests from the relatively narrow concept of stress to the far broader and richer concept of the emotions. I explain why this would be clinically more productive.

Section B, *Coping in Emotion*, contains an article from *Psychosomatic Medicine* in which I compared trait and process approaches to coping and try to reconcile them.

Finally, in the *Epilogue*, I try to pull everything together by drawing on the themes that began the book. I make an effort to reconcile the diverse outlooks about science that have tended to fractionate our discipline.

I

Starting Out with A Bang: My Dissertation

When I left the Army in 1946 after World War II ended, I sought advice from Gardner Murphy, a widely appreciated professor at CCNY where I received my batchelor's degree, about where I should go for a doctorate degree. I had been enrolled for a semester before the war at Columbia, and could have returned automatically, but Murphy advised me that my interests in psychodynamics would not be well served there.

He suggested Clark University, but when I applied I was turned down though, ironically, I later became an associate professor there. Murphy also recommended Pittsburgh because Wayne Dennis had been transforming psychology into a first-rate department. So I applied there, was accepted, and began my graduate studies in 1946.

When it came time for me to do a dissertation, I had an idea for a project that excited me. In those days, projective tests, such as the Rorschach Inkblot Test, were greatly favored for clinical psychodiagnosis. At that time too, it was generally accepted that unproven psychodynamic concepts should be evaluated by laboratory experimentation, and I shared this viewpoint wholeheartedly.

For my dissertation, I wanted to evaluate the Rorchach concept of color shock. One reason this interested me was its connection with emotion, which I saw as a central process in human adaptation. Color was said to be akin to emotion. Rorschach theory stated that neurotic individuals would respond emotionally to the color and, as a result, display three performance deficits—mental blocking, low productivity, and a poor quality of the percepts constructed from the inkblots. These were said to be evidence of a syndrome of impaired functioning in the presence of color, known as color shock.

When I began to seek a dissertation supervisor, I thought about a well-known and respected member of the clinical faculty, who was one of the reasons I had applied to Pitt. Because what I say may be embarrassing for that member of the department or his family, and because it was based largely on hearsay, I see no point in identifying him by name. Let me call him Professor CP (for "clinical professor").

Because of gossip from a few student friends, and also because of my own vague impression that CP might be difficult to work with, I developed considerable uneasiness about having him as my dissertation chairman. I had no direct experience to substantiate the stories I heard, but I feared that if CP were truly a martinet, he would take over the study and I would lose control of my dissertation. I needed to be advised, not told what to do. Perhaps I was being too cautious, but the dissertation is a first major step in a career and a graduate student can't afford to take a chance with the wrong faculty supervisor, who will also write letters of recommendation when it is time to leave school and get a job.

So I decided instead to ask Roger Russell, a physiological psychologist I knew and respected, to be the chairman of the dissertation committee. Russell knew very little about projective tests, but he was a sound methodologist, and he agreed to be the chairman. I thought, however, that I could have my cake and eat it, too, by having CP on the dissertation committee, but not as chairman. So I asked him to be a member of the committee so I could gain his input, but he declined with evident annoyance and indicated that he would only serve as chairman. This confirmed my suspicion that he could, indeed, be self-centered and controlling.

I think my experience illustrates some of the potential interpersonal problems that graduate students, then and now, can face in seeking a doctorate and beginning a career. I never got to know CP, much to my regret, and he later left the department and had a long and distinguished career elsewhere. Although I was happy with Russell as chairman, not having access to CP's knowledge and wisdom was undoubtedly an important loss for my clinical education.

Russell made important suggestions about how to design the study. At first I found him a bit stuffy, yet thoughtful and fair minded. I remember, for example, when I graduated he said to me, "Now you can call me Roger." I was at first a bit taken aback at this ingenuous but status-oriented remark. Perhaps I was oversensitive, and I suppose, as a young man, I had some problems with authority. He had spent quite a few years in Great Britain, married an Englishwoman, sounded British, and might have picked up British class-centered values and ways. I remember joking with other students about his pronunciation of nomenclature, with the accent on the second syllable, rather than on the third as it is pronounced in the United States.

Still, Roger was a very decent and kind man and I knew he meant well. I have always remembered this moment of awkwardness with affection rather than irritation, and have very warm feelings for him and for his wife, Kay. He later became Executive Director of the American Psychological Association (APA) and served in that capacity for many years. In 1985, on a sabbatical visit to Australia, my wife Bernice and I made a special effort to spend a few days in the city of Adelaide, where the Russells owned a small ranch. We stayed with them, reminisced, and partied with some of their Australian friends. It was a charming and sentimental sort of homecoming, academically and interpersonally, after almost 40 years.

To test the theory of color shock, my idea was to create a photographic version of the Rorchach Test that reproduced it entirely in black and white in order to compare it with the standard version, which contained color in five of the inkblots. The two versions would allow me to evaluate the theory of color shock by using one of John Stuart Mill's cannons of experimentation, the method of difference.

To test a large sample of high-school students, I used the group form of the test, with slides as the stimulus instead of Rorschach cards. It was quite a challenge to obtain permission from the high-school authorities to permit me to do my research in one of the schools in Carrick, Pittsburgh. Perhaps because of my status as a veteran, or of the unusual nature of such a request after the War, or because the research seemed benign and interesting, or just due to dumb luck, I was granted the authority to proceed.

The day of the research was Halloween, wet and slippery, but Bernice and I managed to get there with our materials in time after an uneasy beginning when the car almost failed to start and skidded in the wet leaves a few times before we got going. With Bernice assisting, I administered the two versions of the test in counterbalanced order; the colored version was given first to half the subjects, and the non-colored version first to the other half. As a check against the possibility that some subjects were color blind, Bernice administered the Ishihara Test, and later we eliminated from the data set those whose color vision was not normal.

The findings from the study showed clearly that there was just as much evidence of color shock in the black and white version as the colored one. Evidently, if color shock was occurring, it depended on the shape or shading of the inkblots. Color obviously had nothing to do with it. The only difference between the two versions was the relative absence of pure color responses to the black and white Rorschach slides, such as blood or a pretty sunset. Whatever one might say about the emotional causes of performance deficits, the theory about the role of color was clearly wrong.

When I went to Johns Hopkins in 1948 in my first postdoctoral job as an assistant professor, I submitted an article based on my dissertation to Gordon Allport, who was the editor of the *Journal of Abnormal and Social Psychology* (later it became the *Journal of Personality and Social Psychology*). In those days, when the number of psychologists doing research was still small, journal editors were not only distinguished scholars, but also did all their own reading and evaluating of submitted articles, as well as taking care of the correspondence. Allport summarily rejected my manuscript as too repetitious and wrote in his letter that it looked as though I had pieced it together lazily from my dissertation. He was absolutely right.

However, he said the research was important and worth publishing, and invited me to resubmit it if I could write it up cleanly and tersely. I was so shocked by his criticisms, and ashamed that my laziness had been found out, that I spent the next night without sleep completely rewriting the article, which I mailed the following morning. In a very quick turnaround, he congratulated me on the workmanlike way I had handled the rewrite, and accepted the revision without modification. I wish I had saved Allport's letters to illustrate his sensibility and humaneness to beginners, and the way publishing worked then.

Allport's letter of acceptance made me feel proud, but even more important, it taught me a valuable lesson about not being hasty in writing up research. As I have grown older, I have become even more wary about what I submit for publication. I write and rewrite many drafts so its quality will be the best I can make it before letting a manuscript go.

I try to get help from my colleagues, and when they read and criticize what I have done, I am grateful; it is a most welcome gift, because they are apt to be hard pressed for time. What we all want our critics to say about our manuscripts is something like, "This is a magnificent piece of work; I wouldn't change a word" which, of course, seldom if ever happens. Realistically, what is needed is constructive criticism, that helps us improve on our work.

The ultimate fate of this research is, I think, one of the most significant aspects of this dissertation story. When Joseph Zubin, an important figure in psychopathology in the 1940s, heard about my dissertation, he invited me to speak at an APA symposium he had organized on projective techniques. Because the program was already completely full of papers and discussants, I could take only 10 minutes to give my paper. I had no notes and had to speak extemporaneously, but I was so familiar with the material that this posed no problem. I have the vague recollection, which could be erroneous, that there were thousands of psychologists in a huge room—the topic was extremely popular at that time—with many sitting on the floor in the aisles and others standing.

Scared to death, I nevertheless presented my study and my conclusion that Rorschach theory had been wrong in its assumptions about color and color shock. I didn't have to wait long for a reaction. One female psychologist—again it is better not to identify her by name, though few today would know her—then a bigwig in Rorschach circles, stood up and blasted my study as ill-conceived, misleading, and worthless. To my ears, she seemed exceedingly nasty, and others later confirmed that this impression was, indeed, consistent with her reputation. Her main criticism was my use of the group method of administration of the test, though I now miss the point of why this would have made a difference in the validity of my conclusion.

I am no longer sure exactly what she did say, and at the time, I was chagrined and intimidated by the ferocity of her attack. This was the first time I was publicly savaged, but not the last. I discovered that one needs to cultivate a thick skin to be a research scholar, and I have learned to cope better with strong criticism, though it still sometimes bothers me, as I suspect is true of most people. Anyway, despite feeling greatly intimidated by the task of reporting my dissertation results at that APA meeting, and the assaultiveness with which I had been attacked, I felt good about the beginning of my research career, which was born in controversy and has continued in that vein throughout the years.

As a budding research scholar, this first public experience actually whetted my appetite for intellectual fray, which characterizes many topics about which our knowledge is incomplete, though I share the popular dislike of hostile, ad hominem attacks that are so common in academic life, and worse still, back-stabbing by someone who professes only friendly sensibilities in my presence. Nowadays, with the depletion of funding, increased competition, and what seems like a loss of gentility in everything, the professional atmosphere has grown increasingly venal and hostile.

In addition to the concept of color shock, there were many other features of Rorschach theory in those days that needed to be evaluated, either in the laboratory or in clinical field research. There was, for example, the psychological meaning of the movement response—that is, when motion is projected onto an inkblot. Another important concept was the quality of form in the percept constructed by the person. This quality was judged on the basis of the match between what is constructed by the person and the shape of the inkblot. These and other Rorschach response patterns were the basis of all sorts of judgment about a person's psychodynamics and mental health.

In many instances, Rorschach theory did not hold up well, which contributed to its eventual loss of status. This statement, although essentially correct, must be sharply qualified, because, as will be seen in Part II, my research with Charles Eriksen at Johns Hopkins produced findings supporting the psychodiagnostic significance of particular kinds of Rorschach Test data. Eventually, however, projective tests, including the Rorschach, began to lose favor in clinical assessment, though they may be enjoying a revival in some quarters with the emergence of new scoring systems, such as that of Exner (1995).

Nevertheless, my study, a first of its kind, didn't seem to change what clinicians did or said about the Rorschach Test. For a long time, it remained the most widely used diagnostic test in the clinician's armamentarium, without seeming to be influenced by any of the post-war research. This was the first intimation of what I would see again and again in years to come, namely, that research findings rarely unseat theories or change much until the leaders of the field, and the followers, change the way they think.

But even more important than the fate of particular assessment tools, clinical psychodiagnosis itself seems to have lost the very favorable standing it once had. One reason for this is that confidence in the inferences derived from diagnostic testing gradually weakened as psychologists became more sophisticated about assessment and its validation. Another, even more important reason, is that clinical psychologists rankled at their early image as testers rather than psychotherapists. As they began to compete with psychiatry in therapeutic practice, diagnosis became more and more subordinated to treatment. Therapists relied increasingly on intake interviews rather than formal diagnostic testing to gain an understanding of a patients' psychological strengths and weaknesses, and to plan a course of therapy.

In any case, my research on color shock, and the controversy it generated, endeared me to Chairman Clifford Morgan, who had hired me as an assistant professor of psychology at Johns Hopkins in 1948 after I finished my doctorate at Pitt. In addition to teaching and research, my job was to do psychotherapy with troubled students in the campus clinic. Along with much of the faculty, Cliff was terribly prejudiced against clinical psychology, especially its practice. But my use of experiments to test clinical ideas, though often providing support for the hypothesis, gave me considerable cachet in a very science-centered department, as well as in the growing field of university clinical psychology.

Because Cliff died very young, he never knew that in the 1970s I developed doubts about the dependence of psychology on laboratory experimentation, especially for research in personality, clinical, and social psychology. By that time too, I was beginning to have plenty of company, including even some of the Hopkins faculty, such as my friend and collaborator, James Deese, who in later writings (Deese, 1972, 1985) criticized the epistemological view he and I had earlier assimilated. The field was getting ripe for big changes in the ontology and epistemology of psychology. My disaffection with the laboratory and later transition to field research is the topic of Part III of this book.

II

Why Psychologists Argue: Perennial Epistemological Issues

There are three sections in Part II, each dealing with difficult epistemological, metatheoretical, and theoretical issues.

Section A presents five articles, published during the 1950s while I was an assistant professor at The Johns Hopkins University. It deals with a dissident American movement, called the New Look, which challenged the traditional premises of the field of perception implicitly accepted as doctrine for several decades during the heyday of behaviorism.

In Section B, I try to show that this dissident movement was, in large measure, a reversion to the cognitivism of the classical Greeks, which set the stage for the perennial separation of the basic functions of mind, cognition, motivation, and emotion. This separation, which was less extreme, as seen later, in the Greek version than in the behavioristic movement of the 20th century, fueled the vigorous debate about cognition–emotion relationships that took place in the 1980s. After thousands of years, we are still struggling to find the best way to characterize how the mind is organized.

Section C returns to the 1950s to examine an experiment I performed with Robert A. McCleary that demonstrated a process of discrimination without awareness, a form of unconscious mental activity that we named subception. Because it dealt with the unconscious at a time when this notion was deeply distrusted by hard-nosed psychological scientists, it caused much controversy. In my comment, I try to bring matters up to date by showing how unconscious processes are dealt with today.

Section A. *The Revolt of the 1940s and 1950s: Individual Differences in Motivation and Ego-defense Influence Perception*

INTRODUCTION

After World War II officially ended on September 2, 1945, the day I got married, a great expansion of academic life began, which was to reach a peak in the 1960s. The reasons for this expansion are complex, and undoubtedly have to do with economic factors and the return to school of large numbers of servicemen who were supported by the GI Bill.

In the 1950s, especially in the late years of that decade, professors began to play a game of musical chairs in which universities vied with each other to attract the most visible scholars to their departments. Psychology was no exception. Thus, in 1948, with the completion of my doctorate, I was able to obtain an attractive appointment at a first-rate university. Competition for promising faculty shifted the ante for beginning appointments up to the rank of assistant professor from the lowly rank of instructor which, before the War, had been the first step on the academic ladder.

As faculties around the country grew, an enormous diversity of interest and outlook became increasingly apparent. This diversity has been a great strength of universities in the United States. Given the expansive, competitive environment of the 1950s and 1960s, one could more easily afford to be a maverick and get by, a situation that, here and abroad, depends greatly on time and place and, unfortunately, may be disappearing as public support for higher education and research dwindles.

Like several of the new members of the Hopkins department, I was hired with the understanding that I could not obtain tenure. Even if I were successful in bringing honor to the institution, I would still have to leave in 5 years or so. Honor depended largely on what one published and how it was regarded. At the ripe old age of 26 (the oldest member of my department was 36), I didn't mind and, like many of my ilk, I was motivated to make a name for myself as a significant research scholar.

The department consisted of an impressive collection of hard-nosed young men, mostly prejudiced in favor of behaviorism and learning, and sympathetic to a Darwinian evolutionary outlook. There was a favorable attitude toward physiology, and a negative attitude toward clinical psychology, especially clinical practice. Clifford Morgan, a physiological psychologist, was the chairman, and other visible psychologists included Al Chapanis, James Deese, Charles Eriksen, Wendell (Tex) Garner, Elliot Stellar (also a physiological psychologist), and others perhaps a bit less well known.

As a graduate of CCNY and the University of Pittsburgh, working amongst a group that was mostly from Ivy League and Big Ten schools, I felt I had grown up on the wrong side of the tracks. These young men all became important leaders in their respective fields. Cliff Morgan, who had a remarkable, intuitive sense of who would succeed, had brought this faculty together, and it was truly an outstanding group. I wasn't sure I was up to its lofty standards, but I persisted. Eriksen, a personality psychologist, and I, were heavily influenced by the New Look movement, though I believe Jim Deese might have been too, but in somewhat different ways as a psychologist concerned with learning.

I collaborated in research with both Erik and Jim, and did mostly stress research with Jim, which was formative for my later empirical and theoretical work. My joint research with Jim is not represented here only because it did not meet the criteria for reprinting in this anthology; but the ideas promulgated in that research are central to my later work.

Before the late 1940s, research and thought about how we perceive the world followed three long-standing traditions: First, influenced by an evolutionary interest in our fate as a species, perception was conceived and studied *normatively*—the emphasis was placed on how people in general perceive their environments. Second, perception of the physical and social environment was said to be *veridical*—it was regarded as, in the main, *accurate*, which made it possible for people to survive and flourish. Third, perception was treated as a *cold* process, which means that there was little or no interest in the *motivational or emotional determinants* of adaptational success and failure.

Because speculating about what was going on in the mind was anathema to a behaviorist, perception was viewed as an instantaneous process in which an environmental stimulus display gave rise immediately to a perceptual response, without going through a series of stages of intervening mental activity. Only later, when the ontology and epistemology of psychology was loosened to permit expectations to play a role, did perception psychologists begin to conceive of multiple processes at work between the stimulus input and the response output. But by then, interest had begun to shift from perception to a superordinate concept of which perception is a part—cognition.

Beginning in the 1940s and extending into the 1950s, a number of venturesome psychologists, such as Gardner Murphy and Jerome Bruner, as well as some others, moved boldly to develop a radically different way of thinking about human perception. Murphy was a professor at CCNY who was lionized by most of his students, including me. We were impressed by his humanity, scholarship, and his fairness and balance in teaching about diverse schools of thought (e.g., Murphy, 1947/1966). When I went back to school after World War II, he helped me join the APA, though I had only a bachelor's degree, and he advised me about graduate schools, as I noted in Part I.

These scholars created a renewed metatheoretical and research agenda, referred to as the *New Look.* The movement had three basic themes, each diametrically opposed to the premises of the earlier approach.

First, it emphasized *individual differences* in the way we perceive the world, as opposed to a normative approach, which refers to people in general. Second, perceptions could *deviate from* the population norm without being regarded as pathological. Third, perception, and other processes of human adaptation, were said to be governed not only by the environmental stimulus display, but also by *motivational and emotional* factors, which produce the individual differences that are ubiquitous in everything we do. To put it a bit too simply, to some extent we engage in wishful thinking; we are also capable of having dark thoughts that influence how we see events and objects.

Although seeming to be in contradiction, the traditional and the New Look outlooks both captured an important part of the truth, and although they were never reconciled by any of the perceptual theories of the day (see, for example, F. Allport, 1955), Lewin (1946) was crystal clear about the need for reconciliation when he wrote:

> "The problems of general laws and of individual differences frequently appear to be unrelated questions which follow somewhat opposite lines. Any prediction, however, presupposes a consideration of both types of questions...problems of individual differences...and of general laws are closely interwoven. A [scientific] law is expressed in an equation which relates certain variables. Individual differences have to be conceived of as various specific values which these variables have in a particular case. In other words, general laws and individual differences are merely two aspects of one problem; they are mutually dependent on each other and the study of the one cannot proceed without the study of the other." (p. 794)

I consider Bruner to have been the most important progenitor of New Look thinking and research. Throughout his intellectual life, he emphasized meaning in the study of the mind, and more recently promulgated a narrative approach to psychology, which has gained considerable popularity in certain quarters (Bruner, 1990; see also Josselson & Lieblich, 1993; Sarbin, 1986; Schafer, 1981; and many others, such as those cited by McAdams, 1996).

I first met Bruner at The Johns Hopkins University. He came there to give a colloquium talk. I thought he was a tremendously impressive, self-confident, and lively man, interested in everything, and articulate about expressing what he thought. He wore glasses with very thick lenses, and when I first saw him, his choice of perception as a main interest struck me as more than just intellectual. I never had the chance to ask him about this, but I assumed that some serious visual defect must have moved him against the reigning tradition toward a more individualized view of perception. Scientists often choose topics for their research because of personal experiences and problems. Only recently did I discover from his autobiography (Bruner, 1983) that he had been born blind. His sight was restored by surgery at age two, and heavy glasses were needed to correct his vision to near normal.

To give a feel for his research in those days, in one of his early, celebrated studies, Bruner and Goodman (1947) effectively demonstrated, with later replications and extensions by others, that children overestimate the sizes of coins compared with other circular objects of no value. Poor children overestimated these coin sizes more than rich children, all the more so when the monetary value of the coins was high. In accord with the New Look outlook, the evidence showed that personal values and needs influence how we see objects, events, and people.

This way of thinking about perception, and by extension, problem solving, decision making, and action, fitted well with my own psychological premises and commitments, which leaned toward psychodynamics, subjectivism, and holism. So when I started out as an assistant professor, I became involved in research along these lines. What follows in this section is a modest sample of my research in those days, dealing with the influence of personality dynamics on perception—especially motives, ego-defenses, and individual differences in stress.

With respect to the articles presented, section A begins with a study I did with two undergraduates on the role of hunger in the perception of food and nonfood objects.

Next, I make a defense of the perceptual defense concept, which had been criticized by Postman, Bronson, and Gropper. Two studies with Charles Eriksen, a colleague at Johns Hopkins, follow. In my view, they provided strong empirical support for the hypothesized role of ego-defensive processes in perception and thought, and in the symptoms of emotional dysfunction they displayed.

What was then commonly referred to as "perceptual defense" was first hypothesized to raise the threshold for perceiving threatening stimulus material, in accord with the Freudian concept of repression, which should lead a person to avoid thinking about or seeing what was threatening. Subjects favoring avoidance should be less motivated than others to perceive threat, presumably because they want to keep it out of consciousness. However, along with quite a few others, Eriksen and I viewed this as only one of two coping strategies.

The second strategy, referred to in those days as "perceptual vigilance," should, in contrast, result in a lowered rather than raised threshold for perceiving threat. Subjects favoring this type of coping want to perceive the threat as quickly as possible, presumably to neutralize it by means of intellectualization or distancing. Our research showed that the choice of one or the other coping method could be predicted from our subjects' clinical symptoms. Without such a prediction from independent personality data, making an interpretation about defensive avoidance and vigilance would be tautological.

The fourth study, also done with Eriksen, arose from the same vein of ego-defense theory in that it demonstrated that the content of neurotic patients' emotional disturbances could be identified from the tendency to reject Rorschach concepts reflecting that content. The rejection was indicated by avoidance of this content in the percepts they constructed from the inkblots. The same process was manifest in the projective test and in daily living. Findings like these supported the premise of projective tests that what is seen and not seen can have psychodiagnostic significance.

The fifth article deals with individual differences in stress, and by implication, coping. In my research at both Johns Hopkins and Clark University, I regarded variations in personal goals and beliefs as the main explanations of ubiquitous individual differences displayed under similar environmental circumstances, and this theme shows up often in my later research and writing.

Below are the five articles reprinted for Part II, Section A.

HUNGER AND PERCEPTION

Richard S. Lazarus
Herbert Yousem
David Arenberg
The Johns Hopkins University
1953

During the past several years the reader of psychological journals has been bombarded by a series of experiments and discussions on the relationship between hypothetical need states and various measures of perceptual behavior. The evidence is quite impressive that such relationships do exist under a variety of conditions. However, little systematic attention has been given to a critical analysis of the criteria of perception which have been employed in these experiments, and to the conditions under which these relationships are found. To understand the mechanism of this phenomenon (or perhaps these phenomena), a careful analysis of the circumstances under which positive and negative findings occur, as well as a study of the kinds of perception which are capable of being molded by personality and need variables, is required.

Particularly prominent among the studies persistently cited as providing evidence of the motivated nature of perception is a series of experiments using hunger as the independent variable **(1, 2, 5, 6, 9, 10).**

In this article these half-dozen studies on hunger and perception will be discussed briefly. In addition, two experiments will be described which may throw some light on the question of the criterion of perception and the mechanism of need and perception relationships.

Some years ago Sanford **(9, 10)** performed two experiments in which he systematically varied hours after eating and studied the effects upon such imaginal and associational variables as the content of word associations, the interpretation of ambiguous pictures, and the completion of words and drawings. In general, Sanford found a positive relationship between food deprivation on the frequency of food responses, although this increase was not in direct ratio to the increase in time.

A later experiment related to hunger and perceptual behavior was performed by Levine, Chein, and Murphy **(5).** These authors tested a small group of experimental and control subjects on the interpretation of simple drawings which were presented behind a ground glass screen to make them completely ambiguous. The experimental subjects were instructed to fast for various periods of time prior to the experimental sessions. The authors found that the number of food responses given for achromatic drawings increased at three and six hours of deprivation, decreasing at nine hours. For chromatic drawings, food responses increased at three hours' deprivation, but then decreased at six and nine hours from the last meal. These curves were questionably interpreted to mean that greater ambiguity of stimuli (the use of chromatic drawings suggested greater ambiguity to the authors) favors some kind of reality process that operates in contrast to autism (perceiving in accordance with the hunger need).

More recently, two experiments were performed with adult Navy personnel by McClelland and Atkinson **(6)** and Atkinson and McClelland **(1).** In the first experiment (6), the authors projected blank images and smudges on a screen with varying amounts of hints to the subject concerning what they might be. They measured the effects of varying degrees of food deprivation on the frequency of food-related responses. The main finding was that as hunger increased from 1 to 4 to 16 hours of deprivation, the average frequency of food responses increased in a negatively accelerated way. Moreover, the introduction of smudges decreased the number of food responses compared with the blank screen condition. Instrumental responses (knives, plates, etc.) showed the greatest increase in frequency. In the second experiment (1), this work was extended to the Thematic Apperception Test in an effort to study the clinical procedure of inferring needs from projective behavior. The essential results were that as hunger increased, there was an increase in the percentage of subjects showing food deprivation themas, story characters expressing a need for food, and activity which was successful in overcoming the deprivation, as well as a decrease in the amount of goal activity and friendly press (involving help in getting food).

The final experiment which is directly relevant to the present issues was performed during the last war and reported by Brozek *et al.* **(2).** Thirty-six conscientious objectors who had volunteered as subjects were exposed to a study of prolonged semistarvation. During the semistarvation period, observations of the men indicated severe personality changes and a marked increase in the food drive. However, in spite of the extreme interest in food and the obvious signs of such preoccupation, only limited aspects of the free association test (starved subjects gave significantly more unusual responses to the food stimulus words than controls) showed any systematic effects of the starvation. Measures of restricted association, dream content, Rorschach projections, and the results of the Rosenzweig Picture-Frustration Test provided essentially negative results.

BASIC ISSUES RAISED BY THE HUNGER AND PERCEPTION EXPERIMENTS

For the purposes of this article, there is little to be gained by an intensive critique of each of the experiments cited here. Some of the findings among the experiments are not consistent. While Sanford, McClelland and Atkinson, and Levine, *et al.,* found food associations are a function of hunger, Brozek, *et* al. obtained no such relationship in most of the functions they measured. While McClelland and Atkinson avoided giving their subjects a "food set," Levine, Chein, and Murphy made a special effort to introduce such a set. Levine, *et al.* found food interpretations decreasing at six and nine hours' deprivation, depending on the type of stimuli. Sanford found at least no increase above four hours, and McClelland and Atkinson's data showed a negatively accelerated function. Except for Brozek, *et al.*, there is some rough agreement that the relationship is either not monotonic, or at least is negatively accelerated.

It is also true that these experiments, taken individually and as a group, have been the subject of much just criticism. Indirectly, Pastore (**8**) has seriously questioned the propositions upon which they were based. He offers a critique of the need in perception experiments in general, and a separate criticism of the experiment by Levine, *et al.* It seems more relevant at the present time to raise two basic issues (focusing upon those studies using hunger as the independent variable), and to develop them with respect to the experiments to be reported here.

The first issue concerns the way one should define perception and the second has to do with the nature of the process in the need in perception relationships. Both questions are intimately related. The answer to both must have to do with the kind of stimulus and response situations that enter into the perceptual behavior studied. Therefore, in considering the experiments that have been cited and the studies to be reported here, we may focus upon such experimental variables as the type of stimulus, the means by which the stimulus is made ambiguous or is presented at below and above the threshold of recognition, and the nature and conditions of the responses that the subject gives to the perceptual situation.

The experiments cited above all appear to be dealing with a special kind of perceptual behavior which ought to be differentiated from other kinds of perception. While Sanford made no claim to be dealing with perception, the above experiments have been repeatedly cited as demonstrating the interdependence of need states and perception. There is a growing tendency to define perception practically without reference to a stimulus of any kind. While Sanford spoke of the "imaginal processes," Levine, *et al.* used the expression "perceptual distortion," McClelland and Atkinson used the term "perception," and Brozek, *et al.* spoke of "perception and association." And yet, *the closest any of the experiments came to considering* perception even partly in terms of a stimulus variable was that of Levine, Chein, and Murphy, who projected chromatic and achromatic drawings distorted behind a ground glass screen. Most of the time McClelland and Atkinson projected nothing on the screen, and, at times, nothing but vague smudges. What we are dealing with, then, in all these experiments, is something more like projection, association, interpretation of formless objects, imagery, phantasy, or some such psychological behavior, rather than, strictly speaking, perception in terms of the identification of objective stimuli. There are probably important differences between these intimately related forms of perceptual behavior which determine the kinds of conclusions one may draw about the principles in the area of needs and perceptual selectivity.

Now it is clear that attempting to make the kind of distinction that is sought here is no simple matter, and is likely to get the person who makes it into all kinds of epistemological confusions. However, it does seem that the interpretation of ambiguous blots or of blank images on a screen does involve some differentiation from the identification of objective stimuli which have a common label and about which we can get almost universal agreement. We must ask if it makes any difference whether we show the hungry subject nothing, ambiguous blobs, real objects, pictures of objects, or words which stand for the objects. This is the case whether we are dealing with needs like hunger or hypothetical ego-defense processes as in some very recent experiments.

At this point a few comments would be in order concerning the much-used but little-discussed notion of ambiguity in perceptual experiments. It seems to the writer that ambiguity, which is thought to be a necessary requisite of need and perceptual relationships, must be thought of in terms of the number of reasonable alternatives that any given population may offer in response to the request to identify or interpret a stimulus. This point has been made in a previous article dealing with perceptual defense (3). If everyone agrees that the stimulus is a typewriter, then it is hardly ambiguous. However, as the number of interpretations that are possible increase, say to include a roast ham, an office desk, a cigarette lighter, etc., the stimulus may be said to be more ambiguous. Even a blob or smudge on a screen may have some structure, however slight, because most viewers would agree that it could not be certain things while it could be others. The concept of the popular response and the good form response on the Rorschach Test takes recognition of the incomplete ambiguity of the ink blots. While it may be dangerous to argue that if most people perceive a fountain pen as a fountain pen the stimulus really is a fountain pen, it is eminently practical, and there could be no society if we did not. The distinction that the present author is proposing, therefore, between the two kinds of interrelated perceptions, that which seems to be more oriented toward imagination, association, or projection, and that which is more stimulus-oriented, depends upon the criterion that the experimenter uses to score the response. If the correctness of the response depends largely upon the characteristics of the stimulus, rather than some hypothetical state in the organism, then this might be called perceptual behavior in the strictest sense. The criterion depends upon using stimuli about whose identification nearly everyone will finally agree. In the case of words, the subjects will all finally arrive at the correct recognition, at least given an unlimited exposure. The technique for introducing temporary ambiguity may be the rapid exposure of the words or letters on a screen, or the use of subliminal illumination which is progressively increased to what is called "above threshold levels." At early stages in this progression the perceptual process may indeed be closely analogous to the interpretation of completely ambiguous blobs. In the case of pictures, all subjects should eventually arrive at the agreed upon designation of the stimulus. If this is not so, then the stimulus is truly ambiguous, and cannot be used in an experiment on perception in the strictest sense. In the experiments by Levine, et al. and by McClelland and Atkinson, stimulus ambiguity was half-heartedly varied in terms of chromatic vs. achromatic drawings, and smudges vs. no image at all, respectively.

In dealing with the question of the mechanism of the need and perception relationships, it is also appropriate to point out that explanations have been offered in terms of the concept of response frequency. Some writers have argued that needs affect perceptual recognition by virtue of the recency and frequency of certain words, and that greater frequency of experience with a word results in lower recognition thresholds (**11**). This argument has been most extensively used in connection with experiments on needs and the visual recognition thresholds of words. While the interpretations of the relevant data are controversial at the present time, this is one of the hypotheses about the need in perception relationships that bears investigation, and on which the experiments to be cited here have a bearing.

Two experiments will be reported. Both were designed to be comparable with some of the earlier experiments on hunger and perceptual recognition. However, they employed pictures of food and nonfood objects which, when exposed fully to subjects, would be unequivocally identified by all. The two experiments are nearly identical, except for the fact that in one the subject was free to make his own guesses with no restrictions imposed upon his responses, and in the second experiment the subject was forced to choose from among a limited group of alternatives which were always before him, and which represented the range of stimuli which were being used in experiment.

PRESENT PROCEDURES

The subjects in these experiments were 110 and 51 volunteer male college students, respectively, who were individually tested at a time when they indicated they were free to participate. They were told that this was an experiment in the perceptual recognition of familiar objects, presented in the form of photographs by means of a tachistoscope and variac, which were described to them. They were told that we were interested in the effects on recognition of such variables as illumination, time of day, fatigue, etc. The ascending method of limits was explained to them with the caution that we would finish with each picture before going on to the next, and that, since the number of exposures was fixed in advance, the presentations would continue, sometimes even long after the subject had correctly identified the object. Presentations were all made at one-fifth of a second, on a ground glass screen, using a small lantern slide projector with a camera shutter attached. By means of the variac, the first exposure began at 22 volts and proceeded upward in one-volt steps, always going past the point of recognition by the subject. For example, if the subject identified the stimulus correctly five times in a row (which was the criterion of success), exposures continued for a variable period of about 10 to 15 times thereafter. If a subject failed to recognize the object after 59 exposures, he was allowed to view it for longer periods until it was established that he could, indeed, identify the object. But in that case, he was given no score for that slide since carrying the item to a slower speed would have resulted in a fairly meaningless score to represent his perceptual threshold. Any subject who did not recognize at least 60 per cent of the slides within the 59 exposures was discarded. There were nine such cases. After each exposure, the subject always made a guess, even if he claimed to have no idea whatever concerning the nature of the stimulus.

The stimulus materials consisted of 10 slides which were made from photographs of a series of food and nonfood objects. Food pictures consisted of a bunch of grapes, a dish of pancakes, a two-thirds-full bottle of milk, an ear of corn, and a double-decker toasted sandwich. The nonfood objects were a pocket cigarette lighter, an open portable typewriter, a mountable pencil sharpener, a bellows-type camera, and a household hand iron. The presentation of these slides was always the same for all subjects, and randomized in an ABBA order. The first trial always consisted of a practice slide in which a photograph of an electric clock was used.

When each subject had completed the series, he was interviewed concerning his eating habits, given the word-association test used by Sanford in his second experiment, and asked to rate his degree of subjective hunger on a five-point scale. None of the subjects appeared to have guessed the purpose of the experiment, or realized that half of the pictures were food objects. From the interview data, it was possible to place the subject accurately in terms of time since his last meal, using the beginning of that meal and the start of the experiment as points of reference.

The only deviation from the random testing of subjects occurred with some of the students falling in the five- and six-hour deprivation period. After the collection of considerable data it became apparent that there was a shortage of subjects who had not eaten in five or six hours. This group was supplemented by collecting some subjects just before lunch would normally occur, and, with their consent as well as under the pretext of having them wait under carefully controlled visual conditions, managing to delay testing of these subjects until the proper hunger interval had been reached.There were some subjects who, for some reason or another, had been without food for longer periods, from 7 to 23 hours. Because these subjects were so scattered in terms of hours of deprivation and represented a small group, they were not included in the sample of data reported. It might be noted, in passing, that these subjects were exceedingly variable among themselves in their performance and present no analyzable trends.

The second experiment was performed in exactly the same way as the first, but with the exception that in front of the subject at all times was a verbal list of the 10 pictures, plus an additional six dummy items, half of which were food and half of which were nonfood items.

Moreover, the word-association procedure was abandoned. The subject was told to select his guess from this list of 16 items, which was always before him, and the order of which was constantly changed to prevent list-order preferences.

RESULTS

The findings of the present experiment may be separated into the basic perceptual data including the relationships between recognition thresholds and hours of deprivation and subjective hunger, data dealing with prerecognition guesses and word-association, and qualitative observations. The perceptual recognition thresholds were scored in terms of a formula used by McClelland and Liberman (7) in an experiment on the need for achievement. For each subject, the mean number of exposures (before the criterion) of the food pictures was subtracted from the mean number of exposures of the nonfood pictures. This difference score was divided by the sigma of the nonfood pictures to take account of the effects of differences in the variability of perceptual recognition among the subjects.

One final fact must be pointed out before the data are presented. Experiment 1 was repeated twice, once with an N of 41, the second time with an N of 69, to make a total of 110 cases for the combined experiments. Each experiment was performed by a different experimenter and upon different subjects. The purpose of this repetition was to check on one special aspect of the results. Since, in the first study, the relationship between hunger and perceptual recognition was not monotonic (viz., Levine, *et al.*), it was felt that a kind of cross validation was necessary before any confidence could be placed in the observations. Although the data have been analyzed mainly in combined form, some of them will be presented with a division into parts A and B because of the additional confidence added by the repetition.

Basic Perceptual Data

The main finding from Experiment 1 concerning the relationship between hours of food deprivation and perceptual recognition score is diagramed in Figure 1. This figure shows the curves of recognition for parts A and B of the experiment, and the total combined curve. The lower the score, the lower the relative recognition threshold for food pictures. You will observe that food-recognition thresholds steadily decrease until three- and four-hour deprivation (just before and during ordinary meal-time for these subjects), but take a sharp rise at five and six hours.

Because of heterogeneity of variance, the scores were transformed into logs in order to perform an analysis of variance on the grouped data. The results of this analysis may be seen in Table I. The F test for these means