

THE COLLECTED WORKS OF PETER G. OSSORIO



VOLUME I: *PERSONS*

PERSONS

PETER G. OSSORIO

DESCRIPTIVE PSYCHOLOGY PRESS
ANN ARBOR

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PREFACE TO THE SERIES

The Collected Works of Peter G. Ossorio

Peter G. Ossorio's works are unique.

In a trivial sense the same can be said of anyone's work—it is Jones' work, nobody has the same interests and style as Jones, thus the work is unique. But Ossorio's works are unique in the most profound sense possible and on several counts: in the breadth of his subject matter, the depth and rigor of his analysis, the power and clarity of his exposition, and the absolute coherence of his conceptual framework. Most importantly, they are unique in their significance. Peter G. Ossorio has accomplished what nobody else has seriously attempted: he has articulated a rigorous and coherent framework for understanding persons *as* persons.

If past experience is any guide, this claim will strike some as impossibly overstated, while others wonder why *that* would seem to be a worthwhile accomplishment. These reactions say a great deal about the intellectual climate of “behavioral science” in the second half of the twentieth century—and they are substantially the same reactions which greeted Ossorio's first book, *Persons*, in the early 1960's. To those who doubt the possibility of such accomplishment, this series serves as a reality check: read the works and judge for yourself. The second group may be reassured by scanning the list of Ossorio's publications; you will discover that the concept of “persons *as* persons” includes behavior, language, culture, the real world, and the *doing* of science, psychotherapy, computer-based simulations, and many other significant social practices.

Indeed, Ossorio's work—which has become the foundation and core of a discipline called Descriptive Psychology by its

practitioners—has had profound influence in a remarkably broad and diverse set of arenas. Directly, Ossorio has influenced the practice of psychotherapy and the conceptualization of psychopathology; the teaching of numerous aspects of behavioral science including personality theory, projective testing, and multi-cultural studies; the understanding of language, verbal behavior, and its technical implementations within computer environments; the practice and philosophy of science; the understanding of cultural differences and their implications; the technology of information storage, retrieval and utilization; and, most recently, the creation of robots that exhibit increasingly the important characteristics of persons. Indirectly, through his students and colleagues, Ossorio has influenced many other fields; among them are the theory of organizations and the practice of influencing organizational culture; the development of computer software and artificial persons; the conceptualization of spirituality; the theory of consciousness, hypnosis and altered states; and much more.

Any editor of a series of “collected works” faces an obvious question: why collect the works? Why not let them stand on their own, as published? The answer in this case is simple to give: the large majority of these works (including the first, seminal volume, *Persons*) have been published only in limited circulation working editions. These works, with few exceptions, were literally unpublishable within the “mainstream” of behavioral science when they were written. Ossorio was making, literally and intentionally, a “fresh start” on the doing of behavioral science, for reasons which he clearly articulates in *Persons* and elsewhere, and which have become increasingly cogent over time.

Metaphorically, Ossorio was talking chess to tic-tac-toe players, who responded, “That’s all well and good, but does it get you three-in-a-row?” Suffice it to say that the tic-tac-toe players decided what was worthy of publication in mainstream journals and books. And to extend the metaphor a bit further, it is evident that the mainstream of behavioral science has

progressively realized that tic-tac-toe is a no-win game, and we perhaps should have been playing chess all along.

For those who have tired of the trivial insularity of tic-tac-toe behavioral science, the present series represents a substantive and substantial alternative. Descriptive Psychology Press intends to publish this series at the rate of at least one volume per year. In the spirit of making a fresh start, let's begin.

Anthony O. Putman, Ph.D.
Series Editor
Ann Arbor, MI, March 1995

PREFACE

If there was ever a volume in psychology that merited the appellation, *sui generis*, it is the first volume of Peter Ossorio's works, *Persons* (1966). When, in 1965, the first chapters were shared with those of us in Boulder, they came like a presentation from another world. He simply was not talking or writing in the way that ordinary psychology was conducted. *Persons* was neither a theory presented as possibly true and certainly testable empirically; nor a methodological treatise on how to conduct some varieties of psychological research; and it certainly was not the summary of a series of empirical studies. If *Persons* was not one of these three recognizable forms of professional writing, what was it? The other categories of writing that come to mind—personal essay, mystical insight—all invited the dismissal of the work as not serious and surely not the work of a sophisticated professional. But those of us who had conversed and argued with Peter knew that he was both sophisticated and serious, and that *Persons* represented his attempt to share a vision of how to think clearly and systematically about psychological topics.

Because Peter was starting fresh—not building directly on previous psychology theory—the initial problem in understanding was that readers would assimilate *Persons* (1966) to something that they already knew. In my case, I initially tried to see it as an extension of the conceptual portions of Fritz Heider's (1958) *Psychology of Interpersonal Relations*, and, while there are some relevant kinships, it would be inaccurate to portray *Persons* (or Descriptive Psychology) in that fashion, for Peter's work built in no way on Heider's. (Peter's account of intentional action was informed by Anscombe's *Intention* [1957], and the similarities to Heider's discussion of “desire,” “know,” “can,” and “try” reflect his appeal to the same deep structure that Heider had apprehended rather than to direct intellectual influence.) Similar kinships could be seen with Chomsky's

Syntactic Structure (1957), with Ryle's *The Concept of Mind* (1949), or with the analyses of several of the philosophical psychologists who owed their insight to the late Wittgenstein (1953). I am thinking of R. S. Peters' *The Concept of Motivation* (1958), Strawson's *Individuals* (1959), and A. R. White's "The notion of interest" (1964), among several others.

What I, along with many others, found was that the analogies that help me to get beyond puzzlement and irritation were drawn from instruction in a new set of conceptual tools—as in a newly invented mathematics or in double-entry bookkeeping (for those to whom this way of doing bookkeeping is a new way of thinking about the world.) When I could free myself to approach *Persons* that way, then the structure, interrelatedness, and comprehensiveness of the presentation in *Persons* quickly came to have meaning. Not long after I made this breakthrough, I began to see how the distinctions intrinsic to intentional action and to the larger concept, persons, came to repeat themselves in each theory of behavior that I examined. First with Peter, and then on my own, I found myself able to use Descriptive Psychology concepts. The seven illustrative analyses contained in Part Four ("Theoretical Persons") and in Part Five ("Ubiquitous Persons") were a crucial starting place for me. These seven topics/issues are:

1. Mediation S-R Theory (Osgood, 1957).
2. Subjective Expected Utility Theory of Motivation (Atkinson, 1964).
3. Perceptual Defense Research (Goldin, 1964).
4. Prescriptions on Psycholinguistics (Miller, 1965).
5. Social Psychology of the Experiment (Orne, 1962).
6. Cognitive Aspects of Emotion (Schacter & Singer, 1962).
7. Ideographic vs. Nomothetic Approaches to Personality (Allport, 1937, 1961).

In these short analyses, Ossorio was able to show that the Person concept contained all the elements of logical structure required to describe, represent, and explain each case. Further-

more, the Descriptive Psychology analyses did not generate the conceptual confusions that were inherent in the (sophisticated) homunculus theories and in the characteristic insistence by these seven mainstream theorists that real science requires explanations in terms of underlying processes.

My first attempt at a critical analysis of research using a Descriptive Psychology approach was contained in the chapter “The self, intentionality, and reactions to evaluation of the Self” (Ossorio & Davis, 1968). That paper played a modest role in stimulating a critical reexamination of what research was being done, what significance it had, and of how to think about the theory–empirical research relationship within social–personality psychology. Gergen’s *Toward Transformation in Social Knowledge* (1982) and Smedslund’s *Psycho–logic* (1988) have each taken a somewhat more radical stance concerning the nonempirical quality of much social psychological research—see the critiques from a Descriptive Psychology point of view by Davis (1991), Mitchell & Davis (1985) and Ossorio (1991)—but several major figures in the field have come to recognize many of the hypotheses purportedly tested within the pages of mainstream journals are in fact derivable from tautologies or near tautologies (Wallach & Wallach, 1994; and the entire issue #4 of *Psychological Inquiry* (1991) edited by Pervin). And even though it has taken thirty years for positions such as Ossorio’s to become acknowledged, that should not surprise anyone.

As I revisited *Persons* to complete this labor of love, I found that I could see brief sections which would become the kernel of entire monographs. The discussions of “description” in Parts Two and Three lay some of the key ground work for the full-length treatment in *Meaning & Symbolism* (1969). The development of the concepts of part- and partial-descriptions invite the systematic presentation of forms of behavior description in “Notes on Behavior Description” (1967/1981), and the need to represent behavior as part of the real world invited a presentation of the transition rules (appearing first in *Outline of Behavior Description*, LRI Report No. 4a [1967] followed by the

fundamental descriptive formats appearing first in “What Actually Happens” [1971/1975/1978] and subsequently modified in “The State of Affairs System” [1971].) The need for explicit rules, making it clear how to use the conceptual elements of the Person concept consistently, led first to the statement of maxims in various parts of *Persons*, then to nine maxims in “Notes on Behavior Description” (1967/1981), and finally to nearly 100 principles for Status Dynamic explanations in *Place* (1982). In a very real sense, all of Descriptive Psychology is there in *Persons*. Subsequent elaborations have arisen from the great variety of issues which former students and colleagues have approached with Descriptive Psychological tools. The *Advances in Descriptive Psychology* series contains many of these; others form the basis of the Descriptive Psychology Press’ publishing activities. Another major theme of Ossorio’s subsequent work is the explicit systematization of Descriptive Psychology concepts.

Persons is one of those books that must be read and reread, thought about and used, argued against and fought with. But one ignores it, even today thirty years later, at peril to one’s sanity, coherence, and personhood.

(I am indebted to Thomas O. Mitchell for reading these introductory comments carefully and suggesting several changes that improved their clarity and force, and for confirming my sense of the initial reaction of readers of *Persons*.)

Keith E. Davis, Columbia, SC, September 1994

This publication of *Persons* has been a true labor of love. Many individuals donated their time and money to make this publication possible; among them were David Bender, Ray and Laurie Bergner, Keith Davis, John Forward, Jane Littmann, Tom Mitchell, C.J. Peek, Mary Roberts, Carolyn and Paul Zeiger. In particular, Jim and Garnet Holmes, Mary McDermott Shideler and the membership of The Society for Descriptive Psychology all contributed substantially to financing this work.

Joe Jeffrey took on the laborious task of creating the Index. Anthony Putman was the final proof-reader. Jan Cohen designed the type conventions and the cover.

Two individuals truly made this book possible: Peter G. Ossorio, who wrote it, and Lisa Putman, who produced it. Over a period of two years, beginning from barely legible Xeroxed copies shot through with typographical errors and convention inconsistencies, Lisa performed Herculean labors to produce the volume you now hold. Had she known in advance what she was facing, it is hard to understand why she would have gone ahead—but the end product is worth the toil, and more. But then the same could be said about *Persons* itself, or the entire endeavor of Descriptive Psychology. We all owe a great debt of gratitude to those who have stayed the course. Thank you all.

Anthony O. Putman, Ann Arbor, MI, March 1995

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Foreword

This report calls for more than an ordinary degree of charity and dispassion from the reader. It is, for one thing, neither easy nor pleasant to read. It has not the pin-stripe rectitude of academic prose, nor yet the terse virtue of a measured research report. Rather, it bears the mark of its construction piece by piece and with impatience. At best, it may be said that no jury-rigging or baling wire is required in order to hold it together, for it is all of a piece. But even that would be of more comfort if it were less of a challenge.

It requires some charity because it lacks the promise, which we have come to expect in scientific reports, of presenting something that is true or something “useful” or something that is both. The conceptualization presented here is neither true nor useful. That is not a matter of opinion or evidence, for it *could* not be either. But then, it needs neither, and so if anything is missing, it is not *that*. That fact will serve to indicate the magnitude of the change in style of thought required of the reader.

Such a change is doubly necessary, since the presentation is supported by neither argument nor evidence (at times, it may be difficult to believe that). But after all, these could support only a claim to truth. Instead, the conceptualization is simply presented and illustrated, and that is taken to be enough to carry the point of its basic relevance for Psychology as an *empirical* science. Such a procedure is hardly less unprecedented than the conceptualization it goes with, and so of itself it requires a significant change in style of thought in order to deal with it responsibly. An appreciable portion of the readers of the original manuscript (Part One—Part Five) have been unable to see that such a shift is called for. They have seen illustrations, examples, and analogies as arguments, or as evidence,

or assumptions, or whatever. And they have been enraged, suspicious, perplexed, disdainful, egregious, or simply impolite. It is easy to do that.

The presentation requires dispassion because it is paradoxical. In dealing with it, as in the case of mirror-writing, our previously acquired automatisms are most likely to lead us astray. For example, technical terminology is avoided as a matter of practical necessity, and the illustrative procedure is adopted because the subject matter is one we all know, but since it is one we have not thought about in certain ways, what is being said is *not* something we all know, and so the familiarity of the language is both necessary and misleading. Too, the concern with language, necessity, and human limitations has not uncommonly appeared as a wholesale attempt to define things into or out of existence, to “legislate facts,” whereas a major point of that concern is to provide a corrective alternative to what is seen as our *present* widespread tendency to act as though saying that something is so makes it so (because “words mean what I want them to mean”). Or again, the reflexive character of the conceptualization generates considerable perplexity and suspicion of verbal sleight-of-hand. The presentation is a delineation of a concept which subsumes (is instantiated by) a delineation of the representation as one which delineates itself. That it is of this sort, however, is one of its most basic features and one which eliminates (rather than solves) some of our most perplexing present problems in formulating explanations of human behavior, for *those* problems are seen to reflect a verbal sleight-of-hand, and the method of resolution is to keep everything out in the open. For although the conceptualization which is presented could not itself be true or useful, since in one aspect it represents the general conditions for anything being true or useful, the presentation will be useful if, as now seems possible, it helps to eliminate a variety of problems concerning explanation and research in Psychology and suggest some novel procedures and practices with respect to both (illustrated with a substantive example in Part Two).

The presentation itself must be understood as just that—as an action, or performance, associated with a product, rather than as a set of statements. (Analogously, one might think of a teaching device which provides reminders, illustrations, and evaluations and results, if successful, in the acquisition of skills rather than in the accumulation of information.) One of the presented concepts an appreciation of which is most strategic is that of human behavior as participation in some form of activity, or social practice. Both the presentation *and* the reading of *Persons* are to be seen in this light. We have heard frequently about the “participant observer,” but what we shall need instead is the concept a participant whose observations are already and literally a part of his participation rather than something that goes on surreptitiously (magically) and in addition. And as with observing, so with presenting. Accordingly, the irritation and impatience sometimes expressed in the presentation is *expressive* rather than merely symptomatic of a bad temper.

But the proper beginning for the presentation is an apology to the reader herewith for requiring so much more than usual, for not waiting until it could be done better, and for the suggestion, with respect to a report of this length, that a single reading might not be enough.

P.G.O.



INTRODUCTION

The present monograph is the precursor of a more systematic effort designed to provide the initial substance of a new psychological discipline, designated as Descriptive Psychology.

The task of Descriptive Psychology is to delineate the subject matter of Psychology as a science. The recognition of this need is already a departure from existing psychological mores. For the standard way of handling the issue of subject matter has been, of course, to say merely that "Psychology is the study of behavior." From this starting point, the remainder of psychological activity has, by and large, been taken to be either (a) empirical, hence to be settled by experiment or (b) theoretical, hence arbitrary and binding upon no one. In light of a general acceptance of this dichotomy, it would hardly be surprising if an attempt to give a substantially articulated description of psychological subject matter were to appear arbitrary and presumptuous. Nevertheless, there are some significant advantages to be gained from any such description which is even approximately adequate substantively and which does more than to say, in effect, that the subject matter of psychology consists of the various topics that psychologists study. Several such advantages are suggested below. ❖

I. Advantages

A. *Coherence*

Any reasonably detailed description of the subject matter would provide an overview which was independent of the variety of technical approaches to that subject matter. It would, for example, provide a means of identifying that portion of the subject matter which was dealt with by a particular theory or a particular investigator without requiring a prior mastery of the terminology of the theory or the investigator in question. At the present time an independent identification of this sort is

well-nigh impossible. To be told, for example, that M has a theory of motivation is to be told next to nothing, because there is no descriptive framework within which motivation is distinguished from anything else or related to anything else. The restrictions on what qualifies as a theory of motivation are implicit, unclear, and apparently almost completely idiosyncratic. One theorist may deal in neurophysiology, another in biochemistry, another in explicitly normative concepts such as “guilt,” others in quasi-normative concepts such as “self-actualization,” “dissonance,” or “guilt *feeling*,” and another in quasi-physiological concepts such as “drive,” “TOTE,” or “mediating response.” It is not clear why we call all of these theories theories of motivation, nor is it clear that there is any point in our doing so. And it is not at all clear what we have when we have such a theory. Similar considerations apply to our theories of “learning,” “psychopathology,” “personality,” or “perception.” We recognize that motivation, learning, personality, perception and psychopathology are not unrelated, but it seems fair to say that no systematic account of the relations among these various subject matters has been given. To be sure, it is sometimes claimed that some theories, e.g., of the psychoanalytic or the S-R variety, provide an *explanatory* account of the relations among motivation, learning, personality, perception, and psychopathology, but in the absence of a descriptive account it is difficult to make any guess as to what, if anything, is *explained*. No doubt the most overworked and least clear of our basic concepts is none of the foregoing, but rather, “behavior.” Because of the central role of “behavior” in distinguishing psychology from other sciences, the present descriptive effort involves a primary focus on the concept of human behavior and more basically, persons, as the subject matter of psychology as an autonomous science.

B. Comparability

A second advantage provided by a descriptive account of psychological subject matter is that it makes possible the identification of alternative viewpoints and facilitates their

comparison. For example, to have a descriptive account of “emotion” or “personality” as a subdivision of a psychological subject matter would make it possible to recognize when two theories dealt with the same subject matter, i.e. emotion, or personality. It would also provide a basis for assessing the adequacy of such a theory. That is, a personality theory would be adequate to the degree that it provided an account of the basic phenomena identified *descriptively* as “personality” and facilitated the empirical study of “those aspects of the phenomenon which were both nontrivial and empirically decidable.” In the absence of a descriptive account, we are involved in the following sort of muddle:

Q: Are Allport’s theory and psychoanalytic theory competitors? That is, do they provide alternative accounts of the same thing?

A: Yes. They are competitive. They explain personality differently.

Q: What is it that they both explain differently?

A: Personality.

Q: And what is that?

A: Well, of course, that has to be defined within a personality theory (cf. Hall and Lindzey, 1957).

Q: Then how can they both be talking about the same thing?

A: They’re both theories of personality.

Q: What is that?

. . . Etc. Etc. Etc.

C. *Cumulativeness*

A third advantage of a systematic description of psychological subject matter could be to facilitate research and theorizing that was maximally cumulative by making research and theorizing more comparable, hence more significant. The present situation is one in which (a) it is difficult to compare theories except in respect to specific experimental paradigms, and (b) the relation of a theory or a finding of one kind (e.g., learning)

to a theory or a finding of a different kind (e.g., personality) is unclear and uncertain. Because of this, it is relatively difficult for any investigator to make good use of work done by investigators who have different theoretical preferences or work in other substantive areas.

D. *Substantive Adequacy*

A fourth advantage of an adequate descriptive account is that it would permit an individual investigator to pursue significant empirical questions independent of *any* theoretical formulation. He could do this without paying the price that is now commonly thought to be inevitable, i.e. that of (a) fooling himself, because he really is using some kind of theory, only implicitly, and (b) achieving results which are divorced from the "main stream" of organized thought and empirical knowledge in psychology and therefore make no real or lasting contribution. For example, as soon as we have a descriptive account which says that we learn primarily by experience and only secondarily by inductive or deductive procedures, then a significant range of empirical investigation is identified by the question "Under what conditions does who learn what?" The question is significant because "learning" is not an isolated concept, but has a place in the systematic descriptive account of the subject matter of psychology, and so, when we find out something about learning, we know what we have found, and it is not a "bare fact." So long as the data is collected systematically and the systematization is expressed in terminology which has an accepted use, the data is the *kind* of data which has significance in that it adds to our understanding of human behavior, and it is the kind of data which other investigators may well make use of, because among other reasons, they understand it. In particular, there is no basis for the assertion that unless the systematization is expressed in exotic technical terminology having a putatively explanatory value we have a "mere heap of facts."

To assert that a Descriptive Psychology would provide the basis for significant empirical investigations independently of any theorizing is to contradict the widely cherished thesis that

"Theories are *necessary*." However, the contradiction is to a large extent merely apparent. This is because the "thesis" that theories are necessary is to a large extent merely apparent. That is to say, nothing of significance is being asserted or denied when one asserts such a "thesis." For the "necessity" of a theoretical approach has to be defended by asserting that *any* systematization in description and *any* selectivity in collecting data is *ipso facto* a theoretical enterprise. Thus, to say in *this* sense that "theories are necessary" in psychology is to say roughly, that psychological investigation is not a senseless or random activity or behavior and that a useful scientific account of behavior is not a random collection of sentences. But *that* is not something which has been questioned by those who have questioned the necessity of theories. The genuine question about theories is a different one, and so the "thesis" is simply beside the point.

It is far from being the case that we consider every significant sentence about behavior to be a theoretical sentence or every thought about behavior to be psychological theorizing—we are only tempted to such extremes when arguing methodology. The accepted exemplars of "Psychological Theory" are moderately small in number, and could be given in a list. (Freud's theory and its modifications; Jung's theory; S-R theory and its variations; Tolman's theory and other Expectancy theories; Rogers' theory and other Self theories; Meyer's, Murphy's and other psycho-bio-social theories; etc., etc.) That such a list can be given does not imply that it cannot be added to in the future. On the contrary, there are few predictions that could be made with greater confidence than this: That at some future time we will refer to what Smith said as "Smith's theory," whereas Jones, who may have said as many thoughtful or useful things as Smith, will never have his words referred to as "Jones' theory." The fact that we find it impossible to *say* what it takes to be recognized as a theory, in this sense, does not prevent us from recognizing theories when we encounter them and it does not prevent us from distinguishing theories from nontheories. In connection with classifying something as a

theory in this sense it would be a naive sophistication to say "But aren't we all a little bit theoretical," and it would be as otiose as saying "But aren't we all a little bit mad" to the admitting physician in a psychiatric hospital.

It is in this latter sense, the sense in which psychological activities and achievements are already divided into "theoretical" and "nontheoretical," that a descriptive account would permit an individual investigator to pursue significant empirical questions without theoretical formulations and without penalty. With respect to the methodological "thesis" in which "theoretical" simply distinguishes sense from nonsense, there is very little to be said, except perhaps to remind ourselves of how *very* little it says.

E. Autonomy without Isolation

A more speculative, polemic, and no doubt, controversial proposal is this: That a descriptive account of psychological subject matter would contribute materially to the establishment of psychology as an autonomous science, which it is far from being at the present time. In particular, it would help to free psychology from its present dependency on (and encumbrance by) both philosophy and physiology.

It seems safe to say that at the present time most psychologists accept some sort of account about "the place of psychology in the hierarchy of sciences." The autonomy which is apparently granted to the several sciences by the stipulation that they differ in their "level of description" is a spurious one, as it has turned out historically, because the *de facto* implication has been that the lowest level is "the real thing" and as we move upward in the hierarchy we are more and more dealing with "mere interpretation."

The connections here may be exhibited schematically:

- (a) the contrasting term for "interpretation" is "description";
- (b) if there is no description of psychological subject matter, then that subject matter must consist of mere interpretations;

(c) if that is so, then those must be interpretations of phenomena of a more basic sort (of a lower “level”), i.e. of a physical sort (“movement”) or a physiological sort (“brain process”);

(d) and then psychological explanation must consist of showing these interpretations to *be* interpretations of those more basic phenomena. (Here “interpretation” is used descriptively, *not* in the technical sense in which models “interpret” theories.)

Thus, neurologizing, whether actually, conceptually, analogically, or in the guise of “performance models,” has commonly been accepted as the mark of rigor and adequacy in psychological explanation and experimentation. Indeed, this tendency is so pronounced that a recent philosophical writer (Fodor, 1965) could seriously propose that

Explanation in psychology consists of a functional analysis and a mechanistic analysis: a phase one [functional] theory and a determination of which model of the theory the nervous system of the organism represents. . . . [and that] It is sufficient to disconfirm a functional account of the behavior of an organism to show that its nervous system is incapable of assuming states manifesting the functional characteristics that account requires.”

Since this account is proposed by Fodor as a piece of technical philosophy, not as a contribution to psychology, it might properly be dismissed by psychologists as an interesting curiosity, but psychologically irrelevant, reflecting the peculiar concerns, standards, and fashions of philosophers, as contrasted with psychologists. That it is not unlikely to be taken seriously by some psychologists reflects a second sort of constraint. That is that attempts to deal conceptually with significant psychological concepts such as “experiment,” “observation,” “say,” “same,” “response,” or “person” are almost certain to be identified by psychologists as “philosophizing.” This is not an accident, since the “empirical or theoretical” dichotomy leaves no significant room for conceptual analysis or for description of anything

other than apparatus and “operations” and “results.” It is doubly not an accident, since it is a tendency that is fostered by philosophers, and most clearly so by those philosophers who are wedded to the “unity of science” viewpoint. An example is found in a recent paper (Feigl, 1965) by a philosopher who is one of those best known to psychologists. Here, he states that

The [mind-body] problems may be fairly clearly divided into scientific and philosophical components. The scientific task is pursued by psychophysiology, i.e. an exploration of the empirically ascertainable correlations of “raw feels,” phenomenal patterns, etc. with the events and processes in the organism, especially in its central nervous system (if not in the cerebral cortex alone). The philosophical task consists in a logical and epistemological clarification of the concepts by means of which we may formulate and/or interpret these correlations.

It may well be questioned whether many psychological investigators would want to take “raw feels” and “phenomenal patterns” as their subject matter. What is disturbing is that it is not nearly so clear that psychologists would object to the suggestion that their task is limited to the collection of data whereas the thinking is to be carried on by philosophers. Although the passage from Feigl may be counted among the more overt expressions of this attitude, it is not difficult to describe the same attitude among both psychologists and philosophers. The result, historically, has been that psychological science has been largely carried on as though commentary by the logical positivists and their philosophical heirs, the logical empiricists, were *prescriptive* rather than descriptive, interpretive, or merely fanciful in regard to the kind of conceptualization and explanation appropriate to psychology as a science.

We might well ask, thereat, “With friends like this, who needs subject matter?” And the answer proposed here is “with friends like this, we *need* a subject matter.” To say this is not to propose a rejection of theses concerning the unity of science. On the contrary, in spite of what might on casual examination

be taken as arguments or “assumptions” which rule out reductionism and the “unity” position, a careful reading of the descriptive account will show that every provision is made for the *legitimate* possibility of success in reducing psychological concepts to other kinds or in demonstrating any degree of dovetailing of the various “levels” of science. What the emphasis on a descriptive basis for psychological science does represent is a prudential suspicion of “unity” as a self-fulfilling prophecy. It is to say that any “unity” which is demonstrated by following the prescription of unity, i.e. by deliberately stage-managing our psychological accounts of human behavior so that they do dovetail with current neurological thinking, is too methodologically cheap an accomplishment to view with professional pride as a psychologist.

It would seem to be a methodological truism that the only *definitive* standard for a psychological theory is that it should be an adequate attempt (trivial or ludicrous attempts will not count) to give an adequate account of human behavior as such. To attempt to dovetail a psychological theory with the findings and theories of physiologists, or of physicists, or linguists, or economists, or any other discipline whatever is to impose an additional standard. To make a serious effort to meet the secondary standard is to take the serious risk of falling short with respect to the primary standard. If there were not a central core of psychological science as an autonomous science responsive only to the standard of giving an adequate account of human behavior as such, then there would be no adequate check on the *psychological* adequacy of a neurologized (etc.) psychological theory. No doubt we would have a tidy picture of *something*, for we could see to it that there were no loose ends on the behavioral side. But then, why should anyone feel inclined to refer to such functional neurology as “Psychology”?

Since methodological theses generate considerable emotionality among psychologists, it is worth being repetitious in the interest of keeping these feelings on target: It is neither the possibility of a thoroughgoing unification of the sciences, nor even the avid pursuit of such a goal by *some* psychologists, that

is seen as stultifying for psychology as a science. Rather, that danger is found in (a) the vehemence with which such secondary concerns as physiology are proposed as primary and as criteria for psychological explanation and (b) the degree to which these proposals have been so widely accepted by American psychologists, without effective dissent, that they have become the dominant view in psychology today.

II. Difficulties

Granted that there would be significant advantages in having a descriptive account of psychological subject matter, it is equally the case that a number of difficulties are to be encountered in an attempt to give such an account. No small part of these are the difficulties which may be anticipated for a reader who is attempting to understand such an account. These difficulties may be summarized by saying that whereas normal psychological communication consists roughly of 10% of content which is stated explicitly and 90% of content which is shared by reader and writer and is simply taken for granted by both, these proportions are more nearly reversed in the present monograph. What we normally take for granted is that which has to be reexamined critically. Past experience has shown that certain kinds of misunderstanding are much more likely than others, and these are of such a kind that it seems more appropriate to take explicit account of them than to try to modify the presentation of the concept of a person in an attempt to minimize them. The relevant topics dealing with the relation of Descriptive Psychology to technical philosophy are discussed in Appendix A. The relevant methodological topics, having to do mainly with description, are discussed below.

A. *Theory vs. Description*

At the outset, there is some difficulty in distinguishing between observing, describing, and reporting on the one hand

and inferring, explaining, and theorizing on the other hand. Many psychologists would say that the distinction is an arbitrary one having no particular point. For example, a psychological theorist might say that his theory was simply a conceptually economical, higher-level description. And if the theory were a good one, very likely that would be correct. But it would not be *simply* correct. We should have to ask what it was a description of.

For the purpose of formulating some issues, it will be convenient to use as special-purpose terminology the locutions “theory *of* . . .” and “theory *about*.” Ordinarily, when we say that Theory R is a theory about X, we do not distinguish this from the statement that Theory R is a theory of X. We use the two expressions interchangeably, but in either case there is an ambiguity—we may be referring to either one of two states of affairs. In the one case, X simply comprises the concepts of the theory. For example, in this sense, S-R Theory is a theory of stimuli, responses, habit strength, drive, reinforcement, extinction, etc. and the theory provides an account, e.g., of particular responses coming to occur in the presence of particular stimuli. For the present discussion, this state of affairs is designated by saying that Theory R is a “theory *of*” P, where “P” is a surrogate for the theoretical concepts of Theory R. The second case is one in which X is not any part of the theory but rather is an antecedently known and separately describable phenomenon, so that Theory R provides a second way of talking about that phenomenon. This second state of affairs is designated by saying that Theory R is a “theory *about*” Q, where “Q” refers to the separately describable phenomenon. For example, S-R theory might be regarded as a theory *about* learning. Learning is separately describable as the fact that as a result of engaging in certain activities and not others and/or being exposed to certain circumstances rather than others, persons and most other sentient creatures come to be able to do certain things (rather than others) that they could not do previously. We may later wish also to distinguish cases where Theory R is merely a summary

(a more economical description) as against those cases where it is putatively explanatory as well.

We may note in passing that a “theory” which is merely an uninterpreted calculus can have no scientific standing, though it may have some *potential* relevance. Only when there is some number of persons who know how to use the theoretical terminology to characterize observable phenomena does a “theory” become a theory, i.e. a part of the professional practice of science. (See Hesse, 1963, and Spector, 1965, for more extensive discussions relevant to this point.) The extent and degree of the competence to which theoretical terminologies lend themselves will vary. If the scientific practitioner knows how to use a given terminology only in very special circumstances (e.g. in regard to certain laboratory experiments) then of course, he doesn’t know much when he knows that (though it might still be important to know that). And if he knows how to use the terminology with precision only in those restricted circumstances but can also use it impressionistically or imaginatively in a variety of other circumstances, then very likely he knows a good deal less than he supposes. In any case, we shall need to consider only those theories that someone knows how to use.

If Theory R is merely a theory *of* P rather than a theory about Q, there does indeed seem to be little point in the distinction between theory and description, for in this case, they come to the same thing. We can have alternative theories about learning, but we can not have alternative theories about stimuli, responses, habit-family hierarchies, etc. so long as these are simply theoretical concepts. A theory *of* P defines its own subject matter, and so it can have no competitors unless it is also a theory *about* some Q.

Thus, as a description of Q, the subject matter, becomes more and more noncommittal, the differences among a theory *about* Q, a theory *of* P, and a description of P approach zero. Clearly, a theory which is a theory *about* “some phenomenon” is merely a theory *of* P rather than genuinely a theory *about* some Q. A theory *about* “behavior” is not greatly different from this, though perhaps some commitment has been made. A

theory *about* “learning” is apparently even more committed and even more clearly *about* some subject matter (assuming that we have an adequate description of it), though here it may be well to recall that our better-known “learning theories” are primarily theories *about* “behavior.”

The distinction between a theory *of* P and a theory *about* Q becomes particularly relevant when we try to assess the explanatory value of a particular psychological theory and its relevance to those other human activities which do not consist of psychological theorizing. It seems clear that a theory, R, which is merely a theory *of* P has no explanatory value relative to any preexisting Q (and human behavior antedates psychological theorizing) or relative to any Q which is not describable in terms which are equivalent to those of R. And if a theory *of* P is claimed to be also a theory *about* Q, the claim may be immediately discounted to the degree that the description of Q is ambiguous or noncommittal (e.g., “Q” = “behavior”). In general, any such claim has a dubious status until such a time as an acceptable description of Q has been given.

It might be supposed that the requirement for a description of Q could be eliminated so long as instances of Q could be identified. For example, we might identify instances of “behavior” by pointing to them. Could we not *then* take “behavior” as our subject matter without having to give any descriptive account? But pointing could only succeed in identifying a locus of study, not a subject matter, if the latter were not *already* known.

(a) If I point to an instance of “behavior,” I also point to an instance of “organism,” “object,” “movement,” “number,” “physical object,” and to instances of “cost,” “color,” “size,” “heredity,” and some unknown number of other concepts. Thus, pointing will not distinguish one subject matter from others. But neither will it pick out any subject matter at all for:

(b) I may point to city halls, university campuses, and railroad cars. If I have a theory *of* triangles, circles, squares, etc., I may study instances of these latter when I encounter them *in* city halls, *in* university campuses, or *in* railroad cars. That is, I

restrict my study to the sort of thing I have pointed to, and I apply my theory of P (P = circles or squares or triangles) *there*. It would be pointless and misleading to say that what I had done then was to study city halls, or university campuses, or railroad cars. And it would be equally egregious to say that what I had then was a theory *about* city halls, university campuses, and railroad cars. Yet, it would seem that the same sort of claim is made by a psychologist who says that “behavior” is his subject matter when in fact all he has done is to study reinforcement, habit strengths, etc. as they occur (if at all) *in* instances of “behavior.”

B. *What is Descriptive?*

In our less thoughtful moments we are apt to take it for granted that giving a description is the same as reporting an observation, and therefore, what is said is self-evident, so that (a) mistakes are ruled out and (b) everyone ought to agree. This kind of doctrine is sometimes expressed by saying that in giving a description one is “merely reading off the features of what is actually there”.

No doubt in some cases “reading off features” is an informative way of characterizing what is involved in giving a description. “That’s a red cup,” “The cat is lying down,” “The picture is hanging crooked,” are examples which illustrate the fact that some of the judgments we make involve phenomena which we “can tell at a glance” and therefore might well be classified as cases of “reading off the features of what is actually there.” We may move from these cases through a range of phenomena exemplified by “He drank deeply from the cup” and “The halfback made an end run,” and pass on to more difficult cases such as “There’s a knock in the motor,” “He made a sarcastic remark,” and “This is a defective memory unit from an electronic computer.”

As we survey this range, several features of the phenomenon of describing become clear enough to be read off. First, what we can tell by observation is not always something that

we can tell at a glance. It may require careful attention, particular sorts of competence, or more or less prolonged or repeated observation. For example, the judgment that the halfback made an end run or that Big Ben chimed six times ordinarily requires attention and concentration over an appreciable interval. The judgment that this was a sarcastic remark frequently requires observation of the circumstances which provide the occasion for the remark. And the judgment that this is a defective memory unit for an electronic computer may require careful examination and certainly requires special experience or training.

Because some descriptions require careful attention, prolonged observation, or special training, an observation may be careless, hasty, or naive, and so the corresponding description might be incorrect or mistaken in any of these ways or for any of these reasons. From this alone, it would follow that complete or universal agreement among observers can not be a necessary feature of making an observation or giving a description.

There are other bases for disagreement, too. Recall the various descriptions of what I pointed at when I pointed at an instance of "behavior." Two observers might offer alternative descriptions, both correct, of "the same" phenomena, and each might reject the proposition that the other *was* correct, especially if the two had very different training and ability. Again, if two observers differed in training and ability, or if their interests and preoccupations diverged markedly, one of them might be able to "tell at a glance" that here was a Ruy Lopez opening or that here was a sarcastic remark, whereas the other might have to be reminded of relevant features of the phenomena in order to be able to make the same observation.

The preceding cases have taken an observation report as the paradigm of a descriptive account. But descriptions can be given when what is described is not present to observation at all, and it is clearly among descriptions of the latter sort that a descriptive account of psychological subject matter (or any subject matter) must be found. The foregoing examples are intended to serve as reminders that even in the simple case of observation

reports, it is seldom the case that giving a description is profitably characterized as “merely reading off the features of what is actually there.” The latter terminology is a remnant of the empiricist myth of “the given” in perception. Its primary virtue appears to be that it helps to keep clear the distinction between observation and inference. It does so by making metaphoric use of the word “read”—it directs our attention to the fact that what we establish by observation has the same *immediate* intelligibility as a verbal passage which we literally read and understand, hence it contrasts with what we establish by inference. But this metaphor, which *might* have remained harmless, has been put to the dubious use of referring to hypothetical ultimate constituents or ontological grounds for the very sorts of performance (the successful exercise of skills) from which the metaphor draws its sense. In its mythical use it implies a uniformity and universality which is simply nonexistent. To recognize that one man’s “given” may be another’s laboriously achieved inferential conclusion is not to move in the direction of eliminating the distinction between observation and inference. (If one man buys an automobile and another man steals one, is there therefore any less reason to keep the distinction between buying and stealing?) Rather, it is to recognize that the judgment in question is an achievement (since it is necessarily subject to standards of correctness) rather than a gift, and that is to have reason enough to reject the concepts and terminology of “the given” as unserviceable for the present discussion. (See Walton, 1963, for a critique of perceptual “inference.”)

To give a descriptive account of a subject matter is clearly not the same as reporting an observation, for what we actually observe is at most some illustrative cases, and those are not identical with the subject matter, which would have to include all cases of the kind in question. However, the description of subject matter could be related to the description of cases in the following way: The description of a single case would, for present purposes, be equivalent to the description of the general case, which would be equivalent to a description of the subject matter, if it were a description of what it was about that case

that qualified it as an instance of that subject matter. If I pointed to a pawn, or to a person, that would not be enough. I should also have to say what it was about that individual that made it a pawn or a person. To say that about a pawn would involve more than might be immediately apparent. In fact, it would involve the entire set of rules for chess, including those rules referring to other pieces, to the board, and to the criterion for winning or losing. It would require all the rules which are constitutive of chess as a game, because what makes a pawn a pawn is that it is a particular element and has a particular role in *that* game. To say what makes an individual a person is one of the major concerns in the following chapters, and to do that is not less complex than formulating the rules of chess.

We may say, then, that to give a descriptive account of the subject matter of psychology is to formulate the conceptual system which is constitutive of that subject matter in the way that chess rules are constitutive of chess facts. To anticipate a bit, that subject matter can be summarized as “persons and human behavior,” or simply “persons.” (But it should also be noted immediately that this description does not rule out the study of physiology or nonhuman behavior.)

This descriptive account is therefore characterized as a description of P (P = persons) which comes to the same thing as a theory *of* P (or, alternatively, a theory *of* P', P' = person concepts) which is not also a theory *about* any Q. The term “person concept” will be used primarily to identify those concepts the intelligibility of which depends on the concept of a person. Examples of this type are “intention,” “action,” “skill,” “attitude,” “wants,” “human body,” “believes,” “self-concept,” “afraid,” “guilty,” “surprised,” “sick,” etc. (Many such terms are not *uniquely* associated with references to persons, for they are also applied to other animals. However, one of the claims made in the present account is that persons provide the paradigm, or “full-fledged” case with respect to which the application to other animals, infants, and nonverbal individuals generally, represents an intelligible *extension*.) Locutions involving terms

designating person concepts will be referred to as being “person-descriptive” terms. In order to avoid excessive circumlocution certain other words and concepts will be included under “person concepts” and “person descriptions.” These are concepts which pertain to the common world of persons and objects, hence are not *distinctive* aspects of the concept of a person. Major examples of such concepts are “state,” “part,” “episode,” “history.” Whatever controversy may exist with respect to statements which will be made later about person concepts and person descriptions will not hinge on the inclusion of these nondistinctive concepts, and this is the justification for not explicitly excluding them in talking about person concepts.

Philosophers have made proposals which amount to saying (in the present way of talking) that a description of persons or a theory *of* person concepts is a theory about movements (= behavior?). But when I say, “He ran because he was afraid” I do not know, nor do I care, what movements occurred (in rare cases where I might care, that would be an *additional* issue.) And generally speaking, no observer of an action knows what movements occurred. In observing his fearful behavior I do *not* begin with a description of a movement which has occurred and then try to account for *that* movement. Thus, irrespective of whether the movements *might* have been antecedently known and separately described (which is a much more dubious supposition than it appears to be at first glance) it is clear that our use of person descriptions in no way requires any antecedent knowledge of this kind. And that is taken to be reason enough to reject the notion that person descriptions involve a theory *about* movements.

In providing a description of persons, and a theory *of* person descriptions, we are providing the descriptive apparatus for specifying that the subject matter of psychology includes those characteristics which we have no reason to doubt have always been regarded as being distinctively exhibited by people as against other sorts of individuals. (Person concepts include what Strawson (1958) and others have called “person predicates,” or “P-predicates.”)

It is further the case that the descriptive apparatus for formulating a theory of person descriptions itself consists almost exclusively of person descriptions. This situation is a very unfamiliar one, and unfortunately, attempts to understand it by assimilating it to more familiar situations and terminologies are more likely to engender confusion than clarification.

For example, the fact that essentially no technical terminology is introduced in the descriptive account has led some readers to suggest that this is “merely folk-psychology . . . not scientific.” But calling a descriptive account “folk psychology” will not make it disappear nor make it irrelevant for scientific psychology. If “folk psychology” is what psychological theorists hope to supplant by a more authoritative “scientific psychology” then the relation between the two is that of competing theories—it is not the relation between a description and an explanation of the same state of affairs. Those who speak about “folk psychology” do appear to consider it a theory *about* human behavior rather than a theory *of* human behavior.

The reference to “folk psychology” is misdirected in almost every respect. The descriptive account is a presentation of the *concepts* which are constitutive of a particular type of individual, i.e. a person. “Folk psychology” is properly applied to commonly accepted *empirical truths* about persons. It would be suicidal to include under this designation the concept of a person and exemplars of that concept, for without these there would be nothing that warranted the title of “psychology” at all.

There is a further error implicit in the usual references to “folk psychology.” A favorite pedagogical pastime for instructors in introductory psychology classes is to point out the “inconsistencies” in “folk psychology” as a preliminary to emphasizing that this is why we need psychological experiments—to find out what is really the case. An example of the alleged inconsistencies is “Out of sight, out of mind,” versus “Absence makes the heart grow fonder.” When the occurrence of such pairs in a single community is placed in a semantic, or truth-seeking framework, the conclusion that here is a contradiction seems inescapable. However, when the use of such locutions is

placed in a pragmatic or rule-following framework, that usage is entirely intelligible if the locutions are regarded as maxims rather than as statements embodying truth claims. Indeed, it is seen to be *necessary* for “contradictory” pairs to be available whenever possible, since the alternatives reflect the possibility of more than one course of action and the maxim gives the *policy* embodied in the course taken (as contrasted to giving *evidence* which justifies the choice). The assertion, implicit in many references to “folk psychology,” that people who are not scientists are inconsistent, stupid, and conceptually confused may be emotionally satisfying to some scientists, and there may be some evidence to support it, but in the present case a more parsimonious account is available.

If the present descriptive account is rejected as descriptive by a psychological theorist, this will, on the face of it, amount to an admission that his theory has nothing to say about human behavior except incidentally and indirectly. There is an alternative, however—the present account is not *stipulated* to be the only possible one. The alternative would be for that theorist to present a different descriptive account which (a) would be recognizable *as* a descriptive account, and this would involve some *already established* terminology, and (b) depart significantly from the dead-level ambiguity of such subject matter identifications as “behavior.” At the present time no such alternative is visible, but it would be a significant contribution for the present descriptive account if it brought out into the open psychological disagreements (if there were any) as basic as those about the subject matter of psychology, and if it stimulated efforts to formulate the alternatives as clearly and comprehensively as possible. Such efforts would form the core of Descriptive Psychology.

The misapprehension of the descriptive account of persons as presenting a (disguised) stipulative definition, and of “persons” and “human behavior” as (disguised) technical terms is a natural consequence of the earlier error of supposing that a description is a case of “reading off the features of what is actually there” and therefore is self-evident, infallible, and commands

immediate universal assent. Since, so the thinking goes, these features are evidently absent, the so-called descriptive account is merely an arbitrary statement of someone's concept of psychological subject matter and is therefore more accurately described as a stipulative definition or just another psychological theory. But the fact is that in the descriptive account, every effort is made to stay within the limits of established usage for the person descriptions which figure in the account. This, and the fact that the possibility of the present descriptive account *being corrected because it was wrong* is explicitly admitted, distinguishes the present account from a definition and from a psychological theory of the usual sort. It would be particularly egregious to conclude that if the descriptive account is mistaken in some respect this is evidence that it is something other than a descriptive account (e.g., that it is theoretical or definitional).

The converse error is to recognize the degree to which the descriptive account stays within the limits of established usage of person descriptions and to suppose then that what is being attempted is a philosophical analysis of person concepts. But there is a great deal of difference between (a) merely staying within the limits of established person-descriptive usage and (b) exploring the limits of such usage, minutely cataloguing its scope and variety. It is the latter which corresponds to philosophical analysis—the former is simply a reality constraint upon the effort to achieve a *descriptive* account of psychological subject matter. Far from being an analytic endeavor, the major effort and achievement of the descriptive account is that of organizing and classifying person descriptions in such a way as to exhibit perspicuously what we *already* take to be significant likenesses, differences, and relationships. That we already take them to be significant is demonstrated by the fact that we have a long-established terminology for the classificatory, or “structural,” descriptions in addition to having an established terminology for particular person concepts. One of the ends in view associated with the present descriptive account of persons is to ensure the significance of psychological theorizing and psychological investigation by specifying a subject matter which is

antecedently known to be significant as well as being what is generally considered to be distinctively “psychological.”

III. Descriptions and the Use of Concepts

One of the most central, yet most easily misunderstood, features of the descriptive account hinges on the difference between descriptions and concepts and between the application of a description and the use of a concept. We give a description when we say that X is such and such, or that X's are such and such. Giving a description leads us naturally to making truth appraisals—“But *is* X such and such?”; “Is X *really* such and such?” In the past, a good many readers have taken the Person concept, delineated in Part One and Part Two, as a simple description of persons, and it is on this basis that the issue of the truth of the descriptive account is raised (see Appendix A).

“When a description of X is given, all that remains to be established is whether the description is true.” This is a rough formulation of our attitude toward description, but it is accurate enough to highlight the kinds of difference between the use of the Person concept and the usual procedure of describing something.

The first difference is that the concept of “description” is much the narrower of the two, and in fact, in the relevant sense, it is *included* in the concept of “use.” Giving a description is one way of using a concept. To say that X is a such and such is to use the concept of a such and such in giving a description of X, and to use that concept in this way is to use it in only one of the many possible ways in which it can be used. The range of possible uses of the concept will be a subclass of the class of intentional actions (presented in Part One).

Confusion arises because we habitually take the use of a declarative sentence as the mark of giving a description. As a heuristic example, we may consider the following statement: “A physical body will move in the direction of an applied force.”

This sounds like a description of physical bodies. However, taken as a description of the objects which we typically observe and *treat as* physical bodies, the statement is obviously false. In all likelihood it is universally false in that no one has ever observed anything of the sort to occur (the “Nth decimal place” argument). However, universality is not an issue at this point (but cf. Part Four). As a description of the objects we observe, the statement is sometimes false because we apply forces to an object which is not then observed to move. It is sometimes false because the objects to which we apply forces do move, but not in the required direction. And, for example, when we observe the latter, we will say “There must have been other forces at work.” What we do not do is give up the statement as being false. That is to say that we do not treat that statement as a *description* in the semantic sense.

The reason we do not give up that statement is that we *have a* use for the concept of a type of individual that moves in the direction of an applied force. That use is not primarily to describe what we observe, but rather, to guide our behavior, for example in deciding what forces to apply to an object or in deciding when to stop looking for forces that might have been operative on a given occasion.

The use of that concept may be codified by a more elaborate statement: “A physical body will move in the direction of an applied force—unless there is another force acting on it and tending to move it in a different direction.” (Compare in Part Two and Three: “If a Person has a reason to do X, he will do it—unless he has a stronger reason for doing something else.” That is not a description, either.) And the latter may be replaced by an unqualified universal statement: “The motion of a physical body is in the direction of the vector resultant of all the forces in the field.” In the latter, however, the appearance of observational constraint has vanished. There is no possible observation which is logically incompatible with that statement. At most, an observation might lead us to conclude that there were as yet unidentified forces operating or that we needed a better way of computing resultants. (That we sometimes do

give up such universal statements is another matter entirely—cf. Part Three.) But we see too, that the first version, “A physical body will move in the direction of an applied force,” will do every bit as well as this third version—there would be nothing forced or unnatural about *using* it in exactly that way rather than as a description.

We have a use for the concept of a type of individual that moves in the direction of an applied force because we have ways of behaving (including those we call “applying a force”) which qualify as having treated an individual as being of this type. We have only one sort of fact here, not several. To say that on a particular occasion (a) we are using that concept, or (b) we are treating something as being of that kind, or (c) that concept guides our behavior, is to say the same thing, and it is to say that thing literally and precisely, not either metaphorically or loosely. (The elaboration of detail that might be required to clarify the negative case of (b), i.e. treating something as being *not* of that kind, is not at issue here.) The range of occasions on which it is appropriate to say that we are using that concept is far greater than the range of occasions on which it is appropriate to say that we are using that concept in giving a description of something. Both would include saying “The particle moved in direction $\Theta_1\Theta_2\Theta_3$ under a force of N dynes,” but only the former could include hitting a home run or asking “Which way did it go?” To repeat: giving a description only *exemplifies* a certain *kind* of use.

Since the qualifications on that moving object could be (and of course, are) extended to include all of physical theory, it should be clear that the *use* of theories by scientists is simply an instance of the *use* of concepts by persons, though it would not do to say that theories are concepts.

In general, an observer, O, describes a phenomenon, P, as “P.” The physicist who describes particles, the biologist who describes nerve cells, and the psychologist who describes response acquisition exemplify this general form of human behavior. To date, no scientific description, “P,” of some scientific object of study, P, permits the description of one particular

object which calls for scientific study, namely the use of “P” by the user of “P.” One reason for this is not far to seek. Our scientific behavior is guided by a concept of “objectivity” which makes it a virtue to be able to formulate descriptions without reference to people. For most sciences it is not specifically deleterious to let the person remain “understood,” since P, their subject matter, does not include the use of concepts by persons. Here the major disadvantage of failing to mention that the application of scientific descriptions by scientists is a case of the use of concepts by persons is that, failing to mention it, we fail also to keep it in mind, and then that fosters the illusion that we have a kind of knowledge of the world that is independent of people and their activities. It is as though, having seen that the rules of chess and descriptions of chess games could be formulated in terms of “Black” and “White” and without any explicit reference to players, we then concluded that we had finally achieved an objective description of the world independently of people and that chess is part of what the world would be like even if there were no people. With talk of physical bodies the difficulty is neither so simple nor so obvious as with talk of chess, but it appears to be the same difficulty.

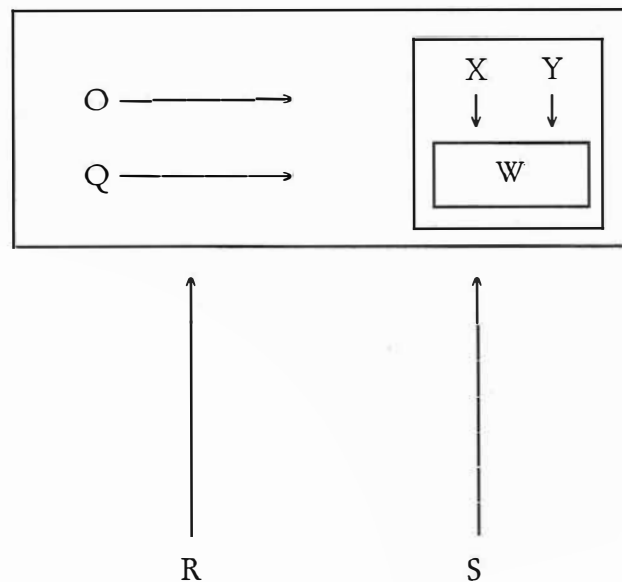
There is one science, however, for which this phenomenon has a particular significance. For Psychology, the study of human behavior, it is *prima facie* a mark of failure if its descriptive resources do not permit the description of a very significant form of human behavior, i.e. giving psychological descriptions and using psychological concepts, and more generally, giving descriptions and using concepts.

Here, then, is the second basic difference between (a) the usual notion of applying a description to a phenomenon and (b) the use of the concept of a Person, which is delineated in Part One and Two and is the major constructive contribution of the present descriptive account. In contrast to the use of physical, biological, and neurological concepts, and in contrast to the possibilities afforded by currently available theories in psychology, the descriptive account, exemplifies the *use* of the concept of a Person in providing an account of P (human behavior)

which is at the same time an account of the use of “P” by the user of “P” (which is equally a case of human behavior).

Perhaps the clearest illustration of the significance of this difference lies in the contrast between the present descriptive account and other accounts of human behavior in regard to the phenomena of “error” and “individual differences.” Figure 1 is designed to provide the relevant context for discussion.

Figure 1. Inter-observer Comparison



W a phenomenon of some kind

X a person who observes and describes W and treats it as being of that kind

Y a second person like X

O a person who observes and describes X and Y and treats them accordingly

Q a second person like O

R a person who observes and describes O and Q and treats them accordingly

S a second person like R

X and Y, being behaving human beings, exemplify the subject matter of psychology. If O is a psychologist he is likely to apply a certain kind of description (in terms of “laws of behavior”) to X and Y. He will discover that his description of X is different from his description of Y. He will then search for “determinants” of these differences and regard his task as unfinished as long as X and Y have different descriptions. (Compare: “The laws of motion apply to all physical objects,” with “The laws of behavior apply to all behavior.”) However, if O has been socialized by reference to a slightly different set of professional standards he will take individual differences as a “given” and will be devoting considerable effort to the effective classification of such differences. As it happens, there is no end to classificatory schemes of the kinds which have been proposed. Nevertheless, it is likely that O will use one or another psychometric scheme in the hope that the measurement it permits may be used *eventually* to detect those determinants, and of course, in the interim it can be used to guide behavior in “practical” decision-making contexts. As it happens, schemes for classifying individual differences by and large involve the use of concepts which are quite different from those which figure in “laws of behavior.” Not too surprisingly, “law-like” concepts and “individual difference” concepts have been taken to be competing alternatives in causal explanations. Nor is it surprising to find efforts to formulate one sort as being “really” of the other sort (e.g. Allport’s traits and, conversely, the psychoanalytic formulation of character structure as a “binding” of energy). Such efforts are not notable for either plausibility or *effective* coverage of the other domain.

The kind of individual differences which are ascribed to X and Y by either O or Q are intolerable when it comes to O and Q in their descriptions of X and Y. If O and Q differ in their descriptions of X and Y, then R (a psychologist, or possibly a philosopher) will conclude either (a) that O and Q are studying a different subject matter or that (b) either O or Q, or both are in error, for there must be a *true* story to be told

about X and Y about which all (qualified) observers will agree, or (c) et cetera.

As it happens, R's description of the differences between O and Q involves the use of still a third set of concepts (philosophy of science, methodology) which is neither of the kind used in formulating laws of behavior nor of the kind used for formulating "individual" differences among X, Y, and others of their kind, nor yet of the kind used by X and Y for describing themselves and others differently. And so differences between R and S, when they describe O and Q differently, are of still a fourth kind (metaphysical? cf. Appendix A).

In the light of the prior discussion of the use of concepts, there is nothing intrinsically puzzling or disturbing about this state of affairs. Certainly several types of concept are to be found here, and several kinds of individual differences. But then each type of concept has its characteristic use, and the uses are obviously different as noted above, and so what of it? Indeed, there are all manner of differences concealed in "the differences between X and Y." For X and Y represent both all the other sciences and the vast range of nonscientific activities, so that it is not four types of concept, but an indeterminately large variety that we are dealing with here. Of course, we have known this all along.

And of course, certain further conclusions will follow: First, all our systems of "laws of behavior" are fundamentally inadequate, and necessarily so. Second, our systems of descriptions of individual differences are fundamentally inadequate, and necessarily so. Finally, our "philosophy of science" is in all likelihood irrelevant at best and perniciously misleading at worst with respect to psychology, since it is inductively based on a range of cases (*other* sciences—as noted previously, its relation to psychology has been prescriptive, not merely observational) which differ fundamentally from any adequate study of human behavior (i.e. in the need to account for the use of concepts) in a way in which they do not differ from one another. Our "lawful behavior" systems and our "individual difference" systems are inadequate because there is a significant range of human

behavior and individual differences in such behavior which each must ignore, as is illustrated by the stratification of the X-Y, O-Q, and R-S pairs.

A reasonably decisive “quick test” of this conclusion with respect to behavior theories is that no such theory can be applied to the use of that theory as such. To characterize the user’s behavior in terms of his own theory is to *explain away* his behavior, not to explain it. For example, the psychoanalyst has a theory of ego function and object cathexis, and he uses this in deciding what to say to a patient. Let us suppose that on a particular occasion his decision is a sensible one and the course of action he undertakes thereupon is successful. There is no way to give an account of *that* phenomenon by recourse to psychoanalytic theory (see Wick, 1965; and Griffiths and Peters, 1962 on this point also.) In the latter there is a place for ego function, secondary process, object cathexis, and instinctual satisfaction, but there is no place at all for decision, sensible judgment, and successful action. It is not that a successful action is an exemplar of “instinctual satisfaction” or any other psychoanalytic concept (for example, a successful action may be a case of instinctual *frustration*). Rather, it is *we* who treat *this* successful action as a case of instinctual satisfaction (if we are psychoanalysts), and we could not do this in the way we do if we did not already have and use the concept of a successful action independently of psychoanalytic theory *in toto*. Thus every *use* of psychoanalytic concepts presupposes the use of a different, non-psychoanalytic set of concepts by a user necessarily described in nonpsychoanalytic terms. Hence the use of psychoanalytic concepts is inadequate to provide a general account of the use of concepts, hence inadequate to provide a general account of human behavior. And it is no different—or likely worse—if we substitute S-R theory, dissonance theory, or any other psychological theory in place of psychoanalysis.

It is commonly assumed that to give an adequate account of a phenomenon (e.g., making a sensible judgment) it is enough to be able to redescribe instances in terms of the particular conceptual system (e.g., psychoanalytic theory) which is involved.

It is in this way that we “generalize” our behavior theories (cf. Part Four), and it is in this sense that such theories have been claimed to give a *general* account of human behavior. But if that were all that was involved, then we might as well advert to the more elegant and classic cases of “All is one,” “Everything is either red or not red,” and “What happened was the will of God.” With such locutions and their systematic elaborations we can also redescribe all instances of human behavior, and do so more perspicuously and economically than by reference to psychological theories.

However, what is required for an adequate account of a phenomenon is an account of *both* relevant differences and pertinent similarities. Cases of sensible decision and cautious decision may be redescribed as cases of ego functioning (etc.), or of “not red,” but the *relevant* differences between them are not differences of ego functioning—instead those differences depend on certain standards which have nothing to do with psychoanalytic theory. This is why to give a description of either decision in terms of ego function (or habit strength, etc.) and to say that this is what is *really* happening is to deny the validity or relevance of the distinction between sensible decision and cautious decision. To do this is neither to contribute to our use of concepts such as “sensible” and “cautious” nor to explain the phenomenon which we thus describe. Rather, it is to make a crude preemptive bid for the use of one sort of concept rather than another sort. Successful use of a different sort of concept is not explanation. For example, astrology and alchemy have been *superseded* by astronomy and chemistry, but the latter do not explain astrological or alchemical phenomena. And if we sometimes deal successfully with a case of cautious judgment by treating it as a case of defective ego function (or as a case of economic behavior or whatever), we have not *thereby* explained or better understood the cautious judgment.

The conclusion appears to provide a dilemma with respect to theory and explanation. In terms of the previous distinction between theory *of* and a theory *about* Q, where Q is a phenomenon of which we have independent knowledge and a separate

description which—in effect—defines the phenomenon. But now it seems to follow that no theory *about* Q is possible at all, that if we meet the condition that Q is separately and definitively describable, then any other descriptive system will, when applied to instances of Q, be only a different way of talking, but not an explanation. It would follow further that the only cases which are cases of genuine explanation are those which occur *within* a theory of P. To this, we might answer, “No, but . . . ,” or, equally, “Yes, but . . . ,” and either would require an explanation.

The immediate conclusion is not that explanations which bring together two descriptive systems (“P” and “Q”; “sensible decision” and “ego function”) are impossible, but rather, that no complete and explicit description of such a state of affairs could be given without reference to a person whose use of “P” and of “Q” permits him to use “P” to give himself (or someone else) an explanation of Q. The metaphorical and elliptical aspect of “‘P’ explains Q,” is emphasized if we compare it with the entirely analogous “‘P’ asks a question about Q.” Without people, nothing explains anything. In most cases, as noted above, no difficulty in principle arises from leaving the contribution of the person “understood.” (See also the discussion of “partial description” in Part One.) Psychology was identified as the exception just because it comprises the study of human behavior and the use of concepts by persons does fall within this scope, hence the latter is part of the subject matter and cannot be left *merely* “understood” in any adequate conceptualization of human behavior. Thus, one of those two answers is, “No, but it *is* the case for Psychology: an adequate theory *about* Q (an explanation of Q, Q being some form of human behavior) which is merely a theory *of* P is impossible.”

Accordingly, any adequate psychological theory, “P,” which is to explain a psychological phenomenon, Q, must involve a conceptualization of the use of “Q” (no less than of “P”) by persons (note that if “P” is a theory *about* Q, Q is independently known and separately describable, so “Q” *has* a use). But

if the use of “Q” is conceptualized as part of P, then Q is describable within “P,” and then, simply, Q is part of P and “Q” is part of “P,” and so P is a theory *of* Q. Ordinarily, this would come about by virtue of “P” being a theory *of* a set of things, P’ which included Q, with the user of “P” not knowing that it was so or how it was so. To arrive at an explanation of Q would in this case be to arrive at the point of being able to show Q as being part of P (or as a specific part of P), and to understand Q would, correspondingly, be to arrive at the point of being able to treat Q as part of P (or as that specific part of P designated in the explanation). And so “P” would have a descriptive use relative to Q. (Appendix B indicates why no restrictions relative to our present practices are thereby introduced.) Thus, the second of those two answers would be “Yes, but for Psychology *that* would not prevent us from giving adequate explanations of *any* of those phenomena (human behavior) which we have aspired to explain; nothing *prevents* us—we simply haven’t done it yet, nor have we really tried.”

We have not really tried, because we have accepted the stratification implied by a philosophy of science which casts science in the image of semantic theory. (Compare: “object language,” “meta-language,” “meta-meta-language” with “thing language,” “theoretical language,” “philosophy of science.”) As psychologists we have had to *learn* to be bored by otherwise disturbing facts, for example, the fragmentation of human behavior into disparate and incommensurable languages and subject matters the coordination of which requires a mysterious, off-stage entity, i.e. a “scientist” or a “philosopher” or, more generally, a person: “Psychology ignores the person because Science is objective and abstract.” “Psychology is piecemeal because it is empirical and complex.” (But we think wistfully of another Newton.) “Better theorizing will do the job.” (Or perhaps, better measurement.) “When we know more about neurology (or genetics) then it will be different.” But how could it be different *then*, except by accident?

By virtue of its widespread acceptance, the semantic model and its implications have been grafted onto our common practices and expectations with respect to the giving of descriptions. The present descriptive account is a rejection of that model. It is a way of being disturbed, rather than bored, by the view of Psychology as a Rube Goldberg collection of basically inadequate fragments made respectable by its dependence on more substantial disciplines. It is a way of treating that state of affairs as undesirable rather than explaining it away as inevitable. It is a way of trying to change that undesirable state of affairs by presenting, via the concept of a Person, a *single* conceptual system which *does* provide a general account of all human behavior, including the X-Y, O-Q, and R-S kinds, in which relevant differences of all kinds are genuinely accommodated rather than being explained away. It is hardly to be expected, therefore that the descriptive account is a “description” in the highly simplified technical sense associated with semantic theory. “Description” here must be understood as a *pragmatic*, not semantic conceptualization. Roughly speaking, a descriptive account of X is the most conservative available account of X (but see also below). And roughly speaking, the contributions to a single general account come about in the following way (see also Appendix B):

(a) The delineation of intentional action in Part One introduces the use of concepts as an essential *feature of* human behavior (rather than, e.g., as a special kind of behavior or as the inner cause or hypothetical “determinant” of “visible” behavior). It also provides the equivalent of “the laws of behavior” (the demonstration of the equivalence is illustrated in Part Four and Five).

(b) The delineation of the Person concept in Part Two provides the integrated conceptual system which is used in describing individual persons and for identifying individual differences among persons. Since intentional action is the uniquely important element in this conceptual system, there is no forced division, and no competition, either, between general (lawlike) features of behavior and individual behavior—on the contrary,

each contributes something essential to the intelligibility of the other.

(c) The further discussion of the status of the Person concept and the interaction of Persons in Part Two exhibits the reflexive character of the Person concept in providing an account of its own use. Given that the use of concepts is a logical component of the concept of intentional action and that the latter is a logical component of the concept of a Person, we need only to add that the use of a particular concept, i.e. the concept of a Person, by an individual is what is required in order for that individual to be a Person. *With this specification, the basic formal requirements for an adequate general account of human behavior are met.* If P does use the Person concept (if that concept guides his behavior) then he will qualify for recognition as a Person by another individual, O, who is also using the Person concept. It is because the behavior of both P and O is guided by the same concept that the behavior of each is in response to each and is relevant to each. Thus, human action and continuing interaction are exhibited as being mutually intelligible among Persons and actually possible for them without any implication that there are underlying causal processes which *produce* the behavior or that there is something called “the objective truth” about a person which we have to guess at (but never really know) in order to have an effective basis or a rational basis for dealing with him. (Recognizably, the first of these two *non sequiturs* is the Excalibur which theoretical-experimental psychology has aspired to wield, and the second is the Holy Grail that eludes pursuit by psychometric-experimental psychology). Given this as the paradigm case (see below), the remainder follows.

The general view presented of the interaction of Persons is that person descriptions are logically “richer” or “more powerful” than any other kind, hence cannot be empirically pinned down through the use of other kinds of description in which we might formulate our observations. The dilemma is the same as the one we face (1) in trying to solve three equations for four

unknowns and (2) in having to make a move in chess, considering that the rules of chess do not determine that any particular move must be made. Note that the hopelessness of (1) in contrast to (2) is generated by the notion that there is a unique, *correct* answer. It is the logical discrepancy between types of descriptive resources which is *prima facie* incompatible with both underlying causal processes and "objective truth" formulations. It is this discrepancy which *requires* a contribution by an individual observer and makes possible different contributions by different observers. Since the participation of Persons A, B, and C in human interactions with one another are taken to be consequences of the person-descriptive appraisal of each participant by each participant, human interactions are seen as a multivocal lock-and-key phenomenon rather than a univocal billiard-ball-collision phenomenon. Hence the mutual implication of "lawlike" and "individual difference" descriptions of human behavior, codified in the paradigms of intentional action and of the Person (Part One and Part Two). In this formulation lies the potential for a deterministic account of human behavior, but it would be of a different kind than that to which we have aspired. Hence the characterization (Appendix A) of the present formulation as Copernican and relativistic as contrasted to our current monolithic building-block picture of the world. In this light, our present insistence on "observer agreement" as a condition for "objectivity" (see below) is seen as a vestigial anthropocentrism, for in a relativistic framework it amounts to the proposition that there is a *correct* point of view or that there is a *unique* individual difference description which is the *right* one. That the present formulation of Psychology as an autonomous science is also an insular approach may be illustrated by contrasting (1) present formulations of the relevance of genetics to human behavior as a case of "accounting for K percent of the variance" of particular classes of behaviors with (2) the formulation of person-descriptive individual difference parameters as discrete, identifiable loci of genetically "determined" variability and the mode of contribution of each such

parameter to human behavior as the key “mechanism” of natural selection in human populations; in conjunction with the potential for the computer simulation of human activities implied in a previous report (Ossorio, 1964) and dealt with explicitly in a forthcoming report. We may conclude that by virtue of its autonomy, the Person concept formulation generates *increased* potential significance for cross-disciplinary investigation.

So rather than being a simple description of Persons, or even a general description, the Person concept *codifies the logic* of descriptions of Persons. Since the basic phenomenon is the use of the Person concept rather than something called the “meaning” of “Person” or something else called its “referents,” the use of the Person concept is the most that *could* be described. It is not as though there first are persons and then we “read off their features” or discover the appropriate descriptions for them (even children, in growing up, learn these rather than discover them). But also, it is not as though we first encountered something called “the use of the Person concept” and only later discovered what it was. It requires the use of the Person concept to recognize its use and to say what that use is, hence the “reflexivity” of the Person concept. To say what that use is is not a *mere* description, in the semantic sense, but carries authority, and this, too, is a function of the reflexivity of the Person concept.

Compare: An observer who describes the rules of chess describes a set of authoritative statements, for the rules determine what a description *must* conform to *if* it is to be a description of a chess game and what a person’s actions must conform to if what he does is to be a case of playing chess. That kind of authority goes with what is described as “the use of the Person concept.” In the case of chess, we may agree that the rules of chess have that authority, but we may question whether the observer has described them correctly, and if he has not, then neither does what he described carry that authority. And the observer himself may be tentative or even diffident in his formulation. In contrast, since the description of the use of the Person concept is itself a case of the use of the Person concept,

the description itself must have the same authority as what is described. It is, because only a person who could claim that authority (intentional action requires both the relevant concepts and the *competencies* necessary for their use) would be in a position to give that “description,” that the only intelligible presentation is an authoritative one—there is room for disagreement, or revision, or for silence here, but not for diffidence, or tentativeness.

In this connection we may consider the status of the publication of the final product of a constitutional convention, assembled under the laws of the land for the purpose of systematizing the laws of the land in a new constitution. That product would be senseless if presented as the *opinion* of those men that this was in fact the law of the land. It would be equally senseless and much more objectionable if it were presented as merely those men’s *decision* that this was to be the law of the land. (Compare: “I am the law,” with the theorist’s “Words mean what I want them to mean—no more and no less.”) Rather, it would have to be *presented as* the law of the land, with the presentation itself, no less than the codification, carrying the authority of that law by virtue of (a) being subject to that law and (b) the law being what it was, i.e. one which legitimized a constitutional convention. This analogy may come as close as anything else to summarizing how it is with the presentation of the Person concept. The frequent appearance in the presentation, of the locutions “codify” (e.g. “language codifies what people know how to do”) and “formulate” reflects the pertinence of this analogy, and it reflects, too, the fact that such apparent alternatives as the “descriptive–prescriptive” polarity is too simple, equivocal, and overly specialized to provide the descriptive resources required for general use in a pragmatic framework.

Because it is not simply descriptive, the Person concept formulation more resembles an axiomatization of references to human behavior than it does a general description (which would only be one kind of reference), and it does so for both first person and third-person references. For first-person use, the use

of the Person concept is more aptly compared to the use of a maxim, whereas the third-person use is more easily assimilated to the use of axioms, and this is part of the complexity of the nontechnical concept of "description." (Compare: The chess player need not use the rules of chess to say what it is he does, though he may, and others may, but he *must* use the rules in this way: he *follows* them. What others describe and what he does is *the same*.)

Thus, if we made use of our familiarity with axiomatic systems in order to facilitate an understanding of the Person concept and its presentation, we should have to say at once that as an axiomatization it differs from all our other axiomatizations in that it axiomatizes its own use. Because of this, its use in no way depends for its intelligibility on any of the semantic and *informal* pragmatic props provided by philosophers of science for the users of other conceptual systems (e.g., other sciences, current psychological theories) which are fundamentally incomplete in this respect. On the contrary, one of the uses of the Person concept is as a philosophy of science, for after all, the various sciences are simply various forms of human activity (cf. Part Three), and the Person concept formulation subsumes the relevant differences as well as the similarities.

(d) As soon as the distinction between giving descriptions and using concepts is clarified, and more particularly when the systematic *use* of individual difference concept is brought into play, the concept of "objective" and the methodological status of "observer agreement" in the practice of psychological science appear in a new light. For in the general context of human behavior it is the use of precisely those individual difference concepts which permit us to understand one another and interact effectively with one another *without having to agree with one another*. Finding such use to be implied by an objective, general account of human behavior immediately raises the suspicion that the requirement of observer agreement in the way we have understood it is simply part of the price paid for the absence of an adequate account of human behavior. The discussion of psychological research in Part Three suggests

that simple, explicit observer agreement is best regarded as a simplified limiting case, that although we could not carry on as we do if we never agreed in this way, the amount and kind of agreement required for carrying on various activities differs greatly from one activity to another and is never to be regarded as a poor substitute for an ideal of perfect agreement. The linguistic research used in Part Three to illustrate the different flavor of Person concept methodology in psychological research provides some sobering *empirical* evidence in support of this view. An effort is made in Parts Three and Four to illustrate and characterize a mode of communication (featuring maxims and decision messages) which offers some prospect of reducing the requirement of agreement in whatever degree is made possible by our current skills and psychological acumen. This is not a luxury. It is not that requiring agreement, though possibly uneconomical is at least a way of “playing it safe,” for the evidence presented in Part Three strongly indicates that in actual practice, requiring agreement may *prevent* experimental access to significant psychological phenomena. In light of this, a new premium is to be placed on rigor and sophistication in methodology and psychological acumen for an evaluation of the agreement requirement in particular cases, in contrast to our present near-exclusive valuation of expertise in the computational and procedural technology of experimentation.

(e) one of the differences between giving a description and using the Person concept is that the latter generates an endless number of descriptions in much the same way that a recursive definition of “sentence” generates an endless number of sentences (another point of resemblance to an axiom system). Of particular interest and importance in this connection is the technical device of paradigm case formulation. Briefly, the latter consists of giving an account of a “complete” or “standard” case, which is frequently relatively complex, and then using this as a basis for describing other cases by reference to how they differ from the paradigm case or how they illustrate it. Frequently, the reason for using the paradigm case in this way is that it is the one which we know *by observation* and understand best,

because we learned about it first-hand, and so it *can* be used to understand other cases. In any case, the paradigm case approach is in most respects in direct contrast to an atomistic approach in which we try to build up complex cases out of elementary constituents.

The paradigm case approach permits *the same concept* to be used in describing and understanding a range of phenomena *for which no single, simple description can be given*. The difficulties in giving descriptions of human behavior which are both general and nontrivial are well-known to both psychologists and philosophers. They are genuine difficulties, which require for their resolution descriptive resources of the kind provided by the paradigm case formulation. Those who have been unable to treat the Person concept formulation as anything but a putative “statement of fact” have characteristically been quick to challenge its truth by reference to proposed counter-examples which apparently do not satisfy the “simple description” given in the descriptive account. For example, the analysis of “basic human needs” as *derivable* from the concept of intentional action, hence nonempirical (Part Two), was illustrated by a typical list of “basic human needs” which included “the need for security.” The latter has been challenged on the grounds that some men court danger, hence it cannot be the case that all men need security. This is fairly well comparable to challenging the truth of the statement that “a physical object moves in the direction of an applied force” by pointing out that some physical objects are not observed to do so. (Compare: “There must have been another force operating,” with “He must have had a special reason for doing that.”) To date no “counterexample” has been suggested which is not disposed of readily by undergraduates having an elementary grasp of the explicit use of the Person concept. Ironically, it is generally the case that the stipulated conditions which generate the apparent counterexample (another example, the paralyzed man who “knows how” but cannot demonstrate it) are precisely the conditions which provide the point of application of the paradigm case

technique—in effect the construction of the apparent counterexample is itself an exercise in the application of the paradigm case technique.

It is because the contrast between standard and nonstandard is already embedded in the concept of a Person that most of the counterexamples fail. (Compare: “In tennis the server stands behind the service line and” “That’s false—on some occasions the server has a foot over the line.” “Oh, but that’s a foot fault—that’s different. On a foot fault the server loses that serve—that’s the penalty.” And contrast: “In chess, the bishop moves diagonally. . . .” “That’s false. I saw a game the other day where the bishop moved straight ahead.” “Then it wasn’t chess you saw.” Finally: With Persons, it is as in tennis rather than chess. With Persons, nonstandard “moves” are the rule, not the exception, and it is our individual difference concepts which determine their logic and permit us to deal with them.)

It is because the contrast between standard and nonstandard is part of the concept of a Person that paradigm case formulations are an effective and economical resource for presenting the concept of a Person. *That the presentation depends heavily on paradigm case formulations*, for much of the working out of detail or accounting for special cases is left “understood.” Thus, it is highly unlikely that an understanding of the Person concept could be achieved if these paradigm case formulations were taken as simple, general, falsifiable descriptions in the semantic sense.

An important special case, introduced in Part One, is that of a “part-description.” Here the reference to the standard, or paradigm, case takes the form essentially of referring to something as an incomplete paradigm case, i.e. as being like the paradigm case, *but with something missing*. It is in this way that the Person concept formulation is able to subsume such diverse phenomena as animal behavior, infant behavior, abnormal behavior, and even the behavior of nonbiological individuals under the same account as that given for normal adult behavior. We simply identify these kinds of individual by reference to what they lack that the paradigm case Person has. The use of

part-descriptions is the methodological mirror-image of atomism. But the former makes it possible to reach any “level” of description without any appreciable tendency to minimize or exaggerate either similarities or differences, whereas it has been historically the case that atomistic approaches makes it virtually impossible *not* to exaggerate similarities and explain away differences (the “nothing but” phenomenon). Paradigm case formulation is therefore regarded as methodologically more conservative (see Appendix A).

And although it is not made explicit there, the concept of part-description is used in Part Four and Part Five in accounting for such plausibility and apparent success as our current theories and methodological formulations in Psychology have achieved. That is, such theories are shown (in more or less detail individually) to be *incomplete* formulations of the Person concept, linguistically disguised by virtue of being paraphrases couched in an engineering, biological, or phenomenological idiom. If the Person concept formulation is accepted as a description of the subject matter of psychological science, then our extant psychological “theories” of human behavior will be seen as having the methodological status of homunculus explanations. For the essential characteristic of homunculus explanation is that the phenomenon we observe is explained as the outcome of something “else” which works exactly the same way but is invisible (e.g., because it is “inside”), and that is what our “underlying process” theories of behavior come to relative to the concept of a Person. This, too, is a sobering thought, and for those who would consider it *prima facie* incredible, an explanation is given in Part Five of how this could come about without supposing that psychologists have acted perversely or foolishly. But a reflection on the recent appeal of “existential” approaches in psychology and a perusal of such *Zeitgeist* barometers as the *American Psychologist* in recent years may suggest that this, too, is something we have known for some time.

IV. Evaluation of a Descriptive Account

Several features of a descriptive account of the subject matter of psychology have been mentioned. It was characterized generally as an effort to provide a nontrivial description of the phenomena which provide a meaningful subject matter for explanation, elaboration, and investigation by psychological scientists.

It has been noted that the account which follows is “descriptive” in two ways. First, it is a description *of* P, as contrasted with a theory *about* P. Second, the account is given with a terminology that has an established use, and it is given in accordance with that use. In addition, one of the theses of Appendix A is that although some of the concepts and arguments presented in the descriptive account are substantially “the same” as some which may be found in certain philosophical contexts, the material which is presented here is not to be justified by reference to a putatively conclusive prior philosophical argument which has eliminated any other alternative, but rather by reference to its suitability for presenting the descriptive account and illustrating the use of the concept of a Person. Thus, it would be particularly desirable to indicate what considerations appear to provide appropriate bases for assessing the adequacy of the descriptive account.

One basis for such an appraisal, clearly, is the degree to which the descriptive account is descriptive in the sense of staying within established usage. (The danger of supposing that such an achievement is guaranteed by the use of familiar words is discussed in Part Four.) For example, to what extent does the concept of a Person correspond to what we would already have said a person was? To what extent do person descriptions, as identified in the present account, correspond to what we would already have said were the characteristically *psychological* phenomena? And to what extent does the concept of a Person get at what we would already have said were the *important* things to know about people?

A second basis for appraisal is the degree to which the descriptive account is seen to have (or have definite promise of) substantial advantages of the kind proposed above. That is, to what extent does it safeguard psychological investigation against triviality and confusion? To what extent does it facilitate the comparison of theories and support theoretical research by providing a *comprehensive* and *coherent* delineation of subject matter? (With respect to “cumulateness” and “substantive adequacy” no immediate appraisal is possible except in terms of consistency and plausibility.)

A third basis for the appraisal is the degree to which the subject matter identified by the descriptive account *includes* the problems, activities, and theoretical efforts which have gained acceptance as belonging to the technical subject matter of Psychology. Relevant material is presented in Part Four and Five.

Finally, in the light of the previous discussion of the formal identity between a description of P, a theory *of* P', and a theory *about* Q, it should be clear that someone who rejects the descriptive account as *descriptive* is nevertheless free to take it as a theory *about* “behavior” or some equally ambiguous subject matter. In this case, the basis for appraisal would be the comparison of the descriptive account and existing psychological theories in regard to scope, substantive adequacy, economy of descriptive apparatus, methodological coherence, and an estimate of its empirical fruitfulness. A basis for concluding that the descriptive account does not suffer in comparison with existing theories in these respects is discussed in parts Three, Four, and Five.



PART ONE

Intentional Action

In the recent history of psychology there have been a variety of methodological disagreements. One major current issue has to do with the feasibility and the appropriateness of giving a central place in psychological theory and clinical practice to concepts such as intention, self-concept, self-actualization, expectancy, and belief. (Such concepts will be referred to as "person concepts," and their application or linguistic form as "person descriptions.") What is found objectionable about person descriptions is that person concepts are so vague and subjective that their scientific use is incompatible with the scientific status and standards of psychology, and therefore inappropriate.

On the other hand some clinicians and others have taken a positive stand on the matter. For example, Mischel (1964) has presented a rationale for a rule-following model of human behavior by reference to which we understand an action by seeing its *choice* as *intelligible*. This model is contrasted with the predictive, nomothetic model in which we understand behavior by seeing its *occurrence* as *predictable*. However, his explication of the logic of clinical activity seems likely to perpetuate the perplexity and dissatisfaction which it was designed to alleviate. The difficulty is illustrated by juxtaposing three of the points he makes:

(a) The clinician has the *empirical* task of construing the client's constructions (Kelly's theory [1955] is used as the primary vehicle for Mischel's presentation).

(b) "For it is not the clinician's criterion, but the patient's own construal (i.e. his use of rules) which determines whether what he says on one occasion is really similar to what he says on others."

(c) "It is not logically possible for the facts ever to falsify my construct—not even subjectively." (This goes for both client and clinician.)

Although none of these points is here taken to be literally incorrect, it is difficult to see how they could do otherwise than

to mislead a psychological audience whose professional and historical bogeyman is the problem of "other minds." Because the picture which is evoked is that of each man standing isolated on the island of his own constructions: Since each of us is *the* criterion, and the only one, for what his own constructions are, we are likely to be mistaken about one another (point b), yet our constructions can never get at the truth of the matter, because the facts cannot falsify our constructs (point c), and yet we have to understand each other, and do it empirically (point a).

At about this point the properly socialized psychological investigator wanders off to look for a meter reading or a test result. Because the foregoing is just the dilemma of "other minds" all over again. And that is the same as the dilemma of "direct experience" and the same as the difficulty raised by person descriptions. (For example, my "real intentions" and my "real feelings" are just as elusive as my "constructions").

What keeps the problem of other minds on our consciences is that in ordinary circumstances, *everybody talks that way*, including psychologists, and although we sometimes encounter perplexities in the course of this kind of talk ("I wish, sometime, I could be *sure* what he thinks"), no one would seriously suggest that we now have anything other than person descriptions that can do the job that person descriptions do.

Not too surprisingly, our response to the bogeyman has been the creation of a mythology which comforts us by giving the bogeyman the status of something unreal. It is a linguistic mythology of a peculiar sort. The first principle in the myth is that "saying so doesn't make it so." Thus, just because in our talk, we mention, e.g. intentions, it does not follow that there are any such things. The second principle is that what is real ("what there is in the world") is what is both nonverbal and observable, i.e. physical objects and events, including human bodies and "behavior." That is not enough to support a science however. The basis for science is what is both observable and public. (Sometimes "observable" is taken to imply "public.") But

then, it must be communicable, hence verbalizable. Thus, “physicalistic language”; and now the game of science is on. But we notice that the effect of this maneuvering is to sever the connection between language and reality. Now, the connection can only be arbitrary. Words mean what we want them to mean. But we approve of that, because we want to use words in new and better ways. And so, dinosaur-like, we have developed an incredibly expensive and elaborate etiquette for *talking* about declarative sentences (for example, for calling them “true” or “false,” or “probable,” or “confirmed,” or “implied”). And with that goes an elaborate ritual for making up declarative sentences and then admitting them as counters in the game, which we have made primarily a theory game. Thus, paradoxically, what is commonly heralded as a hardheaded, empirical pursuit of factual knowledge, turns out to be almost exclusively a set of contemplative (observational) and verbal exercises.

But need not free ourselves of our language (which would be impossible in any case) in order to be linguistically more effective. What we have to do is to become better acquainted with it instead of pretending to order it about. Becoming better acquainted with it is not like giving ourselves or others a proof that something about language is the case. Something like that could be done only if we took a great deal for granted, and thus it would be to repeat past mistakes. As Carnap (1947, p. 206) points out, only within a *particular* linguistic framework is anything like a proof or verification possible. And, we might add, only within a general linguistic framework is the distinction between verbal and nonverbal possible.

Let us begin by taking seriously something which we evidently do not believe, though it is frequently said nowadays, and we usually give it some lip service: That we have no way of recognizing an object or an event for what it *really* is in contradistinction to what we could say about it (cf. Rhees 1954). We do not first notice a bare event or object and then decide what kind it is—what we notice is that such and such event occurs or that such and such object is there. And to notice this always carries implications—for example, implications as to

what that thing is not. This is one of the central principles of information theory, and it has sometimes been expressed by saying that “every description is theory-laden.”

If we are still taking language for granted when we hear such statements, we will take them for a lament—a lament over what we are lacking, namely a genuine knowledge of objective facts, and a lament over our limitations, namely the limitations imposed by our having only “theories” rather than genuine knowledge. And if we do not like that prospect, we may refuse to take such statements seriously. As is the case. But these statements are not a lament for what we lack—on the contrary, what they tell us is that *nothing is lacking here*. (A cup of coffee that has no firing pin is not lacking anything, either.) There is no difference between a mere description and a *real* description. And so we are not always in doubt about what a thing is.

But how can that be, since we can be *mistaken* (and isn’t that what science minimizes the risk of?) To be sure, we can be mistaken, but then, if we did not already have descriptions, we could not be mistaken in their use, either. This raises, the question of what it is to have a description.

We recognize a thing as an X only in the light of a descriptive system in which X’s are distinguished from Y’s and Z’s. But there are still two things left unsaid here. First, in order to have such a descriptive system, we must know how to distinguish between something which is a Y or a Z—it is not enough to have *merely vocal* distinctions among X’s, Y’s and Z’s. (On the other hand, it does not matter *how* we are able to make the distinction.) Second, it must make a difference to us whether a thing is an X *rather than* a Y or a Z, otherwise it is only a mock distinction—a gesture, a pretense. It is only a mock distinction unless there is something we sometimes *do* differently *when* it is a case of X and because it is a case of X rather than a Y or a Z—there must be something we know of that would constitute *treating something as* an X, and something *else* that would constitute treating something as a Y, etc. (Calling a thing an X *is* a case of treating something as an X but it is the “degenerate” case—if all cases of treating a thing as an X consisted

merely in calling it an X, there would be no such thing as calling a thing an X either. Thus, any account of language which makes it merely a tool for referring to what we observe is one in which the basic phenomena of language are left unexamined.)

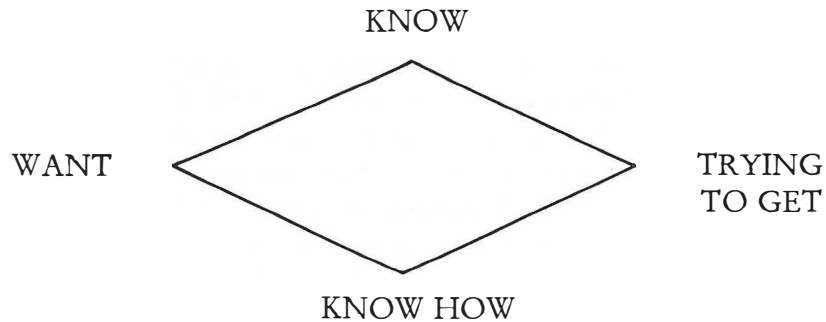
It is only when we have come this far that we have at all come to the position of being able to tell the difference between words and language, on the one hand, and, for example, noises, designs, or marks on paper, on the other hand. If we have not come this far, then it will be surprising if we do not confuse sounds with words and if we do not confuse making a vocal distinction or a mock distinction with the fact of having said something. There is more to be said, of course, and it will be useful to examine (a) the *use* of a descriptive system, taking as basic the case where we need not assume the prior or auxiliary use of some other descriptive system, and (b) some of the features of complex descriptive systems which will be relevant to later discussions. First, however, it will be appropriate to present briefly a descriptive system within which we talk about intentional actions. ❖

I. Intentional Action—a brief description

A paradigm for intentional action is shown in Figure 1. The paradigm, PI, is not a diagram of an intentional action (although used in this way it would have the merit of showing that there is not a direct connection between wanting and trying to get something.) Rather, the diagram is simply a convenient visual aid for representing “all at a glance” the descriptive system which accounts for the existence of intentions and intentional actions. A description of an intentional action is one that has the familiar form “X did A in order that B,” “X’s purpose in A-ing was to achieve B,” or a recognizable variation of one of these. (See Anscombe, 1957, for a more extensive and

rigorous account of the relation of “intention” to linguistic forms, especially “why” questions.)

Figure 1. PI: Paradigm for Intentional Action



To say that PI provides descriptions of intentional actions is to say that any instance of intentional action has five logically primary aspects:

(1) An intentional action is always something performed by a Person. (This does not lend itself to visual representation. The concept of a Person depends fundamentally on the concept of intentional action, which it includes, and so the terminology is introduced here, although even the initial presentation of the Person concept is not completed until Part Two. The capitalization of “Person” will be used to indicate an individual who represents an instance of the concept of a Person. When not capitalized, “person” will not imply that the individual is or that he is not a Person—that is left open.)

(2) The Person wants something, or has a reason for doing something, which the action is intended to achieve.

(3) There is an observable behavioral episode which constitutes an overt attempt to achieve the result.

(4) The Person knows something relevant to the action. At a minimum, he knows the difference between what it is he wants and other things.

(5) Finally, the overt attempt represents the exercise of some ability, skill, or competence. The overt attempt is neither accident nor coincidence, but rather, something the Person knows how to do—it is repeatable, with variations under various appropriate circumstances.

A distinctive descriptive system is one which cannot be translated into another descriptive system and cannot be replaced by another descriptive system. “Replacement” here is a pragmatic concept, not a semantic or logical one. A descriptive system is replaceable by a second descriptive system (or some combination, which would have the same result) when (1) a person who knows how to use both systems can use the second one in every case where he would normally use the first, (2) the use of the second marks the same differences for him on that occasion that the first one would (allowing for some *additional* contribution from the second system). And (3) the person knows ahead of time that he will be able to use the second in place of the first (for example, if he knows that on each occasion the second descriptive system provides the description of the *complete basis* for the use of the first); that is, there is nothing *ad hoc* or *post hoc* about the use of the second in place of the first. Replacement is a weaker condition than translation, and most of the historically important theses concerning scientific terminology have been theses about replacement (with the notable exception of “operationism” which involves translation). For example, “construct validity” involves replacement in the pragmatic sense, because the conditions for the application of a theoretical term on a given occasion (and also, the observations which verify the predictions which validate the application of the term) must be completely *statable*, and statable within a descriptive system which is other than the system to which the theoretical term belongs.

Given two apparently different descriptive systems, the assertion that one is translatable into the other or replaceable by it has no initial plausibility whatever—such an assertion always carries with it the burden of *demonstrating* that that is so. There has been a good deal of effort directed toward the

replacement of person descriptions, e.g., by physiological descriptions or physical descriptions. In spite of such efforts we have at the present time no reason whatever to believe that person descriptions are replaceable by any other kind of description (cf. Chisholm, 1955). Thus, until further notice which seems unlikely ever to be served (cf. Part Four), person descriptions must be accepted as being generated by a *distinctive* descriptive system.

II. The Use of the Concept

The use of a distinctive descriptive system such as is implied by the concept of a Person, or of a physical object, or of an intentional action, constitutes a skilled performance on the part of the user. The ability to perform correctly in these ways is not directly inherited—it is something we acquire by learning. We learn here as we learn to play a game—not deductively, not inductively, but by experience. The learning of concepts and skills precedes other kinds and is required for other kinds. For example, to learn inductively is, basically, to count cases, but that requires that we know what to count and how to count, and *that* is the sort of thing we learn only by experience.

If a conceptual system provides a distinctive type of description, the applications of such descriptions cannot always be the result of an inference. The basic cases will be precisely those in which no inference occurs, because the pattern of inference is a pattern which generates an infinite regress of the “vicious” kind. To infer is to proceed from premises, which are something we already *know about* (e.g., as a matter of evidence, definition, or assumption) to a further conclusion. If all knowledge were inferential, then those premises must themselves be conclusions drawn from still other premises, and the latter would need still other premises preceding *them*, etc., etc., etc. There is no beginning to such a series and so nothing can get started here—what

such a series provides is a *reconstruction* of the process of extending knowledge; it cannot be a description of what we do in acquiring knowledge. The all-inference situation does not even provide a basis for conviction or belief, since it can provide a basis for nothing whatever. (In some current cognitive theorizing, “inference” is used in such a way that the premises need not be something the person knows about. For example, the premises might be causal conditions of perception or frequency data from which “correct” responses could conceivably be *calculated* with a high degree of accuracy. This technical use of a familiar English expression has the disadvantage of obscuring the fact that on most occasions to say that someone has made an “inference,” in this technical sense, is to say that no inference at all has been made.) Thus, the “ground floor” of human cognition and behavior is not definition, not proof, and not inference machinery. It is, rather, the ability to recognize when something is so, and the ability to accomplish something. And the value of *that* is precisely: whatever difference it makes (information theory again).

If the paradigm, PI, reflects a correct analysis of intentional action, then it is something people use. And then there is a distinction made among “want,” “know,” “know how,” and “trying to get” types of concept and there are criteria for applying the corresponding descriptions. These criteria must be public. If it were a matter of one person’s say-so, nothing he said could *possibly* be wrong. But then, just because of that, he could not possibly be right, either. There would be no difference between his being wrong and his being right. And so he could *say* nothing, even to himself, and of course, we could not understand him either.

In general, the criteria for the application of person descriptions, including descriptions of intentional action, are not explicitly statable. If they were, the descriptive system underlying person descriptions would not be a distinctive one. It would be replaceable by the descriptive system(s) in which the criteria were stated. (Any conclusion which follows from X will follow from Y if Y implies X, and this is what would be the case on

each occasion where “Y” represents all those facts *by virtue of which* “X” is asserted on that occasion.) Even in those cases where we do speak of some evidence, Q, relevant to the appropriateness of a person description, the appeal is almost universally to agreement rather than to an inferential rule. That is to say, the things we bring in as “evidence” are brought in as a way of inviting agreement from someone who already has the ability to use person descriptions—it is not backed up by a rule of inference “Q implied X” (X being a person description) which could be used to demonstrate the validity of the judgment “X” to someone who had not the mastery of person descriptions. And if we call it an inductive inference, “Q, so probably X,” this will be like permitting the psychotherapist to “verify” an interpretation by using as evidence other, later interpretations—or earlier ones, for that matter (cf. Wisdom 1962). The only facts that are established in this way are facts about what we are inclined to say.

Thus, like any *distinctive* descriptive system, PI depends fundamentally not on verbal links to other kinds of terminology, but on the existence of people who *know how* to use it, who have mastered the criteria (have the requisite skill) for applying the concepts which are involved—a set of people in whose lives the application of the system has a place. The sufficient condition for there being criteria, statable or not, is that there should be general agreement in judgments in individual cases and that another person should be able to learn to make correct judgments of that kind (Bambrough, 1961).

A brief note on “criteria”:

(1) To say that there are criteria for the application of some description, e.g. “He is angry,” is merely to say that the concept is not a fictitious one—there are cases where “He is angry” would be correctly used, and there are people who would agree on that as well as on cases where “He is angry” would *not* be correctly applied, and it makes a difference to them whether or not a person is angry, and another person could learn to make correct judgments of this kind. “The criteria for ‘P’” refers to whatever is required in order for the application of “P”

to be fully justified (which is *not* to be confused with “true,” “known for certain,” or “highly probable”), and “statable criteria for ‘P’” refers to any locutions other than “P” which designate the criteria for P. In terms of any description other than “P,” what is required for the application of “P” to be fully justified will normally differ from one occasion to another. In general, there is no reason to suppose that the criteria for some P *can* be given in any way other than “P.”

(2) To say that a person has mastered the criteria for the application of “P” is simply to say that he knows how to use “P” correctly. (That is like saying that he knows how to do sums, or how to fix radios, or how to track a deer, or how to play chess.) Among other things, this implies that he can recognize when that description is fully justified, when it would be incorrectly applied, and when there is a question as to its applicability.

(3) The “criterial use” of a description is the kind of performance which demonstrates the mastery of the criteria for that description.

(4) To say that a person has mastered the criteria for “P” is not to say that he makes no mistakes and knows all there is to know about the use of “P.” That would be like saying that a person does not know how to speak English unless he knows every word in the English language and never makes a mistake in usage or grammar. So there are criteria for “has mastered the criteria for ‘P’,” and what will count as mastery of some “P” will reflect what is a *workable* requirement for that specific “P.”

(5) Without the foregoing, we could not raise questions regarding truth or evidence and we would have no way to settle them—there would be no such question. To recognize *what* would constitute evidence, to recognize how much and what kind of evidence fully justifies saying that it is *true* that he is angry—these are all part of knowing how to use the description “He is angry.” To establish some fact in evidence (which then involves the use of some other description) presents the same kind of situation as was the case for “He is angry.” If I have not

mastered the criteria for the application of that other description, then *that* will not be something I can think of as evidence or count as evidence. Thus, evidence and truth are special and secondary considerations for the behaving person and for the characterization of human behavior, whereas the mastery of criteria is a general and fundamental feature of both.

It is not on the basis of evidence that we count cases, and there is no evidence, either, that something is a pawn, or a person, or an action. Neither is there any lack of evidence—we are at the ground floor here. In the paradigm case, to call a thing a pawn, or a person, or an action, is an essential part of treating that thing as a pawn, or a person, or an action. The latter is the difference it makes to describe it as we do. There is constraint here, but it is one of ability, not evidence. Treating something as, e.g., a pawn, is something I know how to do—or else I don't. What I designate as a pawn may be something that would more commonly be recognized as a penny or a piece of chalk. But if I know how to play chess, I will probably have very little trouble in treating those things as pawns. Whereas, if I designate that mountain as a pawn, nothing that could be expected to follow would constitute having treated something as a pawn, and if I know people, I will also know what to count as having treated something as a person or as an action. "An action is what a person *does*."

III. Confusions and Resources in Complex Systems

The simplest use of a descriptive system is to provide a set of verbal pigeon-holes for independently occurring phenomena. It is a taxonomic use. Color names, compass directions, and time measurement provide familiar examples which approach a simple taxonomy. This is also the picture most commonly associated with information theory, i.e., the picture of N

independent categories to which an incoming signal may be assigned. Descriptive systems associated with complex, organized phenomena offer additional potentialities which may facilitate description greatly but sometimes also result in confusion. Two such features of descriptive systems are particularly relevant to person descriptions and the concept of a Person. They are designated here as “partial-descriptions” and as “part-descriptions.” Both stem from part-whole relationships in the complex phenomena which corresponds to the descriptive system as a whole. That complex whole will be referred to as the “primary context.”

A *part-description* is one which refers to the separate or apparently separate occurrence of something which is normally found in the primary context; what distinguishes a part-description is that the object or event which occurs separately cannot be *described* independently of a description of the primary context. For example, “the kind of remark only an idiot would make,” “the smell of bacon,” and “the corner of a brick building” are all part-descriptions. They all refer explicitly to the primary context in which such a remark, such a smell, or such a corner, respectively, would be found. What is not given is a direct, independent, “pigeonhole” characterization of the remark as such, and the corner as such. Yet each of these *can* occur in the absence of its primary context. The remark need not have been made by an idiot; the smell might have come from a bottle of perfume, and the rest of the building might have been destroyed or never have existed (e.g., a movie set). The important contribution of part-descriptions in the use of language is that they eliminate the necessity for undergoing a new set of learning experiences (mastering criteria) for every distinguishable element of the real world which can be talked about. **In summary: In a part-description that primary context is mentioned explicitly, whereas the “part,” though it occurs separately, is characterized only indirectly as the kind of thing which occurs in the primary context.**

A *partial-description* is one which refers to an object or an event which cannot occur except within the primary context; in

spite of this, the partial-description provides a separate, explicit characterization of the “part” without mentioning the primary context at all. Partial-descriptions would be unintelligible except in their systematic use. They are frequently “shortcut” expressions which would properly be replaced by more complete expressions. For example, “He bought Q,” is an elided form of “He bought Q from A by paying P for it.” “He bought Q,” is a partial description because nothing of the sort could happen if that was all that happened. One cannot buy a thing, or even appear to, except from a seller and for a price. Similarly, one could not play a trump if that was all that happened. Only in the context of an actual game could that piece of cardboard *be* a trump, and only in the course of a game could I *play* a trump. The important contribution of partial-descriptions is that they “streamline” our language by permitting us to drop out of the conversation whatever can be taken for granted. **In summary: In a partial-description, the “part” is the only thing that is mentioned, and the primary context, though it is a necessary accompaniment, is left “understood.”**

Part-descriptions are a potential source of confusion indirectly, because they lend themselves to symbolic use (cf. Part Two). Partial-descriptions lead more directly to confusion and perplexity as soon as we treat them as “pigeonhole” descriptions and forget that the *necessary* context for what partial-descriptions refer to is always “understood.” This is not to say that partial-descriptions refer to something fictitious. On the contrary, there is on each occasion an observable something to which the partial-description refers (e.g. a piece of cardboard that counts as trump, a set of motions that counts as a purchase). The observable something is the referent of the partial-description, not its meaning, and its meaning is defined by the *external* relationships of the referent, i.e. by the part played by the referent in relation to the primary context. (Similarly, the role of, e.g., a policeman, is defined by the relations of a policeman to other individuals in other roles—it is not defined by the individual characteristics of a given policeman.) Thus, when we

forget how much is taken for granted in using partial descriptions, and this is likely to happen if we are scientists, we *are* likely to treat partial-descriptions as pigeon-holes, and then we will ask what is it about the observable something that validates the partial-description. Whereas there is nothing about that observable something that validates the partial-description. (And if I own some furniture there is nothing about those observable somethings, those pieces of furniture, which validates the description “*my* belongings.”) So that, for example, to pick a known instance of a partial-description and use the description of *that* observable something as the “operational definition” of the partial-description would be pointless and misleading. It is not the kind of procedure that could be expected to lead to consistent results if one is interested in the phenomenon designated by the partial-description. (I can sit by myself and practice laying that card on the table all day long, but *that* will not be a case of playing a trump. And the way to investigate my belongings is not to examine everything that looks like furniture.)

Person descriptions, it seems clear, are like “trumps”—they are partial-descriptions. And because partial-descriptions are separable from the conceptual systems which give them meaning, one can learn to apply them in specific cases in the proper context without recognizing their necessary relation to their conceptual context, the concept of a Person. More than anything else, it is the translation of this ability into a “pigeonhole” investigation of person-descriptive phenomena that accounts for the “elusiveness” of person concepts and the perplexities encountered in dealing with them as psychological subject matter. What is required, then, in order to reduce the appearance of necessity in this linguistic shadow boxing is an explicit formulation of the concept of a Person. The major contribution of the present delineation is to permit the gross structure of the Person concept to become visible as a unit. It will be seen that both in the formulation of the concept of a Person and in the use of this concept in life situations the effective use of partial-descriptions plays a crucial part.

IV. Criteria for PI Concepts

The criteria for “want,” “know,” “know how,” and “try to get” concepts are permissive rather than prescriptive. That is, they select what is *prima facie* allowable rather than what is true or necessary. It is the PI system as a whole which comes into play in judging what is actually the case. The cases referred to below are intended to *illustrate* the different kinds of PI concepts and the different relevant criteria.

It should be made clear at the outset that it is the logical structure of the Person concept that is being illustrated here by reference to the more specific content with which we are familiar. Such content may be expected to vary on a cultural and subcultural basis, much as language does, although no simple parallelism between linguistic communities and Person concept contents is assumed.

WANTS

(a) Not everything that a person may mention is intelligibly ascribable to him as simply “what he wants.” I can be said to want *to go hunting*, without any further question being raised thereby. I can be said to want *a million dollars*, but only if I have a further end in view. Aside from the many further ends which would be like “to go hunting,” I might simply want *to have* million dollars, i.e., to be in the position of one who has as contrasted with the position of one who hasn’t got a million dollars. In the case of “a million dollars” we would have few qualms about treating it practically as an end in itself because it is so easy to think of many genuine ends toward which that could contribute. In contrast, I cannot without further explanation be said to want exactly two ounces of mud, or a twig of mountain ash three inches long, or the thigh bone of a hen turkey, or any of a wide variety of objects, events, or situations.

Some criteria apply to priorities among wants. These are generally less restrictive than the criteria for wants as such, but they are there. For example, I could be said simply to want to

smell that rose, or to save that child's life. I cannot, without further explanation be said to prefer the former to the latter or allow it to take precedence in a choice situation. In general, the further explanation will consist of adducing other ends or a further end in view.

The employment of criteria for wants and their priorities embodies a view of human nature insofar as such employment constitutes a delimitation of what a person can be understood as wanting without any further end in view. Such a view is not a *theory* of human nature—it is not something we could *discover* to be wrong. It is part of the concept of a Person, and the employment of these criteria expresses our standards for what it makes sense to say.

What it makes sense to say is what makes a difference in our lives—the social practices involved in the use of words are what give them meaning. If what we say makes no difference, then we have not said anything, and then there is no sense in asking, either. (“Is it five o'clock on the sun?” “Well, what time is it on the sun now?”) The grammatical resources of our language make it possible to merely *go through the motions* of saying something. (Roughly: Like going through the motions of designating that mountain as a pawn.)

(b) We have criteria for what a situation calls for, i.e., standards for judging when the situation in which a thing is done is *reason enough* for doing that thing: If I pass an acquaintance some morning and he says, “Good Morning,” that is reason enough for me to reply in kind. It *gives* me a reason, and I do not need a further reason, and if I reply in that way, he need not then ask, “I wonder what he meant by that?” If someone insults me that is reason enough for me to become angry, and if the insult is freely offered, it is reason enough to reply in kind. If I see an automobile bearing down on me as I cross Broadway, that is reason enough for me to jump out of the way.

To say that a situation gives me reason enough to act in a certain way is not to say that it *justifies* my acting in that way, although the latter is a frequent concomitant. Rather, it is to

say that one can understand, that it is the sort of thing a person would do in those circumstances. ("Shucks, m'am, any red-blooded American boy would have done the same.") Conversely, if I did *not* say "Good morning," or become angry, or jump out of the way of the automobile, and if I did not do anything else that had the same significance, *that* would require some further explanation.

There is some degree of asymmetry between "having a reason" and "wanting." Of the two, having a reason appears better suited for use as the fundamental concept, since it is easy to see that wanting something gives one a reason for trying to get it, whereas it is not so clear what it is that a person wants on those occasions when he has reason enough to do something. However, it appears that one could plausibly say what he wants on any specific occasion when he has reason enough to do something. Thus, in the following exposition, "wants" and "has a reason to do . . ." are used as alternative constructions in order to take heuristic advantage of the familiar motivational connotations of "want."

TRY TO GET

To try to get something is to engage in overt, *observable* behavior which (a) could be expected to result in having that thing; or (b) is the kind of thing that is done with the aim of getting that thing. If there is a camera in the car, then walking to the car qualifies as trying to get the camera. (It also qualifies as trying to get anything else in the car, or the car itself.) If there is a glass of water on the table, then reaching toward it qualifies as trying to get the glass of water. If I am confronted by a crocodile and I run away, this qualifies as trying to get away from, or trying to avoid, the crocodile.

Generally, "try to get" descriptions are clear cut cases of part-descriptions. The overt attempt, e.g., to escape from the crocodile is recognizable as such because it is recognizable as the sort of thing a person *would* do if he were *deliberately* trying to escape from the crocodile. We learn this kind of thing

by experience, and the experience required is simply the experience of some number of instances which were accepted as actual cases of trying to get. (The beginner will normally take at face value the veteran's statement, e.g., that "that was a perfect overhead smash" or "that was an evasive response," especially if the activity they are engaged in is the teaching-learning activity. And the child will normally accept what parents and other adults say, at least until he has a reason not to. It is also convenient in this regard to have some conventional expressions, e.g., facial expressions signifying emotions. These, too, give us *prima facie* cases.)

Trying to get a million dollars illustrates an important class of cases in which there is no single thing that could be described as "the sort of thing one *would* do" to achieve that end. But we can take any overt attempt or series of attempts, and judge whether it is of a kind which could be expected to bring one *closer* to the given end. For example, investing one's money would pass the progress check; robbing a bank or merely saving one's wages would probably be considered borderline; giving one's money away or becoming a beachcomber definitely would not qualify.

KNOWING HOW

To know how to do a thing is to have the capacity, the skill, the competence to do it. However, not all capacity concepts are "know how" concepts. Instinctive, reflexive, or in general, unlearned, capacities fall outside this range. (But native capacities developed through training do qualify.) To know how to do a thing is not merely to be disposed to engage in a certain sequence of motions. It is to be able to do so on appropriate occasions and to be able to adjust the performance in hitherto unpracticed ways to meet the exigencies of a particular occasion. (Think, for example, of knowing how to drive a car.)

In the paradigm case, a capacity is demonstrated through a performance. Sometimes more than one is needed, e.g., if luck or chance need to be ruled out (Compare: knowing how to climb a ladder, knowing how to do arithmetic, knowing how

to run a farm). Ordinarily, I will have some knowledge of my capacities and, for example, that will influence my choice of overt attempts, but that is another question. I may be entirely convinced that I know how to play chess, or tie my shoelace or conduct an experiment, but the *criterion* for whether I know is not my conviction, but instead, an observable performance.

KNOW

Here, it is important to distinguish between being aware *of* something and being aware *that* something is so.

To be aware of something (e.g., a chair, a dangerous situation, the green color of tree leaves) is to be able to participate successfully in the standard social practices which turn on the distinction between that thing and other things. Except that what is specifically not *implied* is the ability to say what that something is. How much one has to participate successfully in is given for particular P by the criteria for “aware of P.”

To be aware that something is so is to be able to say that it is so and to appreciate the difference it makes. It is to be able to engage in the corresponding social practice *including* being able to apply a certain description correctly. Thus, “being aware of something” operates as a part-description relative to “being aware that something is so.” When one is merely aware *of* R, one does just the sort of thing one would do if one were aware *that* R was the case. We do not have a second way of recognizing what one does when one is only aware *of* something and is not aware *that* it is so.

The distinction between knowledge and belief is not central to the present discussion. Roughly: If what I take to be the case is appropriately grounded in observation or evidence, then I know it: Otherwise it is merely something I believe. Knowing something is not the same as either having a strong belief or having a true belief (cf. Ryle, 1949).

V. PI as a Standard

PI provides the paradigm of intentional action. There is a *logically* necessary relationship among the quadruple of PI concepts (**wanting**, **knowing**, **knowing how**, and **trying to get**). It is like the relationship between buying, and selling—in our normal use of these terms, one implies the other. (And a pawn *implies* a bishop, rook, queen, etc., as well as a 64-square board and practices such as capturing pieces and checkmating.)

PI serves as a standard because any event, in order to be seen as human behavior, *must* be seen as falling within the limits of appropriateness determined by criteria such as those illustrated in the previous section. Any apparent exceptions must be merely *apparently* exceptions. (An apparently married bachelor must merely appear to be so—either he really is not married or he is not really a bachelor.)

To try to reconcile an apparent exception is to try to provide an explanation, and to succeed in reconciling the exception is to provide an explanation. To need or want an explanation for a person's behavior is not a permanent condition. The acquisition of knowledge may remove the need or it may create the need for explanation where none existed before.

"If I see a car bearing down on me as I cross the street, that is reason enough to jump out of the way." This is a situation which fits PI without apparent exception: What I want is to avoid the danger of the car, and that is something which a person might be said to want without any further end in view. My overt attempt is just the sort of thing one *would* do, having that end in view. What I am aware of is that the car is bearing down on me, which is something I could be expected to have observed, considering that I looked. And finally, there is no capacity involved in jumping out of the way that I have not demonstrated in performing the act.

"If I did *not* jump out of the way, *that* would require an explanation." Perhaps I simply stop and look casually up at the sky. Here, there is nothing I am overtly attempting except, perhaps, to see something in the sky. Since seeing the car bearing

down on me is *reason enough* for me to get out of the way, to say, e.g., “He’s looking for an airplane up there,” is not to give an intelligible account of my behavior. And to say further that, “He’s more interested in seeing the airplane than in getting out of the way of the car,” is still not to give an adequate explanation, because this order of priority would require further explanation. A different special explanation would be the following:

(a) Special knowledge—I recognize the driver of the car, know that the car has special brakes and can stop on a dime, and I believe that he is trying to frighten me;

(b) This is reason enough for me to want to frustrate his attempt;

(c) To stand in the road and show no fear is to do something incompatible with his intentions for me—it is the sort of thing one would do in order to frustrate that intention;

(d) To stand idly in the street requires no capacities that I am not demonstrating thereby (including, for example, the capacity to inhibit fearful impulses).

Now PI applies without apparent exceptions.

VI. Types of Action

Because of the quadruple differentiation of the concept of an intentional action there are many parameters which could serve as a similarity basis for classifying actions into types. Among the aspects which could be so used are the following:

(a) What the person is aware of;

(b) What he knows about;

(c) The basis for his relevant knowledge (e.g., knowledge vs. belief);

(d) What he wants;

(e) The presence of a situation which provides reason enough;

- (f) The function of the action (the further effects which are relevant to the person, e.g., related to his needs);
- (g) The discriminable features of the overt attempt (e.g., style, and omission vs. commission);
- (h) Present capacities, demonstrated or assumed;
- (i) Antecedent conditions—capacities acquired or demonstrated, and dispositions, acquired or demonstrated.

It is of greater interest, however, to use these aspects in combination. Proceeding in this way provides an effective means of reducing the appearance of a *Deus ex machina* operating in the guise of “provides reason enough.” More importantly, it permits a cogent formulation of some of the most important uses of intentional language. That is, cases that are explained by reference to fear, anxiety, guilt, anger, jealousy, etc.

For example, if we ask what *must* be assumed or known about a person in order for the attribution of fear to him on a given occasion to be *intelligible*, we find the following (Gosling, 1962):

- a) He must have learned to distinguish between dangerous and nondangerous situations, objects, events, etc. (And to know something as dangerous is to know it as something to be avoided.)
- b) He must have learned how to perform some actions which were effective in escaping or avoiding dangers. (This is not to say that he must be able to recognize or avoid all dangers.)
- c) He must have an acquired disposition to take steps to avoid danger in the absence of deliberation. The criterion for having such a tendency is to have exercised it in some past performances. (This third condition is what distinguishes fear from, e.g., caution or prudence.)

Thus, in saying “X did A because he was afraid of P,” we are saying that (a) he is aware of a danger; (b) he wants to avoid

it; (c) he knows how to engage in overt attempts to avoid danger, both deliberately and otherwise, and (d) what he is doing now is an exercise of that capacity—he is trying to avoid the danger he is aware of. In the simplest case, when the quadruple criteria are met, this is to describe X's action in a way that fits PI without apparent exception. (A more complex case is described below.)

This account makes clear why it is that "He acted fearful because he was afraid," is not a reification and why "X did A because he was afraid of P," makes X's action intelligible and intelligent instead of merely promoting the postdiction of a visceral event or a qualitative aspect of direct experience. It emphasizes the point that intelligent behavior is seldom accompanied by prior episodes in which the planning of the behavior takes place (cf. Ryle, 1949). It accounts for why we consider fear, anxiety, terror, apprehension, etc., to be the same *kind* of feeling. (Gosling: The same kind of prior learning is assumed.) and different from envy, anger, or others (different learning). Condition C also accounts for the fact that feelings generate *impulses* which must be controlled if we are not to be carried away by them, and it helps us to understand why it is that even though our feelings are ours, they also have the aspect of something that happens to us.

A parallel set of conditions can be advanced for attributing guilt. The following will indicate what changes need to be made:

- a) He must have learned to distinguish between right and wrong actions and between good behavior and bad. (To see an action as right or behavior as good is to see it as something to be done; to see an action as wrong or behavior as bad, is to see it as something to be avoided.)

And a parallel formulation can be given for anger:

- a) He must have learned to distinguish circumstances which are needlessly frustrating and situations which constitute being attacked without sufficient reason. (To

see something as a needless frustration or a gratuitous attack is to see it as something to be eliminated.)

- b) He must have acquired some capacity for identifying sources of frustration or attack and some capacity for eliminating frustration or attack by means of an attack on the source.
- c) He must have an acquired tendency to attack, without deliberation, sources of needless frustration or gratuitous attack. The criterion for having such a tendency is to have exercised it in the past. (This third condition distinguishes anger from, e.g., self-interest or persistence.)

Several remarks are to the point here: First, the point of the preceding exercises is not to exhibit elegant substantive analyses of these concepts, but rather, to illustrate the logic of such analyses and to show their place in relation to the concept of intentional action.

Second, it may be assumed that even exhaustive efforts along the lines indicated above will not succeed in delineating adequately all the significant features of “feeling” concepts. For this, additional resources are required. For example, to distinguish between “stranger” and “weaker” forms such as anger-resentment or fear-timidity, it would seem that we need to be able to refer to some degree of balance between wanting and wishing. (Wishing is conceived of as a part-description relative to wanting: Whereas wanting has three correlatives, knowing, knowing how, and trying to get, wishing has only the first of these. We wish for things without trying to get them and we can wish for things without knowing how to get them—for example, we can wish for impossible things. (Cf. Peters and Mace, 1962; and Webster, 1957.) Or again, for the ingredients required to distinguish, say, an angry action, an angry mood, a hostile temperament, and the direct experience of anger, we need to go from the concept of intentional action (PI) to the concept of a person (PII—see below).

Third, it is instructive to review the characteristics of the “standard form” common to the conditions for attributing feelings of anger, guilt, and fear: Condition **a)** expresses a tautologous relationship and built-in significance. What is dangerous *is* what is to be feared, and what is dangerous *is* something to be avoided. Condition **b)** describes the implementation of that significance, i.e., the acquisition of the capacity to act appropriately in the light of **a)**. And finally, condition **c)** describes a tendency to act spontaneously in ways appropriate to **a)**. In the light of earlier remarks on the fundamentally public character of the concepts of “Person” and “intentional action” it is worth noting here that the fact that conditions **a)** and **b)** refer to *learned* discriminations and capacities has the consequence that these discriminations and capacities are public in nature since the criteria for these are public. Also the tautologous nature of **a)** sheds light on how a situation can provide “reason enough,” e.g., to be angry. And in the light of earlier remarks in regard to treating something as a pawn, it is also worth noting that the present formulation of feelings such as anger, fear, and guilt amounts to saying that (1) to have such feelings is a way of *treating something as* a dangerous thing, a frustrating thing, a wrong thing, etc.; (2) moreover, when we do this, we are doing what comes naturally; and (3) to do so is to exercise a capacity or competence with respect to which we may expect both individual differences and differences during the life of a single person. (We might anticipate, therefore, that this would be a significant reference point for conceptualizing psychotherapy.)

VII. Some Psychopathology

To be able to identify some intentional actions as, e.g., fear behavior and distinguish these from other significant types of intentional action contributes to a parsimonious account of the phenomenon commonly described by reference to ego

defense mechanisms and unconscious motivation. The following is an example of such application.

Let us hypothesize a young mother with a two-year old son. The presence of the son severely restricts her freedom and she resents this strongly. She punishes him often and severely. When asked why she is so angry at the son, she replies in surprise, “I’m *not* angry at him. *Really*—a boy *needs* discipline or he’ll grow up to be a juvenile delinquent.”

If we do not take the mother’s description of her own behavior at face value it is most likely because we perceive a discrepancy with respect to “trying to get.” What she does is not “*just* the sort of thing one *would* do if . . .” The conclusion would follow, “The reason she gives *can’t* be her only reason—there must be another one instead or, more likely, in addition. What could it be? Well, what is she overtly trying to do? Attack the boy? Does she have a reason for wanting that? Well, to be angry at him would be reason enough to want to do that. Does she have reason enough to be angry at him? Yes—he frustrates her constantly in significant ways. Does she know how to express her anger? She surely does. Does she know she is angry at him because he frustrates her? No, unless she’s simply lying to us, and we have no reason to think that.”

At this point, the special explanation advanced to resolve the original “trying to get” discrepancy has succeeded, but at the cost of generating a new discrepancy in the “know” position of PI. Thus, neither description, nor the two in combination, provides an adequate account of this woman’s behavior. It is here that we turn to concepts such as fear and guilt, the classic motives for repression.

We continue the explanatory account: For a person to be angry at another person is *reason enough* for him to know that this is so. (In contrast, having, e.g., a high white blood cell count, or being in a state of physiological arousal, is not reason enough for a person to know that *that* is so.) If she is angry and doesn’t know it, there must be a reason. Does she have reason enough for not knowing she is angry at him? Well, yes. Since the remainder of PI is satisfied, for her to know that she is

angry would be for PI to apply without apparent exceptions, i.e., it would be for her to be attacking him deliberately. That would be for her to be in a dangerous position *vis-à-vis* her husband and the surrounding community. (Here is a case where having reason enough is not a justification.) And it would be for her to be doing something *wrong*. And *both* of these states of affairs are things we may assume she has an acquired disposition to avoid without deliberation (i.e., the avoidance requires only the awareness *of* something but not knowledge of *what* is the case here.) At this point the mathematician would be inclined to say “and all the higher-order derivatives vanish.” That is, as soon as a person has a reason for not knowing that he is doing a certain thing, he also has a reason for not knowing that *that* is the case, and a reason for not knowing that he does not know that that is the case, etc., etc., because otherwise the entire structure of ignorance would be subverted (and this may help us to understand a discontinuity in “availability” between what is “repressed” and what is merely hard to remember). Q.E.D.—Her behavior fits the PI without apparent exception.

On the bases of her further behavior and, e.g., the relative obviousness of the initial discrepancy, we would very likely draw further conclusions about how much she wanted to avoid the danger or avoid being wrong, or, how dangerous or how wrong she took it to be, or, conversely, conclusions about her capacity for treating her husband et. al., as dangerous and herself as wrong, as well as her capacity for controlling her anger.

It is important to keep in mind that what is generated by removing apparent exceptions is an explanation, *not* as such, a “true” explanation. Our degree of conviction about any explanation is relative to what we know. If, for example, we know that the mother in the example above had been socialized into a group which professed the same kind of belief that she expressed, then we would consider the application of the “ego defense” description to be questionable rather than fully justified, because there would be a plausible alternative in terms of “special knowledge” on her part. If we wanted to decide between the two alternatives we would probably pay particular

attention to (1) the consistency of her behavior across situations including what she says—does she consistently do just those things which a person *would* do who held that belief, and (2) her reaction to information about (a) the differences between her professed belief and the beliefs of others and (b) statistics about delinquency as a function of differential child-rearing practices. But we might proceed by treating her as though the one or the other explanation was the correct one, but taking care to make it as easy as possible to recognize whether and to what extent we had not succeeded in treating her in that way. And upon inspection, it should be clear, that the former approach is merely a special case of the latter.

In general, my unconscious motivations can be formulated as apparent PI exceptions which are explainable as the intelligent (but not deliberate) avoidance of knowing something that I am doing. It is because knowing what I am doing involves knowing what I want out of it that it makes sense to speak of unconscious *motivation*. One might better speak of unconscious *action* here. It should be clear, however, that for me to avoid knowing that I am doing A is not for me to be unaware of what I am doing at the time when I do A—in general, I am doing B (the mother is *disciplining* the boy), and I may be able to describe my actions in great detail. The A-B discrepancy leads us to characterize B as a distortion, and the *kind* of discrepancy it is is summarized by the kind of ego defense we say is exhibited. (The mother is rationalizing, denying, repressing.)

In connection with ego defenses and unconscious motivation, asking about the function of an action or kind of action plays approximately the same role as is illustrated in the example by asking what the mother was overtly trying to do. Identifying functions appears to be most appropriate when no single or simple overt attempt is involved (like the case of trying to get a million dollars).

It is because there is no uniquely *real* description (as opposed to a mere description) of a thing that denial, repression, rationalization, etc., etc., can be so easily accomplished. One need not be a liar or a fool or have empty places or submerged

places in one's life history where the repressed events "really are." All that is required is a workable description of what one does. To say that a person distorts in an ego-defensive way is to say that he does not *entirely* succeed in treating himself or others in the ways that he describes. Between designating a penny as a pawn and designating that mountain as a pawn, there are intermediate cases.

VIII. Review

The problem of the use of person concepts in Clinical Psychology was discussed pragmatically, with particular reference to the methodological notions of "partial-description" and "part-description." Person concepts were identified as partial-descriptions, inasmuch as their use is intelligible only in the sight of their systematic function within the more inclusive concept of a Person. Much of the perplexity generated by the use of person concepts was attributed to a general tendency to take both the use of language and the concept of a Person for granted.

Because "Person" is necessarily a public concept, to see an individual as a person does not imply any distinctively subjective or phenomenological procedure or outlook. It does not involve putting oneself in the other person's place, though it is compatible with that (indeed, it throws light on what empathy is and how it is possible).

In this initial portion of the discussion, the concept of intentional action was formulated as the key constituent of the concept of a Person. A preliminary measure of the descriptive adequacy of the formulation was provided by showing how "ego defense," "unconscious motivation," and various specific "feelings" such as fear, guilt, and anger can be derived by reference to the concept of intentional action. In Part Two of the discussion, the concept of intentional action is elaborated into the concept of a Person.



PART TWO

Individual Persons

In Part One the notions of (a) a distinctive type of description, (b) a partial-description, and (c) a part-description were presented as a basis for explaining the elusiveness of "person concepts" such as intentions, feelings, and self-actualization as objects of psychological investigation. Briefly:

(a) A conceptual system provides a distinctive type of description when descriptions of that type are not replaceable by descriptions of some other kind. In general, criteria for the application of descriptions of a distinctive type cannot be fully stated.

(b) A partial-description is one which refers to a phenomenon that could occur only within a larger framework, the "primary context"; in spite of this, the phenomenon is directly described without any mention of the primary context.

(c) Certain phenomena can and do occur by themselves; however, they are not describable independently, but only as being the kind of thing which is found in some "primary context." A description of this kind is a part-description.

It was suggested that person concepts are elusive because they are essentially partial-descriptions relative to the concept of a Person, which in turn provides a distinctive type of description, and that the greatest present need is for substantial conceptual clarification in this area. Toward this end, the concept of *intentional action*, summarized in the paradigm **PI**, (consisting of a quadruple of concept-types: "want," "know," "know how," and "try to get") was presented as the basic, distinctive element in the concept of a Person. The ordinary use of PI was presented as carrying with it (a) the ability to recognize when an explanation of a person's behavior is called for, (b) a procedure for constructing explanations from whatever information is available, and (c) the ability to recognize an adequate explanation. On this basis, together with some elementary formal features of PI which serve to distinguish *types* of intentional action (see Part One, Section VI) the following derivations were made: (a) behavior motivated by a feeling such as fear, guilt, or anger was derived as a species of intentional

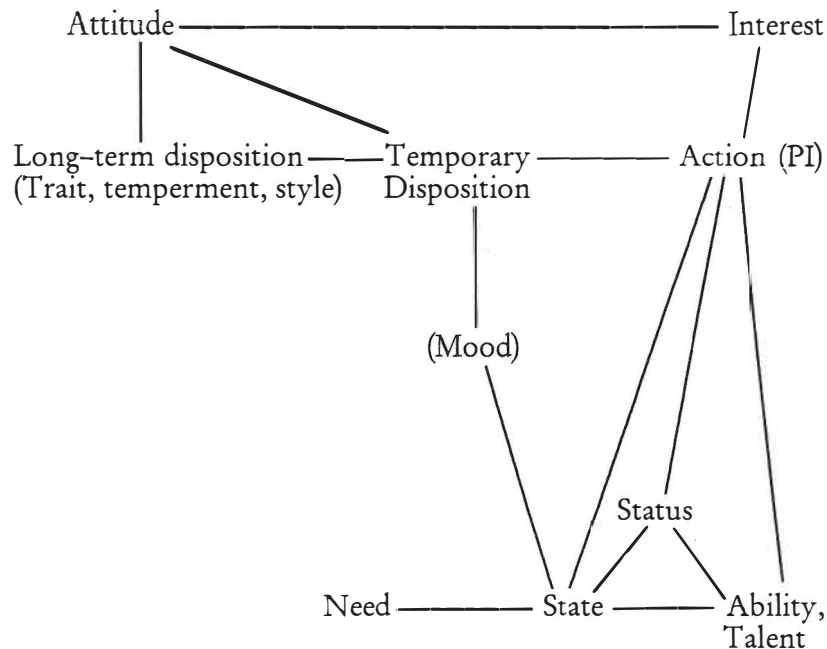
action, and (b) behavior in which ego defenses and/or unconscious motivation are exhibited was derived as a species of intentional action.

The present discussion deals with the derivation of PII, a paradigm for the concept of a Person, from PI, the paradigm for intentional action. In general, PII is derived from PI by constructing various series, the elements of which are intentional actions each of which is of the same type, or species (See Section VI, Part One for ways of generating “types”) performed by the same individual. Thus, in contrast to a *type* of intentional action, which designates an unlimited number of actual and hypothetical actions, the series in question consist of a finite number of actions each of which is part of the life history of a single individual. (One exception to the foregoing, needs, will be noted below.) The concept of a Person is the concept of an individual whose history is represented by a collection of series of the kind shown on Figure 1, i.e., it is a history of intentional action, successful, unsuccessful, and abortive, the totality of which is articulated into the format labelled PII. As in the case of PI, PII as shown in Figure 1, is a visual aid for showing an overall structure and main lines of articulation “at a glance.”

And as in the case of PI, the point of the following brief discussions of PII concept-types is not to present a definitive analysis of certain readily segregated descriptive units. Rather, it is an attempt to formulate an intelligible and maximally familiar set of interrelated reference points such that following the conceptual pathways for which they provide a paradigm will stimulate a heuristic recollection of—and summarize—what we all do in using various specific person-descriptive expressions on specific occasions. It is an attempt to “assemble reminders for a particular purpose.” Any serious attempt to become more explicit about the concept of a Person requires that we throw off the trauma suffered in the freshman-year discovery that there are umpteen thousands of such expressions. The number of such expressions is of no great consequence so long as the principles according to which they are generated and the ways in which they are used can be effectively stated. ❖

Figure 1

PII: Paradigm for the Concept of a Person



I. Types of Series

If a single action is describable as, e.g., *angry*, then we can talk about the class consisting of all the angry actions of a given person. Such a class is also a series. A person's hostile actions will be datable and clockable. That is, they are episodes in his life which begin at one time, last for a certain time and stop at a later time. (Even acts of omission can be treated as episodes because the significant opportunity for the act which was not committed is both datable and clockable.)

We may now conceptualize an anger series which (a) contains an excessive number of elements, and/or (b) contains an excessive number of elements which satisfy the additional description that their occurrences involved an initial PI discrepancy of the “not reason enough” variety. (*Persons*, Part 1), (c) covers a long enough stretch of calendar time and (d) is not selectively associated with particular other individuals. (Note that (a) and (b) are analogous to White’s (1964) criteria of “insatiability” and “indiscriminateness” as criteria of neurotic behavior.) These are the conditions under which we would say “This is a hostile person,” and in so saying, we would be attributing a *trait* to him,

A trait has the logical status of a disposition. To avoid confusion, dispositions are here distinguished from tendencies. Descriptively, the dispositions involved in PII are merely summaries of frequency information, whereas a tendency has the characteristic that when it is present it will be exhibited overtly unless something prevents that. In this respect, a tendency is like an impulse.

If a single action is describable as involving a particular object or type of object in the “want” position of PI, without any further end in view, this provides the basis of conceptualizing the series consisting of actions of this kind performed by a single individual. The application of standards of appropriateness and frequency will enable us to judge when such actions occur with excessive frequency or priority. These are the conditions under which we say that the person *has an interest* in that object or type of object (an interest in Jane, in chess, in politics, etc.). A derivative case is that in which the interest in the object serves a further end. Here, we would be inclined to say it was not a real interest. (For a more detailed account of the concepts of interest and attention, see A. R. White, 1964.)

Attitude concepts combine the logical features of interests and traits. That is, to have a particular attitude is to be disposed to engage in certain types of intentional actions with respect to an object of interest.

We may single out for special attention those portions of a series, e.g. an anger series, which consist of many members and few gaps over an appreciable period of time. These are the conditions under which we say that the person was in a certain *mood* during this period of time. To be in an angry mood is to be likely to be angry and nothing but angry (or irritated, annoyed, etc.) with everyone and everything. To be a moody person is to have a particular trait, i.e. the disposition to have moods, especially those involving unpleasant feelings,

We may select a series of intentional actions which consists of those actions the performance of which demonstrates a certain capacity. If these performances can be graded according to difficulty (which they can be, if only by comparing across individuals) then we may characterize the series by reference to its upper range, and say that the person has that degree of the *ability* in question. We frequently say, "That took talent," and when we say this we are not speaking about someone's employment of a tool—it is to say "Only a talented person could have done that."

Some of the most important capacities attributable to a person have to do with inhibiting or controlling certain tendencies. Feelings, for example, were formulated as including a disposition to perform certain kinds of action without deliberation. In light of the further requirement that the disposition be demonstrated by performance, this is to say that feelings involve tendencies to act (impulses). Yet feelings are not always directly expressed; more often than not, a substantial amount of control is involved; and certainly, when feelings are *not* expressed, some control has been exercised. In turn, capacities are related to states, in that one of the most important ways in which being in a particular state makes a difference is in regard to a person's capacities.

To say that a person is in a particular *state* is to say that overall expectations in regard to his actions are to be adjusted in a systematic way relative to his normal activity or else relative to the normal activity of others. The latter is particularly to the point in connection with permanent or quasi-permanent

states. A state resembles a mood in that it preempts an appreciable interval of time and so is concerned with a series of consecutive actions the whole of which represents a discontinuity or displacement with respect to what was otherwise to be expected during this period of time.

In fact, a mood *is* a state. For example, to be in an angry mood is to be in an angry state, or a state of anger. In general, it appears that any feeling (analyzed previously with respect to PI as a type of intentional action) may give rise to a distinguishable “emotional state.” We speak, for example, of being in a state of fear, or of bliss, or of being beside oneself with glee, or with jealousy, etc. One logical connection between feelings and emotional states is provided by the impulses (tendencies to act without deliberation) which are necessarily involved in having particular feelings. In some cases the emotional state represents an inability either to control the impulses or to give them appropriate expression in action. Being in a state of fear (frequently this corresponds to what many psychologists would call “anxiety”) provides the classic paradigm here, and this corresponds to what Ryle (1949) has called an “agitation.” In other cases the emotional state represents the uninhibited expression of the impulses (giving way to one’s feeling). Here we may think of an outburst of anger or of the joy that reigns at the end of a winning football game. We may note, also, that part of the concept of being in an angry mood, not mentioned above, is to have a lessened inclination or a lessened capacity to control impulsive expressions of the feeling.

There are other varieties of states. To be in *pain*, or to *itch*, is to be in a particular state, one in which one’s capacities for many kinds of performance are reduced. Pain differs from emotional states in that there is not a distinctive type of intentional action corresponding to being in a state of pain, whereas there are feelings corresponding to emotional states. This is not to say that there is no way that one could *pretend* to be in pain—there are recognizedly typical postures, gestures, and facial expressions for this, as well as typical manifestations of incapacity.

The direct experience, or the sensation, of e.g., pain or anger or itching is derived from the corresponding feeling or state. My direct experience of pain is the direct experience I have when I am *in* pain. But isn't pain or anger *really* a particular quality of experience? Well, perhaps any one person can discover some quality of experience that typically accompanies his being in pain, but his being in pain is not logically contingent on his having any particular sensation. (Factually, we may note reports, for example, by hypnotized or lobotomized persons, to the effect that "I feel the pain, but it doesn't bother me any more.") It is *being* in pain or being in anger that has significance, and so it is not as a peculiar quality of experience that pain or anger is of psychological interest.

In general *pathology* concepts are "state" concepts. To be sick is for one's capacities to be reduced and for one's behavior proclivities to change. Likewise, for being, e.g., exhausted, intoxicated, maladjusted, or crippled. Being crippled, or maladjusted, or being mentally retarded are examples of permanent or quasi-permanent pathological states.

What a person expects is what he now believes will occur or fail to occur, and this is properly part of PI, since it falls under the "know" concept-type. But if he is waiting for something to occur, including, sometimes, when he is preparing for it, then he is in a particular state, a state of *expectation*, and he has an expectant attitude. If he discovers that what happens is different from what he expected, then he has received a surprise. If he is not *merely* surprised, then he is also thereupon in the particular state of being disappointed, or shocked, or aghast, or overjoyed, or relieved, or dismayed, or something of that sort. In general, expectation will serve as an initial basis for taking a course of action only insofar as the person can change what is expected. What is more important for selecting a course of action is what the person believes *would* happen if It is in the conditional mode that alternatives are generated. Once a course of action is initiated, expectations become appropriate, and those expectations which serve as a basis for "progress check" feedback are particularly useful. But it is belief (PI

“know”), rather than merely expectation, which is required for an adequate conceptualization of human action.

Certain conditions which would qualify formally as states are better described as statuses, mainly because they do not exhibit the kind of discontinuity or contrast which makes it informative in other cases to speak of “states.” Examples would include male, female, child, adult, aged person, blind person, mentally retarded person, and stranger, or foreigner.

II. Needs

One *could* say that to have a certain need is to be in a certain state, but the case of needs is distinctive enough to warrant separate discussion. Statements of the form “He has a need for A,” refers to a state of affairs, A', such that if he fails to achieve A' or, more generally, if A' fails to come about, *that* state of affairs will have one of the following consequences: (a) the person enters into a pathological state; (b) a pathological state of the person is maintained; (c) a pathological state of the person becomes worse. Thus, “need” is conceptually related to “pathology.” In the positive cases (e.g. “He needed the queen of hearts in order to make a royal flush.”) the pathology may be merely a temporary frustration. A need involves some associated time period which is the latency period of the pathology in case (a), above—a need the satisfaction of which could be postponed forever with impunity would not be a need. Also, the satisfaction of a need is accomplished in a certain degree which may range from “completely” to “not at all.” Differences between cases (a), (b), and (c), above, are associated with different degrees of need satisfaction.

Needs differ from states in that having a need does not imply a difference in behavior except via pathological states. Specifically, needs do not *imply* anything about motivation, though it is frequently the case that a person who has a need will try to satisfy it. Moreover, in the case of certain kinds of needs,

which are of particular interest, there is no displacement or contrast involved either for one person or across people. These are the needs which are generally identified as “basic human needs” (cf. Coleman, 1964; Maslow and Murphy, 1954).

The following are representative examples of basic human needs: (a) order and meaning, (b) security, (c) adequacy, (d) love, and (e) self-actualization. Such needs can be formulated in terms of PI as soon as intentional action is seen as a participation in a body of social practices, which it necessarily is seen as. (For example, the criteria for the application of PI descriptions are necessarily public, and the differences that such descriptions make are knowable only through other descriptions the criteria for which are also necessarily public. And the learning of such criteria implies the social relationships within which the learning occurs.) We may therefore discuss these basic human needs in the following way:

1. The “know” and “know how” concept-types of PI directly imply that intentional action is possible only in an ordered and meaningful world, and it is possible only to the extent that the person’s world is ordered and meaningful.

2. For me to be insecure is to act in the light of the belief that my overt attempts are likely to be unsuccessful in achieving what I want. Insofar as I am insecure I do not have a “know how” repertoire relevant to what I want. And if I am as insecure as a person could conceivably be, intentional action will not be possible for me at all.

3. Likewise, if I have no competence at all, then I cannot act intentionally. I might be said to make overt attempts and have them succeed but that would be luck. (And to the extent that I could come to know what kind of luck I could count on, I would then “know how.”)

4. Love is rarely regarded as just another human relationship. In most cases it is regarded as either the prototype of all later relationships (initial object cathexes, terry cloth mothers, etc.) or the archetype or ideal type, i.e. other relationships are intelligible *as* relationships in the light of how they

resemble and how they differ from this relationship. In either case, it would follow that my capacity to enter into the going variety of human relationships would be arbitrarily impaired in the degree to which I had not yet achieved a love relationship. And if the difference that intentional action makes lies in a human relationship that I cannot enter into, then I cannot perform that action either, though I might still go through the motions.

5. Finally, we may point to two kinds of criteria: criteria for successful performance (outcome) and criteria for excellence of execution (process, “good form”). And we can conceptualize a second-order capacity, that is, the capacity to act consistently in accordance with PI, being limited only by potential capacity, opportunity, and (to a minimal degree) priorities among wants, and not limited arbitrarily in any of the ways described above. A person might be successful in the exercise of this capacity and still be able to do better (outcome vs. process), so that there would always be something new to be tried. It is in such terms as these that concepts such as “personal fulfillment” and “self-actualization” may be analyzed.

In general, basic human needs are presented here as logical derivatives of the concept of a Person rather than simply empirical insights into the nature of human beings. Such needs are basic, not because failure to satisfy them *leads to* maladjustment, unhappiness, or other forms of personal failure, but rather, because to fail to satisfy them *is*, in that degree, to be maladjusted, or unhappy, etc. i.e., to fall short of being a Person. (We would not say that an individual *had* failed to satisfy a basic need *until* we would also assess personal failure, and it would be on the same grounds.) This approach to “basic needs” has the advantage of employing a clear-cut, nonarbitrary basis for distinguishing basic needs from other needs; it has the further advantage of being capable (or so far, apparently capable) of assimilating any of the commonly accepted “basic needs” to a single conceptual system which throws light on the relations among basic needs and on the question of how each one of

several lists containing nonequivalent entries can legitimately carry conviction as being basic. It also clarifies the question of priorities among basic needs by implementing the distinction between logical priority and motivational urgency. For example, biological needs in general must be satisfied “before” psychological needs not because they generate stranger wants (though that may be true also), but because they have a shorter “pathology latency.” And “order and meaning” is most basic among psychological needs because it is a prerequisite for *any* intentional action. And it is not the case that self-actualization (the other end of the line) can not be satisfied until *after* other psychological needs have been satisfied. Rather, success in satisfying this need can not be achieved except *by* achieving successes with respect to other psychological needs whereas the converse is hardly the case at all.

III. PI and PII Concept-types

Since PII concept-types were formulated as series consisting of intentional actions of a particular type, it might seem redundant to characterize an intentional action by reference to its occurrence in a series. But to see this as redundant would be to overlook some important aspects of the situation.

An action is a particular and the series of which it is a constituent is a particular. A given action might be the first element in the series, or an element late in the series, or one preceded by numerous other elements, or a distinctive one, or a typical one, etc. Its having any characteristic of this kind in no way depends on its being one type of PI action rather than another. Thus, series membership gives rise to descriptions which are not redundant with respect to descriptions of distinctive types of PI actions.

The preceding would hold for membership in any series. There is a further contribution stemming from the multiplicity of PII concept-types. To describe an action, e.g., a “hostile

action,” as an element in one type of PII series is to offer a description which is different from what would be given by describing the action as a member of another type of series. For example, a sarcastic comment made by someone who was in an angry mood would be different from a sarcastic comment which was the typical mode of expression of an old grouch even if the observable characteristics of the episode (e.g., words, manner, circumstances) were the same.

Also, for example, the recognizable symptoms of an emotional state acquire a status not unlike that of an “overt attempt” in regard to describing intentional action. If I see his eyes narrow and his jaw thrust forward and his face flush, I will be inclined to say that what he is apparently doing is controlling his anger. (But I might say that what he is doing is *showing* his anger.)

Finally, it should be clear that to a large extent any of the PII concept-types can assimilate any of the others, either directly or by way of a part-description. For example, a trait may be the disposition to (a) enter into certain states, (b) express certain attitudes, (c) demonstrate certain abilities, (d) show an interest in certain types of objects, (e) satisfy certain needs, etc. This is direct assimilation. And the object of an interest might be (f) the kind of object toward which a certain attitude is directed, (g) the kind of activity which expresses a certain temperament or exhibits a certain kind of style, (h) the kind of object that would satisfy a certain need or would be expected to put a person in a particular state, or (i) the kind of activity that requires a certain talent or skill, etc. (And now, substitute cases (f) through (i) in (d), above.) The convertibility of one kind of series into another via direct assimilation or part-descriptive assimilation has no formal limits, so that PII is a recursive and “generative” system which provides an unlimited number of descriptions. And we do make some use of this added descriptive power in our normal social interactions, although, as with language generally, we do not make much use of the more complex possibilities.

Perhaps the most significant consequence of being aware of the generative character of PII will be to help us to understand the pervasive sense of personal identity which we do have and which seems both inescapably private and yet in no way conceptually disconnected from the overt, easily communicable things that we do (which it seems would have to be the case if it were truly private). Neither the number nor the complexity of person descriptions is limited, but the use of person descriptions comes about through the skills acquired in participation in social practices and, like the use of language in general, is itself participation in a set of social practices. But the exercise of these skills does have human limitations. Participation in situations of unlimited complexity seems quite clearly to be outside the range of human capacity (cf. Miller, 1961) even when only familiar skill components are required, and this holds equally for description performances and for the performances which are described. Thus, it is not surprising that our self-knowledge should have much of the general character of “feelings,” since the latter both are a critical aspect of person characterizations and, on the performance side, involve skills which can be exercised without requiring deliberation and thus could continue to be exercised past the point where deliberation was no longer possible. Participating in a complex of social practices is what a person spends his time *doing*, and so it marks the difference between first-hand experience and what a person merely knows about. The person who knows himself is like the person who drinks by himself—the performance is solitary, but the practice is a social one. Better, the person who knows himself is like the person who works out a novel chess combination—here again, the performance is solitary and the practice is not only a social one, but is one for which the rules are reflected in explicit statements, but here, nevertheless, the achievement is a unique one. A Person has, not something simple like chess practices, but a life history of participation, decision, and expectation, and an impressive repertoire of skills to draw upon.

A further point, parenthetically, in connection with the complexity of PII: The notion of “symbolism” is frequently

encountered in clinical theory and practice. It would seem that the general condition under which we speak of the use of symbolism is that some object, activity, or circumstance should be mentioned which both has a direct and usually innocuous, description, and in addition instantiates a significant part-description which refers to what is symbolized, and the part-description may be either the PI or PII variety. For example, a knife may be seen as a phallic symbol because (a) literally, it is recognizable as somewhat “the kind of thing which . . .,” and this is to say that under some description it is “the same” as a phallus. Similarly, plowing a field can symbolize intercourse because to plow a field is to treat the earth in a certain way, and to do that is, in part, to engage in a certain kind of activity, and this is “the kind of activity which . . .” is involved in intercourse. Thus, “symbolic satisfaction” is not a substitution of thought for reality. Rather, symbolic behavior has the same logical structure as intentional action. It is not any “overt attempt,” but rather, an overt attempt under some intentional *description* which is the intentional action of PI. So a man who is plowing a field is literally engaging in the kind of activity which is involved in sexual intercourse, and in one sense, this is no less than what is done by the man who is engaged in sexual intercourse. But although he is doing that, his participation is not of the same kind, since it requires a different kind of skill, knowledge, and supporting circumstances. And so, on the face of it, he is not engaged in the same social practice, and it would require a “special explanation” to provide the basis for saying, in a given case, that nevertheless it was the same or that the person was doing that *because* of the way it was “the same.” Since we do have criteria for the use of person descriptions, we are able to take account of symbolic behavior without having to suppose that every such possibility is a fact, or that when it is a fact something very mysterious has happened.

The following sources contribute to the complexity of person descriptions:

(a) Each of the four PI concept-types has a large number of instances (i.e., a large number of specific concepts of that sort);

(b) Types of intentional action are determined by combinations of distinguishable features of the PI paradigm, and the number of combinations is far greater than the number of distinguishable features;

(c) An intentional action is placed in some *set* of PII series, or concept-types;

(d) PII concept-types are further differentiated by virtue of their mutual assimilability. Since these sources combine multiplicatively, and in the latter case, combine without limit, it is clear that umpteen thousand descriptive terms come nowhere near exhausting the logical potential of this conceptual system. Yet the system and its logical potential can be represented in two relatively simple paradigms.

In general, the transition from PI to PII concept-types is the transition from class membership to part-whole relationships. In turn, part-whole relationships generate a new set of class memberships, or types of action. The transition could be described as a partial explication of the existential import of an action. Because a consequence of the transition is that actions which have identical PI type characteristics have a different place in a person's life or in different people's lives. And if they have a different place, they may play a different part. To see an action in the light of PI is to see *what* action it is. To see an action in the light of PII is to see what *person* it is the action of, and this is to have a broader understanding of what action it is, also.

When we speak of what a person intends, what he is afraid of, what he is ashamed of, and what he expects or believes to be the case, we are talking about his "inner life." And there is some problem with this "inner." Sometimes it seems as though one would need a crystal ball to have access to it—and then projective tests, for example, or "dynamic" theories take on the

aspect of a crystal ball. Or else it seems that this is only an inferior, “commonsense,” way of making reference to what goes on inside him—and then talking about his nervous system, for example, or about his body chemistry or about measurement and models, takes on the aspect of superior rigor.

But there is not something extra about a Person which requires such peculiar maneuvering. A person’s inner life is a *Person’s* life, and it is a Person’s *life*.

IV. Interaction of Persons

“The queen of hearts is a piece of cardboard with distinctive red markings and a stylized figure of a woman.” When I see these things, that gives me reason enough to call it that. But these features are not what I *mean* when I call it the queen of hearts—that belongs to the game. And if I see his eyes narrow and his jaw thrust forward, that may give me reason enough to describe him as “angry.” But these features are not what I am talking about when I call him “angry.”

The primary function of the concept of “Bridge” is to guide the behavior of one bridge player with respect to others. And the primary function of the concept of a Person is to guide the behavior of one person with respect to others.

When we try to be as explicit as we can about what actions and what circumstances provide “reason enough” to act in a certain way, the effort leads in the direction of social roles, social structure, and custom. For example, to learn to distinguish what is dangerous from what is not is to come to distinguish certain commonly recognized dangers, and the successful exercise of this capacity will largely consist in distinguishing other commonly recognized dangers (cf. the analysis of “fear” in Part One.) And learning effective means of avoiding dangers will have similar characteristics. And so forth. So that, for example, what there is commonly reason enough for in one society, e.g.,

erecting crosses on graves, may require a very special explanation in another.

We give an explanation for the sake of someone who understands. Ourselves, for example, but also for others. To give an explanation is to remove some uncertainty. To explain what a person does is to characterize him by reference to PII, and to understand a person is to see him in this light. For an observer, to see a person in the light of PII articulates the situation for him, and this serves as a guide to action (a) by providing “reason enough” to engage in some action, or (b) by contributing to what the observer knows, and thereby entering into his intentional actions, either currently or later on. Since what a person knows does not change unless he forgets or finds reason to change his mind, the contribution may occur at a much later date.

This description would apply to the person observed, also. The rules are the same for observers and those observed. The observer may, for example, if he is a scientist, have special tools and modes of observation, but observation does not play a logically different role in his life than it does for the one who is observed. Observation works the same way for both, even though they are pursuing different goals, perhaps. Which is only to say that, after all, observing is something that *Persons* do.

What is observed? Here, we must recall the important differences between (a) a class of actions of a given type, and (b) the series of actions of a given type which are performed by a person. The series is observable in a sense in which the class is not, though neither is observable in the way that an action is. When we have observed all the members of a series, we have observed the series. This means, roughly, there is nothing left to observe.

The criteria for the ascription of moods, traits, attitudes, etc., are characteristics of PII series (frequency, appropriateness, density, etc.). Like the criteria for the application of PI descriptions, they are permissive rather than prescriptive. This has the consequence that we may observe sufficient instances to constitute a trait, attitude, mood, state, etc., without having observed

all of the instances. And even before we have reached this point, we will have passed the point where it would be equally appropriate to say either one of two things: "He used to be hostile, but he's changed," vs. "For a time, it seemed as though he were hostile, but it turned out that he wasn't," or "I started out in an angry mood, but it vanished almost immediately," vs. "For a minute I thought I was going to be in an angry mood, but it didn't happen." Sometimes, but not always, in a case like this, we try to decide which is the correct account by looking to see if there is a reason for the change.

To call him an old grouch on the basis of a sarcastic remark is like calling *this* the queen of hearts on the basis of its markings. Yet, unlike those markings, sarcastic remarks *are* the kind of thing we are talking about in calling him an old grouch. How is this possible?

To begin with, we should want to say that even when we have an insufficient number of instances, to ascribe a trait on the basis of observing an action is not to make an inference. And the point of that is, the action is not a *sign* of the trait, because the element is not a sign of the series—it is a part of the series. (The corner of a brick building is not a sign of the building, either.)

When we have observed enough cases, there is nothing left to be observed insofar as the ascription of a trait (attitude, mood, etc.) is concerned. Then there is no empirical gap to be bridged, and no inference, either. When we have not observed sufficient cases, we may be mistaken in supposing that other cases have occurred or will occur. That is all. And if we have observed sufficient cases, we may still be surprised (cf. "He used to be hostile, but now he isn't"). No amount of cases will be enough to prevent that. It is perhaps in recognition of this point that we are inclined to say "We infer from cases—but we never really *know*." Whereas, it ought to suggest that the point of understanding a person is not to *predict* what he will do. (When I say, "I'm going out for a pint of beer," that is not a prediction, either.)

“He’s the kind of person who would sell his grandmother down the river for a nickel.” I am not making a prediction here, not even a conditional one, and so I need not be wrong in saying that, even if he never makes a sale. Rather, I have used these words to say something about the kind of person he is, and normally, I would thereby have announced my intention to treat him accordingly. If he does sell his grandmother down the river, then, on the face of it, he has *shown* himself to be the kind of person I have said. But only on the face of it. If there is a special explanation for what he does, then perhaps, anyhow, he is not that kind of person. That is one way of receiving surprises. And this is the way with PII characterizations. They are part-descriptions. When I call him an old grouch on the basis of a couple of episodes, I am not making a rash guess at what else he will do, though I will, no doubt, have some expectations. The series corresponding to his being an old grouch is not what I guess is going to happen—it is my conceptual representation of what his life *now* is like—he is “the kind of person who . . .” When I have observed enough instances, then on the face of it, he has shown himself to be that kind of person. He does not just then become that kind, and I do not even then know for certain that he is that kind. I can predict what he will show—that is like an “overt attempt” in PI. I cannot predict what he will be—but that is something I can understand, and when I am wrong about that, it is a different kind of mistake.

It is a mistake of a different kind because there are no criteria for what a person is in the way that there are criteria for what a person is apparently doing or for what he has, on the face of it, shown himself to be. This is part of the mystery of a person’s “inner life.” But this is a logical mystery, not a causal one. It is a real mystery, not the absence of facts—and this is also like saying, it is not a mystery at all. (Compare: The number of objects in a room is not something that can be settled just by observation and counting. We must first decide what is to be counted at all, and what is to be counted separately. Many decisions are possible here even without supposing

any knowledge or practices which we do not now have, and so nothing needs to be mysterious.)

PII is the conceptual domain within which a Person's behavior has significance. The logical complexity and descriptive power of PII is considerably greater than that of PI, which it includes, and what can be established by observation is only *one* of the four elements of PI, i.e. the overt attempt, including failures and mistakes. So there is an unknown number of PII descriptions which would be compatible with any given series of "overt attempts," and each of the PII descriptions would involve many more, and more complex, concepts than that of the overt attempt. So the "iceberg" image of human behavior has a point even when we do not confuse descriptions or explanations with causes.

The primary function of the concept of a Person is to guide behavior, not to establish facts about behavior (the latter is merely one of the things that Persons *do*). Descriptions which are of the logical complexity of PII cannot in general be *established* by observation. (If they could, we would have no need for language—instinct would serve just as well.) In the light of PII, we always act in the absence of proof. But we are not lacking something when we lack this, for we have no concept of proof here. (And a tree that has no carburetor is not lacking something either.) So counting cases only summarizes what we are inclined to do—it does not approximate or serve in place of a proof. PII guides our behavior even when we make a mistake and fail to understand correctly. We do not always pay for our mistakes, but sometimes when we fail to understand, we do the wrong thing. Here, again, the significance of doing the wrong thing is: Whatever difference it makes. So deciding, and making choices, is not something that could be avoided—it is something that Persons do. And so, also, it is neither stupidity nor perversity for a Person to make every effort to treat whatever he does as being successful, although often enough what he accomplishes thereby is what others describe as "distortion."

In addition to the social practices which are "built-in" to specific person concepts or person descriptions, the general way

in which PII guides a Person's behavior, and therefore also, what he concludes from other people's behavior, is illustrated by the following maxims:

(1) A Person has reason enough to do everything he can to treat whatever he does as being successful. (A Person's life is not lived moment by moment—we “play to win,” and each single action has a “follow—through.”)

(2) If a Person has a reason to do something at a given time, he will do it, unless he has a stronger reason for doing something else.

Reasons, unlike causes (including motives, when these are seen as causes), require no maintenance. Nothing *further* has to happen in order for a person to continue having a reason. On the contrary, something further has to happen in order for him to stop having that reason (for example, the achievement of a goal, a change of state, a change in what he knows, the loss or acquisition of a skill, etc.). So there is no special problem of accounting for the persistence of behavior, the resumption of periodic activities, or the continuity of a relationship with another Person.

Because a Person's reasons are temporally extended and from a diversity of sources, a Person generally could not possibly do all the things that he has a reason to do. Hence the importance of choice, the “over-determination of behavior” and the lack of equivalence between what a Person is and what he shows himself to be. All of these may be regarded as reasons why there are in PI criteria of intelligibility for priorities among wants as well as for wants as such.

To see a piece of behavior as an intentional action, as falling under **PI**, is to have an account of why it took place. To see it in the light of **PII** is not to have a second account of why it took place. We do not need that. To say that an action “springs from” a particular attitude (trait, etc.) is to say something, not about its source, but about its significance.

We observe a churlish remark, and we say “He resents Bill's success.” This puts the remark in the context of other hostile

actions directed toward Bill. (The relevant circumstances, Bill's success, which is what he knows about, is the same each time, and so we sometimes speak of the reason for his attitude toward Bill.) To see a person as resentful of another's success makes a difference. We treat him differently, and this also includes expecting differently from him. If it did not make any difference, then there was not *that* uncertainty to be removed, and then, no such description could have constituted an explanation. We do not first observe PII characteristics and then see what sort difference they make. These characteristics represent differences that *we* make.

The difference that is made by my knowing about his resentment of Bill's success is not determined by that knowledge. There are three other PI concept-types which play a part in the story of my reaction to him. And that reaction will play a part in *my* life and therefore will be intelligible to him and others in the light of PII. Etc.

It is in pursuing such accounts of human interactions in detail that we present the cash value of the statement that people's behavior follows rules and that the primary function of the concept of a Person is to guide the actions of one Person with respect to others. It is the kind of account that might be pursued in clinical practice.

V. Individual Persons

The concept of a person has a certain degree of complexity. Enough to make it plausible that a major source of confusion in connection with the use of person concepts is that they are learned in a relatively unsystematic way, so that we come to be able to use the terms more or less appropriately but are never very clear about how they interconnect with one another. Our usage exhibits the connections, but we cannot say what they are. Ordinarily, there is no reason to say what they are.

In the light of the magnitude of the complexity of person descriptions it is clear that there are quite enough descriptions to go around—each individual can have a unique description. But this very flexibility also contributes to the general impression that person descriptions are derived directly from the grammar of the natural language (here, English) together with person concepts (e.g., “angry,” “stingy”) as simple expressions, “pigeonholes,” rather than being derived from the coherent logical structure which is the Person concept. Three of the sources of complexity appear to be particularly significant in this regard.

First, there is the variability introduced by the notion of states. Because of the multiplicity of kinds of states and the frequency with which one state or another is entered into, the conceptual unity of the person tends to be overlooked in favor of the succession of happenings in his life history, and then later on, when as psychologists we feel the need, we begin to look for the causes of these happenings as a way of achieving conceptual unity.

Second is the fact that the same basic term, e.g., “anger,” or “fear,” is used for a variety of logical functions, or concept-types. For example, we have an angry action, a state of anger, the direct experience of anger, enduring anger at a given person, an angry mood, and a general tendency to become angry. Here, too, because the logical relatedness and differentiation of the various concept-types is generally overlooked or only partially acknowledged, instances of the various concept-types are likely to be lumped together into a single conceptual category for which no single account is possible. In the resulting confusion, it may well seem that the only way to make sense of the phenomenon, e.g., anger, is to regard all of the instances of anger concepts as being the outward effects of the same basic cause, with the variety being accounted for by the variety of intervening events. That is like saying that the similarity among “courage,” “courageous,” “encourage,” “courageously,” “discourage,” “encouraging,” etc. is that their instances all stem from the same basic cause.

Third, individuals vary in the degree to which they articulate the Person concept with a variety of specific concepts under each concept-type. All of these several aspects contribute toward making the Person concept well nigh invisible to the naked eye.

As clinicians, we have the distinctive task of identifying and dealing with persons who are in pathological states. These states involve failures in respect to meeting basic needs (and therefore, other needs as well), failures in respect to what the person knows how to do, and failures in respect to what he knows. Which is to say that we can treat him as a Person, but not as just *any* Person—rather, as one in a pathological state, as one “in distress.” And this is not to say that there is a single way in which we always treat such a person. Rather, we understand him differently, and we try to understand the difference it makes. (Compare: how we treat a person who is in pain.) Or, we cannot treat him *entirely* as a person (we would fail if we tried that) because of the way he fails as a Person. Either way.

Curing neuroses or psychoses is like getting a million dollars—it is not something we *know how* to do, although often enough the outcome of therapy is the kind of change which one would count as a “cure.” And so we need something other than we have now. We need an effective technology, and that may come about simply by the development of new practices in the light of the Person concept, together with what we already know or believe. Most likely, however, we will need the impetus of new knowledge. Because we already have the concept of a Person, the acquisition of such knowledge does not depend on theoretical development to give it significance.

We would like, for example, to have some answers to questions of the following sorts:

1. What sorts of effects do therapist in general, or any coherent group of therapists, *know how* to achieve?
2. What sorts of correlational patterns exist among identifiable pathological states, either concurrently or sequentially? And nonpathological states?
3. Are there subtle, but dependable, symptoms distinctively associated with various feeling states?

4. Are there dependable procedures for surveying and summarizing a person's capacities, belief, attitudes, interest, etc.?
5. Are there identifiable aspects of peoples' histories which are substantially associated with later pathology?
6. How are a person's capacities related to one another, e.g., inclusions, exclusions, implications, correlations, additivity, interactions, etc.
7. What are some of the unusual, but surprisingly effective ways that people have discovered for satisfying familiar needs?
8. To what extent and under what conditions can belief, attitudes, and behavior be expected to change (a) simply by being exposed to a person model, or (b) in the light of some significant interaction with a person who may serve as a model?
9. Are there distinctive physiological states associated with pathological or nonpathological states of the person? If so, which go with which?
10. What sorts of things do people want? What objects, events, or situations are commonly the object of specific feelings, e.g., fear, guilt, anger, etc.

To say that we do not *require* theories in order to arrive at significant empirical results is far from saying that efforts in this direction should be drastically diminished. One of the most valuable features of theory-based empirical findings is that, in addition to what they contribute to what we know, they are likely to involve novel practices which augment what we know how to do. (The relation of theory and research to the Person concept is developed in greater depth in Parts Three, Four, and Five.)

In the last analysis the question we ask is, what can be done, what can happen between us that will be of benefit to that person, that will enable him to be more successful as a person. If we *know how*, we will use whatever empirical results of whatever kind are available, and in general, we will know how to use any information which is presented in terms of the concept of a Person.

VI. Review

The concepts of (1) distinctive types of description, (2) part-descriptions and partial-descriptions, (3) intentional actions, as represented in the paradigm, **PI**, and (4) feelings, impulses, and defenses were developed in Part One. In Part Two, the concept of intentional action was expanded into the concept of a Person as represented in the paradigm, **PII**, by identifying a set of concept-types each of which has a distinctive logical relationship to an intentional action, and therefore, also, a distinctive relationship to each of the other concept-types. The expansion of PI into PII is quite analogous to the linguistic variations through which a semantic element can be transformed (cf. Courage, courageous, courageously, encourage, discourage, was encouraging, etc.). Each PII concept, i.e. each example of a given PII concept-type, is a series of identically-describable intentional actions. It is the series of such actions which occur in the life of a person.

In PI, different intentional actions could be distinguished as being of one type or another; the relation of a specific intentional action to its type is the relation of class membership. In PII, the intentional action is already classified as to type; the relation of a specific intentional action to an exemplar of a PII concept-type is the part-whole relationship—the intentional action is a member of a series.

The concept of a Person is the concept of an individual whose history is a history of intentional actions articulated into the series format of PII.

The PII concept-types correspond to what would commonly be called “personality variables.” Because the relation of a PII series to the corresponding intentional actions is that of whole to part, and not either cause, determinant, or categorization, the series is neither inferred nor predicted on the basis of observing intentional actions. Instead, the totality of series, PII, serves as a *conceptual system* which gives *significance* to an observed intentional action, and this is routinely accomplished by

characterizing an action by means of a part-description relative to PII.

The primary function of the concept of a Person is to guide the behavior of one person with respect to others.

Because the concept of a Person, PII, has a considerably higher degree of logical complexity and descriptive power than PI, where what can be observed is only *one* of the four necessary constituents of PI, (i.e., the “overt attempt”) the significance of an action far exceeds the scope of what can be *established* by observation. Therefore, we must always act in the absence of proof, and if there is any probability involved, it is in the sense of “degree of confidence” rather than a summary of case counting. The concept of “basic human need” was presented as a nonempirical derivation from the concepts of “need” (PII) and “intentional action.” Another clinically significant concept, “symbolism” was presented as involving a literal description of an object, activity, or situation for which there is also a significant, unspoken part-description. The potential clinical value of “nomothetic, predictive” findings was indicated.

In Part Three the linguistic basis of the Person concept is developed further, and some connections are drawn between the Person concept, language, and the concept of psychological research.



PART THREE

Psychological Persons

In Parts One and Two, the concept of intentional action and its significance were developed on the basis of a pragmatic account of language and the methodological concepts of partial-description, part-description, and distinctive descriptive system:

(a) We use a partial-description to refer to something which can only occur in the context of a larger set of happenings or in a larger structure (e.g. “playing a trump”). But we do have a terminology for referring to them without mentioning the larger context. Because of this, partial-descriptions are misleading when they lead us to believe that what they refer to are independent phenomena which can be studied and identified separately from any larger context and can serve as “building blocks” for wider conceptualizations.

(b) In using a part-description, we *refer* to something which can occur by itself, but we do not mention it directly. Instead, we identify it as “the kind of thing which” is found in some other, primary, context (e.g. “the smell of bacon”). We do not have a distinct terminology for giving a nonrelational characterization.

(c) A distinctive descriptive system is one which cannot be effectively translated into another descriptive system or have its pragmatic functions duplicated by the use of another descriptive system.

Intentional descriptions of behavior are seen as involving a distinctive descriptive system which is possible by virtue of our capacity to implement part-descriptions. The concept of intentional action is articulated into four types of logical components in the paradigm PI—“want,” “know,” “know how,” and “try to get.”

The significance of intentional actions is summarized in a second group of logical components, or “concept-types,” in the paradigm of the whole Person, PII. A concept falling under one of the PII concept-types is that of a series of intentional actions of a particular, distinguishable kind. These concept-types of PII

then correspond to what are commonly called, “personality variables.”

The concept of a Person is the concept of an individual whose history is a history of intentional action articulated into the series format of PII. The primary function of the concept of a Person is to guide the behavior of one Person with respect to other Persons. The meaningful ordinary language terminology referring to feelings, intentions, traits, interests, needs, and other human characteristics consists of partial-descriptions relative to the concept of a Person. Because the usual experimental approach is to interpret this terminology as having the pragmatic function of either summarizing or predicting observations, the Person descriptions of ordinary language have not provided a generally fruitful starting point for psychological investigation.

In the present discussion the pragmatic character of language is presented in somewhat greater detail. The pragmatic character of the use of language by psychologists is examined, and this serves as a basis for formulating a paradigm for the scientific activity of psychologists. The new formulation is somewhat more general than the commonly accepted “verification” paradigm. The difference that the new formulation makes is illustrated by a series of psycholinguistic studies. ❖

I. Pragmatic Aspects of Language

Language codifies what people know how to do. The meanings of the words we use and the social practices in which our lives are spent imply each other. It is not a simple implication.

The capacity which men have that makes them distinctively human is the capacity to implement descriptions, especially person descriptions. We implement descriptions in our *knowing*—in recognizing instantiations of a description (“He did it because he was afraid,” “He is an old grouch.”) And we implement descriptions in our *knowing how*, i.e., in being able to do or

accomplish what is described (how to treat someone as dangerous; how to be an old grouch; how to get a camera from the car).

Some of the basic capacities which are codified in language are of such a kind that their primary exercise (the performance which demonstrates the capacity) consists of *verbal* behavior. Although one person may know how to do something (how to tie his shoelace; how to treat an old grouch) without either he or anyone else being able to *say* how, he could not know how to do *this* thing if no one could say that that was *what* he did. For in that case, there would be nothing of *that* sort that he did then, and nothing of *that* sort to be known, either.

To say this is to deny that “saying so makes it so.” A familiar way of denying that would be to say “What *happens* happens, and its happening does not depend on what someone knows.” However, the latter is free of paradoxes only in linguistically truncated contexts in which we do not mention human behavior, e.g., in talking about physical objects.

“Those people are playing bridge, only no one knows anything of the sort.” What is being asserted here? If what those people are doing is playing Bridge, then Bridge is something that people play, something they know how to do, and “Bridge” is the term used to refer to that activity. Then the word “Bridge” has meaning, and there are criteria for its application, and one who has mastered the criteria knows how to use the word correctly. That capacity is demonstrated only if the word has been correctly applied in the past. (Naturally, for defined terms, only the correct application of the undefined terms involved in the definition would be required.) Then there must have been other Bridge games, real Bridge games. So what is being asserted is that what those people are doing now is “the kind of thing which” those other people did then when they played bridge (cf. Rhees, 1954).

If no one played Bridge, then there would be nothing which was resembled by what those people are doing now—unless it were something other than Bridge—and in any case, there would be no criteria for the application of “Bridge,” so that to utter the words “Those people are playing Bridge”

would be to have said nothing, because making the sounds of these words would not succeed in distinguishing anything from anything else.

In general, to say that P is doing X but doesn't know it, is to use "doing X" as a part-description. X is "the kind of thing which" is done deliberately on other occasions. This is why it would be paradoxical to say "Its happening doesn't depend on what someone knows." It might be said that *some* event took place there irrespective of what anyone knew, but for human behavior events, what makes it the case that an event of *the kind described* was what took place, is events of the same kind in the primary context of the part-description. And this *does* depend on what someone knows.

It is the criteria for the application of descriptive terms which determine when events are of the same kind. I can say he is the meanest man in town, not because he is a copy of someone else I know, but because I know how to apply "mean" correctly, and that includes being able to distinguish between more and less mean. But there are limits of clear usage, and when we want to exceed them, we have to *decide* to use the same term anyhow. And then it will be appropriate to speak of extending the use of the expression rather than of merely using it. And if we decide to extend it, still, it will be merely a different expression in disguise unless we *succeed* in treating the new "instances" in the way we treat the old instances. This point is relevant to the appraisal of the explanatory value of technical concepts.

Language codifies human knowledge and behavior by making it public. Being public, the varieties can be distinguished from one another. And if one variety can be distinguished from others, then it can make a difference, and if it does, then it has some significance and so has the terminology in which it is expressed. So it is *not* to say that first there is a phenomenon (the knowledge, or know-how), and then we put it into significant discourse, and then it becomes public. Quite otherwise. Roughly speaking, either all of these things happen or else none of them does. (Compare "First he brings it to us, then we take it with us, and some time later we enjoy the use of it," with

“First he sells it to us, then we buy it from him, and some time later, we own it.” (The distinction between process descriptions and outcome descriptions is discussed in Part Four.)

Language makes human knowledge and behavior public. However there are publics and publics. Bridge players are a small public. Their skills and interactions are something other people can identify and talk about, but not engage in. The scientists in a given area of research and theory are a small public also, and sometimes what they do is almost as esoteric as what Bridge players do. Neither scientists nor Bridge players are primarily engaged in verifying predictions or discovering truths about the world. (This is to speak of the significance of what they do, not necessarily about their intentions.) Bridge players are participating in a known form of life. Scientists are, too, but part of their participating includes creating new forms of life (new activities, new practices) in a limited way. What they say, what they talk about, has no general currency unless it can be discussed entirely in ordinary language. But it does have currency among those scientists who have a common interest and understand one another; the practices associated with, e.g., operant conditioning, with small group communication network research, with mathematical models of stimulus sampling, etc., etc., etc., are what give meaning to the discourse carried on with respect to these topics.

II. Some Pragmatic Features of Psychological Language

“If a person has a reason for doing X, he will do X, unless he has a stronger reason for doing something else.” That is a mere common sense tautology. To be sure. Otherwise, it would be a mere matter of fact, and then it could not have the use that it does. It guides the behavior of persons, and it makes a primary contribution to the way they understand other persons. And when the person is a scientist, it does not cease to

have this function, but here its use is not always as perspicuous as it might be.

(1) A survey of those psychological theories in which the determination of behavior figures explicitly indicates that every such theory contains an axiomatic principle of this sort, together with the theoretical equivalent of "the strongest reason at time T." The concepts of "momentary effective reaction potential," "the momentarily most probable response alternative," "the vector resultant of all the forces in the field," "the momentary maximum positive balance of object cathexis," and "the momentary magnitude of Expectancy X Value" all have this character. Each of these theory-specific paraphrases of the pragmatic maxim provides the theory with the basic formula for predicting behavior, and no one appears ever to have suggested giving up this feature of the theory in question, though other aspects of the theory have been questioned. But then giving up *this* feature would leave the remainder quite pointless and divorced from reality.

(2) "The rate of responding is inversely proportional to the ratio of reinforcement." (Here, any general or theoretical statement will do.) This means: Whenever it makes sense to speak of a ratio of reinforcement and a rate of responding, this relationship will hold, *other* things being equal. It is the latter phrase which expresses the principle in question, and it is indispensable, because there are always the exceptions. If a stronger, countervailing principle is operating, then negative results are expected, but that is not counted against the validity of the statement in question.

Just as theories would be relatively pointless without the theoretical paraphrase, so laboratory experimentation would be relatively pointless (one might say, "non existent") without the "other things being equal" paraphrase. The thesis that "the basic principles of behavior should apply to *all* behavior," and the faith that these principles should be more clearly exhibited in simplified laboratory situations than in complex and poorly controlled real life situations is the combination which provides the basic rationale for laboratory research. But the first of these

is misleading, though tautologously correct, and the second illustrates *how* it can mislead.

(a) “In order to exhibit the principles which operate in chess play, I will remove all the pieces from the board except the king on each side. When play begins, they will be following all the rules to the letter, including those which apply to the moves of those missing pieces. Of course, there will be no occasion to illustrate *those* rules, but after all, the situation I have set up here is one which could come about in the course of a game so it really is a ‘real life’ situation.”

(b) “General Motors is rather too complicated a business enterprise to study effectively. Since the basic principles of business enterprises will apply to *all* business enterprises, I will study that boy who is selling lemonade, and there I will discover these principles more easily.”

Here the relevant point is not that both the boy and General Motors *could*, without self-contradiction, be said to illustrate “the basic principles of business enterprises.” (And both skipping dinner and having one’s pocket picked *could* be described as cases of “deprivation.”) What is to the point is that the selection of the boy selling lemonade could hardly have been made except by someone who already knew what the basic principles of business enterprises *necessarily* are, i.e., who had mastered the criteria for applying the description “business enterprise.” And it would be surprising if he could, from studying that boy, show anything relevant to General Motors and other paradigm cases of business enterprises which he did not already know and use in his *selection* of the boy as a simplified case. We do not count cases on the basis of evidence, not even simplified cases. (And it is not in that other simplified case that the part played in chess by the king could be discovered.)

“Complex, real life behavior” is the paradigm case of human behavior, the case in which the phenomenon is most clearly and *simply* exhibited. It is the existence of *that* phenomenon that leads us to conduct experiments, and it is already intelligible by reference to the concept of a Person. So the “simplified” case is not unlikely to be also the degenerate case which only

vacuously illustrates the basic principles of the phenomenon and is therefore the worst possible case for deriving an understanding of the phenomenon.

(3) "But laboratory situations *are* real life situations for the subjects—they do not behave in a peculiar way in laboratory experiments in contrast to the rest of their *behavior*."

The results of recent interest in the psychological experiment as a distinctive behavior setting (Orne, 1962) should discourage any primary reliance on this thesis. They *are* real life settings, and they differ *significantly* from other real life settings just as the latter differ among themselves.

However, the primary question here is not whether the behavior of subjects is peculiar, but whether it is correctly described, and what that amounts to. Laboratory experiments resemble projective test situations in that in neither case could the subject's behavior be one of the paradigm cases for any ordinary description of human behavior (at least, none beyond the most nominal and innocuous kinds of description, e.g., "He said ' . . . ' " or "Then he put the paper in the third box.") With respect to such terminology as "anxiety," "hostility," "conformity," etc., the concepts and practices embodied in descriptions of this kind were developed elsewhere and have their primary use elsewhere. So in all cases their application in these technically psychological settings needs to be evaluated as to whether an application of existing linguistic usage or an extension or gross modification of it is involved. The latter would seem to be the rule rather than the exception, considering the emphasis put on "objective" and "precise" measures by psychological investigators. Whatever else may be involved here, these two terms generally imply a contrast with the standards of ordinary usage. In this connection, one thinks, for example, of (a) GSR or test measures of "anxiety," (b) paying money to subjects in order to achieve "ego involvement," and (c) guessing of test responses as a measure of "empathy."

The converse problem with respect to technical terminology which has its primary use in experimental settings is, of course, a familiar one. For such terminology there is no general

reason to believe that it would be anything but an empty gesture to extend its use beyond these settings—empty because we have no effective way of implementing such linguistic innovations except, perhaps, by doing what we have been doing all along (Chomsky, 1959; Mason and Bourne, 1964; Jones, 1965). Ordinary prudence requires an appraisal of each such extension.

Because the role of the scientist involves linguistic and non-linguistic innovation, the scientist as such has a warrant for deviating from accepted linguistic practices. He operates in a sphere of linguistic irresponsibility much akin to poetic license. But both poets and scientists are evaluated by how much they achieve that is not *merely* an exercise in linguistic irresponsibility. It is generally agreed (cf. Carnap, 1956) that this kind of achievement is in no way guaranteed if a scientist *merely* acts in accordance with the professional rules of thumb which are current among his fellow scientists, e.g., those rules of thumb associated with such terminology as “objective,” “predictive,” “construct validity,” “operationalized,” “repeatable,” “confirmable,” “statistically significant,” “experimental controls,” etc. There will be times when fruitful scientific endeavor requires some extension and innovation with respect to *those* linguistic and nonlinguistic practices. What counts is that the innovation should be more than a form of utterance (more, too, than merely chalking up a “+P” or a tentative “T” or “F” alongside some sentence or two), and that its significance should extend beyond the boundaries of psychological experimentation. The *way* it counts is, of course, whatever difference it actually makes. Naturally, the relevance of an innovation is generally not fully disclosed at the outset, but this is not to say that we should forbear to ask “what for?” To speak of “apparently worthless” in appraising innovations would seem to avoid both a “vulgar pragmatism” (Kaplan, 1964) and the equally and oppositely polarized notion that there is a peculiar kind of activity, “pure science,” which consists in the unfettered search for truth.

Thus, the thesis that experimental situations are *real* life situations misses a second important point, that these situations are not “the real thing” *for the experimenter*, since it is only

insofar as the conceptualization (law, theory, hypothesis, etc.) which is relevant to an experiment has at least a potential relevance beyond the instances of its experimental validation that there is any point in the experimental effort or the conceptualization itself.

The paradigm of psychological investigation which is most commonly accepted at present is a semantic paradigm expressed in the following sequence: theory—hypothesis—operationalization—confirmation. It is appropriately described as a semantic paradigm insofar as the primary concern here is with the appraisal of the truth value of statements. The fit of this paradigm to what psychologists are apparently doing is like the fit of a suit that is an inch too small in all dimensions—almost any single deficiency can be compensated for with a tug or a wiggle or an awkward posture, but it is fundamentally inadequate. For example, the psychological scene is almost totally devoid of anything that could be called a theory, if by “theory” we mean a set of statements that is necessarily logically incompatible with *some* statement describing an empirical outcome. What we do find are models and descriptive systems, both of which have the general characteristics of being linguistically insulated from descriptions of fact, so that no report of experimental outcomes is more than informally related to a change in the model or descriptive system as such. The response to negative experimental results can perhaps best be described as the exercise of ordinary prudence in not continuing to make factual statements which contradict the descriptions of experimental outcome. If the conclusion is reached that a given model is not particularly appropriate to a given phenomenon, that conclusion is not particularly a matter of logic and it is only indirectly a question of evidence, and anyhow there are always other phenomena on which the model can be tried out. In a word, the norm of experimental activity is not that of exposing theories ruthlessly to test the truth, but rather, that of demonstrating the successful application of a given model to one of the recognized “problem areas” in psychology, and of making whatever verbal and procedural adjustments toward this end are suggested by experimental outcomes.

That this should be so need not be regarded as the simple sociological consequence of existing norms concerning the publication of negative findings. It *may* be taken as reflecting an awareness that truths are cheap and infinite in number, that almost any theory could be kept alive indefinitely so long as words mean what the theorist wants them to mean, that one can always derive as “consequences” of a theory results that could have been predicted with confidence anyhow, or follow up only the successful pilot studies, etc., and all of this is formally impeccable. Nothing in the semantic paradigm of theorizing, hypothesizing, operationalizing, and verifying represents any safeguard against triviality, capricious practices, and other linguistic excesses. The considerable attention devoted to a formulation of criteria for empirical meaningfulness is relevant to only one of an indefinite number of loopholes associated with the semantic paradigm.

There are safeguards, of course, but these lie in the way psychologists are trained, in the restraint they exercise on one another and the assistance they give to one another, and on their *pragmatic* appraisal of what the state of psychological science provides reason enough for psychological scientists to do. The criteria for what it is worth being successful at are not truth criteria. A psychologist, no less than other Persons, has reason enough to try to treat what he does as being successful.

It seems clear that even a minimally adequate codification of what psychological scientists do requires a pragmatic statement, not a semantic one. As an initial contribution toward this end, the following paradigm is presented as a simple generalization: **Conceptualization–Decision–Action–Vindication**. Each of these four terms is the pragmatic analogue of the corresponding term in the semantic paradigm, and each includes its semantic analogue as a special case:

(a) To theorize about something is to conceptualize it as being a certain sort of thing, but not all concepts are theories. For example, the concept of a Person (or of an organism, or of a physical object) is as complexly articulated as many theories, but it is not a theory (no more than the rules of chess are a theory about chess).

(b) To form the hypothesis that X leads to Y is an instance of making a decision (the decision to treat X as leading to Y), but there is a whole range of other decisions also involved in conducting an empirical study.

(c) To operationalize a description by engaging in specific experimental procedures is an instance of following a certain course of action, but not all of an investigator's experimentally relevant actions are appropriately included under the heading of "operationalizing" (and under the pragmatic formulation, none of them *have* to be).

(d) Finally, to verify a significant prediction to the effect that X leads to Y is to vindicate the decision and course of action involved in treating X as leading to Y.

The last point, (d), makes explicit the condition that standards of significance are applied at some time, and that without such an appraisal scientific activity is unintelligible except as a *fragment* of a significant activity. To see such a fragment as a complete unit would be to see the actions of scientists as scientifically pointless, though allowing, perhaps, for some purely personal significance.

The third point, (c), provides the linguistic basis for the explicit recognition that a variety of decisions and procedures on the part of the investigator are all relevant to the successful outcome and that it is this "package," rather than, e.g., simply the making of one prediction rather than another, that is vindicated by a successful outcome. In turn, such recognition may serve to motivate the formulation of more effective rules of thumb for what and how to change in the light of experimental outcomes. As matters now stand, the exclusive focus on the prediction can be implemented effectively only on the condition that experimental procedures should in general *not* be open to question, and the predictable consequence of having to protect an over-investment in prediction has been that what would be appropriately used as procedural rules of thumb are elevated to the status of standards of acceptability.

The replacement of "confirmation" by "vindication" reflects an explicit recognition that an empirical procedure which is

successful in contributing to our understanding of a phenomenon need not consist in establishing the truth or the increased likelihood of some statement, and this goes with the recognition that it is primarily concepts, rather than merely statements of theories, which guide behavior. (Any statement of theory codifies the way certain concepts—primarily the “theoretical” concepts—are *intended* to guide behavior. But, of course, there is no guarantee that that will happen.) This conclusion is by no means peculiarly the outcome of the preceding discussion, but it probably does have a greater-than-base-rate association with a pragmatic outlook.

The investigator who acts in the light of this pragmatic paradigm will characteristically engage in the following procedures: (a) He identifies a significant goal toward which his effort is intended to contribute. (b) In relation to that goal he identifies an immediate objective, which is what he aspires to achieve in *this* effort. (c) He formulates his conceptualization of what the relevant considerations are—a description of the phenomenon under investigation and a specific identification of any features of the situation which are regarded as troublesome, problematical, significantly constraining, etc. (d) He decides upon a course of action for which the considerations given in (a), (b), and (c) jointly provide reason enough. (e) He implements this decision as well as he can, i.e., he tries to get what he wants in the light of what he knows and what he knows how to do. Finally, he reappraises the significance of his action and its outcome. This includes not merely decisions as to degree of success, but also, a consideration of the potential relevance of his achievement to other goals that had not previously been brought into the picture, resolutions for what to do differently next time, what to do next, etc.

One of the distinctive features of this procedure is that the relation of (d) to (c) is not *in general* a deduction of any kind, e.g., a deduction of theoretical consequences via correspondence rules or operational definitions; although there is nothing to prevent a person from making deductions of this kind in going from (c) to (d), there is also no necessity for any deduction to

be made. What replaces the deductive aspect of the semantic paradigm is the application of standards of rational behavior, and the standards which will normally have the most immediate relevance will be those of the scientific "public" which is most immediately concerned with the subject matter of the experiment. If there were no such standards to be applied, no deduction could occur and no investigation would be informative or have any point.

A second distinctive feature is the use of maxims, particularly in connection with (c) and (d), above. Although they have not been explicitly mentioned previously, the use of maxims as part of the pragmatic approach seems inevitable because maxims provide a natural and effective way for an investigator to be both clear and specific in distinguishing between (a) what he is treating as obligatory and what as a matter for decision, (b) what he considers within his capacity and what he does not, and (c) which conceptualizations of phenomenon, procedure, or outcome he is treating as questionable and which he is not. In the semantic approach the burden of all of these pragmatic functions rests primarily on the statement of assumptions or postulates. The latter have some drawbacks in this regard because they are likely to imply more than is intended or needed, so that it is also likely to be unclear how they are to be taken. (This drawback is illustrated by a road sign which says "There are rocks on the pavement," when its intended function is directly accomplished by the familiar, "Watch for rocks on pavement," which involves no excess baggage in the form of factual commitments. It will be argued later that this drawback is also illustrated by a familiar conceptual road sign which says "Nature is orderly.")

If we act in the light of the pragmatic paradigm rather than the semantic paradigm we may be expected to do things differently in some respects. Of course, if we already have a reason for taking the pragmatic approach rather than the semantic, our doing so would not then also have to be justified by reference to what we came to do differently. Such a reason may be found in an appeal to the well established principle that if we are

explicit about what we do we are better equipped to deal with new situations and to profit by experience. Because it is easy to see the point of claiming that the pragmatic formulation *is* the more explicit and coherent one. A formulation in which conceptualization, evaluation, competence, and interpersonal agreement are given explicit and *systematic* connectedness in what scientists do is to be preferred to one which goes, roughly: “What scientists do is formulate theories and conduct experiments to find out whether they are true.” “Oh, by the way—unless these truths are worth something, which may or may not be the case, it’s all beside the point.” “Oh, and then, too, unless enough of them agree on which truths to talk about, you can’t hardly tell the scientists from the patients, and it’s even a bit of a guess as to what the difference is between the truths and the nontruths.” “Oh, and another thing, there are these criteria for meaningfulness . . . ,” etc. This is a pointed sort of vignette and perhaps it is overdone. But perhaps not—perhaps it only evokes an initial impression of indecent exposure, attributable to the centrality of our socialization into the multiple proprieties of covering up that last inch.

In any case, there *is* also some point in providing a concrete example to show that in some cases, at least, adopting the pragmatic approach does make a significant difference. The following is an abridged description of a series of five studies reported in detail elsewhere (Ossorio, 1964). The present description parallels the “characteristic procedure” described above in connection with the use of the pragmatic paradigm.

III. Second-generation Psycholinguistics

1. The significant parochial and nonparochial *goals* of the studies were:

(a) to increase our technical psychological capacity for implementing descriptions of the form “He knows about X,” or

“He is treating something as an X”; i.e., the concern was with the form of cognition implied by the concept of a Person;

(b) to contribute to the alleviation of a currently acute and constantly worsening social pathology, i.e., the choking up of the channels of communication between producers and consumers of (primarily) scientific and technical knowledge.

The connections between (a) and (b) are provided by the following maxims:

A. If you want to find out something, the best thing to do is to ask a person who knows and who understands what it is you want to know and is willing to tell you. As a practical ideal for codifying what is known within a sizeable public, aim for a machine that behaves like a person in this respect, but which can also bring together the knowledge of many persons.

B. Because the time and volume aspects of the communication problem (b, above) are so extreme, only a completely automatic, computer-based information processing system *can* provide a solution.

C. What makes a document or piece of information relevant to, e.g., the design of an airplane wing section, is that the people who are involved in that set of practices (the people who know how to do that) are willing and able to treat it as part of that set of activities: Therefore, don’t waste time analyzing or classifying what are commonly called “objective” features or documents or messages; instead, try to duplicate the crucial features of what those people do with these documents or messages—that is what is *both* objective and significant about something that is a document. (In this respect, documents differ fundamentally from pieces of paper with marks on them.)

D. The construction of a physical system which demonstrably duplicates significant human capacities for treating something as being of a certain kind is at least as good as any other currently available method for demonstrating current understanding and furthering subsequent understanding of human behavior.

2. Thus, the immediate *objective* was to demonstrate the successful operation of a set of completely automatic procedures for storing and retrieving information “about” particular topics of interest; more specifically, procedures for doing this by duplicating the capacity of Persons for judging the relevance of a variety of information “packages” to specific topics of interest.

3. Relevant *considerations* included:

(a) the characterization of “relevance” as a primitive term for pragmatic discourse,

(b) a partial sketch of the scientist *qua* scientist as a Person, and

(c) of the scientific community as being highly stratified into “publics,” and

(d) a description of the input-output relations to be expected from the computations which are associated with correlating, factor analyzing, and measuring factors.

(e) Maxim: The judgments of relevance made by persons who “know about” the subject matter in question are the only data that is criterion data other than data provided by observing them at work, which has its drawbacks.

(f) Maxim: Do not try to program complex human judgments—almost any alternative will have greater long-range promise.

4. *Decisions*:

(a) To make use of the organized storage of data as an alternative to programming a simulation of the “process” of judging subject matter relevance;

(b) To accomplish this by factor analyzing criterion data, with the subsequent factor measurement providing the essential procedure for subject matter indexing.

(c) To stratify the criterial judgments on the basis of what the judge knows, not on the basis of what he says. (Maxim: We do not count cases on the basis of evidence, and correlational evidence is no exception.)

(d) To carry out the demonstration with a minimal data base. (Maxim: The demonstration of meaningful results with a minimal data base will ordinarily carry the same kind of conviction as the prediction of a surprising or counterintuitive result. Assumption: If the stratification of the scientific community is as significant a social fact as it is taken to be, then the difference it makes should be *readily* demonstrable if the experimental approach taken is going to demonstrate a difference at all.)

5. *Actions:*

Twenty-four subject-matter fields were selected for defining a subject-matter domain to be studied. These included such fields as “biosynthesis,” “beam theory,” “field theory,” and “spectroscopy.” The fields were selected from the four general content areas of physical chemistry, biochemistry, electrical engineering, and aeronautical engineering. For each of the four general areas, one or more “Experts” (persons competent in the area by virtue of academic training and/or professional experience) were assigned to identify and select a “corpus” for each field. The corpus consisted of 6 “documents,” each consisting of a minimum of 6 consecutive paragraphs of text which was taken from the recognized literature of the field and was judged, upon inspection, to *be* part of the literature of the field. In each corpus, the technical terms were identified by the Experts. From each corpus, 3 terms were selected at random, making 72 terms in all. For each of the 24 fields, a set of 3 or more judges (experts in the specific field) was selected. A total of 77 judges was drawn from graduate student and professional populations in New York, Colorado, and California. Each judge was given the task of rating the degree of relevance of each of the 72 terms to his field of competence. An *ad hoc* 9-point rating scale was used, similar to, but not identical with other scales previously developed and used in psycholinguistic studies.

Ratings by the several judges for a given field were averaged, giving a 72 x 24 data matrix. The 24 fields were intercorrelated and the correlation matrix was factored by Comrey’s

Minimum Residual method and rotated in accordance with the varimax criterion. The resulting factor space (7 common, 7 unique factors) was designated as a **Classification Space**, or **C-Space**, because of its intended use in classifying documents as to subject matter.

A set of 16 “test documents” (each consisting of one paragraph of text) was selected quasi-randomly from the corpora of 8 of the 24 fields represented in the C-Space domain. From each of the test documents, 4 to 6 technical terms were selected as “vocabulary terms.” Both the vocabulary terms and the test documents were rated by a set of judges with respect to the 24 fields. Approximately 25% of these judges also served as judges for the factor analysis data. On the basis of these judgments, both vocabulary terms and test documents were assigned to locations in the C-Space (they were “indexed”), following the usual weighted average (third power) procedure for estimating coordinate values. These locations were designated as the “psychometric locations” of the terms and the test documents. Eight phrases designating some “topic of interest,” e.g., “vector analysis,” “the synthesis of fat,” “types of fields,” and “contraction properties” were similarly rated and indexed in the C-Space.

A third set of judges was selected on the basis of their competence in the 4 fields from which the “topics of interest” were taken. This set of judges overlapped the factor analysis set of judges (33%) and the factor measurement set of judges (67%). Each judge was presented with 2 “test packages,” each consisting of 6 test documents and 1 topic of interest falling within his field of competence. Each judge was given the task of ranking the 6 test documents in the order of their degree of relevance to the topic of interest. These rankings were criterion data and a consensus ranking was computed by averaging over the several judges for each field. Thus, a total of 8 consensus rankings was obtained.

A relatively *a priori* computational formula, containing no empirically determined parameter values, was used to compute the C-Space coordinates of each test document as a function of

4 to 6 vocabulary terms appearing in the document. (For instance, the document was “read” and classified as to subject matter on the basis of words that occurred in the document and were “known” to the “reader.”) For each test package the C-Space distances from the topic of interest to each of the 6 test documents were computed, and the test documents were ranked in the order of their distance from the topic of interest. This was the “system ranking” of the test documents. Each of the system rankings was correlated with the corresponding criterion consensus rankings. A similar analysis was carried out using the psychometric locations of the test documents in arriving at a set of rankings designated as the “psychometric rankings.”

6. *Vindication:*

For 7 of the 8 “test packages,” the correlations between the psychometric rankings and the criterion consensus rankings ranged from 0.896 to 0.984 (Mean = 0.939). The corresponding figures for the best 6 of the 8 system rankings were 0.879 to 0.951 (Mean = 0.920). The seventh dropped to 0.690. This drop represents the main indicator of the degree to which there is still room for technical improvement in approximating human judgments by means of the completely automatic Classification Formula procedure. (All reported correlations above 0.690 were significant at the .01 level for $N = 6$.) In general, these results represented a higher degree of correspondence than that between judgments of relevance made by 8 individual judges who contributed to the criterion consensus (Mean $r = 0.829$). The negative instance ($r = 0.195$ and $r = 0.015$, respectively) among the 8 test packages was of particular interest because it occurred under just those conditions where that result was expected, i.e., where the topic of interest was quite close to the C-Space origin. In such a case, the topic of interest is essentially outside the entire content domain of the C-Space (like asking for sheet music in a chemistry library), which is always a possibility for a library of less than universal scope. Under these conditions a Person would say “I don’t know enough about that to be able

to help you,” and the information processing system under consideration could do likewise, inasmuch as the distance from the origin to the location of the topic of interest is a piece of information which is automatically available to the system. Since both the positive and negative outcomes occurred in accordance with expectations and both are consistent with the successful functioning of a linguistic data processing system in an operational setting, the results of the study were taken to vindicate the decisions and procedures contributing to the results.

Here it is worth noting that what showed such a high degree of correspondence was, on the one hand, (a) judgments by individuals in one field of competence and, on the other hand, (b) a set of distances based on a factor analysis of ratings by 27 other individuals, and on factor measurement provided by ratings by still another set of some 26 to 30 individuals and on the use of a computational formula having no empirically determined parameters to provide a Finagle coefficient. That is, it was agreement between an essentially intrapersonal structure and an essentially trans-personal structure. (The criterion ranking judges for a given test package were treated as equivalent units—any one of them could have been substituted for the consensus ranking without altering the results substantially. On the other hand, neither the factor analysis judges from different fields nor the factor measurement judges from different fields provided equivalent data.) This kind of correspondence is seldom sought and seldom found in psychological research (but cf. Jessor, 1964); it is a genuinely *structural* correspondence as contrasted with the commonplace finding that members of a group have *similar* attitudes, interests, beliefs, habits, etc. Thus, it is entirely consistent with the view, presented earlier, that what is special about linguistic behavior is its irreplaceable signalling function within a structure of participation in existing social practices which are themselves interrelated so that, for example, to engage in linguistic behavior *as such* is already to be participating in a special set of social practices. And it is entirely

consistent with the thesis that what is constitutive of human behavior, including scientific behavior, is just that it *is* participation of this sort, codified in the concept of a Person.

There is more to be said about the relation of the preceding study to the pragmatic and semantic paradigms of psychological investigation. It should be clear that in fact the study could hardly have been conceived under the semantic formulation, although there is no *logical* incompatibility. It is also the case that once begun, it would have been quickly abandoned in the light of some procedural rules of thumb which have come to be widely used as basic standards of “experimental rigor” in connection with the semantic paradigm. Specifically: The data matrix referred to above, consisting of 77 judges by 72 technical terms, is clearly recognizable as a multitrait monomethod matrix. (The study was designed to make use of 3 terms per field and was therefore presented in that form above; in point or fact, 12 terms per field, a total of 288 terms, was used for both the C-Space and the multitrait monomethod analysis—see below). The several judges for a given field would constitute different “tests” purporting to measure the same thing, to wit, the degree of relevance of those terms to that field. As a basic precaution, therefore, it would be important to examine the intercorrelation matrix for the 77 judges in order to establish convergent and divergent validity. Judges within a given field should correlate substantially more highly than judges in different fields. And it would be particularly important to establish this, considering that the number of terms used in the study amounted to a microscopic sample (this would hold for either 3-term or 12-term samples) from the vast, though finite, number of technical terms currently used in the fields of knowledge studied.

The correlation matrix failed signally to conform to the convergent-divergent requirements. Correlations between judges in the same fields averaged 0.45 and ranged from -0.10 to 0.81.

Seventy-two percent of the judges correlated most highly with a judge in a different field. The result of factor analyzing the 77 judges was as chaotic as the correlation matrix. Several

paradoxical combinations of fields loading substantially on a single factor were found; in other cases the judges in one field split up into two or more factors; several other factors could be characterized in a vague way individually, but were difficult to distinguish from one another conceptually. Thus, on this procedural basis, the study was a clear and dismal failure. This appraisal was confirmed by several experimental and psychometrically oriented psychologists, none of whom (a) demurred at the multitrait monomethod characterization, (b) failed to take a dim view of the adequacy of the sample, or (c) knew of *any* basis for *expecting* any salvageable results from the data in the light of the pattern of correlations.

In contrast the factor results obtained from the 72 x 24 matrix which resulted from combining the scores of those disagreeing judges within fields were eminently interpretable with a gross structure conforming to what would be expected from a general knowledge of the fields in question. (The rejection of the multitrait multimethod approach in its entirety was implied by decision (c), above.) Some minor surprises occurred but they were readily interpretable and highly plausible. An example of such a "surprise" was the substantial (0.635) loading of the Physical Chemistry field of "fluctuations and Brownian Movement" on a factor which was designated as "molecular (Fluid) Dynamics" on the basis of its primary association with certain Aeronautical Engineering fields (Aerodynamics 0.848, Air Properties 0.844, and Aircraft Design 0.742). (In fact, the former does deal with the dynamics of molecular particles, primarily in liquids, whereas the three latter deal with the dynamics of molecular particles in *gaseous* fluids.) Six major common factors and one minor one were found. This factor structure was reproduced not merely substantially, but more nearly identically (i.e., not merely the same fields being associated with the same factors, but with identical or near identical rank order of factor loading on 5 of the 6 major factors and a surprising correspondence of the specific numerical values of the factor loadings) throughout a series of experimental manipulations which in

terms of factor analytic rules of thumb could have been expected to result in some very substantial changes in factor structure. These manipulations included (a) replication with samples of 4, 6, and 12 terms drawn from the corpus of each field (so that the 77-judge matrix was comparable to the most adequate rather than the least adequate of these replications. In fact, the data for the 77-judge correlations was identically the same data that was used for the C-Space in which the relevance ranking study was carried out); (b) replication with a different set of judges who showed no greater apparent agreement than the initial set; (c) replication based on a completely different corpus selected from the literature of the 24 fields by the same procedure (but some different Experts) as the first corpus; (d) reanalysis using a different and mathematically nonequivalent method of factoring (from Comrey's Minimum Residual method to Lawley's Maximum Likelihood method) and finally; (e) replication in which sample size, sample source, set of judges, and method of factoring were altered simultaneously. What *was* found to affect the factor results substantially was a thoroughly biased sampling procedure in which the terms judged were selected from only 10 of the 24 fields.

The degree to which replicability was demonstrated may be illustrated by reference to coefficients of congruence (Harmon, 1960) which were computed for the corresponding factors in the 7 analyses involving sample size variations. Six factors from each of the 7 analyses were used, making a total of 126 coefficients of congruence. The *lowest* value among these 126 coefficients was 0.970. In comparison, a value of 0.90 is accepted by Harmon and others as providing an adequate basis for accepting the correspondence of factors.

The contrast between this set of results (together with the relevance ranking results based on these results) and the semantic approach needs no discussion. The adoption of the pragmatic formulation not only can make a difference—it *has* made a difference. To be sure, this is only one example (a quite different study identifying a kind of "placebo effect" of theory on therapists (Brittain, 1965) could also have been discussed here),

but it will serve to establish the principle if it is agreed that the results presented here do not suffer in comparison with typically reported experimental results in respect to carrying empirical conviction, opening up further avenues for investigation, and contributing to our understand of human behavior and our ability to deal with it.



PART FOUR

Theoretical Persons

The concept of a person is the concept of an individual whose history is a history of intentional actions, qualified, elaborated, and interrelated in ways described previously (Part Two). Just as the primary function of the rules of Bridge is to guide the behavior of one Bridge player with respect to others, the primary function of the concept of a Person is to guide the behavior of one Person with respect to other Persons. Just as the rules of Bridge are *constitutive* of Bridge-playing behavior, and not merely a theory about Bridge-playing, the concept of a Person is not merely a theory of human behavior. What keeps us from giving this fact its modest due is that as *empirical* scientists, we want to hold that nothing about the world is a priori—we take pride in being hardheaded (an ambiguous term that): “But how can you tell if it’s true?” “Show me.”

To doubt, to raise a question as to the truth of certain statements, is itself an instance of rule-following behavior, and to make investigations regarding the truth of statements is likewise. It is not always appropriate to engage in that kind of behavior (that is a necessary part of its having any significance).

The negative “never ruled out a priori” is not to be confused with the positive “always appropriate.” This is clear enough in the arena of daily human activity. The results of investigation are given a particular kind of authority. That is because what we accept as an investigation is an activity the results of which we *do* give that kind of authority. In the brief, we have criteria for what is and what is not an investigation. The authority of investigations is no more a feature of the specific techniques of investigation (including saying “How do you know that such and such . . .?”), than the authority of a trump is the feature of the specific coloring and composition of that piece of cardboard (cf. Wick, 1964).

To recognize instances of the concept “investigation” and to implement that understanding in action requires a certain kind of competence, just as it requires other skills to recognize instances of “anger,” “green,” “meter reading,” or “ice cube” and

to react appropriately to them or to references to them. And because it does, it is possible to make mistakes in this regard. Not all mistakes are the same here. Some arise from simple clumsiness, from lack of skill, some from careless observation. These are the mistakes against which we are well warned and prepared.

There is another kind of mistake about which we think and do very little. That is, the indiscriminate use of the performative techniques which have a significant normal use. We have some awareness of this when it comes to the use of statistical methods, experimental designs, or physical apparatus in experimentation; here, we recognize the point of the old saying that if you give a small boy a hammer he will discover that everything around him needs to be pounded on. Yet when it comes to verbalization, it is more nearly the case that "anything goes." To a large extent this may be seen as a consequence of casting science in the image of semantic theory. In any case there is a place for an occasional reminder that the "Law of the Instrument" holds for language as well as for apparatus. We do not express a doubt, and thereby signify that an investigation is in order, *merely* by looking wise and asking, "How do you know that such and such?" or by looking puzzled and asking, "How do you explain such and such?" No more do we perform a marriage *merely* by looking solemn and saying, "I now pronounce you man and wife," or play a trump *merely* by laying a piece of cardboard on a flat surface.

In each case it is the place of the performance within the wider circumstances of its occurrence which determines *what* took place. In the right circumstances a trump was played, a marriage was performed, a doubt was expressed. If the circumstances do not provide reason enough for doubting, then the person who says something which constitutes an apparent expression of doubt will not be doing anything intelligible just because he goes through some familiar motions ("How do you know that such and such?") His behavior will not be intelligible as an action unless he or we can provide a special explanation

which removes the “apparent exception” (Part One) by an appropriate specification of something he wants or knows or is trying to accomplish (and then it may simply be his doubt, but not ours). Without such an explanation, we understand his behavior as a defective performance, a symptom of incapacity, just as we should understand an examination paper which showed “ $18 + 43 = 71$ ” (or, frequently, the way we understand a slip of the tongue).

In this light, it is easy to see that much of psychological experimentation is not investigative in nature but is, rather, an ordinary sort of activity of a generally observational and exploratory sort which carries no particular authority. In Part Three it was argued that the primary importance of this kind of activity is that it is *potentially* a part of a socially creative process. However, this potential will be poorly realized so long as psychologists perceive themselves as engaged primarily in trying to achieve an authoritative account of human behavior (to “explain” it) rather than as trying to increase our understanding of it. Yet it is clear that the former is what most psychologists who perform experiments take themselves to be doing. This is perhaps best illustrated by the way in which experimental findings are generalized to “explain” the behavior of everyday life. ❖

I. The Mediation of Generalization

“What has the same description is the *same*.” The behavior of psychological investigators is highly predictable—they will generalize experimental findings along verbal lines. I present N subjects with Test X and Task Y, and I describe the results by talking about, e.g., “the effect of high drive on performance in a complex task.” To be sure, I *selected* Test X and Task Y with this end in view.

To proceed in this general fashion is one of the things a psychologist knows how to do.

But after all, it is having concepts, codified in just such expressions as “performance” and “complex task,” which involves our having criteria for what is to be counted as being the same. So the other instances of “high drive,” “complex task,” etc., which have not been examined *are* “the same” as the ones we now examine. Then why *not* generalize along verbal lines?

Generalizing is not a verbal exercise. The warrant here is only the warrant to *try* to generalize, and the constraint is one of ability, not respectability. It is the practices that go *with* calling something “an X” that give significance to the practice of calling a thing an X. Ordinarily, what we give the same description to are a set of things which we *do* treat in the same way (the same color, the same intention, the same illness, etc.) Our language reflects this, as it reflects, too, what we count as treating different things in the same way. (To treat *this* thing as a pawn is to treat it *the same* as I treated that other pawn, but that should not suggest, for example, that I make the same moves with it, or that it serves the same end.) Our capacities to agree in these respects are among the basic human capacities codified in language. Without these capacities we could not understand one another, and doubly so, for there would be nothing to understand—there would be no such event as the event of someone having *said* something.

So unless there are practices (or until there are) that go with calling a thing, e.g., “high drive,” or “complex task,” or “impaired performance,” calling it that is a noncommittal gesture, and its being noncommittal here reflects the immunity previously described as “linguistic irresponsibility.” If I thus deploy my terminology in the absence of supporting practices relating, e.g., to the real life “high drive” cases to which I “generalize,” I may nevertheless insist that I have *said* something. But then, others will know how to make allowances for that. If scientists were held to the standards of linguistic rigor which obtain in ordinary discourse their task would be well—nigh impossible. Practices do not spring full-fledged from the scientist’s brow.

But it is not a *simple* linguistic excess to “generalize” along verbal lines beyond any vindication of the attempt. Several additional behavior-guiding concepts are commonly involved in this practice. Central among these is the concept of the “genotypic description,” the “essential common element.” Along with this are the concepts of “psychological process” and “determinism.”

Briefly: A major paradigm for psychological explanation is that the event P (what is to be explained, e.g., seeing an object as red, or as hostile, or forgetting a name, or performing poorly) is the outcome of a deterministic psychological process (e.g., adaptation, repression, extinction, increase in response probability, etc.) It is this process, or perhaps several, which provides the genotypic description (what *really* happens) of event P. And it is the common presence of this ingredient in cases of P which provides the vehicle for generalization; it provides the antiphonal “because . . .” to the query “Why P?” not merely for *this*, but for the other, unexamined, cases of P.

If the event P is my seeing something as red, our explanation will begin, e.g., with the frequency-energy spectrum of reflected light as the (for our purpose) initial determining factor, and it will involve some reference to a capacity for reacting to such stimuli, and some reference to perceptual processes of, e.g., “adaptation,” “summation,” “decoding,” “integration,” “leveling,” or “closure.” And if event P is my treating someone as an old grouch, we will commonly ask “what is the objective basis for the perception of hostility in general and for this instance in particular?” This will normally be like asking, “what is the emotional wave length, the invariant,” of the phenomenon which sets into motion the processes which eventuate in the perception of hostility?.”

What is questionable about this strategy is—everything:

(1) What is implied by the process approach is that there must be something about each instance of hostile behavior *by virtue of which* it is an instance of hostile behavior, and only by reference to which we are justified in calling that behavior hostile; and it is because this something is the same something

in all the cases that we call “hostile” that all cases of “hostile” are *the same*. And there is no question that there is something common to every instance of hostile behavior. What is common is that it is an instance of hostile behavior, and if we observe it what occurs is that we observe an instance of hostile behavior (cf. Bambrough, 1962). Nothing else is required for there to *be* instances of hostile behavior except that we should have the concept and the capacity to implement descriptions of that sort. To suppose that there is a specifiable something *else* that occurs every time is to engage in the remarkable supposition that when it comes to person description everything happens in pairs (and why restrict it to person descriptions and why to pairs?). It may be so, of course, and we may yet find the elusive alter ego of hostile behavior—that is an empirical question. But if the value of empirical studies in psychology depended on there being wholesale coincidences of this sort, psychological science would be not merely a gamble, but sheer speculation.

(2) Psychological “processes” are not processes at all. Or, these “processes” share no significant features with the more familiar, paradigmatic cases from which we derive our concept of a process. The general characteristics of a process are that (a) it occupies a solid interval of time (thus, processes account for the connections between “discrete” or “separate” events, e.g. outcomes), (b) it has a beginning, middle, and end or at least, (c) the state of the process at any arbitrarily selected moment of time is describable, and therefore the difference between the states of the process at any two times during its history can be described, (d) the process is identifiable and describable separately from the initial conditions or the outcomes on any given occasion, so that, for example, we can speak of “the same” process having different outcomes on different occasions, and we can speak of the process being interrupted or arrested. The *flow* of water down a slope, the *burning* of a candle, the *growth* of the pile in the hourglass, and the *filling* of a cup are paradigm cases of processes.

Not all processes have *all* the characteristics mentioned above—that is why we speak of paradigm cases. But psychological processes are distinctive in that they have *none* of these features. Instead, they have the general characteristics of outcomes, or changes of state. They can be dated, but not clocked. (Running a race is a process, and it occupies a period of time, which can be clocked as, e.g., a “four-minute mile”; winning or losing a race is an outcome which occurs at a given time, which can be dated, e.g., 9:05 a.m. Monday, October 14, 1984, but it occupies no time at all.)

This feature of psychological processes is most clearly brought out by asking for descriptions or identifications either of intermediate stages of the process or of the process separately from outcome and initial conditions. What, for example, is the situation part way through the “process” of deciding between alternatives (not of *trying* to decide, which may take considerable time and never eventuate in a decision, but literally, of *deciding*), or of increasing a response probability, or reducing dissonance, or recognizing an object as red, or sampling a stimulus element, repressing an idea, projecting a wish, or making an inference, drawing a conclusion, or adding 18 and 43? In each case, the answer would seem to be—nothing. It is not information, but concepts that are lacking here. It is not that the relevant observations are thus far impossible for technical reasons, but rather, that no observations of any kind are relevant—at least no terminology and no description of hypothetical observations toward this end have been advanced by psychological theorists.

“But if we can treat these things, e.g., increasing a response probability, as processes, as apparently we have, what difference does it make whether they *demonstrably* have the required features?” Precisely. To raise this question is to adopt the functional, pragmatic point of view, and it is to recognize that although there is nothing positively reprehensible in talking about underlying psychological processes, there is also no *particular* point to doing so. That is to say, if it is useful to talk this

way, that is not because our talk *refers* to a process, but at most, because it is *process-talk*.

(3) But it is not just any kind of process that in fact is involved here. It is specifically the asymmetrical, or “underlying,” type of process that has been so sought after, and there is some reason to believe that this search has been primarily motivated by the part that such processes play in implementing the requirement of determinism in the paradigm of explanation identified above.

The principle of determinism appears to be generally regarded by psychologists, though perhaps not by philosophers of science, as a basic assumption about the world which is necessary in order for scientific investigation to be intelligible. The notion here is that without such an assumption, scientific activity would be pointless, if not actively irrational.

There appears to be no evidence to support such an assumption. It would be difficult, if not impossible, to identify anything that could count as *evidence*. But then there need not be, and no such assumption needs to be made. There is necessity here, but it is a linguistic necessity, not a factual one. And, it is properly expressed as a maxim rather than as a substantive assumption: If *x* happens, then “why *x*” is never ruled out in advance, though it may happen that “why *x*?” does not succeed in raising a question. “Why *x*?” may succeed in asking a question without thereby raising a question, e.g., when the answer is “no reason at all” (see Anscombe 1957 and Manser 1961 for a discussion of “why” questions). This is a general feature of any descriptive system which involves “event” concepts (though it is not restricted only to these). In particular, it is the case for physical, biological, psychological, and sociological descriptive systems. Each generates its own questions and makes provisions for the answers—only what can be answered can be asked.

Here it is worth noting that an entire genus of paradigm cases of processes is found in observable human behavior—the “overt attempts” discussed earlier. Getting a camera from an automobile, playing a trumpet, drinking tea, and acting like an old grouch have the “process” characteristics identified above.

The question of the describability and identifiability of these processes separately from the initial conditions and outcomes on specific occasions has already been handled in the expository outline of intentional action (Part One). The question has sometimes been put in a different way (Ginnane, 1960) which is apropos here: A process involves a change *in* something (and to speak of an event is simply to mark such a change), but what is it that changes when psychological processes are involved? The answer which is implicit in the deterministic paradigm given above is: What changes is the internal configuration of the organism. The conceptualization of social participation as (a) constitutive of human behavior and (b) codified, in their separate fashions, by language and by the concept of a Person provides a different answer which is verbally simple but has its own complexity which will need to be explored: What changes is the position of the person *vis à vis* the intelligible world of persons and objects, where the range of possible “positions” is given by the concepts by which the world *is* intelligible.

In connection with this answer, compare:

(a) What changes when a trump is played is the position of that individual (that card, e.g., the queen of hearts) with respect to the other cards in the game, where the range of its possible “positions” is given by the rules of the game.

(b) What changes when the moon suffers an eclipse is its position *vis-à-vis* the remainder of the solar system, where the range of possible “positions” is given by the rules under which the solar system operates.

(c) What changes when I buy something is my position *vis-à-vis* the world of persons and objects. That change is directly characterizable as a change specifically with respect to the practices, or “rules,” embodied in the concept of property or ownership. Because practices are interrelated, that change may also be a change in social status, a change in my capacity for operating a business, etc.

(d) When I come to fear someone, what changes is my position *vis-à-vis* him in respect to—what? Well, we have no other or better way of talking about *this* relationship than to

use “fear terminology”: I am “threatened” by him. And my position *vis-à-vis* the world changes: Now I am in a “dangerous” position, or a “defensive” position; I am “insecure,” where before I was less so or not at all.

We know that what can be treated as a Person can also be treated as a biological object and as a physical object. Persons have bodies and bodies are masses. Presumably it is this certainty which accounts for the commonly expressed conviction that in principle one could give a rigorous, definitive description of the physiological and physical correlates of human behavior and thus “explain” the latter. But that conclusion follows only in a particular way which should provide no motivation for asserting it.

To the extent that we allow that *the same thing* can be described as a person, as an organism, and as a massive, extended body, it is also the case that person descriptions, biological descriptions, and physical descriptions cease to be isolated from one another in their application, though it does not follow that there are no longer the three distinctive descriptive systems involved. The relationship here is quite similar to the mutual assimilability of the distinctive concept-types in PII, the paradigm for the Person concept, except that what we have here are not part-descriptions but something which it seems appropriate to describe as “*explicit* partial-descriptions” or as “relational descriptions.” “The neurological processes which occur when a person sees red,” “the physiological processes which occur when a person is angry,” “the physical processes which occur when a person forgets,” “the unconscious mental representative of the mother,” “the psychological processes which occur when a person learns something” have the logical status of partial-descriptions relative to person descriptions (and person descriptions are partial-descriptions relative to the concept of a Person). That is, they would be unintelligible and would refer to nothing whatever (cf. the discussion [Part Three] of “saying so doesn’t make it so”), if it were not the case that the corresponding person descriptions were *already* intelligible and had a

factual reference. They are explicitly so (unlike person descriptions) because they do mention what it is that the process in question is relative to, hence “relational descriptions.” But they are descriptions of a neurological process, a physiological process, a physical process, etc.

So it is not “in principle,” but here and now, that we can say rigorously and definitively what the physical and physiological antecedents and correlates of person descriptions are. To assert or assume that there is a logically independent, *second* description of these neurological, physiological, physical, etc., processes (for example, to assume that such processes can be described within the simple biological or physical descriptive systems, with respect to which it is the case that it would make no difference whatever whether there was any such thing as a Person) is to go beyond what can be derived from the linguistic necessities of the situation.

If the foregoing gives the appearance of an attempt to secure the advantages of theft as against honest toil, it may be instructive to reflect on the history of “ $\sqrt{2}$ ”. We may imagine an earnest mathematician of now-outmoded vintage saying:

There is nothing in the domain of mathematics except numbers and the relations between them. Then obviously it *must* be possible, in principle if not at present, to say what that number is which, when multiplied by itself has the number ‘2’ for its product.

In fact there was no such number until the concept of “number” was revised to include numbers which have only a relational description, like “ $\sqrt{2}$ ”, and the former “domain” consisting of rational numbers can now be seen as a special case of the real numbers which include “ $\sqrt{2}$ ”.

Here again, it hardly seems possible to identify anything that might serve as *evidence* that there is always another description which has a one-to-one relation to a person description. Nor does there seem to be any linguistic system within which a proof could be given that there is always a second such description *or that there is not* (contrast the existence of proofs

that $\sqrt{2}$ is irrational). Thus, the absence of a proof that there are no second descriptions paralleling person descriptions does not have the force that it is sometimes taken to have. (What would constitute a *proof* that shapes are colorless or that a coffee cup has no firing pin? That we don't mention color? That we say nothing about firing pins? Neither do we mention physical objects in giving person descriptions. But that is not proof.) As to the likelihood that there is always that second description, perhaps the most pertinent fact is that no case of any such description has been presented (see Long, 1964 for answers to some "obvious" possibilities). It would seem therefore, that those who look for physiological correlates of person descriptions in a reductive spirit, i.e. because they suppose that there *must* be those second descriptions, are operating on a sort of blind faith. Likewise, someone who assumes (a) that the description of a human body as a physical object is in any relevant sense prior to or more basic than its description as a human body or (b) that physiological descriptions refer indirectly to "the same thing as" what person descriptions refer to directly or (c) that person descriptions must be given "real definitions" of their "referential meaning," would seem to be begging the question at issue. (For example, Brodbeck, 1963, seems clearly to make all three assumptions. They are not uncommon.)

Nevertheless, there is something to be said, equally, for physics, biology, and rational numbers. That is that even if they were inadequate in principle, they might be quite adequate *in fact*, by providing suitable approximations. We can approximate " $\sqrt{2}$ " quite closely by "1.4141 . . .," and it might be that we could come to approximate those other relational descriptions equally well for whatever we needed. This appears to be a live possibility because the simple descriptions (rational numbers, biological descriptions, physical descriptions) *are* already implementable—we can recognize instances of these descriptions and act on them. Thus, the psychologist who looks for physiological correlates of person descriptions as a matter of empirical inquiry rather than as an expression of a metaphysical attitude

is *not* thereby operating on the basis of faith alone. To follow the maxim “Look for causes of behavior,” will be a rational procedure for psychologists if following that maxim has in the past produced results that psychologists value (no one questions the value of many of our psychophysiological correlations). We do not need to assume that there are causes “out there” to be discovered. With respect to the underlying psychological processes which have appeared in psychological theories, it appears that they are in general *not* implementable except as relational descriptions relative to person descriptions (cf. Jessor 1956, and Chisholm, 1955). Thus, we may expect no essential simplification to be accomplished by reference to them. Although pursuing them may lead to some unforeseen and interesting observations, that kind of activity will not as such carry the authority of an investigation of human behavior, and so it will not provide a general “explanation” of the latter, either.

From the foregoing, several conclusions may be drawn.

(a) The first of these is simply that the concepts, practices, and forms of discourse embodied in ordinary language are not, so far as anyone has shown or given reason to believe, lacking in any characteristic which is essential for the understanding of human behavior.

(b) The next is that the concept of a Person, in conjunction with the pragmatic methodology which stems from the same linguistic conceptualization, provides a basis for understanding human behavior. Further, this basis does not suffer in comparison with any of the currently available theories, models, and descriptive systems in psychology. As soon as we eschew the chauvinistic satisfaction of regarding formulations in ordinary language as prescientific, it becomes apparent that, on the face of it, the concept of a Person introduces conceptual “muscle” in areas where current formulations are recognizedly most deficient, and it does so without any corresponding disadvantages. That is, (1) it provides an articulated framework for dealing with “higher mental processes” and “rational behavior,” including an unmystical and unparadoxical account of how linguistic behavior is importantly different from, and not merely

similar to, other kinds of behavior, and (2) the conceptual channels to physiological, biochemical, sociological, and mathematical methods and formulations are relatively noise-free in both directions via the relational description formulation.

(c) Until and unless we are prepared to give up the concept of a Person *in toto* (and does anyone *know how* to do that?), any explanation of human behavior that is presented in terms of the Person concept is more parsimonious than *any* explanation of the same behavior given in other terms. This is because (1) person descriptions cannot be reduced to descriptions of another kind, and (2) any alternative explanation could, even if it were maximally successful in its own terms, provide only the equivalent of a partial description—it would constitute an “explanation” of something which was itself not intelligible except in relation to the concept of a Person (this is why the alternative would be to have something comprehensive enough to do the job that the Person concept now does).

(d) A conclusion with perhaps more immediate impact is the following:

The Person concept not only gives significance to results and procedures associated with other disciplines, it likewise assimilates the empirical results and procedures of current psychological models and, in a sense, assimilates the models themselves.

In the following section this conclusion is developed further.

II. The Mediation of Responses

What was suggested in the preceding section was that (a) any behavior theory in Psychology can be translated without significant loss into ordinary discourse via the concept of a Person, and (b) the consequences which can be drawn from the ordinary language formulation provide a translation of consequences drawn within the theoretical formulation. Only an

illustrative demonstration can be attempted here. (Something approaching a second such demonstration is given in Part Five.)

For this purpose it seems appropriate to take the “mediation” version of SR theory as presented by an authoritative spokesman (Osgood, 1957). The basic structure of the theory, which would serve as the basis for the initial translation, would seem to consist of the mediation hypothesis and the phenomena of “decision” and “control.” In the work cited (page numbers will refer to Osgood, 1957) these are described as follows:

A. The Mediation Hypothesis (p. 355)

“Whenever a neutral stimulus (sign-to-be) is paired with a significate and this pairing occurs sufficiently close in time to a reinforcing state of affairs, the neutral stimulus will acquire an increment of association with some distinctive portion of the total behavior elicited by the significate.”
 “. . . the self-stimulation produced by this representing reaction can become associated, through ordinary instrumental learning, with various overt responses appropriate to the object signified.”

B. Decision (p. 359)

“. . . decision in the behavioral system is simply the selection of the most probable alternative within any divergent hierarchy.”

C. Control (p. 359)

“. . . combination and patterning within convergent hierarchies modify the probabilities of alternatives, and this constitutes ‘control’ in the behavioral system.”

Assuming the Person concept as presented in Part One and Part Two, the following translation is made:

A. Rule I: “By experience, a person comes to know what goes with what, i.e. he comes to know the significance of what he encounters.” For example, he comes to know (a) what parts go together to make wholes (p. 355), (b) what objects, states, or events go with which, and (c) what objects, states or events lead to which (p. 378). (Note that “know” explicitly does

not distinguish between “being aware of X” and “being aware that X is the case.”)

Rule Ia: “With experience (practice) a person acquires the capacity to implement his expectations and inclinations without deliberation, i.e., with practice, his spontaneous behavior comes to be what he has learned to do in the light of what he knows (p. 356) and wants.” (Note that a reinforcement principle is already implied by the formulation of intentional action: If, wanting X, I do B and achieve X, I will come to know, after one or N occasions, that doing B is the sort of thing I do in trying to achieve X, and from that time on, my wanting X will explain my doing B.)

B. **Rule II.** “If a person has reason to do a thing, he will do it, unless he has a stronger reason for doing something else.” (pp. 359, 381, 387, 391)

C. **Rule III.** “How much reason a person has for doing something depends on the prevailing circumstances.” (pp. 365, 388, 398, 404, 406).

The prevailing circumstances will include, e.g., (a) what he wants (or has reason to do) at the moment (p. 369), (b) what he knows about his current surroundings (p. 389) and of himself, (c) his capacities at the time, (d) his current state (p. 369), and (e) what he expects.

Maxim: Unless *all* the prevailing circumstances are taken into account, an adequate assessment of his reasons for doing a thing cannot be expected (though it might, with luck, succeed) (p. 367).

Strictly speaking, Rule IIIa–e are not required for the translation, since they are merely a set of distinguishable instances of Rule III. (Rule IIIc, “his capacities” does not correspond to anything mentioned explicitly (hence no page reference) but we may assume that it is the person’s behavior repertoire rather than what he is physically capable of doing that is involved in any SR theory.) Of these five, three are recognizable as the three concept-types which, with the “overt-attempt” concept–

type, comprise the paradigm of intentional action. The fourth, the state of the person, is one of the concept-types in the paradigm of a Person. The fifth, “what he expects,” is an attempt to do justice to a concept of “set” which is presented as subsuming such diverse circumstances as experimental instructions or the presence of a member of the opposite sex (both of which seem appropriately included simply among what he knows) and also “private association sequences.” The latter sort of designation is relatively uninformative, and is perhaps best translated generally as a state of the person; however, because it is presented as an instance of “set,” it is rendered as “what he expects” which would fit either a state or an attitude of expectancy.

Some remarks are in order about such a translation. It seems quite clear that a mediating response is conceived of as a physiological event, although its location and extent are unspecified. This kind of event, however, is quite different from other kinds of physiological events. Very simply, there are no physiological identity criteria for this kind of physiological event. (Or, if it is taken to be a physical event, there are no physical criteria of identity, and if it is some other kind of event, X-kind, there are no X-kind of identity criteria.) There is no way that we can raise the question and no way of deciding, either, whether a physiological event is a mediating response or whether an event E_1 which occurs today and qualifies as an instance of a mediating response is “the same” as event E_2 , which occurred five minutes ago and qualifies as an instance of a mediating response. It is not only the extent and locus of the mediating response which is unspecified, but *all* of its individuating characteristics (physiological or otherwise) except one. That one is the relational description which apparently ties it to an object or “significate.” (“Apparently” because there is the same obscurity and potential confusion here that is involved in singling out a “cause” for a piece of behavior. Some of this potential comes out in the anomaly of picking out a single “significate” in dealing with acquisition but insisting that the entire current internal and external circumstances must be taken into consideration when giving an account of “behavior” in the general case.)

Thus, the one and only essential characteristic of a distinct mediating response is that it is “the kind of thing which” is part of “the kind of thing which” has a one-to-one relationship to (was “elicited by”) a distinct something which is only a distinct something for persons who use a certain descriptive system and use that descriptive system in relation to behavioral descriptions. This characterization makes it clear that as a physiological entity and as an event entity a mediating response is a “grammatical fiction.” It is a grammatical fiction because it satisfies the two conditions that (1) the justification for talking about these “physiological events” by calling them mediating responses is not any knowledge, evidence, or assumption in regard to anything physiological or in regard to any event inside any body, and (2) to doubt that there are any physiological events of the kind designated by mediating responses is not to raise a question (*a fortiori*, not an empirical question) about the justification for referring to them. This is “ $\sqrt{2}$ ” again. There are only certain descriptions which now qualify as physiological descriptions, and it is to be expected, though not certain, that none of them picks out the same set of instances as the description, “the physiological event which occurs when mediating response ‘p’ occur.” So we would have to change the criteria for what constitutes a physiological event in order to deny that the kind of talk in which “mediating response” occurs is unassailable in the light of any general facts about physiology or any historical facts about any events whatever except those concerning the commerce had by a person with a “sign” and a “significate” together. The other side of the coin is that no amount of successful use of such talk qualifies as evidence for the occurrence of physiological events any more than the successful use of the word “of” as we now use it would qualify as evidence of the occurrence of physiological events, even if we were to say that “of” referred to a physiological event. (It is only in a pragmatic context that statements of this kind can be made simply and bluntly with any hope of success—success is not guaranteed. In a semantic context, “words mean what we want them to mean,” and so nothing is clear.)

The point here is not to advance any kind of skeptical conclusion about the “reality” of theoretical terms, but to call attention to the fact that the *imagery* associated with words such as “mediating,” “response,” “physiological,” and “event” is entirely irrelevant to the “grammar,” or logical employment, of these words. For then there will be no serious obstacle to seeing that the logical employment of such terminology in stating and using the mediation hypothesis *does* differ basically from the use of the same words in connection with either physiological or behavioral descriptive systems but *does not* differ basically from the ordinary use of a word which normally carries very different imagery and subjective associations, i.e. “know.” Saying that a person acquired a mediating response with respect to a significate does some (and only some) of the jobs which can be performed by saying that he comes to know about the significate including its association with the “sign.” To say subsequently that “the self-stimulation produced by this representing reaction can become associated . . . with various overt responses appropriate to the object signified” seems even more clearly to do only a part of what can be accomplished by saying that a person does what he knows how to do and does it in the light of what he knows and wants. If it makes sense to talk of the self-stimulation which occurs when a representing reaction occurs it makes the same kind of sense to talk of the physiological event (or indeed, the self-stimulation) which occurs when the person acts in the light of some identifiable knowledge. What is missing from the latter is the subjective impression that there is a certain kind of asymmetry between two kinds of talk such that what is talked about one way explains what is talked about the other way. Would it be heresy to say that the knowing explains the occurrence of that physiological event rather than vice versa? There would be a point to that, for example, because we could say, “The dependent variable is the physiological one. I cause that physiological event to happen by manipulating knowledge as the independent variable—I cannot cause the knowing to happen by manipulating something physiological.” Or, it could work the other way some time. But why

should we feel a *general* need to say something about “explanation” here? We may, of course, but that should not lead us to suppose that then we are talking about explanations.

The presence of Rule II has been discussed previously in Part Three. It is the pragmatic maxim which is paraphrased by every general behavior theory, including this SR theory.

Since “convergent hierarchy” is simply a way of referring to the relation of a mediating response to those various “signs” to which it is conditioned, no basic addition to the discussion of the mediation hypothesis needs to be made in order to take account of the joint effect of several currently present “signs” in eliciting the mediating response at its current level of intensity. What is desirable is to have some classification of the different kinds of “signs” which contribute in significantly different ways to the elicitation of mediators and to “unmediated” behavior. Among these kinds is the kind stemming from physiological states of motivation, arousal, activation, etc. Other kinds of signs are also introduced in an *ad hoc* way, as they must be, since SR theory makes no provision for distinguishing *theoretically* among different sorts of stimuli. To identify relevant stimuli on the basis of an *ad hoc* review of the circumstances which might attend an experimental procedure may give the impression that the number of kinds of stimuli is indefinitely large. However, the survey of such circumstances as are brought in in Osgood’s presentation shows that no further basic taxonomy is required beyond what is automatically provided by the articulation of the Person concept in PI and PII.

The earlier discussion of the Person concept has shown that to characterize an “overt attempt” as an intentional action is not to refer to an ineffable something-somewhere, but *is* to relate it in an essential and coherent way to the circumstances of its occurrence, including the learning history of the person whose action it is. That is one *volte-face*. Now an examination of SR theory provides reason to say that in spite of an abundance of satisfyingly concrete imagery it refers to an ineffable something which relates behavior to circumstances only in an accidental, *ad hoc* way which requires the intervention of a

skilled psychologist to make it even verbally plausible. (To claim that this was the whole story about SR theory or to dismiss it out of hand because it is not, would both, equally, be merely to continue an old polemic.) And when we observe the application of the theory we may well be impressed by how little depends on anything distinctively SR in character and how much depends on the logic of part-whole relationships (cf. p. 389) both internally and externally.

For example, internal part-whole relationships are represented schematically in a complex “physical” system characterized by (a) an indefinitely large number of functionally related discrete parts, (b) a variety of independent input sources, each generating an internal spread of events, (c) complete interaction such that what happens at any point is potentially conditional on what happens at some other point or region, including (d) feedback from events at the loci of later stages in internal process to events at the loci of earlier stages, so that self-maintaining or self-canceling effects are possible. It is within such a structure that SR connections find a use in distinguishing one functional pathway from another among those functional pathways assumed to exist. Toward this end, there is no upper limit to the number of conditioning “principles” which may be employed in deriving the current existence of a functional pathway as a function of some set of prior events. (In fact, Osgood makes do with only three—contiguity, frequency, and reinforcement.) It is because the physiological characteristics and the event-like characteristics are carried by this part-whole structure that the selection of functional pathways which is achieved by the SR talk is, in its logical structure, basically like talking about “the physiological events which occur when I know about X” and basically unlike talking in physiological terms about something physiological.

If, as indicated above, the principle of “Control” is “nothing but” the statement that the relevant circumstances make a difference in the reasons one has, and implementing this principle depends almost wholly on part-whole relationships which

are only incompletely formulated in the notion of a “convergent hierarchy,” it might be expected that dealing with totalities is not one of the strong points of the system. This is perhaps most clearly brought out in connection with the interaction maxim (which is itself only informally brought in in relation to the theory). Because of the interaction among the “effects” of relevant circumstances, all of the relevant circumstances must be taken into account, otherwise failure in prediction is to be *expected*. This would seem to be the SR analogue of the logical indeterminacy discussed in Part Two. Because taking all the relevant circumstances into account is not something we *know how* to do. And if we laid that consideration aside and supposed that it might happen accidentally that we had taken all the relevant circumstances into account, we would find that we also do not know how to tell whether something of that sort *has* happened.

“If I predict correctly, then I have taken everything into account.” Well, if I limit my language for describing behavior to, say, two possibilities, “He does nothing” and “He does something,” and if the possible circumstances which I take into account are, e.g., “He is alive,” and “He is dead,” then I will generally do pretty well at predicting, and that may give me a glow of satisfaction. If I describe “behavior” in relation to descriptive systems which are a little bit more complex, for example, if I use the kind of description which is appropriate to a physical object, of to an automaton, or to an animal, I may still do fairly well at predicting. There is sometimes some point to doing that, and to adopt it as a program would have the appeal of the prospect of doing *something* successfully, with its attendant satisfactions. Of course, such a program would be something other than the study of human behavior, too, and could contribute to the latter only indirectly, if at all. With respect to a straightforward study of human behavior, reasons were given in Part Two for concluding that all relevant circumstances cannot be finally established by observation, owing to the discrepancy in the *conceptual* complexity of observations and actions, respectively. (Ironically, this discrepancy is what

makes possible the scientific endeavors of those behavioral scientists who insist that the only adequate explanation of human behavior is a reductive one [cf. Wick, 1964]). It was seen as an essential feature of human behavior (and a feature made possible by rule-following) that neither certainty in advance nor predictive accuracy verified after the fact, nor any belief or conviction about either of these, is essential for effective action, much less for action as such.

(Compare: It is an essential feature of any game that what a player does, even at the most molecular level of a single “move,” is in general not [and in some games, never] determined completely by the rules in the sense that he has no game options *within* the rules in the light of the concrete game-circumstances. Examples of this kind serve to remind us that a person who professes to believe that performance descriptions can be replaced by situation descriptions [i.e., that behavior is completely “determined”] is almost certainly not someone who is then and there engaged in a task which has intrinsic value or requires effective dealing with people, but is quite likely to be someone who is arguing psychological methodology.)

And if actions cannot be established for sure in present tense, they cannot be established in future tense, either, and so then talking about “predicting” an action as a criterion for having taken everything into account gets us no farther ahead.

It would be unfair to suggest that Osgood (or indeed, most experimental psychologists) sees the complete prediction of particular historical events as a goal to be striven toward and to be legitimately evaluated against. But S-R psychology has been frequently associated with positivistic philosophies in which the emphasis is on complete predictability “in principle” and neither Osgood nor other prominent S-R theorists have actively or openly resisted the association. Thus, there is some point in a reminder that the indeterminacy which is openly and systematically a feature of the Person concept is implicitly and informally, but no less definitely, a feature of what is commonly

recognized as one of the most conceptually sophisticated and empirically viable S-R theories extant.

If the translation is accepted as adequate, or at least, as having no significant defects which could not be removed by more assiduous application of the same approach, then it may be superfluous to go on to the applications, for then the basic concepts of the theory will already be seen as subsumable under a *portion* of the Person concept and as being somewhat less coherently articulated than the latter. Under these conditions, it would be extremely strange if we could not derive from the Person concept anything that could be derived from the mediation formulation. (It may be worth noting explicitly that assigning numbers to instances of the concepts in question would not differ in detail as between the two formulations.) However, for illustrative purposes there is some point in going somewhat beyond an “in principle” demonstration although at this point, there should be no doubt that in principle, it can be done.

Rule IV. “If a person has an opportunity to get something he wants, he has a reason to try to get it.” (p. 373) (And, by Rule I, he comes to recognize opportunities as a result of experience.)

Rule V. “If a person wants an X or has an interest in an X (or class of X’s), he will have a stronger reason than most people to treat something as an X. This will also be the case if he is looking for an X or expecting an X or thinking about an X, and any of these latter may be an expression of his interest in an X or of his wanting an X.” (The foregoing rules are regarded as trivial exercises in the use of the Person concept, with particular reference to the PI concept-type “Want” and the PII concept-type “Interest.”) Under any of these conditions, therefore, he is more likely than most to try to treat something as an X. For example:

(a) He is more likely to do it successfully—he is more likely to notice or recognize an X which is present. (pp. 390, 393)

(b) But he is also more likely to do it unsuccessfully—

mistake something else for an X, especially if an X is not present. (pp. 387, 392)

(c) If a person *both* wants an X and is thinking of an X, he is more likely to try to get an X than someone who has not these reasons (because he will do that unless he has a stronger reason not to).

(d) If he *starts* to try to get an X he has a reason to pay attention to what he is doing and to complete his attempt, so he is more likely to do that than if he had not that reason. (p. 389)

Rule VI: A person who gets something he wants by *trying* to speak the language that is spoken has a reason to continue trying. A person who keeps trying to speak the language that is spoken will normally learn to speak the language (from Rule Ia). Assumption: A person who speaks the language that is spoken is more likely to get something he wants than if he does not speak that language. (p. 375) Consequently, when a person who knows the language speaks, he is likely to speak correctly, i.e. he is likely to use words he understands and is likely to say what he intends to say. (pp. 402, 404) And in the light of Rule V and VI: If he wants an X or has an interest in an X or is thinking about an X, he has a reason to talk about an X, and so he is more likely to talk about an X than he would be if he had no such reason. (p. 408)

If the foregoing do not appear to be particularly informative, it should be recalled that they represent merely some specific applications of the general principles. Two other applications were made which are still of some current interest, i.e. “perceptual defense” and the effect of “high drive” on performance:

Rule VII: A person may be aware *of* something (and his awareness may involve an appraisal or evaluation of it) without being aware *that* that something is present or is the case.

This is not derivable from the basic mediation theory but is a theoretically phrased statement motivated by the empirical results of certain laboratory experiments. In contrast, the Person concept includes this principle as an integral part of the

concept, since it is a necessary feature of intelligent behavior, including being a necessary condition for fear, anger, guilt and other feelings (which have evaluative aspects). An example was given previously in relation to "ego defenses."

Hypothetical Rule VIII: A person may enter into a state which leads to the following general change in what he has reason to do, and therefore, in what is to be expected of him: If the reasons he has are assigned numerical values to index how strong a reason each of those is for him, and if these indices have the values N_i before the change of state, then following the change of state, he has the same reasons but their index values are now given by $C \times N_i$, i.e., the new values are a multiplicative function of the previous values. In the simple case, a result of this change is to increase the absolute preponderance of what were already his strongest reasons. It is possible, however, that the change of state as such gives the person a greater reason or a lesser reason for doing some *particular* thing. If the contribution of the change of state to the strength of his reason for doing that particular thing is given a numerical value, then . . . Here all of the numerical apparatus of the "high drive" genre can be introduced, giving the same results. It may be noted that Hypothetical Rule VIII is not, as such, part of the Person concept, but rather the formulation, within the Person concept, of a particular sort of model. Its "model" characteristic becomes apparent as soon as we ask how those numbers get assigned and how we check on the appropriateness of a particular assignment. For we then discover that the only constraint is the persistence of the investigator who can, upon repeated failure in prediction, revise and modify the way he assigns the numbers and continue to say, "But now, this time . . ." The relevant experimental evidence appears to be based on qualitative considerations in situations where inequalities can be guessed at (and second-guessed) rather than on any prior numerical commitments. If we turn to a more objective kind of talk, talk which we do know how to implement, and ask more specifically, what sorts of situations would (and did) provide the basis

for those inequalities, we are able to give a less heroic formulation:

Rule VIII: A person is likely to act differently in different states, and his capacities are likely to differ also. (p. 381) (This is part of the analysis of the “state” concept-type.)

For example:

(a) In a state of moderate concern, or of heightened attention, interest, or alertness, a person is likely to perform many tasks more effectively than usual.

(b) In a state of considerable concern, or pain, or strong emotion, a person is likely to perform any imposed task (i.e., one which is contrary to what he would otherwise have had most reason to engage in) in a perfunctory, automatic way (which is not to say that he will *behave* in a perfunctory, automatic way—it is his performance which shows these features). (pp. 383, 386, 406, 407)

(c) Under these conditions a person is likely to express his feelings, and this is most clearly shown when there is no competing task to be performed. (pp. 403, 406)

(d) A person in a state of extreme pain, shock, fear, rage, ecstasy, or other strong emotion, will lose most of his capacity for engaging in overt attempts to do anything whatever (p. 404); thus he will be unable to accomplish anything which he does not have a very strong reason to do or which is normally not extremely easy for him to do.

The foregoing are not merely the specific sorts of instance on which the “high-drive” research was based. are also statements of necessary relationships, in the sense that if our descriptions of fact did not conform to these requirements, it is the descriptions of fact that we would question. For example, we do not accept any and every feeling state description in conjunction with a given sort of behavior. I cannot casually remark that I have been in agony for the last ten minutes and expect to be believed in the absence of a very special explanation. And we do take it that feelings will be expressed (cf. Part One) unless the person has some reason not to, and it is hardly an empirical assertion that a person who is trying to do two things

(e.g. one of which is to control his feelings) will generally do less well than one who approaches a task single-mindedly.

Finally, mention should be made of the function of S-S and R-R connections in Osgood's theory. These represent sensory and motor integration which are acquired on the basis of frequency and contiguity (spatial, temporal) *rather than reinforcement*. (p. 350-353) These integrations are "... assumed to reflect redundancies in the experience of the organism. It is entirely a 'driven' system." Evidently, therefore, S-S and R-R connections are special cases of Rule I and Rule Ia, above. These rules refer to the person's ability (learning by experience) to *recognize* (S-S) what parts go together to make what wholes, and to *execute* (R-R) overt attempts rather than parts of overt attempts. The R-R formulation reflects the fact that an overt attempt is a performance and not merely a collection of constituent movements. In the light of the formulation of intentional action, Osgood's reliance on S-S and R-R integrations may be seen as reflecting the recognition that "know" and "know how" concepts are no less basic than "want" concepts (reinforcement, drive) in the description of behavior.

III. Parsimony, Understanding, and Psychological Explanations

A pragmatic formulation provides a modified outlook on concepts such as parsimony and objectivity. In this context, *an account of a given phenomenon is parsimonious to the extent that it is given in a descriptive system which is generally and effectively implemented*. Verbal simplicity is not a decisive consideration. Nor is the number or kind of "assumptions" which might be attributed to the account, the giving of it, or the giver. Verbal simplicity and limited assumptions are correlated with parsimony, so that they continue to provide useful rules of thumb. In those cases, however, where they are not associated with

wider or more effective implementation, they have no special merit which calls for the designation “parsimonious.”

This concept of parsimony reflects the recognition that special purpose descriptive systems (including scientific theories and most philosophical systems) have the same kind of drawbacks as those which we recognize so clearly in *ad hoc* explanations. These drawbacks are still relatively apparent in the intermediate case where we invent special-purpose terminology which is *not* systematic—we recognize quite clearly that making up a word for X is not the same as giving an explanation of X, nor even a description of X. Rather, it is perhaps best described as an adaptation to X. If no more than a verbal baptism (either systematic or not) of X is involved, that will be a primitive form of adaptation and its prospects for success will likely not be great. If the new terminology becomes part of a significant activity and that activity is viable, the adaptation is correspondingly successful. (But it will not constitute having an explanation, either, if it is not one of our *old* activities to which we assimilate the new phenomenon.) In that case, too, the new terminology is objective, because it has a public use, and the part it plays in that activity (or activities) is its significance. (And if it has a public use, then people “agree about it.”) This concept of objectivity may be regarded as a generalized version of the special case with which we are familiar, i.e., the notion that the objectivity of terminology is guaranteed by, and only by, a *special* kind of activity, namely, a “show and tell” procedure in which observers use the terminology to report observations of “the same thing” and their reports match (or the terminology is used in giving them instructions, and then they do “the same thing,” or etc.) That two persons, when presented with X in a “show and tell” situation treat it the same to the extent of calling it the same could hardly provide a guarantee that upon encountering X in the course of a different activity they will even recognize its presence or that being aware of its presence, they will treat it the same even to the extent of calling it the same (e.g., for “X” substitute “an insult”). And it would be audacious indeed to suppose that the foregoing refers

to the behavior of experimental subjects in a reliability experiment without holding as well for experimenters in the course of their professional activities.

This is not to say that the “show and tell” kind of agreement has no methodological interest. It would be difficult to imagine people agreeing in the general sense if they did not frequently agree in this particular sort of way. But it is equally important to keep in mind that in human activities reciprocal behavior is the rule and parallel behavior is the exception. Human life as we know it would be impossible if everyone did the same thing (consider the gross cases of parent-child relationships or masculine-feminine relationships). One of the critical ingredients in making this state of affairs practical and usual, and not merely possible, is the use of PII descriptions which permit us to make allowances for the differences between other persons and ourselves. Individual differences of a person-descriptive sort are not the error variance which remains when we have exercised our descriptive and experimental skills, and so they are not something which we need to try to whittle down to zero by improving our measurements or adding new empirical generalizations or devising more convoluted verbalizations in order to be able to give a single coherent account of everybody’s behavior equally. The latter is already given by the Person concept, and the formulation of PII *is* a coherent general account of everybody’s behavior equally and without remainder (but see below under “what can be studied”).

Not too surprisingly, this formulation in which the critical importance of crude verbal agreement is denied and the possibilities of adequate substitutes for person descriptions is questioned leads us to look for, and find a surprising degree of agreement in places where it has not been sought. The remarkable factorial stability and validity correlations in the Classification Space studies are a case in point. A current study by Clapp (1966) shows inter-observer correlations of the order of 0.70 to 0.90 in applying person descriptions to children in free play situations. Comparable figures are obtained by Sidman, whose judges give person descriptions of the self-predicted responses of subjects to

hypothetical situations involving “mentally ill” persons. The behavior-guiding, as opposed to observation-summarizing, function of person descriptions was demonstrated by Schneider (1965) who provided experimental subjects, P, with pathology, ability, and trait descriptions of other subjects, O, and showed that P’s behavior toward O was contingent on the *particular* description and highly predictable in an experimental setting in which a good many alternative behaviors were possible. These studies are consistent with the Person concept formulation that (a) the primary basis for the application of a person description to one person, P, by another person, E, is not *merely* the observation of P by E, but rather, the observation by E of how P treats some other person, O (who may be E), on some number of occasions; and (b) the primary significance for E of having that description of P is that it gives E reason to treat P in certain ways and not others; and (c) E’s behavior toward P is a function of this reason, among others, hence his having this reason is something we can observe. Because his having the reasons he does have is an essential part of our description of him as a person, this accounts for phenomenon (a). And there are not, and need not be, any privileged observers, for in terms of (a), we can describe P as O, E as P, and ourselves as E.

Various exercises of this sort can be carried out, and they have the heuristic value of sustaining the conviction that an objective and significant study of human behavior is an entirely feasible form of human behavior. Contrary to the implicit assumptions of our currently predominant scientific mythos, such a study does not require that we treat human behavior as something other than human behavior, nor does it require keeping the human behavior of psychological scientists as something extra-scientific, hence it does not need to borrow explanatory or methodological authority from physiology, philosophy, or any other extra-psychological source.

In this regard, the prior discussion suggests that (a) psychological theorizing, involving as it has primary attention to notational simplicity and formalism, has failed to provide a more parsimonious account of human behavior because such

theorizing has remained largely a notational variation, either alphabetical or mathematical, on the basic logic of person descriptions in ordinary language, and (b) the attempts to give a more parsimonious, authoritative account have been made at a substantial cost in objectivity (the degree to which the terminology contributes effectively and systematically to significant activities), so that to a large extent, our technical descriptions mean what any investigator wants them to mean (and what he wants them to mean is often contingent on the results of the next experiment, and then the next . . .), and so they mean very little, and that is particularly apparent when they are employed outside of their restricted natural habitat in giving an “authoritative, scientific account” of behavior generally.

Thus, another maxim for psychological investigation is: “Except where specific evidence of incapacity exists, human behavior which can be described as the expression of ‘higher mental processes’ is not to be understood in terms of a more primitive capacity.” This maxim merely reflects the conclusion that person descriptions provide a more parsimonious account of human behavior, hence that a burden of proof is properly placed on any proposed alternative (cf. the use of “aseptic technique” in experimental design, discussed in Part Five.)

And although it was not needed for this, the maxim points the way to a significant alternative to the view that the proper goal of scientific psychology is to provide authoritative accounts of human behavior by reference to deterministic underlying psychological processes. For if to understand an action is to be aware of its significance (NB the discussion of PII), and if its significance is a function of the *implementable* descriptive system within which we place it, then it is clear that the greater the complexity of the implementable descriptive system we employ, the more significance the action has. Thus, if the goal of psychological science is to increase our understanding of human behavior, we may reformulate that goal as “to increase the significance of human behavior for psychologists and other persons.” And this involves *both* finding out more about human behavior and acquiring the ability to do more about it. That is,

both as individual scientists and collectively, our increased understanding will be shown in what we do differently with respect to human behavior, and that change will be a function of what we have come to know or know how to do, and optimally, *neither* of these two components will be null. (Our currently fashionable semantic model of science, by its emphasis on observation only and a *formal* neglect of skills, provides every temptation to hypostatize as “laws of behavior” the consequences of the historical accident that we now have precisely the skills we do have, not those we will or did have.)

Toward such a goal, clearly, the explicitly formulated concept of a Person provides a starting point. What can be studied is (a) the Person concept itself and (b) Persons:

A. The study of the Person concept

The continued delineation of the concept of a person, and the continued exploration of alternative formulations is important because there is an important sense in which the only possible explanation of rule-following behavior is a better statement (than we have when we ask for an explanation) of the rules that were followed. That is, in certain crucial respects, no other characterization can substitute for a characterization within the primary descriptive system for the phenomenon to be explained. To take a trite example, we sometimes give explanations of the form “He can’t read because he has a tumor at Y in his brain.” Implied in such an explanation is that the person is in a particular psychological state which is partially or exhaustively identified by saying that the difference it makes is that it is more difficult for him to read and so much so that in fact he can’t (or, conversely, that his ability to read is diminished to point where he cannot read). Reference to such a state might seem a superfluous verbal interposition to the determined reductionist or Unifier of science but it is important to recognize that the neurological condition is neither a necessary nor a sufficient condition for that psychological state, and if there were *no* psychological state attributable to a particular neurological condition, that condition would have *no* explanatory

value of the kind illustrated by "He can't read because he has a tumor."

The foregoing is not a peculiar feature of the Person concept—it is a general linguistic feature of distinctive descriptive systems. It holds, for example, when we explain physiological or physical phenomena by reference to human actions. If we say, "He wheezed going up the stairs because he ate too much," we are implying a physiological state which accounts for his wheezing and for which his overeating was a significant antecedent. As before, that antecedent is neither a necessary nor a sufficient condition for that state, and if *no* physiological state could be attributed to his overeating (e.g., as against his eating), then that antecedent could not have the explanatory value illustrated above by ". . . because he ate too much." The general case applies also to a person who rolls a ball down an inclined plane or mixes two chemicals, etc. (The content of Appendix B would be relevant at this point.)

No claim has been made that the Person concept, as presented in Part One and Part Two, is complete or definitive. It is decidedly schematic and admittedly incomplete. For example, little has been said about values (priorities among wants) or of the relation of the series in PII to the concept of counterfactual conditionals and the literature on this topic, or of the larger patterns of activities within which particular actions have significance (another part-whole relationship) and which provide the subject matter of various social sciences. Likewise the concepts of "mistake," "wish," "practice," and "status" were mentioned only briefly, yet it appears that they would be of central importance in dealing with phenomena of socialization and psychopathology (cf. Peters and Mace, 1962).

There is at least one important empirical aspect to the Person concept per se, namely, variation across cultures, groups, and individuals. The logical structure given in Part One and Part Two is (provisionally claimed to be) invariant across cultures and Persons. But this is because the schema has to be given specific content consisting of a repertoire of particular concepts within each concept-type, and the repertoire of such

specific content concepts will most certainly show variation across individuals, groups, and societies. Both the stability and variation are important. We understand strangers and foreigners as Persons, because the concept of a Person is no more culturally bound than is the concept of a subatomic particle or a nerve cell. But we understand Persons as strangers precisely because, and when, and to the extent that, we do not know what *particular* wants, knowledge, skills, traits, etc. they have. The comparative study of Person concept *contents* would provide empirical generalizations in regard to groupings of particular knowledge, skills, traits, etc. That is, the co-occurrence of some particulars of this kind may be more nearly a matter of necessity (at least, of the causal-historical sort) than coincidence. This kind of investigation would be illustrated by correlating data on (a) religious values and beliefs and (b) entrepreneurial activities by individuals holding those values and beliefs and (c) economic systems in societies of individuals of this kind as contrasted with other kinds.

B. The Study of Persons

It is only because there are rules of chess which people know how to follow that there are *also* chess questions and chess decisions which are not settled by the rules alone. The statement of such rules, though nonempirical, is not an attempt to *substitute* reason (or language) for fact. On the contrary, such statements, delineating the concept of chess, *generate* empirical questions. Likewise, it is only because there is a concept of a Person, which people know how to use, the nonempirical statement of which is constitutive of a certain kind of fact, human behavior, that there are significant empirical questions and decisions involving that kind of fact.

A. Knowing More about Persons

Because the concept is a complex one, the empirical questions are not of a narrowly delimited sort. For example, the behavioral (action) significance of various states of need, deficit, logical work has been done which involves these relationships

in one way or another. Likewise a thorough, systematic description of a substantial number of Persons has yet to be accomplished. Or again, we might inquire as to what *other* differences, if any, there are between persons who clearly show themselves to be of a certain kind and those who are judged by others to be, but not clearly to have shown themselves to be, of that kind (e.g., “prejudiced,” “generous,” “dependent,” “creative,” “success-oriented,” “abnormal,” etc.). Or, a question that most clinicians and few other psychologists have occasion to ask, “What is the significance of a given description *for* different persons?” For example, what differences does it make to different people that X is “friendly,” or that he is “an old grouch,” “aggressive,” “dependent,” “ambitious,” etc. (By and large, psychologists have studied the evidential basis for giving such descriptions and ignored all the other aspects of the use of such descriptions by persons in their significant activities.) A more familiar question would be, “What are some of the circumstances (if any) which regularly provide ‘reason enough’ for behavior which is of independent interest (e.g., aggressive behavior) or which facilitate the prediction of behavior which is of interest?” Here we are not starting from scratch in regard to either experimental procedures (e.g., Hammond, et al., 1964) or empirical findings (e.g., Jessor, 1964).

A more systematic survey of the empirical gaps created by the nonempirical statements in which the Person concept is formulated may be achieved by proceeding in a more or less mechanical fashion from the two paradigms, PI and PII, used in presenting the Person concept. Some of the questions will be clearly and directly related to the concept, and at the other extreme, there will be a proliferation of questions which depend on the existence of a variety of other descriptive systems:

a. PI descriptions: At time t , what is P doing? What does he want, know, know how to do, and what performance is he engaging in?

b. PII descriptions: At time t , what are P’s traits, attitudes, interests, abilities? What states is he in?

- c. How did P come to have the wants, knowledge, skills, traits, etc. which he now has?
- d. What historical facts about P could have been different, and how, without the answers to a, b, and c (or any given subset) being different?
- e. How does the description of P at *t* relate to his description at earlier times?
- f. What circumstances would change the PI or PII characteristics of P? (Which characteristics, and in what way?)
- g. Other descriptions: What is the physical description of P at *t* and at earlier times? what is his physiological (or chemical, or sociological, or legal, or economic, etc.) description at *t* and at earlier times?
- h. What correlations exist between (1) answers to items a–f and (2) answers to g? Which correlations correspond to manipulations which can be used to produce one or the other kind of event?
- i. How do various P's and groups of P's differ with respect to items a–h?
- j. How do human P's differ from nonhuman P's with respect to items a–i?

B. Doing More about Persons

In this regard, it is clear that many psychologists, especially in the clinical, counseling, and industrial “areas” of psychology, are actively engaged in trying to do something about human behavior of various sorts. They are handicapped in this connection in that the pressure of social need has the general effect of pre-formulating the problems and constraining modes of attack on those problems. A less well-recognized handicap, which appears to affect psychologists generally stems from characterization of certain activities as “applied” psychology.

The fact that we frequently speak of “applied” psychology suggests that there is also a “pure” version which is being “applied.” But it seems closer to the facts to speak of there being two psychologies, i.e. physiologically oriented psychology and the study of *human* behavior, both of which span the range

from verbal innovation ("theorizing") to laboratory experimentation to significant activities in the world at large. Neither unfortunately has had a great deal to contribute to the other. For example, and contrary to what we frequently profess, there is no "general psychology" or "basic scientific psychology" having sufficient human relevance to be effectively used by clinicians generally. On the contrary, most of our general behavior theories are in fact personality theories which were constructed by clinicians and are used by clinicians. And there is no "general" psychology which is "applied" by psychologists who take a psychometric approach to intelligence, abilities, interests, values, placement, etc. Their debt is, rather, to mathematics, statistics, agriculture, and "common sense."

The notion of an authoritative account of human behavior is historically important because it has been equated with "pure" psychological science and has resulted in the relative isolation of that group of psychologists whose primary employment has consisted in the application of philosophical prescriptions for "doing science" in essaying novel verbal formulations, or in the application of statistical and other technology to the performance of laboratory experiments. To a large extent, it appears that the preoccupation with a more authoritative account stems from the notion that person descriptions represent a theory *about* "behavior," rather than a theory *of* human behavior (the distinction "theory of . . ." and "theory about . . ." was made in the Introduction) Thus, it would be a competing theory, since our underlying process theories are purportedly *about* "behavior." Under these preconceptions it is not surprising that psychologists should regard person descriptions as simply "prescientific" and aspire to more authoritative accounts of behavior. But the discussion of mediational S-R theory above, and the further investigations presented in Part Five, provide substantial reasons for concluding that all of our existing underlying-process accounts are assimilable to the Person concept and in that case they are not even importantly *different* accounts (except insofar as they are essentially incomplete), much less being more authoritative.

What seems to be required for a viable symbiosis of conceptual, exploratory, and technological efforts is that it should not be a one-way street from “pure” to “applied” activities. Rather, it seems that there should be a special place in research technology for skills which can be exercised effectively in real-life situations. In this way, those techniques which “work” (i.e., which achieve a significant place in significant activities) will, on the whole, make possible genuinely new research findings (since they reflect an increment in what we know how to do) which in turn will provide the necessary challenge, support, and empirical constraint for novel, yet factually significant conceptualizations. These may then lead to further exploratory activities (experiments) which may require for *their* achievement new techniques which can be adapted or transported to real-life situations, etc.

Here again, the use of maxims and decision-announcements represents a methodological exploration. It is not merely a stylistic device which might or might not accomplish the same old things a little better. Rather, it is an attempt to anticipate the kind of linguistic technology which *could* exploit the expressive resources provided by the generative character of rules and thereby serve the communication needs of a scientific community in which no a priori double-language assumptions or procedural requirements were in force. Such talk, as illustrated prototypically in Part Three, would not require giving up reference to assumptions or to postulates in those cases where such reference would be more effective. This kind of talk has many of the problems, but also some of the resources of both therapeutic communication and the assessment of individual differences:

(a) The relation to therapeutic communication comes about because both are pragmatic forms of communication which do not require the same sort of prior verbal standardization which we have been led to believe is essential for “doing science.” A therapeutic interaction is one in which (if it is successful) communication is *achieved*; the assumption that “we agree” or that we understand each other is not a precondition

for carrying on this kind of activity, at least, not nearly to the extent that we feel is required in our technical discourse. The skills which enter into such an activity may therefore (with more than minimal expectation of some payoff) be tried out (the necessary changes having been made) among investigators, or in communicating with experimental subjects (cf. the “aseptic technique” mentioned of Part Five); the reverse procedure is also possible. And whatever systematization or conceptualization is achieved in any area may be trial-generalized to other areas.

(b) Likewise, maxim and decision messages among scientists would provide additional means for an individual to communicate the rationale for his activities in a way which made the “arbitrariness” of his individual approach public and enabled other investigators to apprehend the general significance of his work by making allowances for his idiosyncrasies in the light of their own, so that there would be a minimal reliance on everyone’s having a standard set of metalinguistic and methodological reflexes. This situation has the same formal structure as the substantive field in which the assessment of individual differences is carried out by appraising the significance of an individual’s choices and interests relative to his frame of reference (cf. Tyler, 1959). Since investigators would be particularly concerned to make explicit their decision structures and since their work would stand as the objective evidence of the empirical implications (significance) of those decisions, it would be surprising if some new techniques for appraising decision-making were not soon invented and codified. Thus, there is from this source, too, the prospect for the mutual enrichment of experimentation and other “applied” activities both as to technology and conceptualization. And of course, the appraisal of decision-performance sequences, here referred to as differential psychology, is also one of the constituents of therapeutic interactions in which communication is achieved. But this should surprise no one. A “decision-performance sequence” is simply a special case of intentional action, and so this is another way of saying that we may expect to understand human behavior (whether of

patients, clients, colleagues, or experimental subjects) by reference to person descriptions.

Innovations inevitably, are at some cost. If maxims and decisions were to gain currency in professional communication, no doubt some degree of initial confusion would have to be tolerated, and perhaps, too, a period in which our ignorance became painfully manifest. But we already know how to achieve tidiness in our professional activities, albeit at the cost of other advantages, so that there seems little reason to defer such innovations in our experimental technology out of apprehension of a “sorcerer’s apprentice” effect.

There are more speculative enterprises which fall under the heading of “doing more about human behavior” but which are primarily constructive and not primarily directed toward meeting recognized social needs. The Classification Space studies were a tentative step of this kind also. To be sure, one justification for those studies was therapeutic, since the present state of affairs in the production and consumption of information is here taken to be a chronic and progressive social pathology. Another motivation, however, was the intent to foster a certain kind of technology and conceptual climate in linguistic data processing which would make certain questions “real life” questions and, for example, one in which there would be a significant place for a full-scale computer simulation of a Person in case the psycholinguistic research program of which the Classification Space studies are a part continues to generate successful demonstrations. A hypothetical example of a similar sort would be to conceptualize and foster a set of new practices which would have its own logic, be assimilable to the body of existing practices, and *incidentally* provide courses of action for potential schizophrenics which they would have greater reason to pursue than those which would lead them to psychosis. (Such an enterprise would be surpassingly difficult, but one thing that would *not* be required is that we should first *establish* what it is that now accounts for schizophrenia.) Here, of course, one thinks of the recent development of community mental health programs and “poverty” programs. It would be a mistake to suppose that

the success of such efforts is contingent on “being soundly based on basic psychological (sociological, etc.) science.” History may record that our initial achievement of anything deserving the designation “basic psychological science” was contingent on the use of skills and concepts, and practices developed in the effective (or ineffective) implementation of such programs.

Even more speculatively, one might essay innovations of such a kind as to alter significantly the concept of a Person (a related issue is one of the central concerns of Part Five). But it would be fruitless to try to prescribe or predict the course of such creative efforts in any very substantial detail. *That* is non-empirical, too.



PART FIVE

Ubiquitous Persons

Even the investigator who finds the conceptualization of the Person immediately congenial will have reason enough to say, "To be sure Of course, that is just one possibility, though an attractive one at this time," and to be sure, that concept has not been presented as the modern moral equivalent of a Kantian category of mind. The concept of a Person is no more of a logical necessity than bridge is. The expectation of internal variability across culture, regions and Persons has been mentioned, and the alternative of giving up the Person concept *in toto* has been raised. The question which must be raised seriously in this regard is an empirical one: *Do we know how* to give up the Person concept in dealing with human behavior?

What gives point to the question is that a survey of the psychological literature provides a good deal of evidence that that literature as a whole is not merely consistent with the Person concept—it is coming more and more to take on the explicit logical form of the Person concept. Examples may be found in (a) the Mediation S–R theory, (b) Motivation theory as presented by John Atkinson (1964), (c) a recent review of the "perceptual defense" literature, (d) a recent prescriptive statement for psycholinguistics, (e) recent development of the "social psychology of psychological experiments," (f) recent discussion of the "cognitive aspect" of emotion, and (g) the still current issues of "ideographic (morphogenic) vs. nomothetic" and the functional autonomy of motives. These examples are presented below and others are suggested more briefly. ♦

I. Mediation S–R theory

The analysis given above of the Mediation Hypothesis as a general behavior theory provides evidence of this kind. The theory corresponds to a fragment of the Person concept. And the move from simple SR to mediational SR theory is clearly a move in the direction of the Person concept.

II. Motivation Theory

In Atkinson's account (1964, p. 295) of the evolutionary results of fifty years of research and theorizing about learning and motivation, the "contemporaneous determinants of direction, vigor, and persistence of action" are given by the expression $M_g \times E_{r,g} \times I_g$ where M_g is the motivation to achieve the goal, g ; $E_{r,g}$ is the expectation that the response, r , will succeed in achieving the goal; the I_g is the relative value of this instance (or of this way, as against other ways) of achieving the goal. In terms of Persons, this is to say that a person's action is explained by reference to what he *wants* (graded as to priority among his wants) and what he *knows* (and perhaps, in addition, by the amount of *interest* he has in achieving the goal—the concept of "motivation to achieve G " does not seem to be sufficiently articulated to permit an answer to this latter question). Thus, like the mediation hypothesis, this Expectancy theory is clearly identifiable as a fragment of the concept of a Person. What is even more suggestive, however, is the "speculative review and prospectus" which Atkinson gives for motivational theory:

(a) He points out that a general deficiency of current "Expectancy \times value" theories lies in their being situation-bound and consequently unable to account for the contribution of the person's behavior repertoire (SR "habit" or, more generally, what he *knows how* to do). *When this deficiency is made good, we have arrived at PI, the paradigm of intentional action.*

(b) He identifies the problem of the persistence of behavior and the resumption of interrupted behavior as further conceptual embarrassments. Toward the resolution of these embarrassments he proposes the principles of inertia and of motive reduction via success. The former consists of the statement that "a goal-directed tendency, once aroused, persists until it is satisfied." The second states that a goal-directed tendency may be reduced by success. In the light of the prior principle that behavior is determined by the strongest goal-directed tendency, we have the following picture: Behavior under a

dominant tendency *persists* until either that tendency is reduced by virtue of success or failure, or a different tendency is increased somehow so that in any case the first ceases to be the dominant tendency, and so the ongoing behavior is *interrupted*. As the competing tendencies are of greater than zero strength, when the corresponding behavior is interrupted, it will persist at greater than zero strength. When the same fate befalls the newly dominant tendency and some number of successors, the original tendency may once more become the dominant tendency, and then the corresponding behavior is *resumed*.

These problems are already dealt with in the initial, “roughed in,” formulation of the Person concept. What is involved here is first, the familiar maxim that “if a Person has a reason to do X, he will do X—unless he has a stronger reason to do something else.” Secondly, the inertial principle is from the outset a formal feature of intentional action: Wanting something or having a reason for doing something do not have to be actively maintained—they are simply not that kind of concept; if a person has a reason for doing X he will continue to have that reason until the circumstances relevant to his having that reason change. This was made explicit in Part Two, as was the “active organism” principle: In general, a Person has many more reasons than he can ever actualize in his behavior. (That conclusion does not require neurological reference). The inertial principle was given a more substantive delineation (Part Four) in relation to psychological processes: “What changes (or persists or is resumed) is his position *vis-à-vis* the intelligible world of Persons and objects. . . .” (The principle of reduction of motivation via success or failure is considered to be implied by either the maxim or the “what changes” statement.)

(c) The Person concept formulation, being the more highly articulated, leaves us in a position to sharpen the “goal-directed tendency” formulation: If I fear that crocodile that has just entered the door, I may have a goal-directed tendency to jump out the window. If his keeper steps in and removes him before I can raise the sash, I will cease to have a goal-directed tendency to jump out the window (won’t I?). But we would

not say that that tendency had been satisfied in any degree, nor yet that it had been frustrated. (And it would be difficult to think of e.g., “relief now” as stronger than “fear then.”) Was something satisfied here? Perhaps a goal-directed tendency to be in a place of safety. Or to be far away from the crocodile. Etc. To say that a goal-directed tendency was satisfied here would seem to imply either a knowledge of my intentions or a lumping together of all my possible intentions. But perhaps my intention *was* to jump out the window. Some concept of simply *eliminating* a goal-directed tendency seems to be required here.

(d) A similar sharpening can be accomplished in relation to avoidance behavior (p. 289). Here Atkinson argues that since the avoidant behavior in question occurs so quickly (upon presentation of some cue) that the animals “do not even have time to be afraid,” fear cannot be the motive. It is the animals who are “performing a response with an expectancy of a negative consequence” who show the visible signs of being afraid. Thus, “fear is a symptom, not a cause.” Here we encounter the frequent failure, pointed out in Part Two, to distinguish between the various logical roles, or concept-types, in which “feeling” concepts occur. As a motivational concept, as an instance of intentional action (NB the analysis of “fear” in Part One), it takes no time at all to *become* afraid, because coming to be afraid is not a process, but the acquisition of a reason for doing something, and that is an “outcome” type of event. In the analysis of “fear” for Persons, spontaneous, unpremeditated avoidant behavior, with or without any *other* appearance of fear, was precisely what was required to distinguish fear from e.g., prudence. Since we do not take a rat to be the sort of thing that deliberates, we do not need to keep this distinction here and so we have a greater latitude in what we describe as fearful behavior. (Our forms of description of animal behavior are *simplifications* derived from the general form of description of human behavior by giving up some of the characteristics of the latter. They are not prototypes from which the latter are contrived by adding nonessentials.) In the present case, the behavior of the rats would qualify as fearful even under the

form of description applicable to Persons. It is the *state* of fear that is associated with symptomatology having generally the characteristics of lessening of control. These distinctions and characterizations are among those for which we do not need to refer to laboratory experiments of any kind.

(e) Finally, there is Atkinson's closing prescription for the "integrative task which lies ahead for the psychology of motivation": It must bring together into a single, coherent conceptual scheme (1) the impact of the immediate environment on behavior, (2) the effect of stable individual differences in personality, and (3) "the constant influence on behavior of the persistent undercurrent of active tendencies to bring about particular effects which can be attributed to previous inhibition or inadequate expression of certain impulses in the past."

In Part Two the function of the Person concept in guiding behavior was illustrated as follows: My reaction to him in treating him as being resentful of Bill's success will not be simply a function of that fact of my knowledge of it—it will also be a function of what else I know and what I want and what I know how to do, and so my reaction will reveal me to others in the light of the concept of a Person, PII, which includes the "personality variables" as well as intentional action. From this, it is clear that *intelligible* and stable individual differences are no less *the* central feature of the Person concept than are the rule-following aspects about which much more has been said. Thus, the second requirement is met. The first requirement is met by the "know" concept-type in PI and the third was dealt with previously and has been reviewed above.

One could hardly present a more convincing case for concluding that the Person concept does represent the "ideal type" for current work in motivation.

III. Perceptual Defense

Goldin (1964) presents a scholarly survey of experimental evidence relating to “repression and defense.” His conceptual synthesis eventuates in the postulation of two “levels” of awareness—“pure” perception and “verbalizable” perception. The distinction appears to be identical with the PI distinction between being aware *of* something, and being aware *that* something is so. As indicated previously in connection with SR mediation theory, we do not need laboratory experiments to draw this conclusion. And no laboratory experiment could reflect the fact that the phenomenon does occur outside of the laboratory. More importantly, we have seen that the capacity to react without deliberation is an essential feature of intelligent behavior generally and that a lack of knowledge of *what* we do is something that can be maintained indefinitely.

IV. Psycholinguistics

The pragmatic formulation of linguistic behavior, which is interrelated with the concept of a Person and is illustrated by the Classification Space studies described briefly in Part Three and in detail elsewhere (Ossorio, 1964), appears to be consonant with Miller’s seven (the ubiquitous number) admonitions in regard to psycholinguistics (1965):

(a) “Not all physical features of speech are significant for vocal communication, and not all significant features of speech have a physical representation.” The latter is the significant admonition. It is one of the primary maxims for the C-space studies and has consistently recurred in the discussion of both language and person descriptions (e.g., as the basis for differentiating understanding from either prediction or inference in Part Two). The positive formulation is given in Part Four (“What changes is his position *vis-à-vis* the intelligible world of Persons and objects”).

(b) "The meaning of an utterance should not be confused with its reference." This may be seen as a direct derivation from the maxim that "we do not count cases on the basis of evidence," and again, it is not merely a verbal exercise—that was the critical step in which the rejection of multitrait multi-method thinking in the C-Space studies was codified.

(c) "The meaning of an utterance is not a linear sum of the meanings of the words that comprise it. . . .The whole is greater than the sum of its parts." This may appear to be inconsistent with the use of factor analysis in the C-Space indexing system. However, it should be noted that, first, it was *relevance*, not meaning that was dealt with there, and, second, the data were not fitted to a linear model on the basis of evidence—they began as criterion data and were *put* into that form, which is why the initial relevant consideration was expressed as a part-description, for instance "*the computations which* are associated with . . ." (and in passing: The Classification Formula was multiplicative rather than simply additive. Interpreting nonlinearity as a Gestalt phenomenon, both the not-otherwise-specifiable nature of the collection of practices codified by any specific terminology and the "what changes is his position . . ." formulation illustrate this feature. Interpreting nonlinearity more literally, it may be said that what this admonition suggests is the recourse to nonlinear relationships which are nevertheless explicitly formulable and therefore potentially do codify the capacities implied by the "generative" character of language. A significant beginning in this direction was made several years ago (Ossorio 1961) and is now seen primarily as an approximative method for providing numerical values (of the kind found lacking in the "high drive" research) to implement statements of the form "'x' and 'y' are similar in meaning," since there is no reason to suppose that the logical complexity of mathematics is adequate for the representation of the logical complexity of human behavior. Thus in the present research associated with the Person concept, the implementation of basic cognitive capacities (codified as "primitive terms in pragmatic discourse") has replaced the pursuit of non-linear mathematical

formulation although the success of the latter is not to be ruled out a priori.

(d) "The syntactic structure of a sentence imposes groupings that govern the interactions between the meanings of the words in the sentence." Thus "... simple theories phrased in terms of the chaining of successive responses cannot provide an adequate account of linguistic behavior." This appears to be a development of the non-linearity principle: with reference specifically to the kind of structures already identified by linguistic disciplines (specifically, structural linguistics). It may be noted that the psycholinguistic studies (Ossorio 1961) referred to in this connection *were* attempts to identify syntactic structures with characteristic linear or nonlinear formulae for expressing the meaning of any instance of a given structure as a function of the meanings of its immediate constituents.

(e) "There is no limit to the number of sentences or number of meanings that can be expressed." Thus, "our knowledge of a language must be described in terms of a system of semantic and syntactic *rules* adequate to generate the infinite number of admissible utterances." The central position given to rule-following in the presentation of the Person concept perhaps makes any comment superfluous. We may note, however, that the complexity of human behavior was explicitly related (Part Two) to the complexity of the range of possibilities (statable, in the light of Part Four, as the range of possibilities for being in different positions *vis-à-vis* the intelligible world of persons and objects) codified by language, a complexity which, it was asserted, far exceeds what can be established with finality by observation.

(f) "A description of a language and a description of a language user must be kept distinct." Thus, "... psycholinguists should formulate performance models that will incorporate ... hypothetical information-storage and information-processing components that can simulate the actual behavior of language users." And "A description of the rules we know when we know a language is different from a description of the psychological mechanisms involved in our use of those rules ... the

psycholinguist's task is to propose and test performance models for a language user, but he must rely on the linguist to give him a precise specification of what it is a language user is trying to use."

This admonition requires some discussion. Miller is apparently raising the question of what a speaker knows how to do, and raising it in this particular form: Speaking, e.g., English, is a standard-governed set of practices. To participate successfully in this set of practices requires the exercise of some kind of skill on the part of the speaker. If we characterize the required skills merely as knowing how to speak English, this is a purely *ad hoc* account, and we might as well just say that the person *does* speak English. What is required is a different description of the skills the speaker has such that (a) we have independent evidence of his possession of those skills and (b) the exercise of those skills has as its empirical outcome the result that English is usually spoken correctly, but it should account for mistakes or shortcomings in speaking English, because it must provide an account of what the speaker *actually does*. For example, how much a person can remember on a short-term basis appears to be related to how he performs in speaking.

First, we should notice that mistakes in performance are almost never taken to require a reconstruction of the descriptive system within which the performance is described. Such a requirement would eliminate all our skill-descriptive resources, simply because no human skill is exercised flawlessly on every occasion in which it comes into play. What we generally accept as an appropriate description in case of mistakes is a description within a different descriptive system which is brought forward as the "relevant circumstance." For example, a missed tackle is a defective football performance, but if we notice that a would-be tackler has just been clipped or if we discover that he had been drugged, we do not then feel that we cannot understand the successful exercise of football skills except under some new form of description in which "clipping" or pharmacological terminology plays a significant part. Thus, the fact that mistakes

in speaking English are sometimes made or that certain restrictions on speech seem to hold over and above those restrictions imposed by the rules which are *proposed* by linguists as being constitutive of speech does not appear to present any special methodological problems or to require an alternative mode of description in which speech phenomena are described as being something other than speech.

Second, the question of what a "performance" is must be raised. In general, a description of a psychological performance will be the description of a "psychological process" and as we have seen, this will provide a classification of outcomes rather than a characterization of a process. But any descriptive system which can be applied to "the same" phenomena as human behavior (and this will include physical and physiological descriptions) will provide a classification of outcomes. Some of them may be more adequate, descriptively, and others less, but all will qualify equally well as descriptions of what *actually happens*. Herein lies the clue. Such description will provide an account of what actually happens, but not what it is the speaker *does*. Only the description of an intentional action can provide an account of what the speaker does, *as contrasted with* what it is that happens. For other kinds of description there would be no such contrast. Thus, if there is any nonarbitrary criterion for selecting the performances which are involved in human behavior it would seem to be via the description of intentional action. And it is a distinctive characteristic of the skills which are featured in intentional action that the descriptions of those skills are implementable. That is, a person can decide to do *that* thing, to engage in *that* activity, to exercise *that* skill, whereas in general, a person cannot decide to instantiate the differential equations that describe his physical motions or the mathematical formulae which describe his information-processing accomplishments, etc. So that to say that what a speaker does, or knows how to do, is to speak English (or exercise component skills) is neither trivial nor *ad hoc*, because it can be established through repeated observations and it picks

out one possibility from among the myriad ways in which it could come about that instances of English *occur*.

Have we, then, still not separated a description of the language from a description of the language user, or the “psychological mechanisms” which are involved in conforming to the rules of the language? Perhaps not yet. But the difficulty here seems to be that this admonition is not purely descriptive in its assumptions, but instead makes the additional methodological claim that an “underlying process” formulation is a necessity. But although the possibility of such a formulation has not been rejected (and no doubt there is, anyway, a good deal of point to finding out what goes on in people’s heads when they talk), the necessity for it has been rejected and its general feasibility questioned (Part Four).

Nevertheless, the basic distinction between what the speaker does and what the language is (e.g., as described by a structural linguist) has been maintained all along in saying both that language is a set of social practices and that it codifies other social practices (cf. Part Three).

What corresponds to “psychological mechanisms” is the participation of the person in those social practices which are codified in language, those codified by language, and those which are instantiated in the use of language. This is what the person *actually does* when he says something in English. That is, he does something (engages in some intentional action) *in* saying something in English; he does something (engages in some intentional action) which is not a distinctively verbal move *by* saying something in English; and in saying something, he *refers* to something which may or may not be something distinctively verbal in nature. His doing any of these (usually, they all go together, which is a basic fact for psychotherapy) is a genuine process (the “overt attempt”—of Part One and Part Four) and a psychological fact, so there is a real point in calling *this* sort of thing a “psychological process.” The linguistic practices of saying something are essentially distinguished from the nonlinguistic practices which they codify or instantiate, but the two kinds of practice are *constituted* by their correspondence [cf.

Part Three], so that they are functionally inseparable. This dual codification represents a kind of reciprocity which makes the domain of human behavior a relatively self-contained one (comparable, e.g., to the dynamic balance of the solar system). It is the tightness of this functional structure which leads to the expectation that the relation of behavioral phenomena to other kinds will be most appropriately formulated in terms of general constraints—boundary conditions, learning or adaptation gradients, inequalities, and “bottleneck” phenomena rather than in systematic, molecular, reconstructive dependencies. (Would it be perverse to remark on the analogy between such a state of affairs and the gross functional relationships which have been thought to hold between the cortex and the lower brain centers? And is one of these states of affairs somehow more *real* than the other? If so, then it is the participation which is the more real, because that is the form in which our lives are lived.)

A gesture at two loose ends: First, the notion of “saying something,” suggesting as it does merely the uttering of a declarative sentence, is far from doing justice to the variety of ways in which language actually enters into significant human actions (cf. Wittgenstein, 1953, especially Part One). Second, the intrinsic connection between saying and doing may be objected to on the basis that it assumes something which is known to be false, i.e., that all grammatical sentences are meaningful. This is simply not the case. The mark of a nonmeaningful sentence is precisely that it cannot be used to say anything. If the need for some explanation is felt here, there appears to be no empirical or methodological reason to suppose that the nonsignificant grammatical sentences are not extrapolations (via part-descriptions) from the kinds of sentence that *can* be used to say something. (And Carnap [1958, Chapter 13], working with formal languages, has shown how it is possible to construct a complete semantic system on the basis of semantic concepts such as “true” and “designates” and “formula” without any requirement of a previously and independently specified syntax.)

If the “participation process” which has been proposed as the alternative to the “underlying process” appears to be somehow less real than the latter, it may help to recall that the Classification Space study, which does reflect a participation process formulation, *apparently* provides substantially more effective linguistic data processing than is provided by any nonclassified technology resulting from years of work based on sophisticated engineering techniques and “underlying process” formulations in spite of the fact that the C-Space procedures were technically prototypical and represented the most molecular, and therefore the most disadvantageous, circumstance possible. (The C-Space taps the single capacity associated with “relevance,” whereas the real power of a participation process system lies in the mutual assimilation of capacities, analogous to the mutual assimilation of descriptions illustrated in Part Two—Piaget *ex machina*.) Moreover, the long-term project of identifying and providing computer-realizable implementation of basic pragmatic forms of discourse would, if successful, provide something very much like a set of underlying processes. That is, it would provide a set of capacities for “treating something as an X” which go beyond what a speaker would necessarily be able to say or decide, but which could be used to reconstruct the latter.

(g) “There is a large biological component to the human capacity for articulate speech.” Here it may be noted that the entire structure of the pragmatic formulation of linguistic and nonlinguistic behavior rests on the bare fact that people *have* the capacity to agree in their judgments and the capacity to learn to agree in this respect. Maxim: The irreducible and inexplicable is constitutional.

In summary, the participation-process account of language is doubtless not the kind of “performance model” envisaged by Miller. Nevertheless, it does not appear to slight the aspects of language which his admonitions remind us an adequate psycholinguistics must take into account. Thus, the linguistic aspect of the Person concept does warrant the technical designation of

“Psycholinguistics,” and it has the apparently unique feature of generating the study of *what* happens when people talk, in contrast to the usual psychological device of studying what *else* happens when people talk.

V. The Social Psychology of Psychological Experiments

Orne (1962) has described some of the “demand characteristics” of the psychological experiment as a social setting. His thesis is that “. . . the subject must be recognized as an active participant in any experiment, and that it may be fruitful to view the psychological experiment as a very special form of social interaction.” This corresponds to the formulation summarized in Part Two as, “The rules are the same for the observer and the person who is observed . . . making observations is something that Persons do.” He suggests some very sensible control procedures which have some similarities to both placebo methodology and the usual “control group” techniques, to provide estimates of the contribution of the “demand characteristics” (which are simply what the experimental setting as such provides reason enough for the subject to do in the light of what he knows), and he remarks, “The study and control of demand characteristics are not simple matters of good experimental technique; rather, it is an empirical issue to determine under what circumstances demand characteristics significantly affect subjects’ experimental behavior.”

In his discussion, Orne appears to waver between (a) an explicit acceptance of the “other things being equal” principle in evaluating the effects of the experimental setting and (b) a recognition that since *all* social settings have demand characteristics, there is the almost certain consequence that there is a significant “Demand Character Effect x Treatment Effect” interaction across the real life settings to which we would like to

generalize, and so we have no recourse but to leave the laboratory and conduct survey-style studies in order to determine empirically the ecological validity of the experimental results.

A similar use has been made of the Person concept as a means of approximating an “aseptic technique” in experimentation (aseptic, signifying uncontaminated variables). The general statement here, of course, is simply the maxim: “The subject is a Person.” As might be expected, the application of the articulated paradigm of intentional action to the experimental situation offers some advantages over the use of only the “reason enough” portion of it.

For example, because of its nonempirical aspects, it provides a means of making allowances for what would otherwise appear as a second-order demand character (a consequence of the fact that the procedures for the treatment group and the demand-character control group are different and hence may generate artifacts in the *control* procedure). This is because we can ask, about any experimental setting, “what does it give the person reason enough to do in the light of what we assume he knows and (we assume) he knows how to do?” The latter, the question of what the subject knows how to do is an element which seems most significantly missing in the “demand character” formulation. One way in which it is relevant to the control problem is the question of the extent to which saying what they expected or *would* have done is something subjects know how to do.

Perhaps more importantly, the “know how” concept-type (and the rest of PI) is also directly applicable to the main experimental design—the experimental setting may present unsuspected constraints as well as unsuspected demand characteristics. For example: Suppose that (a) a subject is trained in giving numerical estimates of an invisible criterion on the basis of observable cues, using an explicit rule, or policy, (b) in a test situation, his estimates are challenged by another subject (a confederate), and (c) under conditions of challenge, options are available to our subject: He may consult a third person who (for different subjects) is described as having been trained (1)

under the same rules as the subject, (2) under different rules, or (3) under an unknown set of rules. The experimental variable associated with the latter three treatment conditions is “the anticipated likelihood that the ‘consultant’ will tell the subject he is wrong.” But the rationale for the experiment includes the view that on being challenged, the subject may feel the need of assistance by the consultant in order to make an effective decision (which is part of the demand character). But now, we have to ask, in that case, *are* there three options present? We cannot see how the subject can be helped by the consultant who was trained under different rules because there is no reason to believe that the subject *knows how* to make that kind of use of that kind of information. Unless we can revise the procedures so as to provide a *real* option here, we cannot regard the three treatment conditions simply as representing different values of “likely to tell the subject he’s wrong.” In going through this procedure we would not be ruling out a priori the possibility that if we went ahead, some of our subjects *would* choose that consultant. Rather, we would have discovered that if any subject did that, *we could not understand his behavior* and so we would in that respect be unable to interpret the results of the experiment.

A related application is one in which we ask, independently of design and rationale, will a person who takes option X be especially likely to do so for a particular reason, reason Z? If so, then is there another part of the experiment where a bias toward having a reason Z will make a difference?

In summary, the kind of experimental control described here is directed toward the ideal expressed by the maxim: “The rationality of the subject should not affect the distribution of the experimental outcomes across the possibilities provided by the experimental design *unless* that can be interpreted as a treatment effect.” And the C-space study illustrates how we can also arrange it so that essentially *all* the treatment effect is of this kind. The suggestion was made earlier that rational behavior on the part of the subjects is the most parsimonious explanation for what they do, if an explanation of this kind can be found.

From this point of view it would be especially important to try to rule out such explanations by suitable experimental controls whenever some other interpretation of experimental results is desired.

VI. Cognitive Factors in Emotion

Schacter and Singer (1962) present evidence and argument to support the conclusion that “cognitive factors appear to be indispensable elements in any formulation of emotion.” Their thesis is expressed in these propositions:

(a) “Given a state of physiological arousal for which an individual has no immediate explanation he will ‘label’ this state and describe his feelings in terms of the cognitions available to him.

(b) “Given a state of physiological arousal for which an individual has a completely appropriate explanation . . . no evaluative needs will arise and the individual is unlikely to label his feelings in terms of the alternative cognitions available.”

(c) “Given the same cognitive circumstances, the individual will react emotionally or describe his feelings as emotions only to the extent that he experiences a state of physiological arousal.”

Here, some clarification is required. Proposition (a) is better expressed in the more “aseptic” form:

If a person is in a state which *we* identify *technically* as a state of physiological arousal, then if he describes his state at all, he will describe it in terms of what he knows, and if he describes his feelings at all, he will describe them in terms of what he knows; moreover, he *will* describe his state and his feelings.

The latter is obviously an empirical proposition, but a safe one, since in the experiment, each subject was *instructed* to describe his feelings, and this description was *taken* to be a description

of his state. What follows the first “if” clause is a partial statement of the concept of intentional action: In describing his state or feelings, the subject is engaging in intentional action, and when he does this he acts in the light of what he knows. Thus, proposition (a) simply states a portion of the Person concept within the context of a specific empirical condition. With respect to the initial condition in proposition (a), it should be clear that if a person is in a state of physiological arousal, that is *not* reason enough for him to know that that is the case—on the contrary, if he does know, *that* requires a special explanation, e.g., that he has had special training or has received special information from an experimenter. And if he has not, in general, reason enough to know about it, it would also call for a special explanation if he *did* know how to give an explanation of his state of physiological arousal. So this portion of the original formulation has the force of saying “under normal conditions.”

Thus, the three propositions are subsumed under two aspects of the Person concept: (A) A person acts in the light of what he knows, and (B) In different states a person’s behavior will show overall differences and his capacities are likely to be altered also. What is added is the survey-type information as to how this works under differences in (a) the state which is involved, (b) information available to the person, e.g., information about his physiological state or about the feelings and actions of a co-participant, and (c) the kind of action in question, e.g., reporting his feelings or interacting with a co-participant. Note that if our general findings were that “physiological arousal,” like, e.g. white blood cell count, could vary over a wide range without having any reportable consequences, we would not consider this contrary to the Person concept formulation—we would simply conclude that no change in the *psychological* state (the state of the Person) was involved.

VII. Uniqueness and Functional Autonomy

The present formulation of the concept of a Person bears a noticeable resemblance to earlier formulations by Allport (1937, 1961), who has long been prominent for his insistence that the study and description of individual persons is a primary task for personality theory. Of the two formulations, that of the Person concept is substantially the more complex: (a) it involves a *systematic* reference to language, concepts, and human behavior *as such*, whereas psychological theories, including Allport's, have involved only informal or secondary reference of this kind; (b) it involves a greater conceptual articulation—the analysis of intentional action, of types of intentional action, and of conceptually distinct, though logically related types of personality variables provides a more differentiated picture of human behavior than does the concept of a hierarchically organized set of traits together with loosely associated concepts of interest, attitude, temperament.

At the same time, the present formulation involves a rejection of several central features of Allport's point of view:

(a) It rejects the “building block” metaphysics inherent in any view which regards physical particles (for Allport, read “biophysical dispositions”) as being all there *really* is in the world. One of its particular forms is particularly rejected, e.g., the form in which the bald assumption is made that a human body is *ipso facto* a physical object (i.e., that it falls within the subject matter of physical theory) so that, for example, if I come to know that he is angry by observing what he does, this implies that the description “he is angry” is validated by a physical-object description (or, similarly, that a physical-object description of what I saw is a description, whereas the person *description* is an interpretation).

(b) It rejects the “common element” metaphysics inherent in the nominalist theory of universals and in the Resemblance Theory which Allport regards as a watered down nominalism (1937, p. 310).

(c) It rejects the view that the crowning achievement of language is to facilitate passive reference to what is already known to be “out there” prior to language (logically prior).

(d) It rejects the view that personality variables such as traits either (1) *influence* the behavior of the person whose traits they are, or (2) are generally, or must be, *inferred* from behavior.

These several differences in scope and form make it possible to accomplish what appears to have been Allport’s primary theoretical aim (to conceptualize a psychology of individual persons) without encountering the methodological difficulties associated with his position.

An example of such a difficulty emerges from an examination of the following statements:

(a) “*Resemblances* are never identities, not even in *certain respects*. If I say that you remind me of some friend, and you ask me why and wherefore, I may narrow down the resemblance to similar build or similar resonance of voice. But even in these respects you are not identical with my friend; your build or your voice is only similar to his. . . . Every experience of similarity implies a paradox. There is both *sameness* and *novelty* in the experience. Two stimulus fields are compared, and though for certain purposes they may be subjectively equivalent, still, they are known to be disparate and nonidentical. A sense of two-ness, not of identical one-ness is always involved.” (1937, p. 283)

(b) “Although traits are real enough entities, trait names are essentially blankets, covering one trait in one person and other (similar) traits in other people. Though perceived as similar and labelled identically, the trait is never, strictly speaking, in two human beings exactly alike.” (1937, p. 310)

(c) “The scientific evidence for the existence of a trait always comes from demonstration by some acceptable method of *consistency* in behavior (the consistency

being not a matter of stereotyped habits, but of equivalent responses). It is simple enough to prove some people self-consistent: They are, for example, almost always *decisive* or almost always *fastidious*. . . . one investigator may be content with relatively low measures of consistency; while another . . . may demand almost perfect correspondence among his measures before inferring a trait.” (1967, p. 330)

The difficulty hinges on the use that is made of “resembles,” “identical,” “similar,” and “equivalent.” In reference to (a), above, it should first be noted that the answer to why you remind me of a friend need not be that you resemble him at all. It may be a matter of association or contrast or for no discernible reason whatever. Second, the specific examples of “build” and “resonance” used by Allport have, ironically enough, the characteristics of common traits, and they involve continuously variable perceptual features, so that they are incomplete descriptive expressions corresponding to Carnap’s “functor” (1957, p. 71). If we look to the examples of individual traits mentioned in (c), i.e., “decisive” and “fastidious,” it is a different story. These are attributes, not functors. A corresponding example in respect to physique is not “build,” but “stocky” or “slight.” And we see that two men who are both stocky, and genuinely so, *might* be correctly said to be neither identical nor equivalent, nor even similar in respect to physique, and yet they are not merely similar and not merely equivalent, but literally identical *in respect to being stocky*. To say that they are not identical in respect to physique is to say that physique terminology provides at least one form of description under which they are *different*. To say that in respect to being stocky they are identical is to say that under *this* physique description, they are *the same*, that under this description they cannot be distinguished, for they both meet the criteria. So it is not the case that one or both of these men is not *really*, or not *quite*, stocky. There is no need for them to be identical in any *other* respect (there need be nothing *else* in common) in order for them to be identical in

this respect. Nor is it the case that the physique descriptions under which they are different are somehow more basic, or carry more authority, than the description, “stocky.” To recognize that though they are both stocky they do not have the same physique is not to have a prelinguistic access to *Reality*—it is merely to have mastered the criteria for both “stocky” and “the same physique.” (This is not to say that language is logically prior to perception—NB Part Three.)

The way in which this difficulty about identity and similarity makes itself felt is illustrated in the following: Trait Y is a “biophysical disposition” (and therefore “real”). What disposition? A disposition to do X (“do X” will be used in place of “act in an X-like way,” which is more accurate in preserving the “focal” quality of traits). What is it to do X? Well, instances of X differ among themselves, but they are equivalent in respect to being X. How does this differ from their being identical in respect to being X? It doesn’t (see above). It is true, however, that if two behaviors are identical in respect to being X, they are not thereby identical behaviors, i.e. they differ in some other respect. But the latter feature is already part of their being *two* behaviors (which is not a question of anyone’s subjective impression of one-ness or two-ness). Now, if two individuals have Trait Y, they both have a disposition to do X. If we know of such persons, they *do* do X, since that is how we know they have that disposition. None of the behaviors which are instances of X are identical with any of the others. All are identical in respect to being X. If we segregate the X’s belonging to one person from those belonging to the other, we identify two sets within the total set. Here, to adopt a familiar terminology, there is neither “within” nor “between” variance among these behaviors *as* instances of X. The nonexistent “within” variance provides the *only* basis for saying that Person A has Trait Y and Person B has Trait Y. Yet the nonexistent “between” variance provides the *only* basis for denying that the Trait Y which Person A has is, strictly speaking, exactly the same *trait* as the Trait Y which Person B has. So there is a double standard being

applied here, one for which Allport has provided no justification and one without which it seems doubtful whether any nomothetic-ideographic controversy would find a point of application.

The double standard provides convincing evidence (there is other evidence, e.g., 1937, p. 297) that for Allport the “biophysical disposition” that a person has is logically, not empirically related to that person. My biophysical dispositions are unique, not because they show themselves to be so (how could they do that?) but because they must be so. Because they are divorced from language, are prior to language, are “really out there,” my biophysical dispositions turn out to be dispositions to do just those things which I do do. And so calling them “biophysical” merely announces a metaphysical commitment without being informative in other ways. For the next question is “And *what* is it that I do?,” and how are we to know or answer that except in the light of the concepts and the language that we do have. What guarantees the uniqueness of my disposition to do X is the uniqueness of those of my behaviors which are instances of X. And what guarantees the uniqueness of those behaviors is not their being X (for in that respect they are not unique), nor yet that each is, somehow, uniquely not-quite-X, but simply that they are particular behaviors *of mine*. That I am unique is part of my being an individual, and that is a matter of logic and language, not something that one could discover to be so or not to be so. If we ask, “Which behaviors?,” we simply pick out a number of episodes in my life history, and each one is a unique historical event. But if we ask, “*What* behaviors?,” the answer must be a description of those behaviors *as* behaviors. Here, the uniqueness is beside the point. What is required is intelligibility, and for that we need descriptions. Descriptions involve concepts, and both are public and general rather than private and unique. Exemplars of a concept are “the same” because it is the concept and the description which codify our standards for what is *the same*. And in the crucial cases, the picking out of behaviors is accomplished by describing them, not by pointing wordlessly to them.

In the Person concept formulation, the phenomena of uniqueness and generality are handled in a straightforward way by distinguishing between descriptive uniqueness and existential or logical uniqueness in a way which (it may be hoped) does not even suggest the possibility of a nomothetic-ideographic clash:

(a) Personality variables (PII), which are by no means restricted to traits, are conceptualized by reference to actions of a distinguishable and identifiable kind. All that is required here is the ability to use person descriptions correctly. That ability is not *logically* parasitical on any other ability. In particular, when I observe an action to be of a particular kind, e.g., angry then (1) I do not need first to have observed that some other event has taken place (no inference is made), e.g., that a movement of a physical object has occurred; and it is not logically necessary that any event of the latter kind *has* taken place either in me or out there; and (2) I do not need to have noticed that that action *resembles* anything whatever in any respect whatever; and (3) its being that particular kind (angry) is in no way qualified or watered down by its *also* being of some other kind.

(b) Personality variables are characterized as series of actions of a particular kind, and so are characterized by part-whole relationships as well as by type-descriptions (e.g., “angry”) of the constituent actions.

(c) Each such series is part of the life history of a Person. There is a second kind of part-whole relationship here—the part that the series plays in the person’s life and the part that each constituent episode plays in the person’s life. (For practical Purposes, we might want to talk about a heterogeneous series of actions, e.g., a series that would form a pattern of some kind, but formally, as soon as such talk is intelligible, a membership-characterization of the members of that series will once more make them actions of “the same” kind.)

(d) Since each Person is an individual, his history is a unique series of episodes, and every distinguishable episode in that series is unique. The uniqueness (the identity) of each episode depends on its being distinguishable from other episodes, and this depends on having concepts or behaviors as

being of particular kinds and the corresponding mastery of the descriptive terminology. So there is no conflict between the general and the unique.

(e) To describe a Person completely is to make every appropriate use of all our person descriptions in characterizing him. When we have done that there is nothing we have left unsaid. There is not then a residual uniqueness which we have failed to describe. If we later achieve a new person concept, we have added to our ability to treat people in one way rather than another, and so we can say more about them, too. What is not the case is that then we have discovered something which we had previously overlooked or been ignorant of. (Maxim: Only what can be answered can be asked.) If there is a residual uniqueness in any sense, it is not a matter of descriptive approximation, but rather, the logical indeterminacy discussed in connection with the use of PII.

A second controversial issue raised by Allport is the principle of functional autonomy of motives. This is stated in the following form (1961, p. 229):

Functional autonomy, then, refers to any acquired system of motivation in which the tensions involved are not of the same kind as the antecedent tensions from which the acquired system developed.

The primary difficulty associated with the principle is that of making intelligible how functional autonomy comes about. Other theorists do not deny, for example, that "Sam seems always to be seeking praise," is not deducible from "Sam has a need-nurturance," or any combination of similar statements (1961, p. 226) and that it is not deducible from a description of Sam's needs at an earlier time. What most of them would say is that Sam's seeking praise is a historical accident, that it is the way in which an earlier need was satisfied and continues to be satisfied, though in addition, that behavior may be shared or preempted by other needs (cf. "fusion," "displacement," "subsidiation," "convergent hierarchy"). It appears to be primarily the methodological difficulty of accepting a transformation from

mechanism to motive which leads other theorists to maintain that (a) the principle of functional autonomy presents a question, not an answer and (b) the answer is that the autonomy is only apparent, and the driving force for the behavior in question is the same old driving force, even though the behavior is different. It seems clear, too, that as long as motives are conceptualized in such inclusive terms (e.g., “need–nurturance,” “life instincts”) that any behavior could be considered an instance of the expression of one or more such motives, no empirical evidence could discourage a determined theorist who wanted to claim that, for all the superficial novelty of behavior, it is the same old needs that operate. It is from learning theory, which need not be committed to any particular set of drives, that we obtain the clearest indication that the principal objection to functional autonomy is not the priority of other motives, but rather, the mechanism–to–motive transformation.

Allport’s final formulation in regard to how functional autonomy comes about is the following (1961, p. 244):

It would be scientifically satisfying if we could give a simple answer to the question of how functional autonomy comes about. We cannot do so, unfortunately, and for two reasons. In the first place, we lack knowledge of the *neurological processes* that may be involved in the transforming of old systems and the maintaining of new systems of motivation. In the second place, present-day psychology has no consistent theory of the *nature of man*. The phenomenon of functional autonomy will never be clearly understood until we know more about the relevant neurological mechanisms and about the correct formulation of the purposive nature of motives.

The specific gestures which Allport makes toward an explanation in the crucial case of “propriate functional autonomy” are primarily the following: (1) If existing motives do not consume available energies, genuinely new motives will perforce arise; (2) growth, competence, mastery, and self-actualization are basic life urges; and (3) the essential nature of man is such

that it presses toward a relative unification of life. However, the first of these would seem to be a promissory note which could be cashed only if we invented acceptable ways of identifying different energy states and assigning numbers or inequalities to them; and the latter two have the verbal characteristics of explanations, but in fact express the decision that no explanation is to be had.

The formulation of the Person concept makes a contribution to this issue in the following way:

A. Neurological findings are appraised as irrelevant to the explanatory task of making functional autonomy intelligible. The noncontroversial behavioral phenomena which raise the issue of functional autonomy serve as a standard for neurological formulations rather than vice versa. No neurological theory would fail to be rejected out of hand if it implied that the behaviors which we know do occur were impossible. Consequently, no neurological theory or findings could do more than tell us what *else* happens when functional autonomy (or any behavior) comes about—it could not put our knowledge about functional autonomy on any firmer basis.

B. Conversely, the earlier analysis of the Person concept does qualify as a consistent descriptive account of “the nature of man,” and as a “formulation of the purposive nature of motives,” and that does permit a more detailed analysis which exhibits functional autonomy as a nonempirical derivation from the concept of intentional action and yet is quite congruent with Allport’s presentation.

The first consideration is that intentional action requires the exercise of acquired skills. The second is that the acquisition of skills is accomplished only through practice. The third is that the actions which constitute the practice which leads to the acquisition of skill A cannot be actions which require the exercise of skill A. (The same methodological consideration is reflected in the concept of “emitted response” in the operant conditioning paradigm.) Analogous preliminary considerations also apply to the “know” and “want” concept-types. We acquire knowledge by experience. The actions by which we then acquire a

particular piece of knowledge cannot be actions which require that knowledge. And the actions whereby we learn to make the appraisal of situations as being of a certain kind (e.g., avoidance of danger) cannot be actions which involve the making of such appraisals. Thus, the concept of intentional action involves what amounts to a built-in growth requirement which stems from the fact that mastery of criteria for performance skills and observational skills must be learned, because the ability to conform to a standard must be learned.

Given that a particular action, B, requires particular knowledge, X, particular reasons, Y, and particular skills, Z, I cannot perform B before I have X, Y, and Z. But as soon as I have them, it becomes *logically* possible for me to perform B. Once I can perform B, I might have any number of reasons for doing so. Some of them might be reasons that I already had before. Some of them might not be. But there is at least one reason which I might now have which I could not possibly have had before. That is doing B without any further end in view. During the time when no action of mine could be B, no reason of mine could be my reason for doing B. And doing B for its own sake is not a reason that could be a reason for doing anything other than B unless it was already possible to do (i.e., other actions might be performed as a way of arriving at a position where B could actually be carried off, but that could not happen until it was logically possible for me to do B). Thus, my being able to do B brings with it the logical possibility of my having a reason for action which I never had before, i.e., doing B for its own sake. The fact that among the reasons I could now have for doing B, the one which it can be stated flatly I could not have had before is doing B for its own sake, enables us to understand why formulations of the principle of functional autonomy have taken the form of saying that what was previously only a means (i.e., the "overt attempt") has become an *end* (done for its own sake). But this can now be seen as a special case of the more general consequences of coming to be able to do B *at all*.

An analogous development can be given for cases where it is the acquisition of knowledge rather than skill that makes the difference. For example, going hunting is one of the activities that might become functionally autonomous. I might be said to know how to hunt without ever having done it (if I know how to shoot, hike, track, etc.). But if I have not yet done it, I do not yet know what it is like to hunt, and if I do not know that, then I could not possibly be hunting now for its own sake. Only from having done it for some other reason could I learn by experience what it is like, and only after that could I do it for its own sake. Moreover, this consideration does not apply only to the initial experience of hunting. Changes in circumstances (e.g., the opening of a new preserve, acquiring a new rifle) may make a difference in what hunting is like, so that although previously it was not something I would do for its own sake, now it is.

In summary, to acquire new skills or new knowledge brings with it the logical possibility and the practical possibility of engaging in hitherto impossible intentional action, and so it brings the possibility of having reasons for action which were not possible previously. Thus, the phenomena which give a point to the assertion of functional autonomy are rendered intelligible by an analysis which is anchored firmly in the logic of behavior description and not bogged down in the paradoxes which, understandably, issue from the shotgun wedding of disparate descriptive systems, the physiological and the psychological. Reasons for avoiding any *a priori* fiat about what the relationship between those two *must* be were indicated in Part Four.

C. A review of the preceding analysis will show that as soon as the possibility of the functional autonomy of motives can be derived from the formal structure of intentional action, no “drive” formulations of competence, adequacy, self-actualization, unification, etc. are required. Here, the Person concept provides an advance over a bare “urge” characterization: As indicated in the discussion of PII in Part Two, (a) that persons have such “basic human needs” as the needs for competence,

order, self-actualization, etc., is a logical requirement derivable from the Person concept; (b) a person's self-concept is a particularized Person concept, and therefore shares the coherence and unity of the latter; and (c) the self-concept falls under the "know" concept-type as a significant determinant of a person's actions, hence the pattern of his activities will also share in the coherent structure of the Person concept. Moreover, we should recognize that there is considerable external pressure and support for growth and consistency and that the social practices which provide such pressures are, by and large, the same ones which determine what kinds of consistency we are able to recognize. Thus, the Person concept formulation is consistent with Allport's conclusion that being self-consistent, self-actualizing, and being both "in depth" is "the nature of man," but it has the further advantage of showing how and why this is so, and requiring no more mysterious ingredients than the kind of learning by experience the occurrence of which has never been questioned.

VIII. Other Suggestions

The reader will be able to continue the list for himself. Some suggestions toward this end are the following:

(a) A variety of theories make important use of the concepts of "feelings," "impulses," "unconscious motivation," and "basic human needs," concepts which have been dealt with briefly as intrinsic, but derivative, features of the concept of a Person.

(b) The movement toward "ego psychology" may be seen as a move to effect the conceptual parity of "want" and "know" concept-types in psychoanalytic theories. Likewise, the recent plea for "effectance" by White (1963) may be seen as a move to achieve the conceptual parity of "want" and "know how" concept types. And when we have done both we have again arrived at PI, the concept of intentional action.

(c) Dissonance theory and self-concept theories appear to be fairly direct consequences of the concept of “treating oneself as a Person.” For instance, we understand ourselves in terms of the concept of a Person, which requires that a Person have *reason enough* to do what he does, and we make decisions (which will in time become implicit and spontaneous) by reference to all of the PII characteristics which we are *aware of* having. Like the system of language and nonverbal behavior (but not as much), the self and self-concept duo tends to be self-closing; for example, the failure to perceive that one is of such and such a kind (e.g., tactless, or selfish, or unselfish) seems frequently to be an important part of *being* that kind. Or it may be an important part of not being of that kind (e.g., guilty—NB, the “ego defense” example of Part One).

(d) Psychosomatic and hypnotic phenomena provide the current cash value of hypothesizing the kind of phenomena discussed in Part Four in terms of the analogy with “ $\sqrt{2}$.”

(e) The Person concept incorporates the major suggestions made by Tyler (1959) toward a “workable psychology of individuality”: It provides for (in fact, requires) choices by the individual and provides the structure (organization) for pursuing the implications of a person’s having made this and other choices; the potentialities of the “interest” concept-type easily span the range from vocational choices to object cathexes as avenues for understanding and characterizing the individual; moreover these are, as Tyler requires, not dimensional characterizations.

The Person concept provides a coherent, articulated synthesis of the major conceptual trends in the history of Psychology:

(a) It is *behavioristic*: The discussion of intentional action makes it clear that person descriptions *necessarily* involve public concepts and practices rather than something inaccessible or ineluctably private in experience or in the body.

(b) It is thoroughly *cognitive*: This should require no amplification.

(c) It is *phenomenological* and *field-theoretical*: The irreducible capacity for recognizing instances of person descriptions by straightforward (though not necessarily simple or simple-minded) observation rather than primarily by inference, and the central place of the capacity to react spontaneously (but intentionally) to what one observes gives the Person concept a basically phenomenological status, and the strategic concept of “what changes is his position *vis-à-vis* the intelligible world of persons and objects” is both phenomenological and field theoretical in nature.

(d) It is a psychology of *individual differences*: The Person concept is the means whereby we characterize individual Persons and correspondingly, distinguish among Persons.

(e) It is a *psycho-bio-social* concept: The public nature of person descriptions and the crucial contribution of social practices makes the Person concept inescapably a social one. Formally the Person concept is distinct from any other kind. However, as presented above, the mutual assimilation of descriptions, at least via relational descriptions, is contingent only on the decision (and our ability to implement that decision) that *the same thing* can be described as a Person, as a biological object, as a biochemical object, as a physical object, and as a sociological or anthropological object. Few of us would hesitate to take that step, at least, initially. Thus, no degree of psycho-bio-social integration is excluded. What is not done is to pre-judge the degree or mode of integration formally by constructing an a priori psycho-bio-social “package.”

(f) It is as strongly worded a *functionalist* position as can be coherently stated—both the linguistic practices, the corresponding nonlinguistic practices which they codify, and the codification itself are functional concepts.

(g) Finally, it is thoroughly *existential*: Psychological theory and psychological knowledge do not “explain” what Persons are, and they cannot explain them away either—their value lies in *adding* to our understanding of Persons. Persons are essentially rational and are therefore capable of irrationality—they adapt, but they are not nonrational adaptive mechanisms.

Persons are inescapably saddled with freedom, decision, and responsibility, though there is a good deal of quantitative leeway here. These aspects of the Person concept appear to cover the central positive and negative theses of the existentialist position in psychology. The Person concept makes the facts of, e.g., ethics, aesthetics, and religion a part of psychology (cf. the analysis of “guilt” in Part One) without requiring reductive translations of those facts and without confusing any of those other disciplines with psychology or conversely.

Perhaps, then, it is time to write “Q. E. D.” If the systematic relationships presented above are not acceptable as constituting at least a *prima facie* case for the conclusion that the Person concept makes explicit what it is that does now and has in the past guided psychological research and theorizing, then presumably nothing is.

These relationships help to make two things clear:

(A) First, the sense in which the deterministic-underlying-process theories have failed: They provide some new practices, and sometimes some new concepts, and often a good deal of new terminology. But they do not provide a new logical structure—they merely provide additional, and in most cases inferior, instances of some, and only some, of the same set of concept-types that we already have. Thus, as explanations, they have much the same limitations as the classical “homunculus” theories.

(B) Second, the apparent success of process-talk, i.e., what it is that gives the illusion of success in “treating those psychological ‘processes’ as processes and thereby explaining behavior”: In the delineation of the paradigm for intentional action, one of the difficult tasks was to disentangle the “overt attempt” from the entire intentional action. This is because we do not normally have occasion to distinguish between the two except when we question whether something was done intentionally, and we do not have a separate terminology for the two. If I say “I drank a cup of tea” I have not distinguished between the outcome (what I accomplished, i.e., downing a cupful of tea) and the observable *process* of doing the drinking.

There is a good reason for this: the outcome is not a *contingent* outcome of the process—it is a necessary outcome, because we *identify* the process by means of the outcome. There are an unlimited number of distinguishably different processes each of which would be the process of my drinking a cup of tea, but we do not have separate names for them all any more than we could have separate names for all the visible shapes that could be drawn. (Maxim: Truths are cheap and infinite in number.) But there is a good reason for that, too, in addition to its being impossible. No process, absolutely none whatever, is the process of my drinking a cup of tea *until* and *unless* the outcome of that process *is* my having drunk a cup of tea. And the logical form of the description of an outcome is the logical form in which we conceptualize something that we *want*, even (as almost every motivation theorist has pointed out) in those cases where we normally identify some object as being what we want. So we do not need two descriptions, and it is not an accident that we do not refer to behavior processes separately from their outcomes or that the outcomes which are most directly codified in language are predominantly those which are intelligible as being something that is wanted or positively unwanted either in itself or by virtue of what it leads to (cf. Anscombe, 1957). Since language is common, it is the means and ends repertoire of the social group (with a substantial temporal depth) which is codified in the language *they* speak. And this adds to our understanding of how it comes about that language codifies nonverbal social practice in such a way that the two are inseparable: those practices are not merely *customs*—they are the organized system of *objective, observable* processes which link means and ends and wants. And if we see that the infinite set of possible processes which corresponds to a given describable outcome cannot be explicitly described and therefore *must* be represented by something like a “capacity” concept, we also see why it is *specifically* “want,” “know,” and “know how” concept types that give to the “overt attempts” the significance of intentional action.

Thus, there is one thing which we all *know how* to do as well as we know anything at all: That is to talk about behavior in a way which: (1) conforms to the logical structure (the “grammar”) of the Person concept, (2) refers to things which are the outcome of observable processes and (3) does refer also to the processes which are involved, but not by saying anything in addition to the reference to outcomes. The first of these is the conclusion drawn in (A), above, supported by the entire discussion of the Person concept. The second and third can be independently established from any armchair by thinking up instances—the purpose of the preceding discussion was primarily to emphasize that no one is immune to this phenomenon and that it is so much “bred in the bone” that no deliberation or decision is involved, and no sophistication beyond the bare ability to speak a language.

Under these conditions, it would be extremely surprising if we could *not* get away with: (a) Using the ordinary language “outcome” terminology as before, but taking it now to refer to the observable process, the overt behavior which we now set ourselves the task of “explaining,” (b) making up a set of nonsense syllables or theoretical terms, and using them in accordance with the logical structure of the Person concept, since using words that way is what we know so well how to do *without thinking*, (c) taking these terms to be referring to psychological “processes” after the manner in which our ordinary discourse refers to *observable* processes, this being what we know so well how to do, without thinking, (d) “discovering” that our talk about psychological processes correlates with our talk about overt behavior (after having distorted the behavior “units” sufficiently, in our attempts to be “objective” and “precise,” so that the correspondence is not great enough to make us suspicious), so that (e) if we take our nonsense syllables to be referring to underlying processes we can take our observable behavior to be the outcomes of those processes, (just as the *motion* of a body can be seen as the outcome of internal physical processes), in which case we have “explained behavior” or,

(f) we can complete the full circle and take our ordinary language descriptions once more to refer to outcomes, except that now they are the outcomes of the “underlying” processes, and (g) we have achieved a “scientific”, “authoritative”, empirically confirmed account of behavior and of person concepts.

To proceed in this general fashion is one of the things a psychologist knows how to do.

The positive force of these conclusions may be read back into the discussion of Part Four, where the Person concept is described as a natural starting point for further creative-conceptual-empirical efforts. It provides the necessary *descriptive* basis for such efforts.

The negative force of the evidence is the clear suggestion (1) that a central theme of psychological research methodology has been in effect, the avowal that Persons are not Persons unless it can be empirically demonstrated that they are, (2) that this has been an empty gesture, like designating that mountain as a pawn, because the avowal is not one which we *know how* to implement (we do not have other, real, descriptions of persons that would provide the means for asking the questions, and we do not have *other* practices that would provide the means for settling the questions), although (3) because that gesture was not obviously a lunatic procedure (especially in the absence of an adequate account of either Persons or language), it has required rather extensive observation to make a convincing case for saying that we have not succeeded in treating ourselves or other Persons in the ways we have described.

Here, too, is a familiar perplexity and imminent dismay. It is the problem of counter-transference in psychotherapy. “How do I know that what I claim to be merely observing is not something that is arbitrarily predetermined by my ways of thinking?” (This is different from the related question, whether I have produced the behavior that I claim to be merely observing. [Cf. Martin 1964]) The significance of this question will be derived from what I know (and in the longer run, by what I *know how* to discover) about my ways of thinking, and this includes my appreciation of what difference it makes to see

things one way rather than another. Which is to say, among other things, that the question cannot do service for a destructive skepticism. If “How do I know . . .” is not answerable in terms of current practices, in terms of what I and others know how to do, then I have not succeeded in raising a question by uttering these words. I have not succeeded in expressing a doubt, but perhaps, anyway, I have given myself a caution, or a reminder, or suggested a direction. And then perhaps, I understand differently what it is I am doing, and so perhaps I do differently also. And what is the case in psychotherapy holds for psychological research as well. We need not be *convinced*, or have the *proof* in hand, of doing poorly in order to have sufficient reason for doing differently. We are not always properly in doubt, but that is not the same as standing pat.

Appendix A

Psychological Description and Philosophical Theory

One of the greatest difficulties associated with presenting the descriptive account of psychological subject matter is that it is too often mistaken for a philosophical essay both by psychologists and by philosophers whose primary competence lies outside of psychological philosophy or whose theoretical convictions are of a positivistic sort (in a word, all but a handful of American philosophers). To see the present essay as an attempt at philosophical analysis is only one form of this error. Sometimes, for example, the descriptive account is taken to be the *application* of philosophical arguments to the subject matter of psychology. That there is *something* to be said for each of these conclusions seems hardly sufficient ground for so crude an error.

The formulation of the concept of a Person is psychological in its instigation, in its inception, and in its main concern. The instigation was a very practical one. It was the classic problem of how to teach students something about the interpretation of diagnostic instruments, case histories, and psychological theories, and about the conduct of psychotherapy and laboratory and field experimentation, without requiring that they give up their own conceptual and theoretical preferences in favor of those of an instructor (hence the descriptive focus). A related goal was to accomplish this within a conceptually coherent, intellectually satisfying, and substantively adequate framework, which is something that our current semantically oriented methodology and underlying-process theories do not, in any case, supply. Thus, the remarks on topics such as observation, inference, direct experience, feelings, explanations, etc. which

are made in the course of the descriptive account are made with the intention of delineating verbal moves and inculcating verbal skills which are *actually usable* in a field of endeavor in which there is a very significant distinction to be drawn between mere talk and effective performance. This concern is reflected in the pragmatic, or participant, focus which characterizes the descriptive account, in contrast to the usual semantic, or *mere* observer, focus of current psychological methodology. The focus is on the observing participant, not on the "participant observer."

With this focus go several other characteristic features:

(a) Giving up the posture of omniscience which is involved in semantic theory and relying instead on reference to whatever knowledge (belief, information) the behaving person has available at a given time. It is the latter, and not a guess at, or the ideal of, or the assumption of some Truth, which is required as a condition for behaving.

(b) Giving primary attention to the effective *use* of language rather than to the etiquette of talking about the use of language. It would be informative to say that the descriptive account is deliberately an "object language" account, so chosen in order to take advantage of the reality constraints which operate on the connections between what is verbal and what is not verbal. It would be incorrect to say this, however, since the treatment of language bypasses the technical Semantic distinction between object language and meta-language. Instead, an attempt is made to give an adequate account of the reflexivity of language and of psychological description by using natural language in its normally reflexive character (see below).

(c) Giving a considerable emphasis to the contribution of action (rather than merely observation) to knowledge. This involves making and using the distinction between knowing and knowing how (cf. Ryle, 1949) and also what amounts to the old distinction between practical and theoretical knowledge (being aware *of* X vs. being aware "that" P).

(d) The participant and reflexive emphasis of the descriptive account are reflected in the crucial feature of taking as

one of the *basic* ingredients in the phenomenon to be accounted for the fact of individuals who need, and attempt, and succeed in giving explanations, including, importantly, explanations of the activities of similar other individuals. Thus, the account is indispensably a social one. This procedure is to be contrasted with taking as basic either the content of *particular* explanations (including, for example, philosophical explanations) or the types of individuals which figure in such explanations (e.g., physical particles, neurones, genes, etc.) The alternative chosen here has the advantage (as it is taken to be) of reducing our tendency to reify our explanations, for example our tendency to go from the unexciting linguistic necessity involved in using a causal descriptive system to the supposed *Real Necessity* of every event having a cause, and thence to the further conclusion that any noncausal explanation is somehow incomplete or less than basic.

(e) In brief, the descriptive account is neither nominalism, naive realism, skepticism, linguistic analysis, nor a return to the pragmatic theory of truth. Rather, it is responsive simultaneously to considerations which separately would incline us in one or another of these several directions.

It would be crass ingratitude not to acknowledge a major debt to those philosophers without whose previous work it is unlikely that the present effort would have been a practical undertaking. The initial formulation of the concept of a Person in substantially its present form was made in 1964, stimulated primarily by the writings of Wittgenstein (1953), Ryle (1949), Anscombe (1957), Strawson (1958), Gosling (1962), and Carnap (1958). Since that time a considerable amount of confirmatory evidence pointing to a substantial communality of thought has been uncovered in the form of similar conclusions or arguments advanced by other writers. Notable among these parallels are papers by Bambrough (1961, 1965), Rhees (1954), and Griffiths and Peters (1961). The present claim to giving a *descriptive* account is supported in significant measure by the compatibility

of the present account with a variety of more detailed examinations of “mental” concepts provided by these and other contributors to the literature of analytic philosophy.

Nevertheless, the requirements for a descriptive psychology are different from those for philosophical analysis, and the difference is evident and substantial. There is no question here of a “difference in degree.” They are simply and categorically two different social enterprises. The former requires scope and coherent articulation, whereas the latter requires a great deal of differentiation and specific detail. Consequently, the present descriptive account exceeds the psychological scope of any of the philosophical analyses of mental concepts, and its doing so is an empirical condition for its adequacy (NB the discussion in Appendix B).

The descriptive account does provide a number of results which, at the present writing, would be new to the philosophical literature. Peters (1958) argues that the “rule-following model” is the normal form of explanation of behavior, and is normally the correct form. Mischel (1963, 1964) argues that psychologists do use this model and that it’s all right to do so. But neither of these writers nor anyone else has *presented* anything that could be called “the rule-following model.” As a state of the art formulation, the concept of a Person presented in the descriptive account is “the rule-following model.”

Making the “rule-following model” explicit involves a formulation of intentional action which is itself appreciably novel and has at least two results which might be of some philosophical interest. First, it provides what may eventually be considered to be a satisfactory resolution (or part of one) of the current “issue” as to whether actions can be caused. Second, it provides the logical structure for clarifying the disparate-yet-related uses of “emotion” terminology. For example, it shows that, “He did it because he was afraid of P,” (or any analogous reference to other feelings) is straightforwardly an intentional action description. And it reconciles Kenny’s (1953) and others’ claim that emotions have objects with Gosling’s (1965) claim

that they *need* not have objects (in an intentional action description, the feeling has an object; in an emotional state description the feeling need not have an object).

If such results are indeed of philosophical interest, that will not be trivial. That *will*, however, be incidental to the task and the fact of constructing a descriptive account for Psychology. The “arguments” which appear in this account are not philosophical arguments “applied” to a psychological problem if by that is meant something like applying a rule of arithmetic or a table of integrals to a bridge-building problem. The use of these arguments is more like the use of old paving bricks in the construction of a new oven—they are part of a different enterprise where the standards for suitability and for effective performance are different enough so that it is not a *crucial* issue where they came from or how well they did there. Even this analogy is misleading however, in that it suggests that philosophical theses are “the real substance” of the Person concept in the way that the bricks are “the real substance” of the oven, whereas the Person concept is a more extensive and coherent conceptualization which provides a place for a variety of heretofore disparate philosophical analyses and thereby gives them a significance and intelligibility which they did not previously have.

The following examples may help to clarify further the difference between descriptive psychology and philosophical analysis in regard to standards and methods, corresponding to differences in interest and attitude appropriate to the two disciplines:

I. The Issue of “Dodging the Real Issues”

The interests of philosophers, and the fashions and pressures of the philosophical community, will determine that at a given time certain “questions” are accepted as *prima facie* topics for investigation. They will also determine to a large extent

how each topic is handled. Issues are defined more and more explicitly, and any writer who adopts a position on such a question without answering or acknowledging the current opposing positions will himself be in a dubious position. He will be described as arbitrary and professionally irresponsible. A special case is that in which he fails to observe distinctions which have been made and *accepted* in the course of the communal enterprise of formulating and pursuing the question at issue. Here he will be convicted of vague and loose formulations, and he will be said to have “dodged the real issues.”

Given his characteristically philosophical concerns, a philosopher might find much to object to in the formulation of the concept of a Person. To be sure, some objections would be misdirected from a failure to distinguish between dealing with an issue, and merely talking about it. For example, someone who knew that there was a current controversy about causes of actions but had no substantive mastery of the issue might suppose (because that controversy was not explicitly discussed in the formulation of the concept of intentional action) that here was a case of taking an arbitrary position and “dodging the real issues.” It is a commentary on our preoccupation with a semantic verbal technology at the expense of a concern for subject matter that dealing directly with a problem rather than merely talking about dealing with it comes to be seen as avoiding the real issues. An error of this magnitude would seriously prejudice the possibility of understanding either the letter or the spirit of a practical account in which the giving of an actual resolution is a significant achievement, even if it has some drawbacks, whereas the prescription of a hypothetical resolution has no value whatever.

Nevertheless, a good deal of effort *has* gone into being *quite* noncommittal at a number of points where a philosophical issue (or some other kind) might be discerned. Such locutions as “whatever difference it makes,” “criteria of frequency and appropriateness,” “the part it plays in a person’s life,” “do justice to . . .,” “codify,” and even the basic terminology of “wants,” “has a reason,” “is aware of,” “knows,” and “knows how,” are

expressions of this concern. The irritation and impatience that a practitioner or a devotee of philosophical analysis is likely to experience thereat will also be a measure of the degree to which commitment on certain “issues” has been *successfully* avoided.

It would help here to distinguish two different sorts of “looseness” or, conversely, “tightness.” The formulation of the Person concept is “loose” in that many specific details are left unexamined, and so many alternative possibilities for elaboration are left open. It is “tight,” however, in that there are no apparent loose ends of a kind that would affect the overall structure (see below). The interconnections among the concepts which *are* implicated do not depend on the details which are left unexamined. Nothing would be lost if the concepts involved in the descriptive account were all completely unanalyzable. Devotees of analysis are inclined to forget that “general” terminology such as is illustrated above *has* a use, and it need not be merely a surrogate for a more specific terminology, but may be more informative and intelligible than more specific terminology. That analysis has come to be the preferred treatment for linguistic pathology provides no strong basis for supposing that going through the same motions for an entire language would have any particular merit.

Is this terminological policy a case of “dodging the real issues” by resorting to “loose” formulations? The answer, for the descriptive psychologist, is “Nonsense!” To manage a complex presentation requires rigor and precision in saying what has to be said, *no more* and no less. “Rigor,” “precision,” and “careful” are honorific terms in the scholarly lexicon. But none of them implies going into as much detail as possible.

Equally clearly, it is no part of a responsible descriptive account to try to settle academic controversies, though something of that sort might go along *with* a descriptive account. The real descriptive task is to do justice to what we *now* take to be the case, including the fact of controversy, without merely taking sides, but also (and this is crucial) without taking it for granted that there *is* a solution or answer to be had. (If a

for granted that there *is* a solution or answer to be had. (If a philosopher is working on an “unsolved problem” or “controversial issue” does it then follow that neither he nor we can know or say what he is doing until he solves the problem or until the issue is settled? And if he never solves it, was he doing nothing? To take any such tack *would* be linguistic pathology.) It is the mark of success in a descriptive account to avoid commitment on “questions” to which we neither have nor need the answers. (But we may leave a place for such answers if they are later to be had.) Possibly a separate discussion of the “issue” of causes of actions will illustrate how it was indeed possible to “do justice to the facts” of a controversial question without taking sides and without mentioning the controversy explicitly in the prior exposition of the concept of a Person. To burden a psychological account of actions with the shop talk and extraneous concerns of philosophers would be to do no service to either philosophers or psychologists.

Thus, with respect to those “issues” which are avoided in the descriptive account, there is a clear and straightforward position to be taken: Since we have not the answers to those “questions,” it is quite clear that having those answers is not an essential part, *and never has been any part at all*, of the way human lives are lived. A description of the latter, which is simply a description of persons and human behavior, hence a description of psychological subject matter, *must* therefore refuse any commitment on such an “issue” unless *in addition* to the descriptive account a resolution is being offered. Arguing and developing controversial issues is just one of the many things that persons do, just as they also play baseball, have babies, or invest in stocks and bonds. Since the descriptive account gives a schematization of the totality of human activities, it would be inappropriate to try to catalogue them in such detail as to include the professional concerns of philosophers, brokers, actors, and others.

A different sort of claim is this: Some of those “issues” which are ignored really are basic, and until we deal with them

successfully, no adequate descriptive account of persons and human behavior can be given. (The question of causes of actions might be a candidate here.) And here, different responses are appropriate:

(a) If the claim is that having that answer is *logically* necessary for the success of the descriptive account, then the claimant has already demonstrated his disagreement with the choice of subject matter delimited by the descriptive account. But since he has made it a matter of logical necessity, he is in the position of having taken an arbitrary stand on what is now a controversial issue, hence he “is dodging the real issue” and so we might do well to suspect his judgment.

(b) If the objection is not, in effect, a disguised stipulative-definition, then the claim that the descriptive account will fail may be settled by waiting to see if in fact it does fail. If it does fail, that may not be for the reason proposed, but at any rate some change is called for, and here is a candidate. If the descriptive account does not fail, then the claim was not correct. The appraisal of success or failure must not hinge specifically on whether an answer to the purportedly crucial question was provided, for that would be the same case as (a), above. Thus, the mere claim that some of those bypassed issues are unavoidable and basic requires no *present* qualification of a descriptive account which embodies the decision to ignore those “issues.”

(c) The possibility is not ruled out that convincing reasons might be provided for appraising the descriptive account as defective, and that would call for a change. For example, we might conclude that it allows for undesirable exceptions or extensions, or that it fails to cover distinct phenomena coverage of which is indispensable, or that it exhibits serious ambiguities which are not merely absence of detail, etc. It would be plainly incredible if an initial construction of the scope and difficulty of the description of persons and human behavior contained no mistakes and was as complete as it ought to be.

Nevertheless, it cannot be too firmly stated that *no* apology for ignoring philosophical issues is in order and none is being offered. In one sense, this is simply because “questions” such as

“Can actions be caused?”, “Can ‘mental’ concepts be analyzed into some other kind?”, and “What are the varieties of reductionism?”, are as irrelevant for Descriptive Psychology as they are for Physics, Aesthetics, and Economics. And the problems of philosophy are no more significant for Descriptive Psychology than are the problems of Physics, Aesthetics, and Economics. If the denial that the problems and methods of philosophy have any special significance for Descriptive Psychology appears to be overemphatic, that appearance will be correct. It is overemphatic, for there is nothing, really, to deny. It is not that the connection is plausible, but merely that it has been made by more readers than can comfortably be attributed to “chance,” and so the present effort is an attempt to do justice to *this* fact in a way which goes beyond the simple observation that such an error indicates a lack of understanding of the Person concept, of the pragmatic frame of reference, of the substantive philosophical issues which are purportedly carried over, or all of these.

If the formulation of the Person concept were treated as philosophy, and *if* that were done with understanding, that might be a way of generating some further “issues” relating to the making of assumptions. For example, it may be said that philosophers who have analyzed mental concepts such as “fear,” “direct experience,” “interest”, “attitude,” “emotion,” etc., have succeeded in doing so only because they have left implicit, hence “assumed” the concept of a Person, without which none of these subsidiary concepts is intelligible. Because we all have mastered the use of the Person concept, that omission has hardly been felt or noted in the literature, but it would seem that it has only to be mentioned in order to be acknowledged. In this respect, the present account represents a technical advance which makes good this omission. It is therefore directly comparable to nothing which exists in the philosophical literature, and whatever problems may inhere in it, they cannot literally be the same problems as those with which we are familiar in the tradition of philosophical analysis.

What is distinctive about the Person concept formulation is not that it involves philosophical assumptions, but quite the contrary—that it replaces by an *explicit* account something which is universally *assumed* by *both* philosophical analysts and their positivistic and phenomenological competitors. So that one who understood the Person concept could only respond with a certain sense of grotesquerie to the suggestion that it involved assumptions in the sense of taking for granted material which is discussed explicitly and held open to question in the philosophical literature.

This is perhaps nowhere more evident than in connection with the notion that the present formulation of intentional action rests on assumptions as to whether actions can have causes. For how could any discussion of whether actions can have causes *not* involve assumptions as to what an action *is*? Would we not find it ludicrous and somewhat repelling to find mechanics arguing vehemently over whether an automobile could be made by hand when all the time none of them could even begin to say what an automobile *was*? And if such a person told us that what an automobile was was logically dependent on whether one could be made by hand, what should we tell him? Perhaps that only one who already knew what an automobile was could make such a statement with authority. (Compare: (a) “What a cube is depends on what color it is”; (b) “What a cube is depends on how many sides it has.”) To engage in the dispute without claiming that authority would be simply egregious. But to claim that authority without being able to *say* what an automobile was would be to admit that that knowledge was in the implicit form of something taken for granted—in a word, an *assumption*. And it is not a different case, nor yet a more attractive sight, when it is word-mongers rather than iron-mongers who are parties to the disputation. If we compare “Can actions have causes?” with “Do Freud and Allport provide alternative accounts of the same thing?” there is, methodologically, little to choose from between them.

Because the Person concept is a concept, not a thesis, it provides a refutation of certain theses about what science, or

psychology, or persons, or explanations must be like, not by counter-argument, but by *counterexample*. A refutation of the thesis that a work of art must be beautiful may be legitimately attempted by presenting an item as being (a) a work of art and (b) not beautiful. Under these conditions a *direct* judgment as to whether it is a work of art must be given, for to use beauty as a criterion would be to beg the question. (To introduce other criteria in this example would be an irrelevant elaboration.) The major claim associated with the present account is to have *presented* (a) a science (b) of psychology, which consists of the construction of (c) explanations of the (d) actions (e) of persons. From this follows the minor claim that any putatively definitive statements about either science, psychology, persons, actions, or explanations which are incompatible with the present account are *ipso facto* false.

Because the Person concept is a *counterexample*, methodologically, with respect to certain claims about what psychology, science, etc. must be, it has a substantive use as well as the methodological use as a *counterexample*. The substantive use provides a simple and direct challenge to the putative status of certain questions (Can actions be caused? Can person concepts be replaced? Is replacement the appropriate type of reduction to use here? etc.) as genuine questions which have to be directly answered in *some* way on pain of giving an *incomplete* account. The challenge is not on the basis that "I happen to know the Truth of the matter," nor even the more democratic claim that "One person's opinion is just as good as another's." Rather, the challenge is that these so-called questions haven't yet been *shown* to have, and give every present appearance of *not* having, and so, descriptively, don't *now* have the characteristics which in less controversial cases pretty clearly distinguish genuine questions from the kind of thing a child does in mechanically countering every parental response with another "Why?" and the kind of fumbling that a parent is driven to when he tries to take the later "Why's" seriously as *questions*. These characteristics are, briefly, (a) that we know what would *be* an answer, (b)

that we know how to *get* an answer, and (c) that it makes a difference *which* answer we arrive at.

In the present formulation, a “question” which lacks these characteristics fails to achieve the pragmatic status of a question, irrespective of its syntactic status. To say this, however, is not to offer a modern pragmatic equivalent to the old positivistic ritual: “Unverifiable? Meaningless! Off with its head!” It is not to dismiss something summarily as unimportant or pernicious. On the contrary, it may be *very* important to a practitioner of philosophy (or psychology, economics, etc.) that a locution is a nonquestion at the time it is considered, even though it has all the earmarks of a question. *That* will then qualify as a problem even though the locution does not qualify as a question, and so it is something to be *worked on*, even though it cannot be *answered*. In turn, this formulation is connected with the positive further view (presented briefly in Part Three and Four) of scientific (and philosophical) activity as primarily a matter of creating or inventing social behaviors rather than of uncovering hidden, preexisting truths. (That is, *this* is what ‘the discovery of preexisting truths’ amounts to.) The psychologist who tells us that he is investigating the neural correlates of certain “raw feels,” or the “neurological mechanisms underlying behavior,” and the philosopher who tells us that he is investigating the question of whether actions can be caused, are both in a peculiar (though familiar) sort of position. Their position is essentially that of a producer who is offering stock in a play which has yet to be written and may never be written, much less acted, even though he can “already” tell us the plot. Such a person is *not* in the position of the ticket seller who invites us to attend an *existing* set of performances. In general, then, if we do not yet have those social practices which have not yet been invented, we are not now lacking them, because there is not now anything of the sort to be lacked. And so a descriptive account which fails to mention anything of the sort is not *missing* anything, either. Not now, not yet, and perhaps not ever.

II. Truth, Assumptions, and Philosophical Theory

A major variation on the theme of failing to deal with issues is this: that the descriptive account is built upon unexamined assumptions which are themselves sufficiently controversial or dubious to vitiate the value and interest of the “superstructure.” This is also a variation on the theme that in the descriptive account philosophical arguments are being “applied,” so that the validity of the results depends on the initial validity of the arguments as *philosophical* arguments. Some candidates for “unexamined assumptions” in the descriptive account might be (a) that person descriptions cannot be reduced to other forms of description, (b) that there is only one sort of reduction, and (again) (c) that actions cannot be caused.

Now, perhaps the following claim could not really be defended in *every* respect *ad infinitum*, but there is no doubt that the place to begin is to say flatly that the concept of a person involves *no* assumptions. On the contrary, everything is out in the open, schematically, if not in detail.

A concept cannot easily be presented other than by means of declarative sentences, and when it is as complex as the concept of a person there are bound to be a substantial number of declarative sentences side by side. To a reader who is accustomed to the Beaux Arts style of academic prose and insensitive to regional and personal preferences for more forthright forms of discourse, this is likely to give the impression of rashness, high-handedness, intolerance, or arrogance, particularly in connection with statements about the nonempirical character of what is being presented.

No doubt such a difficulty would arise primarily for a reader who thought that there was any issue here at all. For example, someone who had misunderstood the kind of effort involved in the descriptive account might fail to see that the concept of a Person is just that—a *concept*, not a *thesis*, and then he might also fail to keep in mind that for concepts no question

of “true” or “false” can arise at all, since they are not statements. And because they are not statements, neither can they be derived from any premises, *a fortiori*, nor from unmentioned premises which could qualify as “assumptions.” Nor can they be asserted, and so neither can they be asserted rashly, high-handedly, arrogantly, or dogmatically. Concepts are unquestionable because as nonstatements, it is part of their business to be that way. If they were not unquestionable and imperious to evidence, they could not play the part that they do in the asking of questions and the gathering of evidence. And so, if a concept is *presented* in declarative sentences in which *this* is affirmed and *that* is rejected in no uncertain terms, that will, in the present account, be generally in the service of delineation rather than an impossible and quite irrelevant claim to Truth. (Compare: It is well known, and codified in Carnap’s Lambda notation and Quine’s “. . . X . . .” notation that the locution “F(X)” may be used as a vehicle for referring to that property of X such that F(X). A parallel, but less familiar use of “F(X)” is to call attention to, or present the concept of, an X such that F(X). In neither case is the locution “F(X)” appropriately treated as the *statement that* F(X) or anything else, hence there is no question of truth here.) That this should be so ought, it would seem, to be clear from the presentation itself, for here, consistently, reference is made, not to the truth of statements, but to the *use* of a concept, i.e. the concept of a Person. Moreover, a simple summary of the concept is provided several times, so that there should be no question as to *what* concept is involved (“the concept of a person is the concept of an individual whose history is a history of intentional action elaborated into the format of PII”). However, it is an occupational hazard for philosophers and psychologists (if not scientists generally) that they are likely to develop methodological reflexes which lead them to respond unthinkingly to any declarative sentence with the explicit or implicit equivalent of “Prove it!” Such an attitude has kept a number of readers from ever getting beyond the shocking fact that certain declarative sentences in the descriptive account *were not shown to be true statements*. Here, indeed, is a

pretty how d' you do. Could one ever effectively question the contribution of Truth to behavior and to our understanding of behavior if the first and decisive test that had to be passed by such an attempt were that it should consist of putatively true statements? But anyone on whom that technical Semantic concept exercises such a fascination would be well advised to reflect on the fact that the *assumption* that an adequate account of behavior *must* be given in putatively true statements is an assumption which not merely has never been shown to be true, but is one for which we have no evidence whatever (what would *be* evidence here?).

One approach to the problem (a pragmatic approach which is embodied in the descriptive account) would be to treat that implicit semantic test with the open contempt that it deserved and then hope that the point would get across before the lynching was fairly under way. The point, that is, that nothing is missing from the basic account of behavior when the contribution of truth is left out. (Compare: Arithmetic is not "true," nor yet "false," but we *use* it effectively, and it codifies what we *do*.) Only then, and at some danger of negating prior understanding by appearing to renege, could one hope to communicate anything useful by adding, "And in every way, so far, it appears that the individuals we normally accept as persons *are* exemplars of the concept of a Person." For that is to say that "we," not *primarily* "they," are Persons, and the saying is both a reminder and an invitation to increased participation, not a resurgent claim to Truth. (Presently in preparation is a more detailed examination of the apparent possibility that reference to acting on the belief that "F(X)" is true is literally replaceable, in the strong, pragmatic sense described in Part One, by reference to rule-following and the use of concepts.)

Here again, the difference between analytic philosophy and Descriptive Psychology is crucial. In analysis, a positive statement is either (a) something taken for granted, (b) directly an invitation to agreement, or (c) backed up by a delimitation of alternatives and the elimination of the alternatives. Since the appeal is to established usage, the material required for eliminating

alternatives is presumably always available, though it may require considerable re-working, and so the task is by no means easy. In contrast, in the *construction* of a descriptive account of psychological subject matter, the criterion for selecting statements is simply whether they can function as parts of such an account. (Compare: The criterion for selecting automobile parts from an extensive assortment of parts is whether the part in question can serve as a part of a *functioning* automobile composed of other parts selected on the same basis.) *Consequently, the elimination of apparent alternatives in the way it is accomplished in philosophical analysis is not a necessary requirement for the descriptive account*, though it would provide a sufficient condition if it could be accomplished. (These two carburetors might be equally good, considered merely as carburetors, but perhaps only one of them goes with other available parts to make a functioning automobile. And if one were apparently defective, that would settle the question of which one to try first.)

In brief, the sentences in the descriptive account to which a critical reader might propose “unexamined” alternatives and from which he might thereby conclude that some assumptions had been made, are not accompanied by any prior, independent truth claims. Pragmatic stage directions for their use would be not be “‘p’ is true,” but rather “Here is an ingredient, a component, which has such and such a part to play.” To present such considerations is to present reasons for choosing one component as against alternatives. To present such reasons is to indicate that the selection was *not* made, e.g., by flipping coins, counting votes, or consulting authorities (not even philosophical authorities).

Since having reasons, giving reasons, and asking for them is an integral part of the concept of a Person, the foregoing pragmatic account of the presentation of that concept in the present effort is subsumable under the content of what is said in that presentation, and this is part of saying that “everything is out in the open.” That *this* should be so is, in turn, part of the linguistic aspect of the concept of a person.

To complicate matters somewhat, the arguments presented for certain statements become part of the implications of selecting those statements, so that following the argument is at the same time practicing the application of the descriptive account. For example, if the inferential account of observation is rejected as leading to an infinite regress and a confusion of causes with achievements or reasons, to follow that argument is also to appreciate what is being built into the descriptive account and therefore it is to know better how the descriptive account can be used. This is quite the opposite from a situation in which the concept of a person was somehow a *result* of such arguments. (Compare: Carburetors are what they are for us because we *already* know about internal combustion engines and the part that a carburetor would play in such an engine if it had a carburetor; we do not construct the concept of an engine out of concepts such as the concept of a carburetor in the way that we construct engines out of parts such as carburetors, or the way we construct a description of an engine out of descriptions of parts such as carburetors. But we may *come to know* about motors by first being shown or told how a carburetor works, and then how a . . . ; the pragmatic distinction between the illustrative and the deductive use of language runs parallel to the distinction between observation and inference as the basis for behavior.) When the concept of a person is finally given, it is such that a person who used that concept and knew what he was doing thereby could construct such arguments according to the demands of particular situations. It is the arguments which “follow” from the Person concept, not vice versa, and they “follow,” not as deductive consequences or as conclusions of any kind, but rather, as being *more restricted exercises* of the same skill (cf. Appendix B). The discussions of experimentation, theorizing, and explaining subsequent to the initial presentation of the Person concept provide further illustrations of the use of the concept. Some number of illustrations is all that can be given, for there is not a definite list of uses of the concept, because there is no end to the uses of the concept, and to help

clarify how and why this is so *is* one of the uses of the concept of a Person.

Still, it might be said, claiming the complete absence of assumptions must somehow be a something-for-nothing proposition, the conceptual equivalent of a perpetual motion machine. Perhaps in the long run, this is so, if only to the extent that we *might* always *be able* to talk about assumptions, even though such talk was quite superfluous. But the claim need not be taken absolutely in order to be taken seriously. The point of making the claim is primarily to call attention to the basic difference between rule-following and truth-seeking (which is a very special case of the former), and between following a methodological policy and making substantive assumptions.

To give a descriptive account of a phenomenon involves being as conservative as possible in what we say, given the task at hand. It is to eschew interpretation, elaboration, or explanation as much as we can, given the task at hand. (I might answer the question “What object is that?” by saying what color it was. That might be more conservative, e.g., if it was the judgment that could be made with the greatest confidence or justification. But it would also be to evade the task of saying what object it was. If we do not use the task as a standard, it is always more conservative to say nothing. Note, however, that it is sometimes more conservative to assert than to doubt, e.g., when we are “reading off the features” of what we observe.) To eschew interpretation, elaboration, and explanation as much as possible, given the task at hand is, clearly, to follow a methodological policy. The most instructive locution here would be a maxim or slogan, not a purported statement of fact (but it might well be put in a declarative sentence). Hence any talk about assumptions here is simply superfluous and misdirected. There is not even the assumption that there is some point to following that policy. Instead, following that policy is part of following the more general policy of doing what there is apparently the greatest point in doing, and when we fail, it is the result of inability, not the consequence of assumptions (except, again, as a very special case of the former).

To be sure, persons might differ in their judgments as to what constituted being as conservative as possible in a particular circumstance, and two people might give different descriptions of “the same” thing. But that would be an excellent basis for supposing that they did not have the same concept, and, as indicated previously, a primary value of descriptive efforts is precisely to bring such differences out into the open where they can be negotiated or allowed for.

Following a policy of methodological conservatism involves the refusal to take sides on controversial issues (in part because that would be to assume that there was an answer, and that would be less than conservative—it would be extremely rash, unless, of course, one could *present* such an answer). In addition, however, it provides the basis for selections which may appear arbitrary in other respects and hence may give the appearance of taking sides.

For example, the fact that person descriptions have not been *shown* to be replaceable by other types of locution is sufficient basis, given this policy, to formulate the concept of a person as an independent, or distinctive, descriptive system. For a casual reader or a Truth-oriented reader, this might well appear to be a case of taking sides in favor of the “no reduction” viewpoint. But there is really no alternative here. To have done otherwise would be to have succeeded already in doing what no one could reasonably expect to be accomplished within the next millennium and what many would say was clearly logically impossible. It is one thing to say “Maybe person descriptions *could* be replaced; after all, it hasn’t been proved, and could never be *proved* that it can’t be done.” It is quite another thing to use this so-called possibility in giving a description of persons. Should we, by virtue of this “possibility,” describe persons as being possibly something other than persons? Ridiculous! Talk is cheap, but when it comes to paying off on verbal claims, here, as elsewhere, we face the reality constraints, not of what is True, but of what we know and know how to do. *We* do not know how to describe persons adequately except in person descriptive terms, and it is a discouraging task even then,

and anyone who might claim otherwise will be called upon to *show* it, not merely say so. Conversely, if it were shown tomorrow that the concept of a person is reducible to other sorts of concepts, would the descriptive account presented below thereby be invalidated? Not at all. What would be different is that we would have a new way of saying the same thing, and perhaps we could say more in addition, though that is already extremely speculative. If we had not already achieved an acceptable descriptive account, it is difficult to see how any claim to successful reduction could be very compelling, for there would seem to be the same difficulty then in showing that there could not be a nonreducible person description as there is now in *showing* that there cannot be a reduction of person descriptions.

As in the case of giving reasons, the policy-following pragmatic account of the presentation of the person concept is subsumable under the content of what is said in that presentation, so that everything is still out in the open. That is a reader who understood the presentation, for example, by having read it as plain English rather than as philosophy, would be able to say that policy-following rather than truth-seeking was what had gone into the writing of it, and he would understand why, independently of the purely formal consideration that concepts cannot be true or false.

It should be mentioned, however, that more effort than may appear evident has been devoted to doing justice to the facts which incline us to talk about truth-seeking and assumption-making. Briefly, this is accomplished by distinguishing between knowing (or believing) on the one hand, and demonstrating, proving, or justifying truth-appraisals on the other hand. The former is an essential component of the concept of intentional action (rule-following) but not logically independent of it, whereas the latter *consists of* a series of standard-governed intentional actions and is thus a derivative concept. This formulation enables us to see why, if we failed to make the distinction and used “assumption” for both beliefs and truth appraisals, we might well be drawn into arguments about action or rule-following or an account of either, involving “assumptions.”

To see the distinction is to see why those arguments would be fruitless and why the “question” would have no answer, for the only formal possibility of an answer would be “sometimes, and in a way, yes, and sometimes, in a way, no,” and that is no answer.

This formulation is taken to be preferable to the current fashion in the empirical justification of truth-appraisals especially in the characterization of experimental science. A major difficulty with the Truth appraisals is that the procedure is one which leaves us with the dilemma of an infinite regress or an arbitrary stopping point. We say, in effect, that we will say that “P” is true if and only if it can be shown to be true. For any item of evidence, “shown” amounts to: We perform an operation, R, for confirming “P.” We shall also have to ask whether it is true that R *has* occurred. Thus “R has occurred” must itself be shown to be true. So there must be a second class of operations, Q, for showing that R has occurred. But then, when the question is raised about whether Q *has* occurred . . . *ad infinitum*. The regress is a vicious one for a truth-seeker, for without those *prior* assurances, what assurance could he possibly have about “P”? Classically, we stop the regress by saying “Well, you have to make *some* assumptions,” (which helps to explain the common reaction that any claim to making no assumptions at all must involve some sleight-of-hand). We are well-trained in the etiquette of such situations, and so *we stop—it* does not. For *it* calls for the further question “Is it *true* that we have to make *some* assumptions? And is it true that it is true etc., etc.”

Even if we add the usual assurance, “But of course, any such assumption can itself be tested, by making still other assumptions,” the conceptual situation is still that of the world resting on the shoulders of Atlas, who stands upon the back of the elephant, who is supported on the back of the tortoise swimming in the eternal sea. Is it really any comfort to think that the characters in this peculiar tableau might sometimes exchange roles? Nor will it do to pin our faith on the objectivity supposedly guaranteed by “agreement”—“If we (observers)

agree that operation R has occurred, why then “P” has been confirmed, and that’s the end of it.” For we shall have to ask, is our agreeing itself merely a matter to be settled by our agreeing that we agree (*ad infinitum*)? Or does it have to be *true* that we agree (*ad infinitum*)? Or do we have to *assume* that we agree (*ad infinitum*)?

That such a paradoxical state of affairs has not prevented scientists from keeping to their appointed rounds is not to say that it is a satisfactory state of affairs or that we ought not to do away with it if we can retain the substance without the paradox. One major difficulty with describing behavior as resulting from making assumptions is that that way of talking suggests that there are always real alternatives. (An assumption is the assumption that *this*, rather than *that*, is the case.) The significance of having made an assumption is that there is a truth test which (a) was not made, and (b) there was some point in making, and (c) there was a real possibility of making, not merely a logical possibility. If there was no point in making the test, there is no relevant point to be made by saying that *that* assumption was made. If, as may be the case for many of our beliefs, we don’t *know how* to make a truth test, then to talk about assumptions is not only pointless but also misleading. It misleads us into feeling dissatisfied at lacking something (the Truth suggested by the verification locution), and aspiring to its possession, when there is nothing lacking and nothing to be possessed. The pragmatic formulation permits us to see and to say that nothing of *that* sort is lacking and to give a different, unparadoxical account (rule-following and “knowing how”) of behavior generally, and especially in those cases where talking about assumptions is particularly embarrassing intellectually.

In general, then, the assembling of conceptual components for the purpose of giving a description of persons and human behavior has involved something that could be called borrowing from philosophers and others. What is borrowed, however, is concepts and reasons, not putative Truths. The components of the concept of a Person are not deduced from one another, nor are they derived from some hidden “major premise” with the

help of some equally hidden minor premise. There is no “foundation,” and so there is no “superstructure,” either. Instead, the account proceeds on the basis of being maximally conservative in regard to what is said, and when we have accounted for what is said in this way we do not need to advert to other grounds, e.g., “making assumptions,” in order to have an adequate account. What is given in the descriptive account is not an account of persons which requires that the world *first* be of a certain kind (assumptions) in order to make a *special* place for persons. Rather, it is an account of a world in which the concept of a person has a place which hasn’t been explained away *in advance*. (Such an account is taken to be more conservative than one in which persons *are* explained away in advance by *giving* explanatory priority to physical or physiological terminology.) Hence it is an account simultaneously of persons, actions, language, standards, learning, bodies, and concepts in which each depends upon and contributes to the others. It is therefore a “Copernican” or “relativistic” viewpoint as against the monolithic, absolutistic, “Unity” viewpoint. It is schematic partly because of the limitations of any single author and partly because it is designed to be used flexibly *by* persons and refined and elaborated through explicit use, as has begun to happen in those areas for which it was designed, i.e., clinical practice, assessment, psychological research and conceptual formulations contributing to that research. It is decidedly not to be done up in fine print and hung up on the wall to be admired. It is *formally* completely autonomous but it is conceptually and empirically linked to other sciences and disciplines. The formulation of the descriptive account is designed to keep the reduction *cum* Unity game honest, not to run it out of town.

There is one sense, illustrated briefly above, in which the present concept of a person appears to be more than merely garden variety assumption-free. Ordinarily, a self-contained descriptive system, e.g., a calculus, is said to be assumption-free. Yet a number of important things are left unsaid within the system even when (in the semantic terminology) a “model” for it is found. What is left unsaid (hence “assumed”) is why it should

be applicable sometimes and not at other times (it is the methodologist, talking about “models” and “theories,” and “language,” etc., who says that, and he says it within a different linguistic system). What is left unsaid is that it is persons who must use the descriptive system if it is to have the application that it has and make the difference it makes, and that is leaving a great deal unsaid and a great deal (the concept of a person) implicit, hence “assumed.” Likewise, the presentation of a philosophical argument depends for its significance and intelligibility upon a number of background circumstances, including circumstances which involve persons in an essential way (for example, the formulation of the “problem” which *philosophers* currently *accept*), none of which are anything but implicit and assumed.

The foregoing should not be mistaken for a “sociology of Knowledge” argument in which the validity of philosophical (or other) arguments and conclusions is challenged. It is a much simpler notion that is involved, namely that to describe the activities of philosophers, entomologists, neurologists, etc. is not a philosophical, entomological, or neurological *task*, hence there should be no presumption whatever that an adequate performance in this task depends in any way whatever on the philosophical (etc.) state of the art in respect to generating questions or answers. Such a task seems unquestionably a *psychological* task and it is because the performance in such a task is one of the activities which it is part of the task to describe (and part of an adequate description is to do justice to *that*) that reflexivity and recursiveness are both a central problem in psychological methodology and a necessary feature of an adequate substantive conceptualization of human behavior. This point may be put informally in the form of a criterion of adequacy for any general account of behavior:

An adequate account cannot merely *be* an account of a phenomenon under which the giving of the account can be subsumed—it must codify its reflexivity by *saying* or *showing* that that is so rather than leaving that feature implicit and dependent upon the application of the account *by persons*.

The formulation of the concept of a Person meets the requirement of bringing such features out into the open, and accomplishing this without the use of any auxiliary or *ad hoc* devices. For example, that the application of the descriptive system (the concept of a Person) is fully intelligible only if it is an application *by* a Person and *to* a Person (i.e., the paradigm case) is a fact which is itself made intelligible by the concept of a Person. Likewise the conditions of its application (guiding behavior) and the difference it makes (guiding behavior) are formalized, however schematically, in the concept itself. What is being done in applying the concept is also part of the concept. Etc. Etc. Etc.

Because of this completeness and coherence, it appears to be extremely difficult, if not impossible, to criticize the present formulation in any *general* way (as contrasted to the specific ways mentioned above). The most common attempts at criticism (making assumptions, taking sides, talking loosely, and dodging the “real issues”) are based on assumptions which are not only explicitly and nonarbitrarily rejected, but are actively challenged. It is one thing to be unhappy about the present formulation of the Person concept, since if accepted it would require giving up some preconceptions. To present a mere rejection as legitimate methodological or substantive *criticism*, however, would be to beg whatever question was at issue.

What this apparent invulnerability ought to suggest to philosophers is that if they insist on assimilating the Person concept formulation to the philosophical literature (and both the Person concept and the empirical results reported in Part Three emphasize the impossibility of preventing them from doing so, no matter what the “primary” subject matter was) they should begin by placing it in the category of descriptive metaphysics and only thereafter inquire about its relationship, if any, to philosophical analyses.

It is curious, but perhaps not too surprising in the light of our recent positivistic heritage, that philosophers should need to

be reminded that metaphysics is philosophy, too, but apparently that is necessary. And it is doubtful whether any metaphysician has ever questioned that the concept of human being was a metaphysical concept. Or that the concept of a physical object was also, or the concept of a concept (“universal”).

In respect to Strawson’s 1958 essay, the present concept of a Person appears to have a simple relation—it offers a schematic representation of the totality of P-predicates (or at least a good start in this) and a way of relating P-predicates to M-predicates that goes at least somewhat beyond saying that for persons, both are applicable.

However, the concept of a person is more closely related to Wittgenstein (1953) than to Strawson. To those whose understanding of the *Investigations* is a casual one, it may well appear paradoxical that an account which so clearly has *some* relationship to the Oxonian style of *analysis* should emerge as a *metaphysical* conceptualization. The best suggestion that can be made here is essentially Wittgenstein’s own: “Don’t say to yourself that it must be so, but *look*.” And also, reflect on such a statement as “What has to be accepted, the given, is—so one could say—forms of life.” Following such a suggestion might enable one to see that there is no inconsistency here. And of course, the question of consistency or inconsistency would only be bothersome to someone who had already made the prior error of supposing that the presentation of the Person concept involved the application of (or the prior philosophical validity of) certain philosophical arguments.

For a Truth-seeker, metaphysics is a matter of truth-claims so general that no acceptable pattern of justification for them exists (and so a frustrated Truth-seeker might then call them “meaningless”). For a scientist of this sort, it becomes important to justify the claim that science requires no metaphysical assumptions, and many believe that this has been accomplished (cf. Pap, 1953). For a rule-follower, metaphysics is a matter of *attitudes* so general that no pattern of justification exists. But there is no analogy here to the difficulty with unjustifiable truth

claims, and a philosophical analyst is not immune—his activities may well express such an attitude.

Given this conclusion, the demonstration that scientific activities do not *require* metaphysical assumptions is seen to be peculiarly empty—as empty as the *objective* procedure of settling a truth claim by flipping a coin because that was never the crucial issue. The person concept formulation makes it clear that *no* behavior *requires* any particular assumptions, and so it is a trivial derivation that particular sorts of behavior (scientific) require no special (metaphysical) assumptions. The important question is whether scientists and philosophers can avoid having metaphysical attitudes which significantly and systematically affect what they do *as scientists* or as philosophers. As a matter of fact, it appears that they cannot, though of course, there is no proof of this.

To say that the person concept, in a philosophical framework, is a metaphysical concept is by no means to say that it is *therefore* something new to psychology as it is currently conceived and practiced. The difference lies in *which* metaphysics, not in the presence rather than absence of metaphysics in our professional lives. That we have one, and that it is compounded equally of Semantic theory and material objects is entirely clear. In metaphysics, as in psychotherapy and psychological theory, it is the confrontation of an alternative which was there all along and was ignored that helps us to see our commitments (and those of others) *as* commitments, rather than as a simple reflection of the way the world is, and that may provide us with reasons for changing. Part of the function of the Person concept formulation is to encourage such change. Another part is to make such change intelligible rather than “merely emotional” (“context of discovery,” “purely psychological,” etc.), and a part of this is to make “emotional” intelligible. Finally, it is a function of the Person concept to encourage such change by making it intelligible rather than *merely* persuasive, for that is a paradigm case of treating Persons as Persons.

Appendix B

Normal and Abnormal Behavior

One of the most consistent bases on which philosophical theses with respect to the “rule-following model” have been criticized by psychologists (and thereafter echoed by positivistic philosophers) is that it cannot deal with abnormal behavior. Philosophers such as Ryle (1949) and Peters (1958) have suggested that “psychological” explanations (i.e., technical explanations provided by psychological theories) are not at all *necessary* in those cases where the rule-following model provides an explanation. The response of those psychologists has been to say that the same basic principles must apply in both normal and abnormal behavior (or perception, or learning, etc.) and that it is precisely the attempt to formulate those basic principles that distinguishes scientific psychological theory from common sense and from rational philosophy (or philosophic rationalizing, or muddle-headedness, etc).

There are at least two issues here. The first is the question of what it is that Ryle, Peters, and others have denied. The second is whether underlying-process theories in psychology do in fact have a broader explanatory range than the rule-following model. ♦

I. What is denied?

Ryle, Peters, et al. have sometimes been taken to deny that underlying-process accounts of normal behavior *are* psychological explanations; that thesis has then been subjected to refutation by counterexample. For example:

- (a) Freud explained normal learning by reference to the process of displacement;
- (b) Freud's account is an underlying-process account;
- (c) Freud's account *is* accepted as a psychological explanation;
- (d) hence any general assertion that underlying process theories do not provide explanations of normal behavior is simply wrong.

That such an argument is formally "correct" is perhaps its greatest merit. So, also, is the following argument:

- (a) P explained normal learning by reference to bumps on people's heads;
- (b) P's explanation is a nonsensical account of normal learning;
- (c) P's explanation *is* accepted as psychological explanation (though one that happens to be outdated);
- (d) hence any general assertion that nonsensical accounts do not provide explanations of normal behavior is simply wrong.

Given the historical context of the issue, it is not clear whether such a "refutation" is better described as disingenuousness or a simple failure to see that the issue is the pragmatic one of success in meeting the standards for being an explanation and not the semantic one of whether "explanation" is being used in *one* of its possible meanings.

A Rylean thesis may be stated as follows: Underlying-process "explanations" do not normally function as explanations of normal behavior because they do not provide answers to questions which we normally have about human behavior and

a fortiori, do not provide answers to questions which we *must* ask or are implicitly asking. Instead, those supposedly “basic” questions are like the question “What’s trumps?” in that they are questions which can be asked intelligibly only under certain circumstances by certain people engaged in *special* activities. It follows that the answers to these questions do not provide an account of “what *really* happens”—they do not provide a more authoritative account of human behavior.

Thus, in relation to the thesis that underlying-process theories, because they get at “basic principles,” give us a general, authoritative account of human behavior, including normal behavior, the Rylean (et al.) position is not a counter-thesis with an equal-and-opposite claim to truth, but rather a methodological appraisal of the status of the underlying-process thesis.

II. The Relative Scope of Rule-following and Underlying Process

There is a residual argument to the effect that even if an underlying-process theory were not *needed* in regard to normal behavior, it is not literally inapplicable in the normal case, hence its applicability in the abnormal cases *as well* gives it a greater generality, hence a superiority over the rule-following model. For this thesis there is a certain amount of historical justification, stemming from the fact that the “rule-following” model, though it is often referred to, has not previously been presented in explicit form.

In general, prior references to the rule-following model have been largely restricted to what is encompassed in the present account by the paradigm of intentional action. Discussions of “emotion,” insofar as they have not involved the intentional action type of emotion, have tended to *contrast* emotions with rule-following, thus lending support to the notion that there is more to behavior than is dreamed of in rule-following.

In this respect, the present descriptive account may, once again, be regarded as an effort to “do justice to the facts of the matter” in regard to a controversial “issue.” The central place of the paradigm of intentional action codifies a variety of considerations adduced with respect to rule-following by Ryle, Peters, et al. The further elaboration of PII, the paradigm for the concept of a Person reflects a recognition that there is more to Persons than to intentional action. The descriptive concepts of trait, interest, attitude, ability, state, etc. provide a systematic conceptual and descriptive repertoire which appears to include all of the formal resources of underlying process theories and to be more comprehensive in this respect than any single such theory (for example, psychoanalytic theory appears to have no adequate *systematic* place for individual differences in ability or capacity, for it appears that only those differences which are accounted for by differences in personal learning history are manageable within the theory). In particular, the concept of pathology itself is shown to be an integral part of the concept of a Person, and the descriptive account makes intelligible what must surely rank as one of the more paradoxical features of present explanations of psychopathology, i.e., the existence of a variety of lists of “*basic* human needs,” each list having a status akin to “revealed truth” and being used by some psychologists to account for psychopathology. In effect, then, it is not that the rule-following model is an inadequate framework for understanding *both* normal and abnormal behavior, but rather that to date it has been so far from being adequately formulated that its scope has been substantially misapprehended.

But this is not, in any *simple* way, to uphold the psychologists’ thesis that the rule-following model, as it has been presented in the literature, is inadequate. For the basic place of intentional action even in the context of the second paradigm shows that intentional action provides an explanation of human behavior in a way that the “personality variables” *cannot* duplicate, and that *our* descriptions of the latter sort would not be possible in the absence of intentional action descriptions. The fact that the personality variables cannot do the explanatory job

performed by intentional action descriptions and the fact that a given action, even when classified as to type, is *selectively* assigned to the PII concept types (including “state” and “pathology” concepts) are consistent with the thesis that (a) underlying-process “determiners” of something called “behavior” do not provide the kind of answer we normally want and require in asking “why did he do that?” and (b) do not give an answer to any question that we must ask, or that we are implicitly asking, or that is more “basic” than the normal “Why did he do that?”

It is not clear that any issue remains here. If there is, that must await further clarification, and it may be hoped that the present formulation has contributed toward that.

Once again: the *presentation* of something which qualifies as *an* actual resolution despite possible deficiencies, in contrast to the alternative procedure—of merely talking about what issue the issue is or what a resolution would *have* to be like—illustrates the difference between a Descriptive Psychology concerned with people and an Analytic Philosophy concerned with “issues,” “mind,” and concepts.

Appendix C

Psychological Theory, Persons, and Linguistic Theory

For those who have some knowledge of linguistic theory, it may be of some interest to note the strong analogy between the use of the paradigm case formulation for the Person concept and the characteristic procedures of a (transformationalist) linguist in presenting a grammar or a language.

If we invent a “B” (human behavior) to correspond to the linguist’s “S” (sentence), then the next step would be to replace “B” by “W—K—KH—OA” (intentional action) in the same way that “S” is replaced by “NP—VP” (noun phrase—verb phrase). And just as substitutions for NP or VP provide alternative kinds of noun phrases and/or verb phrases, hence alternative kinds of sentence, substitutions for W or K or KH or OA or combinations thereof would identify alternative types of intentional action (the analysis of “He did it because he was afraid,” in Part One illustrates this). Further, the choice of an individual difference characterization of an intentional action of a given type (recall that the latter is the central element in the individual difference system, PII) would correspond to the application of a particular linguistic transformation to an articulated NP—VP exemplar of the kind referred to by Bever, Fodor, and Weksel (1965) as a basic underlying structure. The order in which transformations were applied, which is a significant aspect of the linguistic theory, would correspond to the indirect descriptions, or “mutual assimilation” of PII concept types, described in Part Two.

The result in the first case would be a fully (structurally) articulated, hence grammatically intelligible, sentence type, and in the second case it would be a fully intelligible piece of human behavior. Finally, a part-description corresponds to what would doubtless be called a “deletion transformation.”

What is left out in these analogies is, among other things, the crucial reflexive character of the Person concept which is a consequence of the pragmatic structure of the latter. Because of this, the presentation can be adequately comprehended as a set of actions by an author, not as a set of statements. But we may say that the “syntactic” aspect of that performance and its product exhibits much the same methodological characteristics as a transformational grammar which merely *is* a syntax. That this should be so is not surprising, for there is continuity here and not merely analogy. Since saying something by uttering words, phrases and sentences is one of the things that people do, i.e., is a class of intentional actions, “S” would, in the most common case, appear in some of the substitutions for “KH” (Know How). That is, in those actions in which something is said, a statement of those competencies of the person which were involved in that action would include reference to the particular, and of knowing how to utter a sentence which is “on target” in that it “says” what the person *says*. (Compare: In those actions in which something is kicked, one of the relevant competencies is that of knowing how to kick a selected target rather than just knowing how to kick, and in contrast to being unable to kick at all.)

It is because there are human practices that are correctly described as kicking something that certain performances may, via a part-description, be correctly characterized as *merely* kicking. (If we had no such practices what we now call “merely kicking” would not then be what it is now, and we would not have the reasons we do, perhaps no reasons or ability at all, to distinguish anything as being *that* sort of performance.) Likewise, it is because there are human practices of saying something that it is passible, via a part-description, to characterize certain performances as *merely* uttering words or merely uttering a sentence.

What characterizes merely kicking is that the performance is one which would have qualified as kicking something had there been something there to be kicked. Likewise, what characterizes the mere uttering of a sentence is that the performance is one which would have qualified as saying something *if* there had been something of the sort to be said there (cf. Austin's and others' examples of "misplaced" performatives).

Performances describable as "kicking" could be systematically articulated and redescribed by mapping them into a geometric frame of reference. We could then distinguish one kick from another, identify kicks that had never been accomplished, or else recognize "the same" kick, under the geometric description. Our level of competence at applying a geometric descriptive system in this fashion would be an empirical matter and would change in the course of our social history (it might increase, decrease, fluctuate, or vanish) However, whereas we should want to ask how a person learned to kick something, we should hardly want to ask what it was he learned that *enabled him* to satisfy particular geometric descriptions.

We should hardly want to ask that because we should not know *what* we were asking or whether we were asking anything in asking that. Unless we had also invented a peculiar new form of answer (perhaps, talking about an underlying process) which might make an honest question out of that dubious locution. For the only answer that makes sense was already given. That is, what he learned was how to kick something, and having an answer (if there is such a thing) to our new question (if that is what it is) is not a *condition* for understanding or explaining *that*. If the new question-and-answer activity gives greater significance to the fact of having learned to kick something, that will be an *unusual* success. Failures in this regard are commonly called "pure science."

To be sure, it might be of some interest to plot empirically the *course* of a person's learning to kick something, using a geometric frame of reference for plotting the data, but it would be highly misleading then to turn around and suggest that his having learned how to kick something is accounted for by his

having learned what we plotted in the geometric framework. And if the empirical information could be summarized in a general formula (a “mathematical model”), it would make no more sense then to say that his having learned to kick something is accounted for by his having learned whatever it is that we say the formula signifies.

Psychologists commonly take the kind of position that is illustrated by the thesis that there *has* to be an answer to the “question” of what the kicker learned that *enabled him* to satisfy those geometric descriptions, and that we must have such an answer in order to understand or explain the phenomenon. (See, for example, Miller’s (1955) concurrence with linguists’ demands that psychologists provide “performance models” that account for the language user’s ability to use the language he uses.)

But this appears to be a simple logical error the nature of which is well codified in the literature of logical theory as the problem of the “substitutability of truth.” In that literature there is no serious challenge to the conclusion that if (a) I believe that that object is a lion; and (b) that lion is in fact harmless, it does not follow that (c) I believe that that object is harmless. This feature of not preserving truth under substitution is a common characteristic of “mental” or “intensional” phenomena. For example, the human activities we describe by reference to “believes,” “knows,” “intends,” or “wants” have this characteristic. The logical error lies in not recognizing that “knows how to do X,” and “learned to do X,” also have this feature. Note that in none of the following does (c) follow from (a) and (b):

- (1) a. I learned that that object is a lion.
 b. That lion is in fact harmless.
 c. I learned that that object is harmless.
- (2) a. I know how to treat that object as a lion.
 b. That object is in fact harmless.
 c. I know how to treat that object as being harmless.

- (3) a. I know how to kick that ball.
- b. To kick that ball is to satisfy the geometric description “z.”
- c. I know how to satisfy the geometric description “z.”
- (4) a. I learned how to kick a ball.
- b. To kick a ball is to satisfy the geometric description “z.”
- c. I learned how to satisfy the geometric description “z.”

The alternative to supposing that psychologists who opted for underlying processes have made a crude logical blunder is to say that in fact they have tried to *deny* the failure of “mental” phenomena to preserve truth under substitution, without knowing that this is what they have been doing. This is to say that they have tried to treat persons as nonpersons, and more specifically, as material objects, since it is in statements dealing with material objects that we find truth preserved under substitution. But that is part of the point of saying (cf. Appendix A) that current psychology clearly does have a metaphysics and that it is compounded equally of affection for material objects and the semantic theory of Truth.

In a similar vein, we could say that performances which are describable as “saying something” could be systematically articulated and redescribed by mapping them into a set theoretical frame of reference. We could then distinguish one case of “saying” from another, identify cases of “saying” that have never been achieved, or else recognize “the same” case, under a grammatical description, which might be as elaborate and cumbersome as we please. However, whereas we should want to ask how a person learned to say something, we should hardly want to ask what it was he learned that *enabled* him to satisfy that grammatical description. For if there is any answer to that, then, considering the derivation of the grammar from linguistic performances, it is already given—he learned to say something. To be sure, it might be of some interest to use the grammatical

descriptive system as a framework within which to plot empirically the course of his learning how to say things. That would be like plotting the course of a person's Rorschach responses over a period of years, using the set of age norms for content, movement, form level etc. as our frame of reference. But it would be a highly misleading procedure then to turn around and suggest that his having learned to satisfy certain grammatical descriptions is what *enables* him to say things or is what *accounts for* his being able to say things. If the empirical information could be summarized in a general formula (a grammatical rule or a set of them) we might summarize our findings by talking about what rules the person learned, but there would then be no more point than previously in asking what it was the person learned that *enabled* him to follow that rule or that *brought it to pass* that he followed that rule.

The present formulation has some significance for the psychological study of language (cf. Part Five, discussion of psycholinguistics). It has been suggested that the task of psychology in the study of language is to provide an account of the mechanisms whereby linguistic structures are acquired and those whereby they are manifested in linguistic performances. Indeed, it has even been strongly recommended (Bever, Fodor, and Weksel, 1965) that accounting for the acquisition of syntactic structures be made a *prima facie* standard of adequacy for theories of learning in psychology. The preceding discussion should provide an indication of the way in which the concept of an underlying structure is the methodological Siamese twin of the concept of an underlying process which was discussed in Part Four.

A further elucidation of the methodological confusion expressed by any general *demand* for underlying structures and underlying processes requires the prior clarification of a set of concepts which include "skill," "ability," "being able to," "achievement," and "performance," and their systematic interrelationships. Such an analysis is undertaken in a forthcoming paper, but one result may be briefly stated:

The structural theory of “S” is an *achievement* analysis. The question of how something got done or what a person learned that enabled him to do it is a question about a performance and calls for a *performance* analysis.

Performance analyses, based on the exercise of skills, and achievement analyses, based on the manifestation of abilities are two radically different methods for identifying or classifying ranges (spectra) of achievements. Only in exceptional cases would the two methods produce identical classifications. And there is no *formal* basis for supposing that *any* set of achievements identified by reference to skills can be mapped into a set identified by reference to abilities by means of any rational function. What relationships exist between such sets is something to be established empirically (cf. the “ $\sqrt{2}$ ” discussion in Part Four), and there being any recognizable relationship is not a criterion for the adequacy of anything—it is merely our potential good fortune if we find any.

The similarity between transformational grammar and the presentation of the Person concept may help to clarify further the reason why it is the philosophical analyses of “mental” concepts which “follow from” the Person concept, rather than the reverse. (See Appendix A) For in contrast to the positivistic semantic model of science as *hypothetical* and *deductive* (we make hypotheses about the True state of affairs and test the deduced consequences), transformational grammar may be clearly seen (perhaps less so by its proponents than by those who have had some reservations about it) as being *descriptive* in spirit and *illustrative* in its procedures. In much the same way, the presentation of the Person concept is descriptive in spirit and the procedure is illustrative, so the “philosophical” arguments and analyses merely illustrate in a restricted way, what is “already” given by the Person concept in a *general* way. Thus, the relation between the two may be clarified by saying that those arguments and analyses are no more a “foundation” upon which the Person concept rests than “NP” and “VP” are the foundation upon which “S” rests.

The similarity between the Person concept formulation and a transformational grammar is not surprising because there is a continuity between the two. That this should be so is itself not surprising, for transformational grammars have explicitly been framed as theories of linguistic competence as contrasted with theories of linguistic performance. It is hardly surprising, therefore, that such theories should find a formal niche within a more fundamental conceptualization in which “skill” and “ability” are structural components. (Because of the pragmatic reflexive character of the Person concept, it is no contradiction to note an additional similarity: the Person concept formulation is also a theory of competence, i.e., competence in the use of the Person concept.)

But it should be noted, too, that although the most common role of “S” is as a substitution under “KH” (know how), it may be substituted under “OA” (overt attempt) as well, and, for grammatically knowledgeable persons, under “K” (know) and “W” (want). For a grammatically sophisticated person would do more than merely know how to say things by speaking correctly in English (or etc). He might, for example, know the difference between a noun and a verb, and if he did, then he might have reason to (might want to) “choose the right verb” give the nominal counterpart of a verb, etc. In general, he might have reasons to utter certain sentences or phrases, as contrasted with having reasons for saying certain things. Because the paradigm of intentional action is the paradigm of a performance, it provides the general descriptive account of *linguistic* performances.

Bever et al. (1955) make the following pronouncement with respect to linguistic performances:

Progress in linguistic analysis of natural language has depended on the careful separation of the theory of the language (“*langue*” or “competence”) from the theory of the use of language (“*parole*” or “performance”). In this way the linguist has insulated himself from the fact that the variables determining the character of speech behavior reflect features other than the formal structure

of the spoken language. For instance, the fact that some sentences are difficult to say, to remember, or to understand, is obvious. Evidently such facts are the consequence of interactions between linguistic variables and variables of memory, perception, motor integration, etc. To fail to so represent these facts would render impossible the representation of *either* the systematic character of language *or* the systematic character of speech behavior.

Several comments are apropos here.

First, the statement would seem to illustrate the garden variety of reification discussed previously. What began as a redescription of some aspect of the phenomenon (people saying things) has somehow acquired the status of a causal influence which “interacts” with other influences to *produce* the phenomenon.

Second, the reference to the use of language illustrates the misconception discussed previously (Introduction) in relation to the use of concepts. As indicated previously, the use of a concept is quite unlike the use of a tool in essential respects. The use of language is equally so, and for much the same reasons, though that is less apparent.

Third, so long as the use of language *is* seen as essentially the same as the use of a tool, to talk this way and also to talk about “linguistic variables” “determining the character of the speech performance” is to use two disparate descriptive systems (action and mechanism) commingled in a confusing and misleading way.

Not surprisingly, Bever et al. attempt to justify their stand by appealing to an analogy with physics:

It is not at all surprising that the analysis of speech behavior should proceed from two empirical and theoretical sources. Indeed, distinguishing among the different kinds of data that constitute superficially homogeneous phenomena is absolutely universal in scientific explanations; it occurs wherever considerations of simplicity

and explanatory power require that the observations be presented as interaction effects. Consider for example, the analysis of a block sliding down an inclined plane. There are two kinds of variables that interact to determine the block's behavior—first, the forces acting downward on the body and determining the acceleration for an ideal system; second, the reactive forces (e.g., friction) due to the character of the particular body and plane under study. The observed behavior is susceptible of systematic explanation only on the view that it is the product of interaction between these distinct systems.

Even a sympathetic reader might question the value of this particular analogy. For example, the distinction between the downward forces and the reactive forces is not a distinction between descriptive systems which are distinct in the way that a grammar and a theory of learning are distinct. What the example presents is a componential analysis of forces within a *single* descriptive system. The calculation of the reactive force would depend on the magnitude of the downward forces (more accurately, the downward resultant force), but it will also depend on the horizontal resultant forces, and on the resultant forces in the direction orthogonal to the other two. The physicist might well regard any classification of forces on the basis of their direction as entirely superficial and removed from the laws of force and energy that he uses. If his theory of force and energy had to be adjusted in the light of observations he made it is hardly plausible and certainly not necessary, that his adjustments would follow this particular typology. The "other variables" mentioned by Bever et al. (memory, perception, motor integration) are all species of competence. Thus, a general theory of competence which would subsume linguistic, mnemonic, perceptual, and motor abilities is what would be required in order to breathe life into the analogy with physics. But even the mention of such a general system may serve to make it quite clear that then there would be nothing sacred about the

linguistic portion and that attempts to account for observed phenomena might as easily lead to adjustments in the linguistic portion as elsewhere.

This is in sharp contrast to the position, taken by Bever et al., that what belongs to Linguistics belongs to the ages, whereas what belongs to Learning is negotiable and expendable. The analogy may become somewhat embarrassing for the extreme linguistic position if it serves to remind us of certain relevant, though commonly overlooked considerations.

There is no question that the notion of frictional forces has been a fruitful one for physics. Bever et al. mention it in a way that could not but suggest that the distinction between frictional and other forces is good because it provides a superior description. But even an informal acquaintance with the history of physical science should provide compelling reasons for doubting this.

The value of the distinction has lain not nearly so much in its merely descriptive use as in its behavior-guiding function. The distinction has been highly important in permitting physicists and engineers to *construct* instruments and other artifacts which are dependable, manageable by persons, and perform novel functions which serve novel further ends. We have adapted our practices to this technology, and assimilated it to our practices too, in such degree that it is easy to overlook the extent to which the “physical” world, even that portion which does not consist of artifacts, is a world of our own devising.

Thus, the *methodological* force of saying that reference to frictional forces provides *descriptive* simplicity and generality is not the simple guarantee of value that Bever et al. apparently take it to be. “Parsimony” is a critic’s terminology, applied after the fact. It is not the name of a mechanism which selects methodologically superior statements or sets of statements. “Parsimonious,” when applied to the concept of frictional force, is a summary appraisal of our success in using the concept. It is not a piece of evidence in favor of notational simplicity as such (cf. the discussion of parsimony in Part Four).

In contrast to the situation in physics, there is apparently nothing dramatic in the way of linguistic technology or successful follow-through which is attributable to transformational grammars. Perhaps some day there will be (e.g., methods of teaching grammar, linguistic data processing), and then it will be different. For the present, however, the characteristics of transformational grammar which Bever et al. present in such ringing phrases as “theoretical simplicity and power,” “fruitfulness,” “fundamental mechanisms,” and “[deeper] explanatory power” would seem to be subsumable, with at most minor loss, under the category of notational simplicity.

The consequences of applying the concept of “parsimony” in a mechanical way are illustrated by the form in which Bever et al. reject an alternative theory:

... this analysis is blocked for English on empirical grounds. We have shown briefly that at the very least such a solution would involve an unnecessarily complex system since there is a simpler analysis which accounts for the relevant facts. In this way we show [it] ... to be logically possible but empirically unacceptable. ... The difficulty with playing fast and loose with simplicity constraints is that, once having started, it is hard to find a way to stop.

One of the difficulties with pretending that “simplicity” has a simple descriptive use is that it makes it all too easy to make the transformation from “some one else’s theory” to “unparsimonious” to “empirically unacceptable.” This is a pragmatic difficulty, of course, and so its methodological force (and its heuristic value, too) may be largely lost on practitioners of science who feel constrained to stay within the traditional semantic framework.

If there were really no reason for rejecting a more complex account in favor of a less complex account other than its notational complexity, and if it were really so difficult to “find a way to stop,” then why should we not admit that there is no important difference and no important issue associated with

complexity? If it were merely a question of needing *some* basis for choosing among alternative accounts (and we may suspect that *that* “necessity” has been grossly overstated), then it would seem to be a rational procedure to decide by flipping a coin or using a random selection device, for in the long run we would have a systematic and genuinely empirical check on the value of particular rules of thumb such as the familiar one which we apply *a priori* and which favors notational simplicity.

A complex formulation which proves to be more useful than a notationally simpler alternative *becomes* the simpler of the two, and one which is more complex or less useful within a narrow context (e.g., linguistic competence) is not unlikely to *become* simpler or more useful in a wider context (e.g., linguistic performance). This is why an appraisal of “parsimony” is an appraisal of *current* success with respect to selected criteria; its prognostic value for success in the future and in other contexts may always be questioned, and in that majority of cases in which we can give no answer, our subsequent actions are the course that *we* have chosen.

In their polemics with other linguists it has typically been pointed out by transformational linguists that to require of a scientific theory that it provide a “discovery procedure” (i.e., a technique for getting the right answers) would wipe out science, since no theory can or *could* possibly provide that (any promise of that sort would violate our empirical conscience). Thus, they make a strong distinction between having a discovery procedure and having a criterion for evaluating the empirical adequacy or “goodness of fit” of a theory.

The theorist’s having such a criterion is what makes his theory “empirically testable.” Observably, “empirical” is the magic word here. But “empirical” is not a magic word—it guarantees nothing and it promises nothing. What is missing is the notion that the criteria for evaluating empirical adequacy ought to be rational criteria. (No doubt there will be some readers who at this point will still believe that it constitutes a penetrating criticism of the foregoing to say “But you haven’t said what it takes to be a rational criterion!” and fail to see that to say

this is to beg the question of what it takes to be a rational criterion. Once we begin to play fast and loose with *that* criterion, it is indeed hard to find a way to stop, and we have gone a substantial distance along that road when we try to present a theory about some subject matter (e.g., “behavior” or “language”) for which we have no descriptive account.

What else could serve as a criterion for discovery rather than invention? Flipping a coin would offer objectivity, empiricism, and simplicity *par excellence* as a way of evaluating the empirical adequacy of a set of experimental results. That is too obvious, however, and we do not do it that way. But an *arbitrary* rule could do just as well. And a *trivial* rule could come as close as possible to guaranteeing in advance that a theory will have “empirical consequences” or that it will be “empirically adequate.” (Something to be kept in mind, too, in regard to “mathematical models” in psychology, particularly when the systematic application of a borrowed computational technology is presented as the “*empirical*” “*test*” of a “theory,” or as “basic science,” or whatever.)

Paradoxically, the overconcern with being “empirical” and “parsimonious” in the semantic sense leads to procedures which are perhaps no more than a hair’s breadth removed from an *irrational* discovery procedure. It has been well known at least since Darwin that given an appropriate selection mechanism the correlative problem of generation or production may be *entirely* dispensable, for the selection may be exercised on variation which is essentially random. We find this also in the more restricted circumstances in which we “shape behavior” and thereby effectively “produce” behavior by manipulating a selective mechanism.

If we think of notational simplicity as a selective mechanism and think of it as being applied constantly and without deliberation in carrying on scientific practice, the net result envisioned would be something like the shaping of behavior with an idiot’s hand at the controls. *That* feature of our scientific behavior has not yet been presented in a rational light. It is a feature which provides us with something that could come as

close as we might wish to being a discovery procedure, for what we “discover” and *accept* will be largely a function of that procedure. All of which argues for keeping “parsimonious” as a summary term for current appraisal and abandoning it as a guide for our scientific activities, at least in the extreme or stereotyped ways which we may see Bever et al. as illustrating.

Thus, we do not need to disagree with Bever et al. that transformation grammars are more “parsimonious” than certain alternatives with respect to selected aspects of verbal performances in order to see how *very* little is implied by that or in order to see the methodological dubiety of trying to justify the rejection of existing alternatives on this ground at the present time. But neither do we have to agree. We do not have to present a grammatical criticism of such grammatical theorizing in order to reject the notion that it should have any more than a suggestive significance for a psychologist interested in the psychological study of linguistic behavior.

It would not be surprising if, following the example of Bever et al., logicians began to demand that learning theories be considered inadequate if they could not supply an account of the psychological “mechanisms” underlying the acquisition and manifestation of the “underlying structures” of the propositional calculus. That, too, is a framework onto which our knowing how to say something might be projected. Subsequently, we might expect that physicists would demand of learning theory that it provide an account of the psychological mechanisms underlying children’s “intuitive” mastery of the systems of differential equations which describe their locomotor competence. At that point, perhaps even those psychologists who think wistfully of Unity as the road to scientific respectability would have begun to appreciate the necessity of accepting the responsibility of practicing psychology as a *formally* autonomous science in order to be *empirically* open to the findings of other disciplines.

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