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Reflections upon the scientific study of personality.

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REFLECTIONS UPON THE SCIENTIFIC STUDY
OF PERSONALITY

A Thesis Presented

By

ROD KESSLER

Submitted to the Graduate School of the University
of Massachusetts in partial fulfillment of the
requirements for the degree of

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Psychology

REFLECTIONS UPON THE SCIENTIFIC STUDY
OF PERSONALITY

A Thesis Presented

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C H A P T E R I

PERSONALITY PSYCHOLOGY TODAY: THE STATE OF THE FIELD

Not long ago when I sat as a graduate students' representative to the psychology department's executive committee I was impressed by a faculty member's remark about upgrading the requirements of our undergraduate majors. More "rigor" was in order to produce first-rate candidates for graduate study and professional careers, he made clear. "We should require more courses in other fields related to psychology; they should take more physics, more chemistry, more calculus." In the discussion that followed no one mentioned more sociology, philosophy, or literature courses. No vote was taken, but I left the meeting vaguely dissatisfied.

The suspicion that my personal conception of psychology was not universally upheld in the field was heightened not long afterwards over lunch with a researcher whose specialty involved implanting brain lesions in mice. "Oh, come on," she said with impatience, "nobody does psychology anymore. Psychology is dead." Her point, if I understood her, was to differentiate psychology from behavioral science. Concerned as it was with "unscientific" notions about mental life, psychology was obsolete; anything worth salvaging could be incorporated into the broadening field of neuroscience.

What psychologists say over lunch is not always implemented back at the office, so it is worth noting that at the department where she now teaches--and among her classes is the introductory course--she recently recommended that no one be hired to replace the single personality-clinical psychologist who had left. He had taught the theories of personality course.

These examples, which can be multiplied, suggest at least superficially that contemporary American academic psychology has excluded from its rigorous examination most of what the non-specialist imagines psychologists know and think about. Instructors of introductory psychology courses are familiar with the gap between students' anticipations, their hopes of understanding themselves and resolving, finally, the puzzle of human nature, and the contents of academic psychology, replete with normal curves, Nodes of Ranvier, Lashley jumping stands, fixed-ratio reinforcement schedules, and so on. It is easy, too, to understand the blank reactions of lay readers who devour with interest books like Passages, Denial of Death, The Politics of Experience, and Becoming Partners when confronted with the table of contents of such professional publications as The Journal of Personality and Social Psychology. Consider the first paragraph from a paper with an uncommonly promising title, "Toward a Causal Model of Love,"

by Tesser and Paulhus (1976):

Some recent work demonstrates that thought about some person, thing, or idea increases the likelihood that one's attitude will polarize (e.g., Sadler & Tesser, 1973; Tesser & Conlee, 1975). Perhaps this effect occurs because thought alters one's salient cognitions, making them more consistent with the initial attitude. Since affect is related to cognitions, thought polarizes attitudes. Given that persons like those they date, thinking will lead to the generation of favorable attributes (Tesser & Cowan, in press). Thus, the more thought about a previous date, the greater the resulting love for that date. Reciprocally, we assume that extremity of feeling about some object increases the likelihood of its being thought about. In the present context, then, thought about a date should be a positive function of one's love for that date.

p. 1095

But if laymen draw a blank at such prose, academic psychologists are rarely chagrined, needing only to remind themselves of the lay public's naivete about scientific psychology. And there is some reason to this view, for psychology is not obligated by the preconceptions or needs of non-specialists.

Among personality psychologists, though, there is another, additional reaction, and that is to lay the chief ills upon the shoulders of research psychologists in the other specialties, especially the behaviorists. It is they whose reductionism trivializes and dehumanizes psychology. Personality psychologists--and I have counted myself one for seven years--probably derive a sense of professional solidarity and group identification simply out of our shared sense of being unlike them, those behaviorists. Because we

utilize a class of variables eschewed by our rat-running colleagues, our "inner," personality variables, we see ourselves somewhat as champions of freedom and dignity.

But the crisis of relevance in psychology cannot complacently be laid on the behaviorist doorstep and simply forgotten, while we pursue personality research, for personality psychology itself is open to claims of triviality, irrelevance--of turning its back upon its proper subject matter. Students in introductory courses in personality are not immune from feeling, several weeks into the course, that they have mistaken their room assignments and have accidentally sat in on some other course. I have encountered students who, having survived the introductory psychology course with some interest intact and expecting to get to the heart of the matter with personality studies, react with the same blank shock to the research work published in the Journal of Personality. What do such neophytes, interested in what it means to be a person, make of such titles as these, the lead articles from the four issues of that journal's forty-fourth volume:

The Verbal Communication of Inconsistency
Between Attitudes Held and Attitudes
Expressed (Wagner and Pease, 1976)

Measurement and Generality of Response
Dispositions in Person Perception
(Kaplan, 1976)

Status Inconsistency, Aggressive Attitude,
and Helping Behavior (Midlarsky and Mid-
larsky, 1976)

Expressive Control and the Leakage of
Dispositional Introversion-Extraversion
During Role-Playing Teaching (Lippa, 1976).

Speaking of the ills of his own field, the eminent sociologist Peter Berger (1963) writes:

It remains true...that a goodly part of the sociological enterprise in this country continues to consist of little studies of obscure fragments of social life, irrelevant to any broader theoretical concern. One glance at the table of contents of the major sociological journals or at the list of papers read at sociological conventions will confirm this statement.

p. 9

Might not the same comment be made concerning personality psychology today?

But if contemporary academic personality psychology is undergoing a crisis of relevance, the fact will not successfully be proven to today's researchers simply on the basis of laymen's and students' shocked reactions. Yet indications exist within the field itself.

When I began graduate study I was invited to accompany a professor of personality to address an assembly of undergraduates interested in psychology careers. When asked to explain exactly what personality psychology was about, he replied in terms which at the time surprised me. Personality, he began, contains so many different topics that it is difficult to see how to define it at all. It is really a miscellany, he continued; if you do not fit anywhere else in psychology, you probably belong in personality.

Sitting there listening, I thought he had misspoken. Anyone with even a sketchy knowledge of the field should know that personality psychology was that branch of psychology which took the person as a whole as its major unit of analysis, whose emphases included understanding normal functioning as well as development, and whose overall scientific goal was an adequate conceptualization of personhood. Surely any of the classical personality psychologists, say, Allport or Murray, would have replied in such terms as these.

Three years have now passed, and I have revised my view considerably on the basis of my greater familiarity with the field. The professor was correct in describing it as a hodge podge of unrelated researches, unalloyed by much of an integrating theoretical umbrella: a miscellany, if you will. The transformation from the classical conception of the field into the contemporary description suggests something drastic has taken place, that something certainly has gone awry.

My experiences as a teaching assistant to professors giving personality courses at three colleges have contributed to my own sense of crisis. The contrast between two of these courses, which ran during the same semester, struck me forcibly. In one the professor began his lectures by stating that while personality courses in the past have

typically presented a series of personality theories, his own course would not discuss them "because research has proven them to be inaccurate representations of human beings." The other course, taught by a practicing clinical psychologist, not a personality psychologist, devoted most of its lectures to the presentation of a series of personality theories, dynamic, phenomenological, trait-oriented, and so on, the professor drawing frequently upon his clinical experiences to illustrate how well some of the theories promote an understanding of persons.

That a professor of personality can without qualms delete significant discussion of personality theorists from his lectures in 1977 is really not so extraordinary, given other trends within the field. While some recent texts such as the second edition of Hall and Lindzey's Theories of Personality (1970) are almost entirely devoted to the discussion of personality theories, today's trend is toward greater emphasis upon research and techniques. Mischel's (1971) text Introduction to Personality serves as one example.

The prestigious Journal of Personality published during the year 1976 forty articles, containing 805 references. Of these 805, only 35 are to what liberally might be considered theoretical sources. Of these 35 references, 77%--twenty-seven citations--are to three theorists alone: Rotter (13), Kohlberg (9), and Piaget (5). Freud is cited in one paper only, and no references are made at all to

the theoretical writings of Allport, Erikson, Kelly, Maslow, or Murray, to list some of the most obvious omissions.

No one will argue with the assertion that the role of general theory in contemporary academic personality psychology has dwindled, but plenty of room for disagreement exists over the significance of the loss. The case can be made that the loss is symptomatic of the field's growing irrelevance to any understanding of personhood, in the sense of some human nature. The seemingly outmoded theories have tended to depict the person as an indivisible unit of analysis, as multifaceted and complex, yet organized.¹ The loss of theory has gone hand in hand with the increase in research employing one or a handful of personality variables, research in personality fragments which add up neither to a theory of personality nor to persons themselves.

Do general theories of personality contribute to the understanding of persons? A partial answer is suggested by the experience of graduate students in a seminar involving its participants in interviews with undergraduate subjects. The idea behind this seminar, of allowing personality graduate students to conduct research consisting chiefly in confronting another human being as such, met with some

¹I do not mean to say that all personality theories are inherently holistic or non-reductionistic; however reductionistic some may be (e.g., Cattell's theory), they focus on the overall organization or personality and provide a sense of personhood.

initial resistance on the part of the personality area, and seminar members were required to include a formidable battery of psychometric devices to insure objectivity and scientific rigor. For many of the participants, this experience was their first in interviewing.

What happened? While all of the graduate students were aware of the mechanical ease of doing questionnaire or simple experimental research, and of producing the kinds of studies that fill current journals, the unusual experience of being confronted with human beings in their own terms forced us back time after time to the very terminologies that "research has proven...to be inaccurate representations of human beings." The frame of mind embodied by much current research, oriented as it is toward the quantification of discrete personality variables, proved unsatisfying in the face of the complexity of nuance we encountered in our subjects' lives, and to the surprise of some we frequently employed loosely-psychoanalytic dynamic theory to arrive at some significant understanding.

The discrepancy between the conceptualizations suggested by the rare exposure to interviewing research and the conceptualizations that arise from the vastly more typical paper-and-pencil or experimental designs was hard to miss; it suggests much about the effect of virtually banishing general theories from contemporary personality psychology.

In the last few pages I have argued the case that the dwindling influence of general theories has contributed to the crisis of irrelevance in personality psychology today, but what remains to be shown is that the kind of research and conceptualizing that has taken its place is itself unsatisfactory. But what might constitute sufficient proof is not clear to me.

On one hand, contemporary work in personality psychology has led to fruitful applications. Employee selection in industry and government has benefitted both from the personality tests and general psychometric expertise of personality psychologists. The Massachusetts Civil Service, for example, has employed to my knowledge both Cattell's 16 Personal Factors Inventory and the California Psychological Inventory to select police and fire-fighter personnel. I have recently heard that the General Motors corporation is seeking psychologists to aid in the identification and selection of, presumably, more productive workers.

On the other hand, as I have suggested, the great popularity of bookstore psychology, rarely the product of the academic psychological community, in contrast to the virtual immunity of lay readers to the publications sanctioned by the American Psychological Association, is testimony to how little we have addressed ourselves to the needs of the common man in his efforts toward self-understanding and adjustment. For the most part, the fruits of academic

personality psychology in recent years have been largely irrelevant to the concerns of the proverbial man on the street.

But there is restiveness, too, away from the street; restiveness in the offices of some personality researchers. On several occasions professors have privately confided their opinions that for the most part the contents of the journals are "trivial," and "not worth reading." Once when I proposed an independent study consisting of catching up on some of the recent journal issues, I was advised that such an effort would be "a waste of time." And once a professor who had taught personality at Harvard, in reference to current research conventions, quipped that "most psychologists couldn't verify the existence of their noses."

Not all the criticisms are privately expressed. A number of writers, both within psychology and within academia in general, have levied the charge of triviality and irrelevance against contemporary psychology. Peter Berger (1963), whose critical examination of sociology is itself richly suggestive of problems within psychology, identifies psychologists' historical concern with their status as scientists as a cause of trivial findings:

At the same time it is quite true that some sociologists, especially in America, have become so preoccupied with methodological questions that they have ceased to be interested in society at all. As a result, they have found nothing of significance about any

aspect of social life, since in science as in love a concentration on technique is quite likely to lead to impotence. Much of this fixation on methodology can be explained in terms of the urge of a relatively new discipline to find acceptance on the academic scene. Since science is an almost sacred entity among Americans in general and American academicians in particular, the desire to emulate the procedures of the older natural sciences is very strong among the newcomers in the marketplace of erudition. Giving in to this desire, the experimental psychologists, for instance, have succeeded to such an extent that their studies have commonly nothing more to do with anything that human beings are or do. The irony of this process lies in the fact that natural scientists themselves have been giving up the very positivistic dogmatism that their emulators are still straining to adopt.

p. 13

Although I disagree with the implication that there is any single process that can be labelled science, at least to the extent of wishing to suggest that the problem may not be that psychologists are too scientific so much as they give their allegiance to an ill-fitting conception of science, the relation Berger points out, that between psychology's "fixation on methodology" and its putative irrelevance, is certainly crucial, and will be discussed in subsequent chapters. The insight is reiterated in these remarks of Noam Chomsky (1965):

One may ask whether the necessity for present day linguistics to give such priority to introspective evidence and to the linguistic intuition of the native speaker excludes it from the domain of science. The answer to this seemingly terminological question seems to have no bearing at all on any serious issue. At most, it determines how we shall denote the kind of research that can be effectively carried out in the present state of our technique and understanding. However,

this terminological question actually does relate to a different issue of some interest, namely the question whether the important feature of the successful sciences has been their search for insight or their concern for objectivity. The social and behavioral sciences provide ample evidence that objectivity can be pursued with little consequent gain in insight and understanding. On the other hand, a good case can be made for the view that the natural sciences have, by and large, sought objectivity primarily insofar as it is a tool for gaining insight (for providing phenomena that can suggest or test deeper explanatory hypotheses).

p. 20

Noteworthy in Chomsky's statement is the implication that it is possible to define and then conduct science in such a way that one's ultimate results lack "insight and understanding," however "objective" the procedures may seem. That this is the case in contemporary psychology will be argued in subsequent discussion. A second implication that anticipates later discussion concerns the distinction between determining one's investigative procedures by the "demands" of one's subject matter and determining them to conform to an externally imposed recipe purporting to be scientific.

Criticisms of psychology are not restricted to scholars from neighboring fields. The British psychologist Liam Hudson (1973) notes:

The discipline's health is suspect: as Zangwill remarked, it has failed to produce a coherent body of scientific law; and its fruits, unmistakably, have about them an air of triviality. Attempts to justify psychological research in terms of social utility at present lead inexorably to bathos. There is

little we have produced in the last fifty years that is, in any sense of that complex word, 'relevant'...

p. 111

Speaking more directly of the field of personality, James Deese (1972) echoes Chomsky and Berger's charge identifying psychological research methods as the source of the problem:

One reason so much current empirical research is trivial and pointless is that the experimental method is inapplicable to many problems in social psychology and the psychology of personality.

p. 24

Deese believes that "much of the fundamental study of personality within the framework of the traditional scientific view is empirical and relatively shallow" (p. 92).

To this small collection of critical statements in reference to the standard research of academic psychology in general and of personality psychology in particular, many more could be added. One thinks of Rae Carlson's (1971) paper, "Where is the Person in Personality Research?," a critique which in its essentials scarcely differs from those Allport consistently makes in his reviews of the field (cf., 1961).

Yet the very fact that such journals as Journal of Personality and Social Psychology and the Journal of Personality continue to be avalanched by the papers of eager researchers suggests that many psychologists to this day

consider research conducted along present lines worth doing.² Certainly in our field it may be said that he who publisheth, surviveth; but, beyond that, there are those who have no complaints about the field today. How can the diversity of reactions to the state of research be explained? How can we understand the complaints of some and the complacency of others?

Although contemporary personality psychologists participate in the same field, their perceptions vary according to their own positions within it. Some researchers have made a greater commitment of both time and selves and have, one may suppose, a greater stake in upholding current practices. Some may have had experiences that were especially disillusioning. The judgement one arrives at regarding the importance, worth, and relevance of contemporary psychological research is likely, in the end, to be a function of one's experiences and position in the field.

My own judgements, of course, are subject to the same kind of influence. I, too, survey the field from a vantage point that represents my own personal coordinates of experience and position. Some statement pinpointing these

²Submission of research manuscripts need not in itself indicate much enthusiasm for the kinds of research currently undertaken. As long as hiring and promotion decisions reflect amount of publications, career-minded scholars are compelled to comply with editorial policy. Moreover, some graduate departments maintain the conservative tradition of insisting upon quantitative, empirical research projects for degree candidates.

is in order. Speaking of the "significant learnings" of his lifetime as a psychotherapist, Carl Rogers (1961) includes the statement, "What is most personal is most general" (p. 26); I am increasingly confident that my own perceptions are neither idiosyncratic nor unique, but perhaps somewhat common.

In retrospect I can see that my socialization within the field had encouraged the formulation of a construct system (cf., Kelly, 1955) that could not adequately accommodate my genuine experience as a human being. The concepts and language I had painstakingly mastered in the ten or so years since I undertook the formal study of psychology had left me increasingly estranged from myself. The acquired experiential categories were alien to the actual experiences of being, for lack of better terms, "fully human." Being by nature serious about my work, I tried and to a considerable extent succeeded in squeezing my perceptions--of self and others--into the ill-fitting shoe my socialized conceptualization of personhood demanded. I had, to overstate the case a bit, quantified my soul to the point of losing it.³

³By laying the chief blame upon the distorting capacity of psychology's constructs, I neglect the possibility of a pre-existing "fit" between the personality or emotional needs of a researcher--perhaps my own, too--and the world view of academic psychology. Perhaps certain persons are drawn to the reality illumined by psychology's constructs? My hunch is there are, and research done into this matter would add to our understanding of the variations in the perceptions of crisis in the field, as well as of the prospects for change.

I reached a point where I simultaneously viewed myself vainly as a reasonably well functioning person in terms of my scholarly understanding of personality, and as emotionally deadened. It gradually dawned on me that the models of personhood implicit in current personality formulations, so amenable to codification and quantification, so readily processable by complicated and elegant statistical tests, were incompatible with a deep and rich inner experience.

If the realization I am describing recalls the experience of the graduate seminar build around the direct interviewing of undergraduate subjects, I would **not be** surprised for among the personal experiences which brought me to the brink of insight was the life-history interviews I conducted of forty-year-old divorced women, under the auspices of the sociologist Alice S. Rossi.

One of these women, describing the break up of her marriage, spoke of her trip to Europe with her stock broker husband. Day-dreaming of travelling together on a vespa, vagabonding from pension to pension, she found herself instead shuttling between jet and taxi, taxi and luxurious hotel. Something inside did not fit; something inside snapped. She finally told her husband, "I'm leaving you. I'm going home. I feel I'm living a lie."

The phrase "living a lie" would not leave me. It took up residence within me, a kind of inner voice that forced me into a confrontation with my own existence. The intimate contact I made with my subjects' lives, ordinary in

some demographic senses, completely more vibrant than my own, shook me up emotionally and intellectually, and forced upon me the realization that the conceptual apparatus of academic psychology encourages one to perceive oneself and others as if humans really were the two-dimensional superficial creatures psychological formulations suggest. To put the matter again in extreme terms, I felt myself a zombie who had caught a glimpse of himself in a mirror.

Teaching personality to undergraduates only augmented the growing dissatisfaction I felt. In my first lecture course I did nothing so much as instill in my students a sequence of new and arcane vocabularies. I provided them with the new conceptual categories, essential for their written examinations, yet seeming not to enhance their genuine understanding of human existence. Teaching certainly provides ample opportunity to observe the socializing function a discipline requires for currying potential new members. By insisting to them, as my undergraduate professors had insisted, that no theoretical statement could be made unless one could answer the question, "Oh? Where are the data?," I observed myself passing on a criterion for verification that was stultifyingly narrow.

Moreover, the inevitable inveighing against the methods of introspection, a part of my teaching catechism, bred an alienation from the lessons of one's own gut. The lesson that scientific proof can never obtain from any inner,

intuitive corroboration conveyed the message that inner intuitions themselves are not part of the true scientist's equipage. As a result, part of the socialization of my students consisted of breaking the link between one's inner experience--one's subjective reality--and one's cognitive formulations. Such a severance, I can see now, had been singularly well accomplished in my own case.

In his criticism of the field, Hudson (1973) states:

Just as novelists draw on their experiences, so too do psychologists. We would both be cut off, otherwise, from the springs of our intellectual vitality. To refuse a psychologist access to his intuitions, even if this were possible, would be as stultifying and as short-sighted as it would be to deny them to a physicist or a painter.

p. 129

I sadly must conclude that we are able to make more progress in reducing access to our intuitions than Hudson believes is possible. Perhaps more than anything, it is the typical confusion between the context of discovery and the context of justification in science (cf., Rudner, 1966) that promotes in the bidding student the mistrust of and alienation from his intuitive experience, for psychologists seek to restrict the title of science to that portion of the scientific spectrum especially appropriate to hypothesis testing.

The message conveyed reads something like this: only when an observation can be externally validated through appropriate quantitative procedures can one be said to have made an observation at all; thus, it follows that the good

scientist will restrict his perceptions to only such kinds of observations.

Such a message is akin to another which, though somewhat oversimplified, can be put as follows: only those qualities of human existence that are quantifiable ought, for the good scientist, even be visible.

In my second personality course I aimed at meeting the demands for relevance that students bring to such courses. The task of translating back into English the real fruits of our discipline's researches proved challenging and difficult. I used a theories textbook, but augmented it with such outside reading as Rogers's Becoming Partners, Freud's Dora: An Analysis of a Case of Hysteria, and Castaneda's Tales of Power. The research journals were of no use. The best, most enlivening and insightful discussions grew out of the readings that reached and drew out students' personal experiences, the readings which did not imply, as so much of the psychological literature does, that one's inner life is no more real than doppelgängers or leprechauns.

The combined result of my interviewing studies, my undergraduate teaching, a good, critical history of psychology course, and other crises in my personal life brought me to reassess the field of academic psychology, as well as my relation to it. I drew back from many intellectual commitments and for the first time in years was able to attend to the long-mute inner voices.

As part of this process of drawing back, I taught for a year a course in freshman rhetoric. It is possible, I was reminded, to deal intelligibly with ideas that are not verified solely in terms of their fidelity to empirical proofs, suggesting that psychology's criterion of knowing is neither divine nor universal. Rather, it represents an epistemological position which itself can profitably be put under scrutiny. The content of our concepts reflects many assumptions and presuppositions about what it means to know, and I believe now that there is a crisis in personality psychology, one that can be related directly to these assumptions and presuppositions.

Certain crucial questions have suggested themselves, perhaps the foremost of which concerns the nature of science itself. The psychology textbooks I have seen have in no way contradicted the implicit lesson of my own education, that there is such a thing as a fixed, universal scientific process. Is this really so? It seems to me now that the charge of irrelevance cannot be made without reference to the methodological commitments of psychologists who place their trust in the scientific method they have been socialized to take for granted. As noted, many critics have leveled the charge that personality psychology's troubles begin with its method-centricity. Gadlin and Ingle (1975) put the matter succinctly:

We ought to begin with a reversal of the present empases: Psychology should initially address itself to phenomena, not methodology. Rather than selecting for research those phenomena suited to our methods, we ought to shape and develop our methods to fit phenomena.

p. 1007

I concur. Psychology suffers from an inadequate understanding of the scientific process, and the procedures we employ have untoward consequences both for the import of our research and for our conceptions of ourselves and others as persons.

The closest formulation of the problem in my view is embodied in the anti-positivist philosopher of science Thomas Kuhn's (1970a) The Structure of Scientific Revolutions. Kuhn's concept of paradigm provides a convenient handle for the diagnosis of present day personality psychology.

To anticipate the argument of the next chapters, the assumptions and presuppositions that constitute personality psychology's present understanding of science and of method can be described as a paradigm, and this paradigm is not something fixed and universal, but, rather, arbitrary and debatable. It is my thesis that the paradigm of contemporary psychology, shared by personality psychologists, is essentially behaviorist and not especially appropriate for the study of personality, especially as personality was understood when the field originated. Personality psychology's history suggests we are in the thrall of a paradigm that accounts for the crisis we as a field are currently experiencing.

C H A P T E R I I

PERSONALITY PSYCHOLOGY'S PARADIGM

I have suggested that personality psychology is a field in crisis and have included the statements of other writers who clearly agree. But the field as yet has heard no general outcry that crisis is at hand. Why is it that the sense of crisis is not more widespread?

One reason is this: today's personality research for the most part "works." Many researchers keep at bay any recognition of crisis through the reassurance they find in adhering to procedures that are unquestionably and generally regarded as scientific, and in procuring from these procedures data that provide significant tests of the hypotheses they conceptualize.

The problem of personality psychology is not that it cannot test or verify the questions it raises for itself. The mechanics of hypothesis-testing and verification as prescribed work well enough. The problem is not a procedural hitch, not a methodological short-circuit.

The problem in a way is that personality research is too do-able. That is, today's research procedures, our legitimate methods, allow researchers to feel they are 'doing the right thing' with their research, and thus serve as blinders that imprison the researcher in certain

conceptualizations of his subject matter while precluding others. Moreover, I would maintain, the conceptualizations we are in the thrall of lead to the phrasing of trivial questions and much irrelevant research.

Personality psychologists, then, have the satisfaction both of 'doing what they are supposed to do' to be scientifically secure and of obtaining results that are appropriate to their questions. The crisis, then, is not brought on because operating by the book leads to inescapable breakdown. It does not. The problem is not within the system. The system runs. The problem is the system itself.

The problem, I would like to say, is the paradigm underlying personality research today. The term is taken from the philosopher of science Thomas Kuhn, whose essay (1970a) advances the thesis that a science develops not by the gradual accretion of more and more proven facts, but rather through a succession of world views, or conceptual pictures, upon which the actual theoretical work of a given scientific period--as well as its taken-for-granted facts--is based. When a scientific community undergoes a 'revolution,' according to Kuhn, what changes is its paradigm. This change is like an irreversible gestalt switch, altering the scientific community's perception of its subject matter, its appropriate procedures, and itself. About paradigm revolution Kuhn states:

...it is a reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications. During the transition period there will be large but never complete overlap between the problems that can be solved by the old and the new paradigm. But there will also be a decisive difference in the modes of solution. When the transition is complete, the profession will have changed its views of the field, its methods, and its goals.

p. 85

Because I will employ the concept of paradigm in a somewhat crucial role in presenting my argument, I shall try to clarify what I mean by it and believe Kuhn, himself, chiefly means by it.⁴

In his seminal essay Kuhn employs the concept of paradigm in several different senses, providing critics with a foothold for levying attacks over the niceties of definition, while providing his adherents with a richer sense of his meaning. Masterman, a friendly critic, in her essay (1970) "The Nature of a Paradigm" counts 21 senses of the term as employed by Kuhn. She remarks that "not all these senses of 'paradigm' are inconsistent with one another: some may even be elucidations of others" (p. 65).⁵ Masterman has distilled these different senses

⁴The application of Kuhn's analysis of science throughout this essay is intended more as an optic to enhance understanding than as any strict test of Kuhn's ideas.

⁵Masterman notes that charges of obscurity in Kuhn are typically levied by philosophers, not scientists themselves. Actual scientists find Kuhn "perspicuous," a circumstance Masterman attributes to Kuhn's having "really looked at actual science, in several fields, instead of confining his field of reading to that of the history and philosophy of science" (p. 59).

of the term paradigm into three categories: metaphysical, sociological, and artefactual.

For a fuller account of these definitional distinctions, the interested reader is referred not only to the Masterman essay, but also to Kuhn's (1970a) postscript to his original essay, published eight years earlier. In this discussion, paradigm refers to the system of pre-suppositions, frequently tacit, that determine how a scientific community construes (perceives and interprets) its very subject matter--and, as a consequence, phrases its problems in the conceptual fashion it ultimately does.

Kuhn gives vivid enough illustrations of the workings of a paradigm in this sense. He claims that were Aristotle and Galileo confronted by, say, a stone hanging on a string, swinging, they would see two different realities: Aristotle would see "constrained fall;" Galileo, a pendulum. Similarly, according to Kuhn:

Lavoisier...saw oxygen where Priestley had seen dephlogisticated air and where others had seen nothing at all.

p. 18

Paradigms operate at the implicit level to give rise to the explicit reality we take for granted.

To describe a paradigm, then, involves making explicit ideas and preconceptions that are normally taken for granted. According to an old adage, fish are the last to discover the existence of water. Personality psycholo-

gists, similarly, may have a difficult time discovering the implicit paradigm presuppositions that endow our present endeavors with their taken-for-granted aura of scientific respectability and inevitability.

Since the quickest and most reliable aid to the fish's discovery of water is its removal from it, it follows that the presuppositions one makes as a personality psychologist might to a certain extent be made clear by transposing oneself into a different field. Something of the sort occurred to me when I became a teacher of rhetoric, a transposition out of empirical science altogether.

I was struck first by the willingness of other academicians seriously to entertain ideas without demanding quantitative proofs of any kind. An idea, it would seem, could have cogency independent of anyone's marshalling numerical evidence to support it. An idea, to put it bluntly, was not necessarily illegitimate just because it was never wedded to an operation.

Take, for example, Orwell's essay "Politics and The English Language." Orwell advances the thesis that corruption of language leads to corruption of both thought and governance, a reasonable idea, certainly, in light of, say, the Watergate locutions. Orwell argues his position well, but there is nothing in the process of argument that approaches what I, as a psychologist, am accustomed to consider as proof. He gives examples of misleading,

treacherous, and ugly usages, but a psychologist would build his case very differently.

A psychologist would automatically begin to ponder the measurement of dependent and independent variables. He might devise a goodness-of-English-usage test, to be administered as a measure of corruption of language. Possibly the subjects would be divided according to how willing they are to use "ain't." A test of high school civics might be adopted as an index of commitment to decent government. The first step in the procedure, it should be noted, is to anchor one's concepts in reality, which in the world--or paradigm--of psychology is the effect of codification and quantification. Operationalizing one's concepts, for the psychologist, realizes them.

The completion of the study whose beginnings I have sketched above can be left to the enterprising imagination; the point I want to make is in reference to certain rules of the game that became clear when I had achieved some perspective on personality psychology. These rules, it seemed to me, had a great deal to do with verification, which is to say method.⁶

Striking differences are immediately apparent in the methods of an English essayist and American psychologists,

⁶Rudner (1966) distinguishes the term method from technique and procedure, defining method as a discipline's logic of justification: the rationale by which a discipline bases its acceptance or rejection of hypotheses or theories. (See p. 5).

but paradigm differences occur among the sciences and, as I hope to show, among the disciplines of psychology.

The student of personality theories has an advantage in understanding Kuhn's concept of paradigm, for the usage is anticipated in George Kelly's (1955) personal construct theory and its philosophical foundation, constructive alternativism. Kelly explained personality itself as the process of sensibly construing the world, and he assumed the world is open to potentially infinite numbers of different interpretations, many of which being completely capable of yielding a fair share of accurate empirical predictions. By extending the individual notion of construct system onto a scientific community, we arrive at Kuhn's conception of paradigm (indeed, Kelly liked to draw the analogy in reverse: his snapshot description of human nature is "Man the scientist.").

When an individual's construct system is transformed, his reality changes. We can understand Kuhn's description of scientific revolution as a catclysmic shift in the underlying construct system not of an individual alone, but of a community of scientists, of an entire discipline. "After a revolution," writes Kuhn (p. 111), "scientists are responding to a different world."

The idea of scientists responding to a different world has been illustrated by Kuhn with examples from physics and

chemistry: Aristotle and Galileo, Lavoisier and Priestley. But we can add one, at least hypothetically, from psychology. Let us imagine in the presence of two of psychology's most famous mythological beasts--the radical behaviorist and the tender-minded humanist--an infant who, in lay terms, is crying its eyes out. Would our two psychologists, representing such diverse perspectives⁷, see the same thing?

The behaviorist, one can reasonably assume, might report "a human organism emitting an operant in obedience with its reinforcement history," while the humanist might describe "a human being in its formative years reacting with displeasure and pain to some unfulfilled need."

That they are seeing two different realities might be made clear by their answers to such questions as, "What, if anything is wrong here?," and "What might a solution be?"

The behaviorist might conclude that the operant emitted is undesirable and should no longer be reinforced. His solution might be not to attend the organism (e.g., provide positive reinforcement) while it continued to cry. The humanist might define the problem in terms of the threat to the baby's sense of trust in the world and confidence in its own efficacy. The humanist, interpreting the crying

⁷ The term "perspective" is employed here rather than "paradigm" to avoid confusion with the application of the latter concept to psychology throughout this thesis. It is certainly arguable that paradigm differences divide behaviorists and humanists, however.

as a meaningful communication, might recommend fulfilling the unmet need. Doing so would engender a sense of both trust and agency, the ideal 'solution.'

The same reality? Clearly not. The humanist, one can assume, would regard the behaviorist's recommendation as a formula for crippling the child psychologically, while the humanist's solution would seem a matter of coddling to invisible demons to the behaviorist. Worse, it would unintentionally reinforce the very problem one hoped to extinguish.

Both of these hypothetical psychologists are responding to different conceptual formulations and, more, to different realities. Is one right and the other wrong? The question is certainly complicated. Each perspective--and the argument holds generally for paradigms--tends to justify itself and be justified in its own terms, while failing completely in the terms of another.

The behaviorist might, should he be granted the opportunity for a test, succeed completely in extinguishing the undesired operant. The removal of the crying and wailing behavior--at once obvious to any and all--is empirically sufficient in the behaviorist view to lend support to the entire behaviorist system. In his terms he is clearly 'right.'

And the humanist, too, given the opportunity, might

succeed completely in raising from that infant an adult with a sense of both trust and agency. The conscious and perhaps subconscious indications of trust and agency, obvious to the perceptive, insightful observer--but perhaps invisible to the perceptive, insightful behaviorist--are sufficient to support the humanist system. In his terms he, too, is clearly 'right.' What is really going on depends in the end upon the perspective--hence, the paradigm or reality--from which one views the situation. Even basic 'facts' are paradigm dependent.

How might the paradigm of personality psychology today be sketched? My experience as a rhetoric teacher suggested that we take for granted that what is real is what is measurable. If we can measure something, it exists; if something cannot be measured, its existence is much less certain. It is paradigmatic that what is confirmably observable is scientifically real. Here we might detect the influence of Watson's early behaviorism: no invisible mental demons for the science of psychology, and that includes personality psychology.

The mention of John E. Watson in the context of personality psychology might strike an anachronistic note for personality psychologists, but I shall argue the case that it is not. Indeed, I shall argue that the paradigm of contemporary academic personality psychology is essentially

behaviorist, and that it originated in the Watsonian behaviorism of the early 1900's, although it did not become the paradigm of personality psychology for another 30 or so years.

Am I going too far in asserting that personality psychology's paradigm is essentially behaviorist? I think not, even though I am aware of how greatly our explicit explanatory formulations differ from those of today's behaviorists.

To approach the behaviorist heart of personality psychology's current paradigm, let us consider our methodological stance. What, for the personality psychologist, constitutes verification or proof? A first approximation to an answer is this: empirical demonstration. Empirical demonstration, we are proud to note, delineates science from whatever it is the novelist--and Orwell would be included here--does.

But to say "empirical demonstration" is not enough, for on close inspection the concept of a single, unitary process that we can identify as empirical demonstration proves untenable. Empirical demonstration can refer to different, perhaps even conflicting, processes in different paradigms. The introspectionists, as Horace Bidwell English (1921) of Wellesley College makes clear in his "In Aid of Introspection," considered themselves

as empirically scientific as the smuggest behaviorist of today:

Introspection is neither an esoteric art which can be practiced only by the initiated, nor an instinct placed by Nature in the breasts of all in order that the study of psychology might be possible. It is a scientific method.

p. 404

The introspectionists regarded their technique as nothing less than the direct observation of their subject matter, and so it was in the light of their paradigm. In the light of the behaviorist paradigm, though, they may have made no scientifically acceptable observations at all.

Empirical demonstration for today's psychologist, and this includes the personality psychologist, essentially means the prediction and control of behavior. In terms of what psychological researchers do, much can be understood if one grasps the verificatory role accorded to the prediction and control of behavior. Because behavior is accorded such a central role in the field's method, calling the paradigm behaviorist seems reasonable.

As I hope to show in later chapters, personality psychology did not originally subscribe to the behaviorist paradigm. The adoption of the behaviorist paradigm has inverted the discipline's relationship to behavior. Before the adoption of the behaviorist paradigm, personality psychologists had as their explanatory goal the scientific

account of human nature and of individuality, and behavior was methodologically important only to the extent it provided insights into what needed explanation (the structure and organization of personality). Today we have a different relationship to behavior. Like other psychologists, we are led by our methodological assumptions into the quest of predicting it. What differentiates us from other psychologists is our willingness to entertain the idea of, and employ, personality variables to better predict and control. Instead of being useful if and when it provided insight into personality, behavior has become an explanatory goal in itself. Now personality variables are useful if and when they enable the prediction and control of behavior.

Today's psychologists take for granted the epistemological sanctity of predicting and controlling behavior, and to get them even to entertain questioning the process for its cargo of presuppositions is hard. In the case of personality psychology, a question rarely asked--or actually posable in terms of the paradigm--is this: What does behavior have to do with personality? Given that the reality of personality variables is intimately bound up with their usefulness in predicting and controlling behavior--or, at least, with their capacity for being even indirectly measured in terms of some observable behavior--the question

seems crucial. Standing apart from today's paradigm, one could reasonably suggest on a priori grounds that behavior is at best only tangentially related to personality. Personality is both inner and mental, while behavior is external and social, evidently subject to many influences other than personality. Not every act--nor for that matter most acts--are especially expressive of personality.

But in terms of today's paradigm a parallel between personality and behavior is taken for granted, and the subject matter of personality in a very real sense is forced to be that-which-allows-the-prediction-and-control-of-behavior. Thus, the disjuncture between behavior and personality creates vexing problems for today's personality psychologists.

Their predicament is illustrated by the controversy that grew out of Mischel's (1968) Personality and Assessment, which suggested strongly--and in the "best tradition" of quantified examples--that personality variables were for the most part incapable even of reliability, the simplest form of prediction. The Mischel controversy involved a threat to the field, for the alledged inability of personality variables to predict behavior in the face of the ability to predict on the basis of situation was interpreted as an attack on the reality of personality variables. Personality psychologists were placed in the defensive

role of trying to establish that personality variables do exist--that is, can predict successfully.

From outside the perspective of the paradigm assumptions about behavior, the entire 'Mischel controversy' suggests an entirely different meaning. If, as the evidence seems to suggest, the regularities of behavior so often emanate from external, situational influences, then it follows that the prediction and control of behavior is not especially useful as a criterion for establishing the legitimacy of personality variables. Putting the point in slightly different terms, to predict behavior is not to explain personality. The entire 'Mischel controversy' depends on our paradigm assumptions regarding behavior.

In addition to the epistemological role accorded to the prediction of behavior, today's paradigm is heavily quantitative. Our quantitative assumptions can be made explicit by encountering a treatment of personality that fails to exemplify them. Consider this statement from Sheehy's (1977) Passages:

It's plausible, though it can't be proven, that the mastery of one set of tasks fortifies us for the next period and the next set of challenges. But it's important not to think too mechanistically. Machines work by units. The bureaucracy (supposedly) works step by step. Human beings, thank God, have an individual inner dynamic that can never be precisely coded.

pp. 36-37

Practicing research psychologists may not necessarily disagree in principle with the statement that individuals

can never be precisely coded, but their research certainly presupposes the codifiability of personality processes. What marks Sheehy's sentiments as especially non-paradigmatic is her obvious approval of the state of affairs in which simple mechanistic formulations will not do ("thank God!").

Today's paradigm assumes a world in which all the important data are quantifiable, and in which the elements will behave in some lawlike, determined, and knowable fashion. The assumption regarding codability is typically corrupted, though, from the position that all the important data can be quantified to the view that only the data that can be quantified are important--and even real.

This emphasis upon codification and quantification probably derives from the early behaviorists' understanding of science, stressing as it did the idea that theoretical statements are scientific only when securely anchored in what is observable. Stated somewhat more clearly, psychologists take the measurable for the existent.

Observable evidence--"hard data"--is important in our paradigm. The superficial history imparted by introductory textbooks suggests that by the 1920's American psychology threw overboard its dreamy mentalism and, taking its cues from physics, demanded more rigor by becoming empirical. But it is an open question whether the empirical procedures

avored by the behaviorist paradigm actually constitute a scientific method deserving of the additional adjectives one and only. What exactly is the relationship among the measurable, the real, and the methods we call scientific?

A number of writers within the social sciences have commented on this point, or close to it. Apparently assuming the view that the present procedures of the social sciences constitute the scientific method, sociologist Berger (1963) concludes that not all of reality is scientifically treatable:

Nothing is farther from the intentions of this writer to come out now with a statement of allegiance to that positivistic creed, still fashionable among some American social scientists, that believes in only those fragments of reality that can be dealt with scientifically. Such positivism results almost invariably in one form or another of intellectual barbarism, as has been demonstrated admirably in the recent history of behavioristic psychology in this country.

p. 124

I agree with Berger that by taking as real only that formulation of reality 'visible' to our current methods we necessarily become intellectually barbarian, or at least barren; but I disagree with the implication that one must go beyond science itself to evolve beyond the intellectual stone ages. According to Berger, "only an intellectual barbarian is likely to maintain that reality is only that which can be grasped by scientific methods" (p. 141).

Perhaps the problem is not the narrowness of science, but rather the narrow interpretation of it to which we now subscribe.

Deese (1972) contends that our present blinders are the result of having adopted as our conception of science the model of late nineteenth century physics:

Less valuable has been the blind transfer of the conceptual apparatus of the physical sciences to psychology as a whole. This wholesale transfer is evident in innumerable ways--in certain kinds of psychological theories (which are usually stated in analytic mathematical form), in reliance upon statistical inference, in the preeminence of the notion of experiment, and in the common use of terms like "independent and dependent variable" to describe the form of scientific exploration in psychology. Of course the commitment goes deeper than these superficial characteristics. A very significant proportion of those psychologists who are leaders in scientific research follow, in some instances almost blindly, a theory of scientific method that represents a philosophic formalization of the methods of late nineteenth- and early twentieth-century physics. Many of these psychologists believe that the development of the main outlines of scientific method stopped in the second quarter of the twentieth century.

p. 3

The old-fashioned philosophical formulations Deese refers to include both operationalism and positivism. (Positivism, it should be noted, runs through today's paradigm like water in quicksand, promoting the acceptance of today's methods and conceptions because of their firm basis in empirical support, mindless of the ease with which other methods and other conceptualizations could, even where totally contradictory, similarly generate empirical

support.)

Evident in Deese's statement is the assertion that other conceptions of scientific method are possible. Not only are they possible, they have already existed. Kuhn has suggested that we be suspicious of a given paradigm's version of its science's history, for, like nations and individuals, scientific disciplines selectively revise their accounts of the past to better legitimate the formulations of the present. The introspectionists, today widely 'remembered' as scientifically naive, as previously noted, were in their own eyes as indisputably scientific as today's researcher with his pocket calculator and computer printout. According to Rychlak (1968):

The great advocate of American introspection, Titchener, proposed that we accept his tool as a methodological standard, a vehicle for evidence which he took to be validation. For him, science was an act of trained observation, followed by analysis of data. The more direct the observation, the better the science. Measurement and experimentation were viewed as "roundabout" ways of practicing observation.... The most direct means of gaining scientific knowledge was through the exercise of trained, disinterested, self-observation and analysis.

p. 203

Does every paradigm pass its own version of science as the true scientific method, then? It would seem so, and from noting it we might obtain a liberating sense of flexibility and relativity in science.

But at the moment we are the seeming prisoners of

our own paradigm which, as Deese has suggested, emulates the model of Victorian physics. Is it any wonder, then, if we convert our subject matter into physics-like dimensions? The operant, to take an example from psychology at large, is a particle of behavior, whose lawlike properties are sufficiently abstract as to exist conceptually independently of time and place or even of the organism whose operant it is. It is, in short, the atom of behavior.

Certain procedures employed in personality research show the influence of physicalistic science. A good example is the idea of a one-step assay, the idea deeply embedded in our research practices that important information about human existence can be obtained by a simple, speedy, one-time measurement. The model here is temperature-taking, or assaying the purity of an ore. We believe we can take our subjects' temperatures for need for achievement or for ego development, obtain a permanent score, and be done in 5 or 20 minutes. Small wonder that so much research in personality involves less than an hour's direct contact of researcher and subject--and often enough that hour is sufficient time for the researcher to assay all his subjects, 50 to 500, in mass testing. Research projects involving no face-to-face contact between subject and experimenter are not uncommon.

Let us step back from the argument and consider what has been said. Methodological commitments have consequences for the kinds of questions raised--and not raised--by a science. If, in psychology, the criterion for reality

becomes equated with the prediction and control of behavior, the nature of the problems individual psychologists work on will reflect that criterion. While the chief riddle of personality had once been the structure and organization of individuality--the kind of problem that might be solved by one or another general theory of personality, today's riddle involves the discovery of that which will enable the prediction and control of behavior. Instead of a general theory of personality, an adequate solution might take the form of a regression equation involving discrete, easily operationalized and quantified personality variables--such as the equations and variables developed by Rotter and Cattell.

We can summarize the effects of today's paradigm by suggesting a kind of parlor game, one that can be played at a cocktail party. The goal is to learn what one can about human existence, and the other persons present are all available as sources of information. But there are limits upon the collection of information. The only questions allowable are those that can be answered quantitatively. Additionally, the truth of any statement is contingent upon its being externally and publicly obvious. Finally, no more than 10 minutes of direct face-to-face exposure to any one person are allowed.

How likely is it that important information can be obtained when we are limited to questions that are quantitatively phrased, and by research designs involving minimal exposure to subjects? How likely are we to delve into the depths of personality when we adopt as a method an outlook quite blind to the non-quantifiable aspects of human existence? Clearly, the methodological commitments of today's paradigm have conceptual ramifications. As Hudson (1973) states:

This wholesale concern with what people actually do with their lives--as scientists, politicians, salesmen, husbands, parents, students--rather than simply with their answers to psychological tests, is something that has been lost almost entirely from psychology. Evidence about people's lives is now treated as though it were vaguely unseemly.

p. 167

In place of "unseemly," I would substitute "invisible."

C H A P T E R I I I

THE BEST OF ALL POSSIBLE PARADIGMS? AN HISTORICAL INQUIRY

Adopting the prediction and control of behavior as its chief validation method, today's behaviorist personality paradigm represents a host of physicalistic, quantitative, and externalistic methodological presuppositions, forming, in sum, a tacit but compelling background reality shaping psychological conceptualizing and research. This paradigm encourages an impoverished conceptualization of personhood, blinding us to both the depth and the organization of inner experience. We have reduced the person into a collection of variables organized to suit the specifications of regression equations rather than any 'natural contours' of human beings.

I am suggesting, then, that our paradigm is inappropriate to our subject matter, a depressing circumstance if true. But to speak in Kuhnian terms, as I do here, would seem to require adopting a relativistic view that would make claims about a paradigm's being well or poorly suited to a discipline wholly arbitrary and subjective. Are there grounds for considering one paradigm more or less suited to a discipline's subject matter than, or better or worse than, another?

The question is tricky. The temptation is great to use the criteria of one paradigm to judge another. We are

commonly told, for example, that the theories of personality failed as examples of scientific formulations because they are inadequate to the task of predicting behavior. This criticism assumes a universality to contemporary criteria which, at least historically, they do not have. This criticism is blind to the possibility that such theories succeed as scientific formulations in terms of the criteria of the paradigm out of which they grew, a paradigm I shall call the personalistic personality paradigm and which I shall elaborate upon below.

The concept of paradigm essentially suggests that we never perceive reality in neutral or non-paradigm dependent terms, but even so, I believe paradigms can be evaluated in terms that are neither necessarily biased nor unfair.⁸

In this essay I shall discuss the adequacy of personality psychology's paradigms through an historical perspective.

⁸The argument can be made, for example, that a paradigm "works" to the extent that it provides sufficient numbers of solvable puzzles for its constituents. Kuhn (1970b) has gone so far as to suggest that providing solvable puzzles is the criterion for determining that a field is a science. The vast amount of research done in personality today is ample evidence that today's paradigm supplies solvable puzzles. But the availability of puzzles cannot simply be equated with their intrinsic goodness, and to argue that today's behaviorist paradigm "works" in this sense is certainly not to demonstrate that it is, for personality, the best of all possible paradigms.

Today's paradigm, I argue, has usurped the place of an earlier paradigm, or, at least, has prevented the full flowering and articulation of an earlier paradigm, now lost. I will argue that this earlier paradigm, snuffed out by today's behavioristic one, was better adapted to the subject matter of personality and more promising than the paradigm now in ascendance.

What I am suggesting, however, seems to contradict the thrust of Kuhn's vision of scientific development. While Kuhn has been explicit in denying any teleological development toward truth, he envisions the sequence of normal science, from anomaly and crisis, to paradigm revolution, to normal science again, and so on, as essentially an evolutionary process. Kuhn (1970a) makes clear the Darwinian parallel:

The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.

pp. 172-173

Kuhn's position, then, would seem to be that whatever the problems of today's paradigm, it ought to be better--in the sense of better adapted--than the paradigm which

preceeded it.

Can the view that today's paradigm is better adapted to the subject matter of personality be supported? What seems called for is an historical comparison of paradigm change in personality psychology with Kuhn's model of scientific revolution. Does the history of personality psychology sufficiently parallel Kuhn's model of scientific development to justify the conclusion that today's paradigm is the fittest?

A consideration of personality psychology's history is a surprisingly difficult task, for I found no explicit history of the field as an academic discipline. This lack has been noted by other writers (Hudson, 1975; Rychlak, 1968). Textbooks are of little help, implying, typically, a chronological development of theories from Freud's to, say, Cattell's, suggesting that with Freud's first publications the field of personality got under way.

Accounts of philosophical conceptualizations of personhood, such as Burnham's (1968) "Historical Background for the Study of Personality," suggest we view present models of personhood as the flourishing in modern hues of ideas that have their origins in germinal philosophical roots, ignoring, it would seem, the issue of how institutionalization into an academic discipline can mark the beginning of a new kind of enterprise for studying personality. Kuhn notes (1970a) in his postscript that the

concept of paradigm is tied in with the existence of a community of scholars. A discipline is an interlocking community, united by a common set of journals, research literature, academic courses, and the like. What is missing is an historical account of the community of scholars who considered themselves personality psychologists.

In The Structure of Scientific Revolutions Kuhn suggests that any science quite normally distorts its field's history in order better to socialize new members into the belief that the present formulations represent only the fruits of the past, and that these contemporary formulations, moreover, are logically mandated by everything that had come before. So it should not be surprising to discover what amounts to a coverup of history in psychology. Our textbooks, as Kuhn suggests they should, indulge in distortions that at times seem shameless. Consider, for example, the impressions Mussen, Rozenzweig, et. al. (1973) give to introductory psychology students of how psychology has developed:

The continual growth of psychology as a scientific discipline makes it hard to define. Originally, about a century ago, psychology was defined as "the study of the mind"--the normal, adult, European, human mind. Each of these limiting adjectives was eventually discarded--psychologists began to investigate abnormal as well as normal individuals, children as well as adults, people in other cultures as well as Europeans, and animals as well as human beings.

Even the noun was changed from "mind" to "behavior." The study of the individual was supplemented by studies of groups and societies.

p. 5

The implication that the shift in focus from mind to behavior is merely another instance of removing cumbersome, limiting adjectives standing in the way of "continual growth" to me is shocking, but such shock presupposes sufficient distance from today's paradigm to recognize the difference between a science of mind and a science of behavior. For the crop of students socialized to the behaviorist paradigm, the belief implied above, that in psychology every day in every way things are getting better and better, is a glib truism requiring not even a second thought.

Without a clear sense of the origin and history of the discipline, personality psychologists stand in danger of swallowing entire the saccharined bromide that the field, like any science, progresses through an accumulation of increasingly refined and time-tested truths, discarding along the way formulations that prove less tenable than their more modern competitors and replacements.

The nebulous sense of history a personality psychologist might pick up would sound something like this: In the late 1800's Freud and his followers, physicians, began treating psychologically disturbed persons, and in the

process developed some creative, albeit largely unscientific, theories of personality. Since then other thinkers, typically psychologists engaged in clinical practice, but not always, have developed competing theories. However interesting, these theories suffered when psychologists in personality became increasingly rigorous in their scientific standards. When it actually came to testing these theories in a scientifically proper fashion, they did not hold up at all. The trend in recent years has been to develop more rigorous, smaller, testable conceptions: more hypotheses than vague general theories.

Part of the beauty of this fuzzy history lies in its justification and legitimation of current practice. The old toots of the past meant well, you see, but they had not quite got the knack of science, which is hardly surprising when you consider the antique modes of conveyance they probably used (Hanson cabs and the like) as well as the quill pens with which they probably indited their arm-chair begotten thoughts.

But that dream of history is false. Personality psychology as an academic discipline in America did not begin with Freud. A reasonable starting place is 1924 at Harvard where Gordon Allport taught what is believed to be the first personality course in American higher education. Let us examine what we can about the history of American

personality psychology; the survey that follows sheds light on the transition of paradigms in our field.

We begin with Allport, for a good case can be made that he brought the field to America and, with his 1937 textbook, for many years defined it.

Allport. We are fortunate that Allport (1968) has provided in his essay "An Autobiography" an account of the personal influences that spurred the development of personality psychology. As an undergraduate at Harvard Allport was exposed to the psychology of his day. He took Hugo Münsterberg's course, reading in the process that professor's 1914 text, Psychology: General and Applied. About the course Allport notes, "I learned little except that 'causal' psychology was not the same thing as 'purposive' psychology" (p. 380).⁹ Allport's training, not surprisingly, was highly steeped in the tradition of Germanic influence in American psychology, that link from Wundt to Titchener. Allport, who won the Sheldon travel

⁹That Münsterberg's class left some deep impression on Allport is indicated by his remarks in his 25th Reunion Report: "Professionally, I have been trying to solve a few of the riddles of human nature. Sometimes, while holding forth from the platform in Emerson D, I ask myself whether we have made much progress since Professor Münsterberg expanded on 'Ze causal and purposive nature of ze mind' to our class at the sleepy two o'clock hour from the same rostrum back in 1916." (25th Reunion Report, Harvard Class of 1919, 1944).

fellowship in 1922, spoke of his decision to study in Germany as follows:

The German tradition in psychology was still strong in America, although Germany itself had been flattened by World War I and inflation. It was only natural for me to head for Germany. William James and E. B. Titchener had immortalized in their textbooks the Teutonic foundations of our science, and my own teachers had studied there.

p. 386

But what he found as a striking influence in Germany was not a continuation of the strand of Germanic psychology to which he had been previously exposed:

I was not prepared, however, for the powerful impact of my German teachers who included the aged Stumpf and Dessoir, the younger Max Wertheimer, Wolfgang Köhler, and Eduard Spranger in Berlin, and in Hamburg, William Stern and Heinz Werner. A fellow student was Heinrich Klüver, who helped me with my halting German, and who has remained a cherished friend ever since even though our paths of psychological interest have diverged.

At that time Gestalt was a new concept. I had not heard of it before leaving Cambridge. It took me some weeks to discover why my teachers usually started their two-hour lectures with a castigation of David Hume. Soon I learned he was a natural whipping boy for the German structural schools of thought. Ganzheit and Gestalt, Struktur and Lebenformen and die unteilbare Person were new music to my ears. Here was the kind of psychology I had been longing for but did not know existed.

pp. 386-387

The experience in Germany enabled Allport to develop a psychology that was simply not a continuation of the American psychology of the time. There was, with the founding of personality psychology, a new root, independent

of both the Titchenerian-Wundtian tradition and of the new Watsonian behaviorists. When Allport returned to the United States he pioneered this new kind of psychology. That his thinking had departed from the kind of psychology done in the American East is illustrated by an incident concerning Allport's doctoral dissertation and the great Titchener himself.

Allport's dissertation, "An Experimental Study of the Traits of Personality: With Special Reference to the Problem of Social Diagnosis," was, according to Allport, "perhaps the first American dissertation written explicitly on the question of the component traits of personality" (p. 385). As his work neared completion, he was invited with other graduate students to Clark University, to attend the select gathering of Titchenerian experimentalists:

After two days of discussing problems in sensory psychology Titchener allotted three minutes to each visiting graduate to describe his own investigations. I reported on the traits of personality and was punished by the rebuke of total silence from the group, punctuated by a glare of disapproval from Titchener. Later Titchener demanded of Langfeld, "Why did you let him work on that problem?" Back in Cambridge Langfeld again consoled me with the laconic remark, "You don't care what Titchener thinks." And I found that I did not.

p. 385

In his own terms Allport saw himself "standing at a frontier" (p. 385), as pursuing "deviant" (p. 385) and "maverick" (p.386) interests. Looking back from the van-

tage point of the late 1960's, Allport could say, "Later, of course, the field of personality became not only acceptable but highly fashionable."

The case I am arguing is that personality psychology's origins were independent of the major traditions warring for control of psychology in the arena of American academic psychology in the 1920's. Although the mistaken notion of history suggests gradual and measured progression and change, the actual history of American psychology is in some ways reminiscent of the warring Goths after the fall of Rome. The big positions were held by the Titchenerian-Wundtian introspectionists and by the new-fangled, American behaviorists. In the midst of this war, personality psychology had its independent origin. Its paradigm, too, would be independent.

It should be clear from Titchener's glare of disapproval that Allport's personality psychology differed sufficiently from the Titchenerian view of what psychology ought to be. It is equally true that Allport's vision of psychology conflicted with that of the behaviorists who eventually eclipsed the introspectionists and whose paradigm established itself as a monopoly in American psychology. While Allport's interest in the pattern of organization of such "inner" variables as personality traits and the self conflicts on the most obvious levels

of conceptual content with behaviorism, underlying differences in the nature of method in psychology also existed, and these differences are noteworthy.

The differences can be expressed in terms of an understanding of science. By the time his 1961 text was published, when American psychology was deeply wedded to its behaviorist paradigm, Allport's protesting views stood in sharp relief. He believed that methods should derive from subject matter, not vice-versa:

Since positivism seeks nomothetic generalizations about behavior it is likely to regard curiosity about the internal order of mind-in-particular as subjective and "unscientific." It somehow seems more scientific to send a platoon of white rats through a maze than to occupy oneself with the complex organization of a concrete personality. It is more respectable to pursue averages and probabilities for populations than to study the life-style of one person. Such preference is not hard to explain in a culture that is technological and machine-centered....

The only real difficulty with the positivist formulation is that it does not know (or rarely knows) that it is a prisoner of a specific philosophical outlook, also of a specific period of culture, and of a narrow definition of "science." Positivism seldom defends its deterministic, quasi-mechanical view of the human person; it merely takes it for granted.

p. 551

Clearly Allport defines himself apart from the dominant paradigm of the day. Examples of Allport's jousting with the methodological prescriptions of the positivist, behaviorist paradigm are common, for Allport was famous for his stand championing the validity of research into

individuality, claiming that we cannot study "personality in general" because individuals do not exist in general, only in particular.

The understanding of science inherent in the behaviorist paradigm stresses the universality of elements: all oxygen embodies the same properties, universal laws relate the rate of fall to the mass of objects, and so on. The operant, to translate this expectation into the current paradigm, regardless of the organism, follows universal laws (e.g., the law of effect). Allport, in his 1937 text, argues against this conception of science:

The person who is a unique and never-repeated phenomenon evades the traditional scientific approach at every step. In fact, the more science advances, the less do its discoveries resemble the individual life with its patent continuities, mobility, and reciprocal penetration of functions.

Starting with an infinitely more complex subject-matter than the other biological sciences, but with the same presuppositions, the psychologist has isolated his fragmentary elements, has generalized and verified his findings in the manner of the austere elder sciences. He has succeeded in discovering orderly processes in the "generalized mind," but the phenomenon of individuality, so deliberately excluded, returns to haunt him. Whether he delimits his science as the study of the mind, the soul, of behavior, purpose, consciousness, or human nature,--the persistent, indestructible fact of organization in terms of individuality is always present. To abstract a generalized human mind from a population of active, prepossessing, well-knit persons is a feat of questionable value. The generalized human mind is entirely mythical;

it lacks the most essential characteristics of mind,--locus, organic quality, reciprocal action of parts, and self-consciousness.

This exclusion of the individual from pure psychology has led to many anomalies. It has, for example, often been pointed out that the psychologist, in spite of his profession, is not a superior judge of people. He should be, but his ascetic and meager formulae derived from "generalized mind" do not go far in accounting for the peculiar richness and uniqueness of minds that are organic and single.

p. 5

The dominant conception of science in psychology, Allport felt, results in an inadequate understanding of personhood. It was his desire that the field of personality sidestep these methodological quagmires. Put simply, he intended for personality psychology something new. Procedures must derive not from a borrowed, prior conception of science, but rather from the nature of the subject matter under study. Allport (1942) wrote, "Whatever contributes to a knowledge of human nature is an admissible method to science" (p. 35).

Allport favored the study of the individual as such, and in his compilation of research methods for personality psychologists (cf., Allport, 1961, chapters 17 and 18) he included case-study interviewing, personal record research, and other procedures open to information not essentially of the codifiable and quantitative variety. Under his editorship the Journal of Abnormal and Social Psychology, which became the very Journal of Personality and Social

Psychology discussed in the first chapter, commonly published case studies, a format unthinkable in today's journal. Allport accepted the reality of the inner experience of selfhood, so his methodological prescriptions could not be blind to subjects' subjectivity. He believed that the most revealing single question one could put to a subject was this: What future are you trying to bring about for yourself?

In this brief account of Allport's influence I have stressed his methodological independence more than I have the originality of his personalistic conceptualizations, partly because the latter are more widely known. Even so, from the passages I have quoted a sense of that personalistic focus can surely be gleaned, and no doubt can exist that the personality psychology Allport envisioned differs from that practiced today.

But in order to speak of an independent, personalistic paradigm predating today's behaviorist personality paradigm, it must be shown that other psychologists shared Allport's independence from the behaviorist paradigm otherwise spreading through American psychology.

Murray. Writing in his Explorations of Personality, Henry A. Murray (1938), another pioneer of personality psychology, makes plain his willingness to explore personality according to lights both new and maverick:

Our emphasis was upon emotional and behavioural reactions, what previous experiences determined them, to what degree and in what manner. This preoccupation set our studies somewhat aside the university tradition. For it has been the custom in academic psychology to concentrate upon the perceptive and cognitive functions of the human mind or, more recently, upon the behaviour of animals.

pp. vii-viii

That the work of Murray, conducted as the first self-styled personologist, lay "somewhat outside the university tradition" is captured by the recollections of graduate study recorded by Nevitt Sanford (1976), one of Murray's former graduate students, now president of the Wright Institute:

In the 1930s, the Harvard Psychological Clinic was housed in a frame building some distance removed from Emerson Hall, the seat of the philosophy department, which included psychology. Teaching as well as research and clinical work was done at the Clinic, which under the leadership of Henry Murray became something of a hotbed for deviant ideas. Freud, Jung, Piaget, and various other European psychologists were studied there, as was the new dynamic psychology of Murray. Students developed strong loyalties to the Clinic and grew passionate about its distinguishing ideas. Emerson Hall, where the tried and true in psychology was laid claim to, tended to be regarded as enemy territory. A student who had done his dissertation at the Clinic prepared for his oral examination by mobilizing his aggression.

p. 757

Murray (1938) believed that in a rough sense psychologists could be categorized into "peripheralists" and

"centralists" (pp. 6ff). The peripheralists represent the paradigm presently in favor among psychologists; it includes an "objectivistic inclination." Such psychologists are "attracted to clearly observable things and qualities" and are positivistic, mechanistic, and elementaristic:

The peripheralists are mostly academic men addicted to the methodology of science. Being chiefly interested in what is measurable, they are forced to limit themselves to relatively unimportant fragments of the personality or to the testing of specific skills. The aim is to get figures that may be worked statistically.

p. 9

The centralists, in contrast, "are especially attracted to subjective facts of emotional or purposive significance." They are "conceptualists rather than positivists" and are holists who "believe that personality is a complex unity, of which each function is merely a partially distinguished integral." Centralists trust empathic intuition and explain human functioning in dynamic terms.

While the research Murray and his colleagues pursued in Explorations was sufficiently varied to include procedures both peripheralist and centralist, Murray's own work falls clearly in the centralist category. Murray was skeptical of the tendency he observed among psychologists to assert their credibility as scientists. This skepticism must be due in part to Murray's own broad professional experience in biology, chemistry, and medicine, which taught him that science was not a matter of specific

procedures nor even specific instruments, but rather a process that must always reflect the 'demand characteristics' of its subject matter. Consider his (1938) statement:

Some psychologists have an almost religious attachment to physical apparatus taken over from the fundamental disciplines: physics, chemistry, and physiology. Working with such contrivances they have the 'feel' of being purely scientific, and thus dignified. Sometimes this is nothing but a groundless fantasy, since what has made these methods scientific is the fact that applied to other objects they have yielded answers to important questions. It is dubious whether many crucial problems in psychology can be solved by instruments. Certainly if physical appliances do not give results which lead to conceptual understanding, it is not scientific to employ them. For the all important characteristic of a good scientific method is its efficiency in revealing general truths.

p. 26

We see in Murray's case, as we had in Allport's, a willingness to question the conception of science otherwise capturing the field of psychology. This quality of standing apart, of seeing a need for a new way of studying human subjects, underscores the contention I am making, that as formulated originally, personality psychology offered a paradigm--or the first gropings of a paradigm--quite distinct from what had been passed down from the battles among the major traditions of American psychology, and which now has carried the day even among personality psychologists.

Murray's position is clearly maverick. His formal

undergraduate training in psychology, unlike Allport's, was cut short by, ironically enough, Hugo Münsterberg's course at Harvard:

At college a bud of interest in psychology was nipped by the chill of Professor Münsterberg's approach. In the middle of his second lecture I began looking for the nearest exit.

1940; p. 152

Murray pursued a career in medicine and science, and his work led him to wonder "why some of the men with whom I was associated at the Rockefeller Institute clung so tenaciously to diametrically opposing views about the simplest phenomena." Murray's curiosity led him to Jung's Psychological Types, which so impressed him he arranged a visit with Jung in 1925. To this meeting Murray attributes his change of professions into psychology:

On the crest of a wave I visited Dr. Jung in Zurich supposedly to discuss abstractions; but in a day or two to my astonishment enough affective stuff erupted to invalid a pure scientist. This was my first opportunity to weigh psychoanalysis in a balance; and I recommend it as one method of measuring the worth of any brand of psychology. Take your mysteries, your knottiest dilemmas, to a fit exponent of a system and judge the latter by its power to order and illuminate your whole being. This assuredly is a most exacting test, to apply the touchstone of your deep perplexity to a theory, to demand that it interpret what you presumably know best--yourself. But then, what good is a theory that folds up in a crisis? In deciding such a test, of course, the temperament and talent of the psychologist (or physician) are often more important than his system; but a healthy and critical inquirer capable of some detachment may succeed in approximately weighing out this

influence. In 1925, however, I had no scales to weigh out Dr. Jung, the first full-blooded, spherical--and Goethian, I should say--intelligence I had ever met, the man whom the judicious Prinzhorn called "the ripest fruit on the tree of psycho-analytic knowledge." We talked for hours, sailing down the lake and smoking before the hearth of his Faustian retreat, "The great flood-gates of the wonder-world swung open," and I saw things that my philosophy had never dreamt of. Within a month a score of bi-horned problems were resolved, and I went off decided on depth psychology. I had experienced the unconscious, something not drawn out of books.

1940; p. 153

Murray, then, came to personality not through a socialization within psychology, but from the outside, a pattern not unusual among personality theorists (e.g., Kelly, Rogers), and suggestive of how independence from the orthodox paradigm comes about. Morton Prince, director of the Harvard Psychological Clinic, provided Murray with a position. As Murray (1940) notes, "No man more ignorant of textbook knowledge was ever admitted to a department of psychology; but Professor Boring was a liberal and I stayed" (p. 154).

Murray's initial reactions to academic psychologists at work indicate his own distance from the reigning presuppositions concerning the proper study of Man:

At first I was taken aback, having vaguely expected that most academic psychologists would be interested in Man functioning in his environment. But not at all: almost everyone was nailed down to some piece of apparatus, measuring a small segment of the nervous system as

if it were isolated from the entrails. I was in the position, let us imagine, of a medical student who suddenly discovers that all his instructors are eye, ear, nose, and throat specialists. The phenomena that intrigued me were not mentioned, since these were not susceptible to exact experimental validation, a standard that rules out geology, paleontology, anthropology, embryology, most of medicine, sociology, and devine astronomy. If my chief aim had been to "work with the greatest scientific precision" I would never have quit electrolytes and gases. I had changed because of a consuming interest in other matters, in problems of motivation and emotion. To try to work these out on human subjects was to become a "literary" or applied psychologist, a practitioner of mental hygiene, outside and looking in upon the real psychologists who, I concluded, were obsessed by anxious aims to climb the social scale of scientists and join the elect of this day's God at any cost. What else could account for their putting manners (appliances and statistics) so far ahead of ends (importance of the problems studied)? No matter how trivial the conclusions, if his coefficients were reliable, an experimenter was deemed pure and sanctified.

1940; p. 154

And what procedures had Murray to offer to replace the isolated studies of segments of the nervous system? Like Allport, Murray was fairly open to any procedure that would provide information about personhood. Murray believed that an adequate understanding of a person would take the form of a total life record or biographical understanding; among the techniques employed in the 1938 study--which sought to generate a theory of personality on the basis of two-year interviews of 50 normally functioning persons--was the psychological autobiography. Murray's team employed structured interviews, questionnaires, symbolic play tech-

niques (Erik Homburger, later Erikson, participated in Murray's project), musical reverie experiences, and a host of other inventive procedures. Murray developed and employed the Thematic Apperceptive Test (TAT), a projective device designed to bring to the surface indications of the subject's subconscious needs and concerns.

The use of many of these techniques, especially the TAT, carries the implicit expectation that the important data of personality are found at a remove from direct, superficial scrutiny. The person is not suited especially to simple, external calibration. Murray was, after all, a believer in the dynamic role played by unconscious, inner elements of self, as the following statement (1940) vividly suggests:¹⁰

I can hardly think myself back to the myopia that once so seriously restricted my view of human nature, so natural has it become for me to receive impressions of wishes, dramas and assumptions that underlie the acts and talk of everyone I meet. Instead of seeing merely a groomed American in a business suit, travelling to and from his office like a rat in a maze, a predatory, ambulating apparatus of reflexes, habits, stereotypes, and slogans, a bundle of consistencies, conformities, and allegiances to this or that institution--a robot in other words--I visualize (just as I visualize the activity of his internal organs) a flow of powerful subjective life, conscious and unconscious; a whispering gallery in which voices

¹⁰ In stressing the importance of the subconscious Murray chiefly takes issue with Allport. Murray once remarked to me in a tone of merry incredulity that Allport thought the subconscious influence "was no larger than a pea."

echo from this distant past; a gulf stream of fantasies with floating memories of past events, currents of contending complexes, plots and counterplots, hopeful intimations and ideals. To a neurologist such perspectives are absurd, archaic, tender-minded; but in truth they are much closer to the actualities of inner life than are his own neat diagrams of reflex arcs and nerve anastomoses. A personality is a full Congress of orators and pressure groups, of children, demagogues, communists, isolationists, war-mongers, mugwumps, grafters, log-rollers, lobbyists, Caesars and Christs, Machiavels and Judases, Torries and Promethean revolutionists. And a psychologist who does not know this in himself, whose mind is locked against the flux of images and feelings, should be encouraged to make friends, by being psychoanalyzed, with the various members of his household.

pp. 160-161

Murray's skepticism that much of an understanding of personhood could ever come from obsessive concerns with scientific-seeming techniques and procedures, his contempt for the externalized view of the individual which, as he remarks, converts one into a "robot," and his emphasis upon the multifaceted arena of subjective, inner life all mark Murray's approach as deviant from the paradigm that guides and determines research and thinking in personality psychology today.

But what is even more distinguishing is Murray's implicit assumption that the goal of the discipline is an understanding of personhood which, as I hope becomes increasingly apparent, is not the same thing as--is not a necessary consequence of--the prediction and control of

behavior through the use of personality variables. Like Allport, Murray believed in the holistic nature of personality, and thus stressed personality organization. Murray's focus upon the person as such is suggested by his use of "personology" to replace "psychology of personality," which he (1938) considered a "clumsy and tautological expression" (p. 4).

Much attention has been placed upon establishing both Allport and Murray's independence from today's paradigm, because the two of them pioneered the field of personality. Hall and Lindzey's (1970) widely respected text is dedicated to Allport and Murray (as well as to Edward Tolman). Daniel Levinson, in his preface to Rychlak's (1968) A Philosophy of Science for Personality Theory, notes:

The study of personality was established as a legitimate field only in the late 1930's, primarily through the writings of Allport, Lewin, and Murray and through the entry of psychoanalysis into the academic scene.

p. viii

The presuppositions underlying the study of personality as it began with Allport and Murray differ from those in force today; these differences include both the overall conceptualization of personhood as well as the discipline's major goals.

Do Allport and Murray alone establish the existence

of a paradigm predating today's? The case will now be argued that their personalistic paradigm extended to other, more recent personality psychologists. My aim is to establish the existence of an independent personality paradigm, now more or less forgotten.

Kelly. A personality theorist whose career pattern, as noted, is typical of the breed is George Kelly, whose reputation was established by the 1955 publication of The Psychology of Personal Constructs. Like Murray, Kelly was not charmed by the psychology he encountered as an undergraduate in the American midwest, in his case the behaviorism of the 1920's. He writes (Mahar, 1969):

In the first course in psychology that I took I sat in the back row of a very large class, tilted my chair against the wall, made myself as comfortable as possible, and kept one ear cocked for anything interesting that might turn up. One day the professor, a very nice person who seemed to be trying hard to convince himself that psychology was something to be taken seriously, turned to the blackboard and wrote an "S," an arrow, and an "R." Thereupon I straightened up my chair and listened, thinking to myself that now, after two or three weeks of preliminaries, we might be getting to the meat of the matter.

Although I listened intently for several sessions after that the most I could make of it was that the "S" was what you had to have in order to account for the "R" and the "R" was put there so the "S" would have something to account for. I never did find out what that arrow stood for--not to this day--and I have pretty well given up trying to figure it out. I can see, of course, that once you step into this solipsism you can go round and round without feeling obligated to come up with anything useful.

Kelly's impatience with psychology led him away to other fields, to math and physics, sociology, and education. His dissertation in 1931 concerned common factors in speech and hearing disorders. He came back to psychology much as did Murray. With his fresh Ph.D. he was given responsibility for providing clinical psychology services to the Kansas school system; he had no previous training in clinical psychology.

No one will accuse Kelly of allegiance to the explanatory concepts of behaviorism, but his writing also makes clear that the methodological presuppositions defining today's behaviorist paradigm were also part of the solipsism Kelly did not care to go round and round in. Because Kelly's conceptualization of the personal construct system is quite close to Kuhn's concept of paradigm, it is hardly surprising that Kelly, himself, identified and labelled the paradigm status of behaviorist psychology:

Out of all this I have gradually developed the notion that psychology is pretty much confined to the paradigms it employs and, while you can take off in a great many directions and travel a considerable distance in any one of them--as indeed we have with stimulus-response psychology--there is no harm in consorting with a strange paradigm now and then. Indeed the notion has occurred to me that psychology may best be regarded as a collection of paradigms wooed by ex-physicists, ex-physiologists, and ex-preachers, as well as a lot of other intellectual renegades...

Kelly was, not surprisingly, aware--admirably aware--of the consequences of subscribing to a paradigm. Illustrating his distance from the presuppositions of the behaviorist personality paradigm is, thus, relatively easy. Regarding the sanctity of physics-like procedures, Kelly (1958) notes:

There is nothing especially revelational about events that happen in an experimental laboratory--other events that happen elsewhere are just as real and are just as worthy of attention. Even the fact that an event took place in a manner predicted by the experimenter gives it no particular claim to being a special revelation from nature. That an experimenter's predictions come true means only that he has hit upon one of many possible systems for making predictions that come true. He may be no more than a wee bit closer to a genuine understanding of things as they really are. Indeed, the fact that he has hit upon one such way of predicting outcomes may even blind him to alternatives which might have proved far more productive in the long run.

p. 35

Especially noteworthy in Kelly's remark is the implied distinction between one's ability to make predictions and one's genuine understanding of one's subject matter. We have encountered this implication before. It is typical of today's behaviorist paradigm to equate explanation with the capacity to predict and control; more characteristic of what I am calling the personalistic personality paradigm is an equating of explanation with meaningful understanding--with insights into the nature of personhood. Throughout the passages included from the writings of

Allport, Murray, and Kelly is the implication that the understanding of personhood is the field's goal.

But returning to Kelly's specific independence from the behaviorist paradigm, we find support again in his remarks concerning the enchantment of psychologists with operational definition:

The writing of the physicist Bridgeman has had considerable influence among psychological theorists. There has been a new emphasis upon the need for operational definition of the variables envisioned in one's experiments. Carried to the extreme that some psychologists would carry it, this would mean that no theoretical statement could be made unless each part referred to something palpable. It is this kind of extremism which has led to the quip that while psychiatrists would rather be abstruse than right, psychologists would rather be wrong than abstruse.

1955; p. 28

Beyond questioning the positivistic certainty of experimental predictions and operational definition, Kelly does not interpret the prediction and control of behavior--indeed, the role of behavior--as the great criterion of verification so characteristic of today's paradigm. According to Kelly (Mahar, 1969), explaining behavior is not the important question:

The languages of western Europe are constructed so as to imply that the logic of explaining behavior is based on the S-R unit. This is to say that the behavioral cycle with which we are concerned is one that starts with a stimulus and asks the question, "What response will ensue?" In effect this means that the stimulus is the question and the response is the answer. This model is implicit in Freudian theory and indeed it is implicit in most dynamic theories. Behavior is the answer;

it is the thing we are seeking to produce. In psychotherapy the object is to get the patient to change his behavior. In learning, also, the object is to get the student to change his behavior. In industry the object is to get the employee to do his job. In politics the object is to get the citizen to support the leadership. Once you are able to produce the behavior you are seeking, you have your answer. Indeed, most psychologists like to say that they are primarily concerned with the production of behavior. I think this is very sad.

But from the standpoint of personal construct theory, behavior is not the answer, it is the question. The personal construct theorist who serves in the psychotherapeutic capacity does not consider his objective the production of certain classes of behavior. He is concerned, rather, with the constructions that man, including himself and his patient, places upon that world and how these constructions are tested out. For him, behavior is not the answer, it is the principal way in which man may inquire into the validity of his constructions.

pp. 219-220

Kelly's distinction between the behavior of a person and his inner construction of experience--his subjective reality--is reminiscent of Murray's emphasis; indeed, of Allport's as well.

Note that associated with the rejection of behavior as a criterion of verification, with the refusal to adopt a physicalistic methodology, we find a consistent interest in understanding the person from an "inner" or subjective standpoint. What is taking shape is a clearer demarcation of the two paradigms in terms of the conceptual fruit they bear. If, as Kelly's personality theory suggests, our very psychological natures reflect the constructs we employ, it

may be that the behaviorist paradigm gives rise not only to an impoverished model of personhood, but also to an impoverished experience of self. Something of the flavor of this argument is conveyed in the following passage from one of Kelly's final articles:

A psychology that pins its anticipations on the repetitions of events it calls "stimuli," or on the concatenations of events it calls "reinforcements," can scarcely hope to survive as man's audacities multiply. More and more it will find its accurate predictions confined to the trivialities of man's least imaginative moments and to the automatisms of persons given in to despair. It seems to me that most of what we know as "modern psychology" is a monotonous tale told of men left behind by the quickening tempo of human undertakings. It is such men, and such men only I suspect, who enact nothing save what has been reinforced, who are carried on by the momentum of their biographies rather than compose their diaries afresh each day, and who become transfixed by their identities. And yet I doubt that there are ever men who are altogether like this. Perhaps it only seems that way from listening to psychologists.

Mahar, 1969; pp. 31-32

Is it too extreme a statement to suggest that, at times, it only seems that way from being a psychologist?

Maslow. Kelly is not alone among the "second generation" personality psychologists to escape the behaviorist paradigm. We may include the obvious example of Abraham Maslow, psychology's reknown humanist, who lobbied against the confinements traditional methodology--what has become traditional methodology--imposes.

Maslow saw great danger in psychology's method-centeredness--or "means centeredness," as he termed it. Psychology should develop its methods to suit important problems, not attack the problems for which its methods happen to be suited:

Means-centered scientists tend, in spite of themselves, to fit their problems to their techniques rather than the contrary. Their beginning question tends to be Which problems can I attack with the techniques and equipment I now possess? rather than what it should more often be, Which are the most pressing, the most crucial problems I could spend my time on? How else explain the fact that most run-of-the-mill scientists spend their lifetimes in a small area whose boundaries are defined, not by a basic question about the world, but by the limits of a piece of apparatus or of a technique? In psychology, few people see any humor in the concept of an "animal psychologist" or a "statistical psychologist," i.e., individuals who do not mind working with any problem so long as they can use, respectively, their animals or their statistics. Ultimately this must remind us of the famous drunk who looked for his wallet, not where he had lost it, but under the street lamp, "because the light is better there," or of the doctor who gave all his patients fits because that was the only sickness he knew how to cure.

1970; p. 13

Maslow saw himself working within and prosyletizing for a philosophy of science distinctly different from that characteristic of the psychology of his day:

We must help the "scientific" psychologists to realize that they are working on the basis of a philosophy of science, not the philosophy of science, and that any philosophy of science which serves primarily an excluding function is a set of blinders, a handicap

rather than a help. All the world, all of experience must be open to study. Nothing, not even the "personal" problems, need be closed off from human investigation. Otherwise we will force ourselves into the idiotic position that some labor unions have frozen themselves into; where only carpenters may touch wood, and carpenters may touch only wood, not to mention that if carpenters do touch it, it is ipso facto wood, honorary wood, so to speak. New materials and new methods must then be annoying and even threatening, catastrophes rather than opportunities. I remind you also of the primitive tribes who must place everyone in the kinship system. If a newcomer shows up who cannot be placed, there is no way to solve the problem but to kill him.

1968a; p. 218

These passages suggest, I think accurately, that Maslow's view of the process of normal science is close to Kuhn's: a dominant paradigm establishes a reign of puzzle solving, where what constitutes a valid puzzle is predetermined. Maslow was keenly aware that the present methodological prejudices exclude important questions, exclude them by pronouncing them unsolvable, unposable, or unscientific. As does Kuhn, Maslow notes (Maddi and Costa, 1972) the process whereby new psychologists are socialized into the presuppositions of the current paradigm:

Most graduate training...turns away from (topics like love, hate, hope, fear). They are called fuzzy, unscientific, tenderminded, mystical. What is offered instead? Dry bones. Techniques. Precision. Huge mountains of itty-bitty facts, having little to do with the interests that brought the student into psychology. Even worse, they try, most often success-

fully, to make the student ashamed of his interests as if they were somehow unscientific. And so often the spark is lost, the fine impulses of youth are lost and they settle down to being members of the guild, with all its prejudices, its orthodoxies.

p. 37

A large part of the deadening orthodoxy concerns the same physicalistic conception of science and technique that we have counted as fundamental to today's paradigm and which has been rejected by the critiques of Allport, Murray and Kelly. Maslow (1970) states:

Inevitable stress on elegance, polish, technique, and apparatus has as a frequent consequence a playing down of meaningfulness, vitality, and significance of the problem and of creativeness in general. Almost any candidate for the Ph.D. in psychology will understand what this means in practice. A methodologically satisfactory experiment, whether trivial or not, is rarely criticized. A bold, ground-breaking problem, because it may be a "failure," is too often criticized to death before it is ever begun. Indeed criticism in the scientific literature seems largely to mean only criticism of method, technique, logic, etc. I do not recall seeing, in the literature with which I am familiar, any paper that criticized another paper for being unimportant, trivial, or inconsequential.

pp. 11-12

Maslow attempted to forge a philosophy of science for psychology that was suited to the original concerns of personality psychology, namely the understanding of personhood. He explicitly saw himself establishing a new way, or Third Force, in psychology:

In the thirties I became interested in certain psychological problems, and found that they could not be answered or managed well by the classical scientific structure of the time (the behavioristic, positivistic, "scientific," value-free, mechanomorphic psychology). I was raising legitimate questions and had to invent another approach to psychological problems in order to deal with them. This approach slowly became a general philosophy of psychology, of science in general, of religion, work, management, and now biology. As a matter of fact, it became a Weltanschauung.

1971; p. 3

Central to Maslow's new psychology were elements already existent in the work of Allport and Murray: a respect for the reality of inner experience--that is to say, subjective experience--and a reliance upon investigative procedures that enable researchers to encounter that inner experience. In Maslow's case, the unstructured interview figured prominently. A sense of how Maslow proceeded is suggested by his (1968b) remarks concerning his research on human sexuality, for which he interviewed women:

But women are really kind of perpetual miracles. They are like flowers, even old ladies. Every person is a mystery to me, but women are more mysterious to me than men. So any woman is a fascinating mystery to question for endless hours.

I interviewed 120 women with a new form of interview. No notes; we just talked along until I got some feeling for the personality, then put sex against the background.

p. 54

Maslow's depiction of self-actualization is based not only upon the lives of historical persons (e.g., Lincoln),

but also upon some casual, one must suppose, or at least informal and indirect interviews of persons Maslow knew and admired. It is noteworthy to add that Maslow's (1970) discussion of self-actualizers provokes more lively and insightful discussion in undergraduate personality classes than any piece of traditionally-inspired personality research I am aware of.¹¹

In addition to departing from the methodological prescriptions so central to the behaviorist paradigm of contemporary psychology, Maslow wasted little time concerning himself with the prediction and control of behavior, another pillar of the current paradigm. His view is close to Kelly's, holding that behavior in and of itself is of little relevance or importance to the personality psycho-

¹¹It should be noted that Maslow certainly did not champion an unscientific approach to research; rather, he hoped to broaden psychologists' conception of science. In his preface to the first edition of Toward a Psychology of Being, Maslow (1968a) wrote, "It is clear to me that scientific methods (broadly conceived) are our only ultimate ways of being sure that we do have the truth" (p.viii). Maslow did not depart from the traditional notion of research without some doubts: "My study of self-actualizing persons has worked out very well--to my great relief, I must confess. It was, after all, a great gamble, doggedly pursuing an intuitive conviction and, in the process, defying some of the basic canons of scientific method and of philosophical criticism. These were, after all, rules which I myself had believed and accepted, and I was very much aware that I was skating on thin ice. Accordingly, my explorations proceeded against a background of anxiety, conflict, and self-doubt" (1970; p. xxi).

logist; behavior is not the end we seek:

Behavior...is means rather than end, i.e., it gets things done in this world. It is a question whether the exclusion of subjective states as a legitimate object of psychological study does not, a priori, make difficult or even impossible the solution of the problem we are discussing. Ends as I see them are very frequently subjective experiences of satisfaction. Without reference to the fact that most instrumental behaviors have human worth only because they bring about these subjective end-experiences, the behavior itself often becomes scientifically senseless. Behaviorism itself may be understood better if it is seen as one cultural expression of the general Puritan striving and achieving point of view we have already mentioned. This implies that to its various other failings must now be added ethnocentrism.

1970; pp. 233-234

Maslow, Kelly, Murray, and Allport represent a personological tradition in personality psychology--what can be defended as the original paradigm of the field, the paradigm that defined the subject matter in the first place. Their lives and work suggest that independence from the paradigms of psychology external to personality is most easily accomplished by avoiding the socialization of a standard psychology education, although independence is certainly possible even for those traditionally educated.¹² In fact, the death of the personological paradigm

¹² Maslow's (1968b) initial reaction to Watsonian behaviorism reads like Skinner's: "Life didn't really start for me until I got married and went to Wisconsin. I had discovered J.B. Watson and I was sold on Behavior-

can be directly related to the socialization up-and-coming members of the field receive, for it is typically true that one is now socialized as a psychologist first, before one specializes in personality.

But here I am getting ahead of my argument. Before discussing how the personological tradition was eradicated from academic psychology, we had best summarize the distinguishing features of that tradition.

The theories of these four men differ significantly, but they share some presuppositions in common which contrast sharply with those characteristic of American academic psychology today. As discussed, they explicitly challenged the methodology reigning in the rest of psychology. Each of these theorists (and the list is not meant

ism. It was an explosion of excitement for me. Bertha came to pick me up and I was dancing down Fifth Avenue with exuberance; I embarrassed her, but I was so excited about Watson's program. It was beautiful" (p. 37).

What brought Maslow to renounce behaviorism were further reading and increased life experience. He attributed his conversion to reading Freud, gestalt psychology, organismic psychology, Bertalanffy, Whitehead, and Bergson, and to studying the Rorschach Test. Beyond those intellectual influences were some more personal observations: "Then when my baby was born that was the thunder-clap that settled things. I looked at this tiny, mysterious thing and felt so stupid. I was stunned by the mystery and by the sense of not really being in control. I felt small and weak and feeble before all this. I'd say that anyone who had a baby couldn't be a behaviorist" (p. 56).

to be exhaustive) defined himself apart from the mainstream. The tradition of personality theorists had always been to play a maverick role, to represent a different way of studying human nature.

The personological personality psychologists were united in a common definition of their subject matter: the understanding of personhood, which chiefly was understood to refer to the elements of personality and their organization and development. The kinds of questions they addressed themselves to involved human nature and the conditions of the good life. While they did not necessarily attempt directly to answers these questions, their sense of mission, their understanding of why the study of personality was important, ultimately had to do with human existence on this planet.

The personalistic personality psychologists were theory-minded, while the behaviorist personality psychologists today, by contrast, are study-minded, or hypothesis-minded. This difference underscores the former's interest in the organization of the elements of personality and the latter's desire to isolate personality variables that enable prediction and control equations.

The former viewed behavior as a peripheral issue, as the tip of the personality iceberg, as it were; the latter view behavior as the royal road to verification.

The former tend to be relatively open-minded and creative in employing techniques and procedures, willing to allow the questions they asked and their openness to the richness of personhood to guide their experimentation with procedures. Though certainly many adherents of the behaviorist paradigm have been as creative and open-minded in their individual researches, the behaviorist personality psychologists as a group have followed a more inflexible, a priori formula of scientific procedure.

The personalistic personality psychologist's goal was the understanding of personhood. Again and again we find that word, understanding. It is a word that has in some respects gone out of fashion with the ascendance of the behaviorist paradigm.

Understanding has given way to the criterion of prediction. Indeed, the nature of scientific explanation itself, for today's paradigm, is intimately bound up with prediction. Let us specifically raise the question, What constitutes a scientific explanation?, for the answer varies from one paradigm to the other, and the issue is crucial for understanding how the very nature of "being scientific" changes when a paradigm changes.

The nature of scientific explanation associated with Hempel is expounded by Dray (1964), a philo-

sopher of history:

Now scientific explanations themselves may be given at various levels of sophistication. It seems generally to be agreed, however, that insofar as they explain particular occurrences, they have one crucial feature in common: they render predictable what is explained by subsuming it under universal empirical laws. In ideal cases, such subsumption exhibits a deductive pattern: a statement asserting the occurrence of what is to be explained is shown to be logically deducible from statements setting forth certain antecedent conditions, together with certain empirically verified general laws.

p. 5

This formulation will be recognized as an ideal held out to students in their socialization as psychologists. It is familiar. What might be overlooked is the qualification, "that insofar as they explain particular occurrences;" it seems to me that in employing this philosophy of science we have forced ourselves to transform psychology into a science dealing with particular occurrences, rather than with personhood which is not especially particular. The personality variables of today's research may be seen as our manufacture of personality particles.

But let us focus on the relationship between this formulation of scientific explanation and the role of prediction. The philosopher of social science, Rudner (1966), sheds light on the issue:

The formal structure of a scientific explanation of some specific event has three parts: first, a statement E describing some event to be explained; second, a set of statements C_1 to C_n describing specific relevant circumstances that are antecedent to, or otherwise causally correlated with, the event described by E; third, a set of lawlike statements L_1 to L_n , universal generalizations whose import is roughly, 'Whenever events of the kind described by C_1 to C_n take place, then an event described by E takes place.'

In order for these three sets of statements actually to constitute an explanation of the event, they must fulfill at least two conditions: first, the E statement must be deducible from the C and L statements together, but not from either set alone, and second, the C and L statements must be true.

p. 60

Rudner goes on to make explicit that this view of scientific explanation is closely bound up with the capacity to make predictions; indeed:

It follows from these considerations that we have an explanation for an event if, and only if (from a different temporal vantage point) we could have predicted it.

p. 60

This view of scientific explanation is that of the behaviorist paradigm. It goes to the heart of the paradigm's method, where method is understood not as techniques and procedures but rather as the field's logic of justification, "the rationale on which it bases its acceptance or rejection of hypotheses or theories." Research today aims at the prediction and control of behavior because the capacity to predict is understood to be synonymous with an adequate scientific explanation.

The personalistic paradigm, in contrast, presupposes a different conception of scientific explanation. I cannot formulate a statement of the personalistic conception as well-focussed and polished as those of Dray and Rudner, and it may be that this alternative conception was, even for them, the personalistic personality psychologists, not always explicitly drawn. But time and time again we find them speaking of understanding, and speaking of it in contexts that clearly differentiate understanding from prediction and control.

Consider Rogers's (1961) statement:

In approaching the complex phenomena of therapy with the logic and methods of science, the aim is to work toward an understanding of the phenomenon. In science this means an objective knowledge of events and of functional relationships between events. Science may also give the possibility of increased prediction of and control over these events, but this is not a necessary outcome of scientific endeavor.

pp. 205-206

The conception of scientific explanation embodied in the personalistic tradition results in different kinds of expectations concerning a satisfactory scientific account of personality. Personalistic psychologists expected the scientific process to end in important understandings, not replicable predictions.

The complacent certainty with which these older theorists are today condemned as insufficiently scientific-

ic derives from the position associated with Hempel; why listen to Hempel? Even within the field of philosophy of science these issues are debated.

Peter Winch, as early as 1958 in The Idea of a Social Science and its Relation to Philosophy identifies the very issue of meaningfulness, virtually synonymous with the understanding sought by the personalistic psychologists, as the reason why the physicalistic conception of science is inappropriate for social science phenomena. A different method, he makes clear, is necessary:

What in fact one is showing, however, is that the central concepts which belong to our understanding of social life are incompatible with concepts central to the activity of scientific prediction. When we speak of the possibility of scientific prediction of social developments of this sort, we literally do not understand what we are saying. We cannot understand it because it has no sense.

p. 94

Winch's point comes to the heart of the matter: the desire for understanding in the sense of a subjective satisfaction is not what one gets from the method practiced by psychologists today; indeed, the method practiced today makes demands that legislate against that very sort of understanding.

Closer to the position of the personalistic psychologists is the verstehen idea of validation. Scientific activity can be envisioned in which the empathic under-

standing of the scientist is counted as evidence that is validating, in the same sense that empirical predictions are counted validational now.

C H A P T E R I V

PARADIGM CHANGE IN PERSONALITY PSYCHOLOGY: WAR OF THE
WORLD VIEWS

The previous chapter argued for the recognition of a paradigm in personality psychology predating today's, a personalistic paradigm dedicated to the theoretical understanding of personhood, a paradigm founded upon a philosophy of science conducive to such an understanding. Something happened, though. Personalistic personality psychologists no longer constitute the heart of academic personality psychology. Their names almost never appear in the references of the leading research journals. Indicative of the changes that have taken place are the complaints expressed by Carlson's (1971) article, whose title itself is revealing, "Where is the Person in Personality Research?".

Carlson, reviewing all the articles appearing in the Journal of Personality and the Journal of Personality and Social Psychology in 1968 (226 substantive articles) notes:

Obviously, no single scientist, no single study, no single research tradition can possibly deal "scientifically" with anything so complex as a whole person. But the attempt can be made collectively and cumulatively. The present impoverishment of personality research is distressing because it suggests that the goal of studying whole persons has been abandoned.

Among the factors distressing to Carlson are many that follow logically and intelligibly from the presuppositions of the behaviorist personality paradigm. First, she points out (p. 205) that "experimental methods predominate in current research, with over half of the published studies employing manipulative procedures." Most of the remaining designs are correlational. These kinds of research are typically of the physicalistic, quantified variety in which human data are briefly assessed--or assayed in the sense spoken of earlier--then simultaneously converted into numbers which, once translated onto IBM cards, are fed into computers for analysis.

As anyone who has done a great amount of this kind of research will testify, more time is usually spent, more manipulations made, more concentration on the researcher's part expended, after the data have been collected and quantified. For, once the data have been converted into numbers, the experimenter essentially begins to play mathematical games, all perfectly legitimate in terms of rules applying to the manipulations of figures. For the rules of numbers are clear enough, and one can sometimes argue a case with a significant chi-square when a t-test of the same data fails to pan out. The weakest link in much quantitative research is the translation of human qualities into integral categories. How angry is 7 on a ten-point anger scale?

But once the researcher has his 7, the rest is smooth sailing. And, after all, "the light is better there."

Carlson does not mention finding case studies. She also laments the discovery that most studies elicit hardly any information about their subjects:

Extremes of a "comprehensiveness" dimension are represented by studies in which subjects left no trace of their personal participation, merely contributing isolated bits of behavior to a data pool, and a few in which subjects provided exhaustive data on a battery of tests and biographical inventories. However, the typical study represented an individual in terms of his sex (sometimes), treatment condition, performance scores, and ratings of partner or experimenter in post-test inquiry. Although the literature as a whole has elicited a wide range of potentially important information about persons, no single investigation either noted or utilized much information about any individual subject. Thus the task performances of subjects in current research remain uninterpretable as personality data in the absence of anchoring information.

p. 206

What Carlson is noting here is the effect of the paradigm change, which has converted the focus of personality research from the understanding of the person as a whole to the attempt to predict and control on the basis of hypothetical personality variables. The studies she reviewed do not need much information about each subject; they need, essentially, an assay of the hypothetical variable and something to predict on the basis of it (performance ratings).

A related observation by Carlson concerns how little time is actually spent with individual subjects:

The time span of contemporary inquiry is short. The vast majority of published work was based upon a single session; less than one fifth of reported studies involved more than a two-week period, and rarer still were the few studies involving follow-up over significant periods of time.

p. 206

The presupposition that natural science techniques can be employed profitably on human subjects has already been noted. How long does it take to determine a solution's temperature? How time consuming is the assessment of a mineral's specific gravity?

Carlson also notes that no attention is paid to the organization of personality or to changes occurring over time:

We cannot study the organization of personality because we know at most only one or two "facts" about any subject. We cannot study the stability of personality nor its development over epochs of life, because we see our subjects for an hour.

p. 207

Of course we cannot! These are not especially important (visible) problems in terms of the current paradigm's world view. In fact, Carlson's complaint is only a complaint from the point of view of the personalistic paradigm. It comes as no surprise to read that Carlson quotes Maddi's (1968) definition of the field of person-

ality, for Maddi, co-author of Humanism in Personology: Allport, Maslow, and Murray, was himself a student of both Allport and Murray who, indeed, is one of very few personality psychologists still to employ the term personologist.¹³

Indeed, Carlson quotes Murray himself:

Over 30 years ago, Murray (1938) noted that "The reason why the results of so many researches in personality have been misleading or trivial is that experimenters have failed to obtain enough pertinent information about their subjects. Lacking these facts, accurate generalizations are impossible. (p.ix)." This comment could stand as a summary of current work--with the important amendment that the accumulation of more "facts" (including much unassimilated data collected through Explorations in Personality) has not provided, nor is likely to provide, the basic generalizations needed in this field.

p. 213

¹³Maddi's definition: "The personologist is interested in universals...in the commonalities among people (as well as)...in the attempt to identify and classify differences among people...The personologist is rather unusual in not restricting himself to behavior easily traceable to social and biological pressures of the moment...Of all the social and biological scientists, then, the personologist believes most deeply in the complexity and individuality of life...his emphasis (is) upon characteristics...that show continuity in time...that seem to have psychological importance...that have some ready relationship to the major goals and directions of the person's life...The personologist is interested in all rather than only some of the psychological behaviors of the person...Finally...personologists...are primarily interested in the adult human being...the fruit of development--a congealed personality that exerts a pervasive influence on present and future behavior" (1968; pp. 6-9).

Carlson complains that "problems are posed by current research conventions" (p. 210), but the problems she describes are the natural expression in research of the pre-suppositions of today's behaviorist personality paradigm. Her complaint stands as evidence that things have changed in personality psychology. From the days of the lengthy case study (White's Lives in Progress, Murray's Explorations in Personality, Allport's Letters from Jenny) we now find ourselves in the midst of a literature on the basis of which one can ask, where is the person in personality research? Something happened: a paradigm change.

Kuhn (1970a) has adopted an evolutionary metaphor for this process of paradigm change, a metaphor seemingly justified by the role he accords anomaly in the cycle of a science's development. For an anomaly, however unintended, is a result of the paradigm it ultimately brings down. Kuhn equates a paradigm with a way of construing the world, and his model of a science's development could be phrased as follows: a new paradigm realizes a new world, one with a certain amount of unexplored promise; normal science is the exploration of that world. A "good" paradigm is a reality that for the most part works: allowing the formulation of solvable problems. But no paradigm will fit perfectly, and eventually a paradigm will come up against its own limits. At this point the community of scholars

confronts anomaly. Eventually a new way of realizing the world will be proposed that will solve the problem--or dissolve the impasse--and the crisis will subside and the field will have adapted.

But did the paradigm revolution in personality psychology occur because the personalistic paradigm foundered on its own anomaly? Although Kuhn's depiction of normal revolutionary science would lead us to suspect so, I doubt that such is the case. Is there any evidence of a crisis in the original paradigm? Were there unsolvable problems which the adoption of the behaviorist personality paradigm dissolved? I think not.

We have earlier discussed the distortion of history in which a field under the influence of a new paradigm indulges, so to consider whether the personalistic paradigm ended in crisis we must especially be on guard against a false sense of history.

Specifically, we can expect researchers today to take for granted the speedy downfall into crisis and anomaly of the personalistic paradigm's research, where that impression derives from the method of empirical prediction and control. What, after all, did Explorations in Personality prove? What was established by Letters From Jenny? The dogma is that the formulations of the personality theorists never did--nor could--stand up to

the criterion of empirical testing.

But such an application of that criterion is, as I have suggested, anachronistic. We project backwards the criterion we currently employ, and erase the memory that different criteria of science were in vogue then. What needs to be demonstrated is the presence of crisis in the personalistic paradigm in its own terms. Was there internal recognition of crisis and anomaly?

There is evidence to suggest no such crisis. At their deaths neither Allport, Maslow, nor Kelly had recanted. On the contrary. Murray, in retirement, is at work on a psychological biography of Melville which, one presumes, suggests a continuing interest in the person as a whole, as well as in the biography as the proper unit of analysis.

But such evidence of continued allegiance to an old paradigm constitutes only a weak case. Kuhn himself has suggested that even during normal revolutionary change there will be old men who never make the transition to the new paradigm.

Yet other evidence that the personalistic paradigm did not fail in its own terms is available. One line of evidence is speculative but fascinating. During World War II Murray was placed in charge of the selection of candidates for the Office of Strategic Services, the OSS,

forerunner of the Central Intelligence Agency. The assessment procedures developed by Lt. Colonel Murray and his staff, recounted in Assessment of Men, published in 1948, are not far different from those employed in the Explorations study, at least in terms of format; for example, in both subjects are placed unwittingly in frustrating situations to determine their reactions under stress.

That Murray should have served as the first chief psychologist for what has become the C.I.A. is curious, and one wonders whether his approach has met with more success there than it has in academic psychology. I believe there are indications that Murray's personalistic approach exerts a continuing impact today, however banished it may now be from universities.

What makes me think so is the text of the C.I.A. report to President Nixon describing Daniel Ellsberg, leaked to the press during the aftermath of the Watergate Scandal. The terms of this psychological description are hardly those of the quantified form typical of today's research and conceptualizations. Instead are suggested the old Murray needs and press, the complexes and dynamic interactions:

This indirect personality assessment is based primarily on background material and current impressions derived from press reports, including newspaper and magazine

articles and television interviews. In addition, selected State Department and Federal Bureau of Investigation memoranda have been reviewed. As the data base is fragmentary and there has been no direct clinical evaluation of the subject, this indirect assessment should be considered highly speculative and in no way definitive.

There is nothing to suggest in the material reviewed that subject suffers from a serious mental disorder in the sense of being psychotic and out of touch with reality. There are suggestions, however, that some of his long-standing personality needs were intensified by psychological pressures of the mid-life period and that this may have contributed significantly to his recent actions.

An extremely intelligent and talented individual, subject apparently early made his brilliance evident. It seems likely that there were substantial pressures to succeed and that subject early had instilled in him expectations of success, that he absorbed the impression that he was special and destined for greatness. And indeed he did attain considerable academic success and seemed slated for a brilliant career.

There has been a notable zealous intensity about the subject throughout his career. Apparently finding it difficult to tolerate ambiguity and ambivalence, he was either strongly for something or strongly against it. There were suggestions of problems in achieving full success, for although his ideas glittered, he had trouble committing himself in writing.

He had a knack for drawing attention to himself and at early ages had obtained positions of considerable distinction, usually attaching himself as a "bright young man" to an older and experienced man of considerable stature who was attracted by his brilliance and flair.

But one can only sustain the role of "bright young man" so long. Most men between the ages of 35 and 45 go through a period of reevaluation. Realizing that youth is at an end, that many of their golden dreams cannot be achieved, many men transiently drift into despair at this time.

In an attempt to escape from these feelings of despair and to regain a sense of competence and mastery, there is an increased thrust towards new activity at this time. Thus this is a time of career changes, of extramarital affairs and divorce.

It is a time when many men come to doubt their earlier commitments and are impelled to strike out in new directions.

For the individual who is particularly driven towards the heights of success and prominence, this mid-life period may be a particularly difficult time. The evidence reviewed suggests that this was so for Ellsberg, a man whose career had taken off like a rocket, but who found himself at mid-life not nearly having achieved the prominence and success he expected and desired.

Thus it may well have been an intensified need to achieve significance that impelled him to release the Pentagon papers.

There is no suggestion that subject thought anything treasonous in his act. Rather, he seemed to be responding to what he deemed a higher order of patriotism. His exclusion of the three volumes of the papers concerned with the secret negotiations would support this.

Many of the subject's own words would confirm the impression that he saw himself as having a special mission, and indeed as bearing a special responsibility. On several occasions he castigated himself for not releasing the papers earlier, observing that since he first brought them to the attention of the (Senate) Foreign Relations Committee, there had been "two invasions," more than 9,000 American lives lost, and hundreds of thousands of Vietnamese deaths.

He also on several occasions had suggested quite strongly that his actions will not only alter the shape of the Vietnam war, but will materially influence the conduct of our foreign policy and the relationship between the people and the government.

Ellsberg's reactions since emerging from seclusion have been illustrative. Initially there was jubilation, an apparent enjoyment of the limelight. This was succeeded by a transient period wherein there was a sense of quiet satisfaction, of acceptance of his new-found stature, as if personally significant actions had accomplished what he sought to achieve.

But then, embittered that Congress and the press had not wholeheartedly supported him, he turned against them. This is not surprising, for there would seem to be an insatiable qual-

ity to Ellsberg's strong need for success and recognition.¹⁴

Is it going too far an extreme to claim to detect within this report a trace of Murray's conception of personality as "a full Congress of orators" complete with "Machiavels" and "Promethean revolutionists"? Certainly one can with relative ease translate the terms of this description into those of Murray's need system. Consider, for example, his (1938) break-down of the need for superiority into need for achievement--"will to power over things, people, and ideas"--and the need for recognition--"efforts to gain approval and high social status" (p. 80).

The conceptualization of a life here, at very least, falls clearly within the personalistic tradition: it is dynamic, holistic, developmental, and suggestive of an inner experience and deep subjectivity, all of which are atypical of today's behaviorist formulations of personality.

The point of this extended line of speculation is to suggest that if personalistic psychology is still practiced, albeit outside academia, the paradigm itself is perforce viable.

The practice of psychotherapy similarly supports the viability of the personalistic paradigm. The adherence

¹⁴"Test of Study on Ellsberg," New York Times, 3 August 1973, p. 10.

after all these years of practitioners to holistic and dynamic formulations of personhood suggests that such formulations promote insight and understanding. Terms and concepts that have vanished from today's research--the entire process of unconscious dynamics--still offer illumination in contexts outside academic research departments.¹⁵

Additional support is found in the flourishing of humanistic education centers within the past 10 or 15 years. Frequently influenced by Erikson, Maslow and Rogers, these programs focus upon understanding and promoting the richness of inner experience. Such programs typically earn their share of condescension or scorn from academic psychologists, who find the lack of rigorous, quantitative methods objectionable. But the viewpoint represented by such schools of education--of viewing the person as the embodiment of a rich subjectivity--seems sufficiently rewarding in its own terms to keep these programs alive.

In a larger sense, the realm of popular psychology--

¹⁵Studies citing the inefficacy of psychotherapy typically do so by employing criteria seemingly more a reflection of the behaviorist paradigm than of the personalistic paradigm. Such studies do not, to my knowledge, assess clinicians' insights and understandings as a function of theoretical persuasions. Rather, such critiques generally attempt to concoct a quantified indicator of successful-functioning behaviors.

of bookstore psychology--suggests the appeal of the personalistic world view. I cannot summarize the gospel according to the popular press, and I am aware that in these writings the reader is sometimes invited to construe himself in metaphors every bit as mechanical and unpoetic as those academic psychologists trade in, but such books as Sheehy's Passages address themselves to the concerns of persons confused about, or interested in, what it means to be a person.

This is to say, perhaps, only that the puzzles the old paradigm posed for itself are certainly posable still. And to the extent that one can accept a certain illumination of thought as a criterion of verification and validity, the insights provided by some of these writers--and Maslow and Rogers both have the readership to be included here--suggests that such puzzles are also solvable.

If the personalistic personality paradigm did not falter because of an autochthonous crisis, how is it that the behaviorist personality paradigm replaced it? The answer on one level is to be sought in transformations taking place within psychology as a whole. The 1920's were not an especially propitious time for the seeds of an independent paradigm to sprout. The revolution in personality psychology was merely one aspect of the great sweep of behaviorism. Being trained within the same institutions, a new generation of personality psychologists were

socialized into the presuppositions of the behaviorist paradigm.

Even if the new wave of personality psychologists refused to accept the reductionist notion of an empty organism, and even if they insisted upon postulating "inner" personality variables, they accepted the formulations of science itself which behaviorism brought upon us.

What I am suggesting is an invisible revolution, perhaps even a revolution from outside. By "invisible" I refer to a quite unknowing absorption of presuppositions. One example has already been given, that of the alteration of psychology's definition into the science of behavior.

For reasons having nothing to do with the success or failure of the new science of personality to solve its paradigm puzzles, I contend, the science of psychology during the 1920's and 1930's became increasingly the property of behaviorism; and the psychologists who specialized in personality, we may assume, were socialized into the same conception of science as were the psychologists who went on to study reinforcement schedules or to look for engrams.

In addition to being invisible, this revolution may be perceived as imposed from the outside--depending upon one's willingness to view the personalistic personality psychologists as actually standing apart from the rest of psychology as a community of scholars, certainly a

debatable point. If the point is granted, one could speak not of revolution of paradigms, but rather of conquest. Kuhn's evolutionary metaphor would seem in good part based upon the apparently dialectical process of a field responding to the crises its own progress generates. If the revolution be "imported," though, that dialectical process might become entirely irrelevant to the establishment of the resulting paradigm, raising questions for any a priori assertions about that new paradigm's evolutionary or adaptive superiority.

The suggestion that the revolution of paradigms in personality constitutes an "abnormal" scientific revolution, however intriguing, rests upon a moot point--is personality psychology outside the community of general psychology--and will not be developed beyond noting an especial danger of "imported" revolution. That is, an imposed revolution could easily throw a field into maladaptive relationship with its subject matter. That this might be the case in personality psychology has already been suggested by the discussion of the 'Mischel controversy'--recall the doubt cast upon the very reality of personality variables that resulted from the difficulty of reconciling them with the demands of a method based upon the prediction of behavior.

Personality psychology could, then, be depicted as a servant to two masters. On one hand are the "demands" of the subject matter, the profound complexity and depth of personhood. On the other hand are the presuppositions concerning method and science which severely restrict what can be recognized as verifiable or real. One could indeed argue that we have been forced to abandon those qualities of the subject matter not treatable by our method, and thus explain the superficiality of both current research and the diminished model of personhood it presupposes.

But as we shall see in the next chapter, the change of paradigm in personality psychology can be understood in quite different terms, terms that suggest that the current paradigm is not so entirely inappropriate to its subject matter, human personality today. Let me conclude this chapter by noting that Kuhn's formulations, which have enabled us to understand the current state of personality psychology, are suggestive in other ways as well. They offer a perspective for understanding the implications for change and are suggestive in diagnosing our present predicament as scientists and as persons. We shall turn now to a consideration of what might be done to bring personality psychology into better days.

C H A P T E R V

PERSONALITY PSYCHOLOGY AND THE MODERN WORLD:
IMPLICATIONS FOR CHANGE

Having followed the argument this far, some readers might question the necessity of employing Kuhn's model of paradigm revolution, for criticisms of psychology similar to those I am offering have been made on the basis of psychology's philosophical presuppositions alone. Many good critiques have laid the blame, as it were, upon positivism, operationalism, and realism. Consider this passage from Rychlak (1968) for example:

Idealism in the form stated at the outset of this section is, if not unheard of, then surely avoided in American psychology. This is because psychology has been dominated by the language of realism in academic circles since the days of John B. Watson (1913). Indeed, his revolt against introspectionism was in a sense a reaction of realism (out there, in the hard reality) to the prevailing idealism (in here, the mind's eye). Over succeeding generations American psychological journal articles have been primarily occupied with problems of how best to map reality "out there." Usually framed in terms of lawfulness, we have shown great concern with measurements ("Let's make our maps highly precise"), operational definitions ("Let's get as close to reality as we can"), and reductionism ("Let's start with simple maps and then work our way up"). Our "toughmindedness" is tied to our realism.

pp. 19-20

The hidden presuppositions that structure how we as

scientists perceive and research our subject matter can be discussed without employing Kuhn's specific formulation of paradigm. But Kuhn's model has implications that are convenient and insightful, and it seems to me a greater clarity concerning psychology's present situation is enabled by Kuhn's analytical system.

For the problem of transforming personality psychology, Kuhn provides an understanding of the process of change. Once we identify the problem as one of maladaptive or, at least, limiting paradigm, we are led to inquire how a field might alter its paradigm.

To those psychologists who recognize the superficiality of today's findings, who admit the triviality of the great bulk of current research, but who feel the solution lies in "better research"--curvilinear models instead of linear, larger or repeated sampling of ever-increasing numbers of variables instead of simple, before-and-after, two variable designs--Kuhn's position suggests a disappointing picture. A science's paradigm is not capable of correction by the empirical research it gives rise to. As Kuhn states, "Paradigms are not corrigible by normal science at all" (1970a; p. 122).

A paradigm whose presuppositions make the realities of mental life invisible can hardly illuminate mental

life, however refined its procedures. Earlier we discussed paradigms by developing the hypothetical example of the extreme humanist and radical behaviorist who both observe the "same" baby crying. In the case of the radical behaviorist perspective--not to be confused with the behaviorist personality paradigm--it is easy to imagine different learning theories competing to explain the organism. Increasingly sophisticated research procedures might support one over the others of such theories (we are not saying that theoretical formulations are not corrigible under normal science), but the behavioral presuppositions are never transcended. And similarly among competing humanist theories. Increasing ability to predict and control the crying behavior will never lead to the perception of trust and agency. Increasing sensitivity to issues of trust and agency will never lead to the perception of the lawfulness of operants.

The point is certainly important: a paradigm is incorrigible in terms of its own research. If the problem is a matter of the paradigm itself, continuing with normal research, or perfecting it, is rather pointless. Thus, the first recommendation to be made on the basis of this analysis is to discontinue current research practices.

If continuing with our present empirical research

will not solve the crisis, what will? How can we solve the paradigm problem? What would be ideal is a restitution of the paradigm that was pushed aside with the ascent of behaviorism. This original paradigm was never brought to full fruition. We might still learn about personality through direct observations of individuals conceived holistically. The case study method has not yet yielded all it might toward an understanding of personhood.

But how easily can a paradigm revolution be engineered through the good intentions of psychologists armed with a vision of a better science of personality? The picture seems grim. To understand how the next paradigm revolution might occur, we might look to the past for an understanding of how the last paradigm revolution occurred.

How is it that the behaviorist paradigm so thoroughly captivated the field of psychology? The question is thorny, and any attempt to answer it to the satisfaction of all is probably doomed. I should like in this section to address some issues that seem pertinent to understanding our acquisition of the behaviorist paradigm, hoping to shed light on the matter of change today.

The success of the behaviorist paradigm has been explained in terms of its promise to make psychology a valid science. Many writers have explained psychology's

espousal of behaviorism in terms of psychologists' desire to be as scientific as physicists. "For many years," according to Deese (1972), "a large and active group of psychologists worked hard to make psychology over in the image of physics" (p. 3). According to Hudson (1972):

Psychologists have a marginal position in the academic community, poised near the borderline between the humane and the scientific disciplines; we have a farouche professional past, redolent of mesmerism, even of witch doctoring; and there still exist widespread misgivings--both in academic life and in society at large--about any attempt to examine the mind's contents. Our response, professionally, has been to over-react: to observe all the outward signs of scientific respectability, taking as our model, incidentally, the Victorian conception of the physical scientist, a model that physical scientists have themselves abandoned.

p. 86

The implication that psychologists leapt to behaviorism because it assuaged their concern over not being scientific suggests an incomplete picture, for it underplays the extent to which psychologists of the introspectionist school saw themselves in their own terms as adequately scientific, and, consequently, masks the presence of a conflict over the very nature of science.

Members of both schools could in good conscience boast of scientific purity. And it is worthwhile to point out that if from the behaviorist standpoint the

introspectionists were not psychologically scientific, the introspectionists considered the behaviorists not scientifically psychological. Robert Watson (1971), the historian of psychology, gives this account of Titchener's view:

He shared Wundt's distaste for the applied aspects of psychology. Behavior is not the concern of a psychology of consciousness. If experience is the sole concern of psychology, then performance (behavior) is irrelevant. Behavior is worthy of study--as a branch of biology, not as psychology. Titchener objected to what he called "the penny-in-the-slot sort of science," in which consciousness is said to be inferred, when it was always there waiting to be interrogated....Behaviorism, which would see study of behavior as paramount, is logically irrelevant to psychology.

p. 402

Because the introspectionists could see themselves as adequately scientific, the conflict between schools was not so much one between unscientific incumbents and rigorously scientific hopefuls; rather, the conflict involved two differing views of science. The behaviorist formulation won the day, and its succession was couched in the legitimating language of greater scientific purity. But our understanding of how today's paradigm came to power will be inadequate if we leave it at the level of the desire to seem scientific. We must question the specific appeal of the behaviorist formulation of science over the pre-existing formulation.

Kuhn's discussion of the factors enabling scientists to embrace a new paradigm suggests that appeals to empirical evidence or to purity of methods do not play deciding roles, for both old and new paradigms define method and can present evidence in a way that benefits itself while damning the other:

When paradigms enter, as they must, into a debate about paradigm choice, their role is essentially circular. Each group uses its own paradigm to argue in that paradigm's defense.
1970a; p. 94

According to Kuhn, "this issue of paradigm choice can never be unequivocally settled by logic and experimentation alone" (p. 94), and even the techniques of persuasive argument must be recognized as important factors.

In this light, the behaviorist paradigm's success was possibly a function of its promise. It is difficult exactly to pinpoint the nature of this promise, but something of what I am getting at is reflected in Maslow's early enthusiasm, previously quoted, which he described in terms of "an explosion of excitement." Of course, not everyone upon encountering J.B. Watson's program dances down Fifth Avenue, but there was something about the behaviorist world view that clicked, that caught on for the time and place.

What was it about the behaviorist formulation that made it so appealing to American psychologists? How can

we understand the magnetic appeal that enabled the paradigm so strongly to establish itself that it completely dominated psychology? Was it Watson's own skill as an advertiser, or something larger than one man's compelling writing style?

Rychlak (1968) suggests one line of argument with his comment:

At heart it is the image of man which is at issue in psychology's internal conflict, let us make no mistake about that. The arguments all come down to this: How shall we theorize about the human being?

p. 2

Can we understand the popularity of the behaviorist paradigm in terms of the suitability of its image of man for the time and place, American society since the 1920's? ¹⁶

What is new about behaviorism is not the discovery per se of laws of learning nor even the application of such laws in contexts such as behavior modification; circus trainers and factory owners have successfully employed them for years. What is new is the model of the person, the image of man it presents.

Paradigms in psychology do present society with an image of man, and in order to take root, the paradigm

¹⁶I am indebted especially to Professor Howard Gadlin whose lectures in systematic psychology have suggested the general outline of this argument.

must present an image congruent with the needs and demands of the larger society. Perhaps this is only to say that the world view of the paradigm must be congruent with the larger reality manufactured by society, an argument which requires essentially the integration of the insights of Berger and Luckmann's (1967) The Social Construction of Reality with those of Kuhn.

A connection that cannot be ignored, I am suggesting, exists between the politics of paradigm change in psychology and social forces in society. The case can be made that in the period of time from, say, World War I to the present, a shift in the meaning of personhood has been mandated within American society, and this shift underlies the rise of behaviorism.

Stated simply, American society has witnessed a dwindling of the richness of the inner experience of selfhood, and this dwindling has been reflected in psychological formulations that have promoted an ever more shallow and superficial depiction of personhood.

If I may intrude upon a field not my own, this change in the quality of personal life has been suggested directly and indirectly in modern literature. Vonnegut (1975) is one writer who claims for his profession a special sensitivity to such changes:

All artists are specialized cells in a single,

huge organism, mankind. Those cells have to behave as they do, just as the cells in our hearts or our fingertips have to behave as they do.

We here are some of those specialized cells. Our purpose is to make mankind aware of itself, in all its complexity, and to dream its dreams. We have no choice in the matter.

p. 228

A succinct statement of the condition of man in modern society is given in Leonard Michaels's recent review of Peter Handke's A Moment of True Feeling, which appeared in the New York Times's Book Review recently:

The Austrian, Peter Handke, who writes poetry, plays and memoirs, is concerned with a familiar subject--the loss of authenticity or innocence. For Handke, this loss characterizes modern life. He thinks we no longer experience things directly, no longer truly feel. All our experience is mediated by cultural formulae, established ideas, cliches of language and manners. Hence, we are alienated from ourselves and left only with the knowledge that everything valuable is gone.

July 31, 1977; p. 7

Michaels's description of this subject as "familiar" would seem to speak for itself.

The sociologist Max Weber's conception of rationalization of society has provided me with a model for understanding what societal forces possibly lie behind the rise of the behaviorist paradigm. Weber, who is especially esteemed for his sociological analysis of bureaucracies, saw rationalization as a process accounting for the basic drift of Western civilization. According to Robert Nisbet

(1976), Professor in the Humanities at Columbia, Weber's concept can be summarized as follows:

Basically, rationalization is, in Weber's sense of the word, the imposition of strict means-end criteria not only upon thought itself but upon art, science, culture, government, war, even religion. It implies the exclusion from thought or act of all that is purely traditional, charismatic, or ritualistic, all, in short, that is not directly related to the means necessary to efficient realization of a given end. Since reason teaches us that the shortest distance between two points is a straight line, rationalization is the process through which we seek, as it were, a straight line, and, thereby, avoid or exclude all that is indirect or circuitous. Weber came to believe that from the late Middle Ages on, more and more areas of Western Culture, beginning with government and finance, had become subject to the canons of rationalization, thus promoting what he called, in a phrase borrowed from the poet Schiller, "the disenchantment of the world."

pp. 111-112

Weber saw in the monopolistic imposition of rational principles and means-ends relationships in the place of traditional, sacred, and folk ways a disenchantment, a loss of magic and poetry. This sense of loss of magic and poetry captures my own estimation of the changes in personhood. Rationalization has caught up with selfhood, and the rise of the behaviorist paradigm is an expression of it. What has taken place is a disenchantment of self.

Some of the early personality psychologists perceived in behaviorist psychology the connection with an

increasingly impoverished societal conception of personhood.

Consider this statement of Allport:

It is certainly unfair to blame the positivist outlook in psychology and social science for the present plight of mankind, although many critics do so. Positivism is more a reflection than a cause of the fragmentation of personality in the modern world.

1961; p. 552

Henry Murray, in several instances, has shown that he, too, was onto the scent. In his Explorations in Personality we find the following included as a footnote to his discussion of the peripheralists in psychology, those researchers in quest of data that can be cast in statistical terms:

This may be regarded, perhaps, as one of many manifestations of a general disposition which is widespread in America, namely, to regard the peripheral personality--conduct rather than inner feeling and intention--as of prime importance. Thus, we have a fabrication of a 'pleasing personality,' mail order courses in comportment, courtesy as good business, the best pressed clothes, the best barber shops, Listerine and deoderants, the contact man, friendliness without friendship, the prestige of movie stars and Big Business, quantity as an index of worth, a compulsion for fact getting, the statistical analysis of everything, questionnaires and behaviorism.

p. 9

This association of behaviorism with Listerine and deoderants, with contact men and "friendliness without friendship" so characteristic of sales relationships (and, too often, of collegial relationships in academic

departments¹⁷) bespeaks the connection between society and psychology's change of paradigm.

What I am suggesting is that American society "needed" or was ready for an image of personhood like that promoted by the behaviorist paradigm: externalistic, superficial, mindless. Let us again attend to Henry Murray (1940) on the topic:

Americans have fashioned a cosmetic culture, in which a pleasing appearance at quick contacts is the thing that counts. It pays--so we are told--to be washed, shaved, manicured, deoderized, tailored (cleanliness is next to godliness), and to smile, smile, smile (agreeableness is next to cleanliness). It is the day of Life, Click, Look, and Peek, of instantaneous effects, candid photography, voyeurism and exhibitionism. A successful personality can be bought (and paid for). The camera makes the man. If you want to be President,

¹⁷ In the fiction of Updike is a passage which, while making again the point about the change in personhood in relation to modern life, hits rather close to home:

"I have the impression, at any rate, that he, as is often the case with scientists and Midwesterners, had no use for religion, and I saw in him a typical specimen of the new human species that thrives around scientific centers, in an environment of discussion groups, outdoor exercise, and cheerful husbandry. Like those vanished gentlemen whose sexual energy was exclusively spent in brothels, these men confine their cleverness to their work, which, being in one way or another for the government, is usually secret. With their sufficient incomes, large families, Volkswagen buses, hi-fi phonographs, half-remodelled Victorian homes, and harassed, ironical wives, they seem to have solved, or dismissed, the paradox of being a thinking animal and, devoid of guilt, apparently participate not in this century but in the next." -"The Music School," p. 139.

there are agencies ready to take your picture milking Bossy and kissing chubby children, to plan your campaign and write sure-fire speeches that will please everyone (and no one). (What if the Gettysburg address had been put together on Broadway?) Our civilization is skin deep, and the best epidermis triumphs. This is all part and parcel of the race for goods, comfort, and social recognition. It is the ideology of big business, now well established in our universities: productivity en masse, the mechanical advance of mediocrity. The wheels turn and psychology is caught up: it takes its place on the assembly line. Move on there! This is no place for rumination! Get busy with the calculator and hand in your results! Who is not familiar with this treadmill? and with the deadening consequences of it? Superficiality is the great sin of American personology. It suits the tempo of the times; it suits industry and commerce; it suits our interest in appearances; it suits our boyish optimism. And it suits the good heart of America, its Rotarian solidarity, its will-to-agree, since it is easier to agree about the surface than about the depths. Perhaps there are no depths. Who knows? There are no depths. Since truth is congenial fiction, and this fiction is most congenial, this is truth. It is no mute thing that the inventor of behaviorism found his destiny in the advertising business.

p. 175

The behaviorist paradigm is, so to speak, as American as mass-produced, mass-marketed, artificially flavored apple pie.

If we accept the argument that paradigms are societally responsive, what are the implications for paradigm change today? Can we expect change to come about in response to cogent criticisms, and continually advance the publication dates of the argument first made by Allport and Murray,

then later by Maslow and, more recently still, Carlson? While the attempt is undoubtedly noble, it is, at the same time, typically frustrated. The would-be reformer is in the position of the pawn on the chess board who discovers that his fate is not, as he thought, in the hands of the king and queen, but rather in the hand behind the board.

The imagery of the chess board suggests my position that the relationship between society and psychology's paradigm is essentially one-way. As society changes, so does psychology's paradigm. Can we argue the case for a two-way relationship, wherein psychology as a field is depicted as influencing society through the production of personality models which become increasingly realized in the actual population? Does the image of man presented by academic psychology serve as an influence to create such men in the real world? Has, for example, the thinking of Skinner influenced the man on the street to regard himself in Skinnerian terms and possibly become a congeries of behavior with a reinforcement history?

The question is intriguing. My own answer is a qualified no. I do not believe that present day academic psychology is in the Mephistophelean business of creating through influence upon the public the kinds of persons that society needs. I think not, simply because the vast majority of our work exists in remotest isolation from the

everyday awareness of the general public. Most Americans have not the vaguest idea of what academic psychologists are up to.

But my "no" is qualified, for there is one group extremely prone to such influence, and they, of course, are psychologists themselves.

Unless he is extremely cynical or capable of depositing his intellectual schema at the office door when retiring from his workday, the personality psychologist inevitably employs his constructs for making sense of himself, his associates close and casual, and his world. How can it be otherwise? Like every serious scientist, he likes to believe his constructs are the best available and, so to speak, the truest. And just as a scientific discipline's basic reality derives from its paradigm presuppositions, so might it be argued that the individual's personal reality, including his inner experience as well as his conception of the meaning of being alive, similarly reflects the constructs available to him.

In the field of personality psychology today there exists an occupational hazard, and that hazard concerns the depth and quality of one's personal experience. Today's model of the person, that consortium of dispositions, either consistent or situationally specific, that soulless intersection of rating dimensions, that predictable, deter-

mined machine, threatens to overtake and become us. We run a risk of believing ourselves the experiential zombies, the emotionally shallow creatures that march over the statistics of our journals. We run a risk to the extent that we strive in our own lives to fit our personalities into the molds required to maintain our allegiance to our own modern paradigm.

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