

The Bogus Science

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Three sorts of psychological science are discussed: Science₁, a natural science with conditional (causal) laws; Science₂, with probabilistic laws; and Science₃, a "human science" trying to capture the complexity of human experience and behavior. Conceptual and empirical evidence are given to support the thesis that Science₁ and Science₂ are not sciences in the sense that they uncover stable laws. Despite this, they are still regarded as sciences for three reasons: the presence of a large professional organization committed to this position; the "empathic ethos" dedicated to bringing about social change by way of marginal "growth" institutions like the school; and the authoritative voices of eminent professionals convinced that psychology is indeed a science with secure behavioral laws. Science₃ is still in the programmatic stage and is unlikely to command much attention because it disregards forces in the milieu which influence significantly human experience and behavior. It is argued that Science₂ and Science₃ should be treated as belief systems which people may find useful for their various purposes.

L'on fournie de l'evidence pour soutenir la thèse que la psychologie n'est pas une science dans le contexte courant qu'elle fuisse découvrir des droits constants avec lesquels l'on explique à la fois le comportement humain et l'expérience. Malgré cela, la psychologie est encore vue comme une science pour trois raisons: la présence d'une grande organisation professionnelle commise à cette attitude depuis son fondement en 1894; l'existence également primitive et reliée de l'application de "mission-oriented" research, desquels les auteurs sont dédiés d'apporter le changement social par l'augmentation marginal des institutions comme l'école; et la démonstration tentative des professionnelles élevées que la psychologie est, en vérité, une science. Plusieurs soutiennent que ces raisons ne puissent pas résister la critique et la théorie "psychologie-comme-science" doit être remplacée par un corps des convictions dans lequel les étudiants sont encouragés d'accepter n'importe quelle théorie empirique qu'ils trouvent pratique pour tout but.

When the original professional psychologists declared themselves to be scientists, they forgot the imaginative and exploratory nature of much previous scientific discovery. Enthused by the salient successes of the natural sciences in the late nineteenth and early twentieth centuries (Cattell, 1966, pp. 4-5), they ignored particularly the establishing of functional relationships by tinkering with variables (Tyndall, 1961, pp. 14-15) and the use of visual models to tease out the general laws of the phenomena in question. Examples of the latter include Harvey's arriving at the concept of the heart as a hydraulic pump, Faraday's discovery of the dynamo (Williams, 1965, p. 196) and, much later, the model of the structure of the DNA molecule (Watson, 1968, p. 202). What the psychologists found was a conventional science replete with laws expressed

in mathematical terms, and they attempted to discover similar laws in psychological phenomena.

This creation by fiat of psychology as a science — labelled Science₁ here — entailed risks because there was no reason to believe that human behavior could be predicted and controlled in the same way as inanimate objects. The conventional method of arriving at the laws at the basis of prediction and control was not necessarily suitable when applied to the study of human behavior, nor were the laws themselves. Specifically, although there are many ways of defining science in general, the function of the natural scientist is to construct a scientific system from the axioms of which laws can be deduced to account for observed regularities (Harré, 1974, p. 241). The cogency and power of such a system lies in its laws, expressed in mathematical terms, which permit the control of the environment by predicting beforehand the conditions to be imposed to guarantee a desired result. Simple laws like $\text{force} = \text{mass} \times \text{acceleration}$ were to be found in all texts in the physical sciences and only hardy materialists could have assumed that human behavior was predictable and explainable in terms of Science₁. For example, Pavlov, heir to a long tradition of Russian anti-clerical materialism and full determinism (McLeish, 1975, pp. 20, 70, 83) treated psychology as a natural science by interpreting psychological phenomena in terms of physiological processes (Kaplan, 1966, pp. 345-346). For him, man was “a system — roughly speaking, a machine — like every other system subject to the inescapable and uniform laws of nature. . . .” (p. 349).

In the United States the most systematic and concrete exponent of Pavlov's views was Hull (1935, pp. 492-498; 1943, p. 17), whose influential theories were demolished by Koch in 1954 (Mills, 1978a). Skinner, who disregarded the physical and conceptual nervous systems because the study of their interactions was beyond the reach of Science₁, managed to produce the only Science₁ law known, a formula for predicting the rate at which a half-starved pigeon will peck a bar to obtain grain pellets (Ferster & Skinner, 1959).

A different kind of psychological science, labelled Science₂ here, was salient at the turn of the nineteenth century. Its protagonists, who began the mental testing movement, saw people as active searchers for, and processors of, information. Although they used a largely correlational rather than an experimental approach to the analysis of performance, they considered themselves as scientists too. Cattell (1890), working in London with Galton, was an early convert to Science₂, which appeared at that time to be a variant of Science₁.

Psychology cannot attain the certainty and exactness of the physical sciences, unless it rests on a foundation of experiment and measurement. . . . Experimental psychology is likely to take a place in the educational plan of our schools and universities. . . . It teaches accurate observation and correct reasoning in the same way as the other natural sciences. (pp. 373-377)

Implicit in this is the unusual stance that the causal (conditional) laws of Science₁ are really of a probabilistic, Science₂ sort. This Humean position was also taken by Pearson (1890), another of Galton's protégés.

... the law discovered by science introduces no element of necessity into the sequence of our sense-impressions, it merely gives a concise statement of *how* changes are taking place. That a certain sequence of events has occurred and recurred in the past is a matter of experience to which we give expression in the concept of *causation*. Science in no case can demonstrate any inherent necessity in a sequence, nor prove with absolute certainty that it must be repeated. (p. 113)

It is noteworthy that the "provable" and the "probable" have the same linguistic root. (p. 139)

If this position is accepted, then the essential methodological difference between Science₁ and Science₂ psychologists lies in the size of the errors of measurement only (and hence in the less accurate causal inferences). The trouble is that cognitive psychologists, the heirs of the mental testers, greatly prefer to do experiments which have the *éclat* of Science₁ without subscribing to the passive state of the organism which is its necessary requirement (Bruner & Olver, 1963, pp. 125-126). Moreover, Science₁ experimentation follows the practice of the physical sciences in which all elements are controlled except one which is systematically varied and a change in the dependent variable recorded. Science₂ psychologists have never managed to accomplish this feat systematically (McGuire, 1973, p. 447). Cattell (1966), a protégé of Galton (p. 23), has smoothed over the cognitive masquerade of using experiments to make Science₂ resemble Science₁.

The indispensable basis for causal inference is not manipulation but evidence for certain invariable sequential patterns established in the events observed. Evidence of this kind *happens* to come very readily as an *accompaniment* of manipulation, but manipulation itself is not part of the requirement. (p. 8)

Science₂ psychologists would do well to accept this escape route. If they do not, then people would accept their pretence of doing experiments to establish causal laws and the mechanism by means of which variable A leads to variable B, a contingency which would necessitate the existence of what they do not have: a common theoretical structure and units of measurement within which the answer to problems might lie.

Even if Cattell's solution is accepted, the absence of stable probabilistic laws in psychology is a matter of common observation. Variants of the correlational analysis preferred by Cattell violate at least some of the conditions which made them originally applicable to non-psychological data (Detterman, 1979, pp. 166-167). Moreover, they refer to a standard situation in a given milieu, not what an individual is potentially capable of doing in changed circumstances. Another very damaging criticism of techniques based on correlations (factor analysis, multivariate analysis) is that, in a critical research situation like Head Start, there is no agree-

ment except among friends about the correct statistic to use (Cohen & Garet, 1975). Finally, even the most stable correlation — that between IQ and socioeconomic status, for example — is subject to various interpretations (Firkowska, 1973; Jensen, 1972, pp. 153-154), which lead to squabbling and an accumulation of issues that are never settled, but are set aside because they become tiresome after a while (Newell, 1973).

The upshot of all this is that, since psychology as Science₁ and Science₂ appears to be ill-secured against conceptual attack, the next step is to examine empirical evidence bearing on the weaknesses of the two sorts of psychological sciences.

CONVENTIONAL PSYCHOLOGICAL SCIENCE₁ AND SCIENCE₂

Experimental Psychology

General and experimental psychologists have devised disparate and competing positions to a degree that is unknown in the theoretically more coherent physical and biological sciences (Koch, 1974, p. 24), and have often tied themselves to laboratory phenomena about which they have no stable interpretations. D. A. Allport (1975), himself an experimentalist, lists some criticisms of laboratory studies:

An uncritical or selective, or frankly cavalier attitude to experimental data; a pervasive atmosphere of special pleading; a curious parochialism in acknowledging even the existence of other workers, and other approaches to the phenomena under discussion — the near vacuum of theoretical structure within which to interrelate different sets of experimental results, or to direct search for significant new phenomena. (Allport, 1975, p. 152)

This is due to the lack of realism that results from experimental methods: Independent variables never switch with dependent variables (as they often do in interpersonal situations) and the conditions manipulated bear no relation to those of the context in which people must operate (Argyris, 1975, p. 475).

But what of Skinner, the most deceptively simple of biological scientists? Worrisome is the marked change in his theories or laws over time (Mills, 1978a, 1978b) and their inconsistencies as revealed by iconoclastic observers (Herrenstein, 1977a). For example, full determinism, prediction, and control were always characteristic of Skinner's later scientific formulation (Skinner, 1953, p. 6, p. 14) until recently (Skinner, 1974, p. 21). Again, "genetic endowment," discounted by Skinner in 1953, is accepted in his later work (p. 188), and some former followers have now agreed about scientific respectability of cognitive variables like "consciousness," "expectation," and "self" (Bandura, 1977). It may be argued in Skinner's favor that a rough approximation to Science₁ is his law of reinforcement, but this has come under severe attack by former adherents (Breland & Breland, 1961; Bandura, 1977; Herrenstein, 1977a, 1977b) against whose strictures Skinner has no defence (Skinner, 1977).

Social Psychology

Social psychologists are also concerned about their lack of contribution to psychology as a science and to the solution of social problems (Elms, 1975). For example, Silverman (1978), dealing with studies investigating the influence of violence in the media — scarcely a problem of great magnitude now (Constantineau, 1978) — comments on social psychologists' "short-term, low impact experiments yielding an array of inconsistent and contradictory results" (Silverman, 1978, p. 356). In an elegant paper McGuire (1973) admits these defects. He traces them to the use of the hypothetico-deductive method beloved by Science₁ social psychologists, and to their desire to support any hypothesis in which they have invested time and money, a desire that is usually satisfied if they face data (printouts) rather than people.

Developmental Psychology

There have been no breakthroughs in developmental psychology, "for the problems on which society wants help are ones that represent massive difficulties in the relationship between people . . . or between individuals and the social system" (Sears, 1975, p. 67). Even Piaget, whose longevity, productivity and general intellectual skills have allowed him to shake off cogent early critics like Hazlitt (1930), is now under heavy siege (Siegel & Brainerd, 1978) and has admitted that his developmental theorizing has a more restricted range of applicability than he had earlier claimed (Piaget, 1972). An iconoclast like Sears is reduced to believing that cumulative research during the last 50 years supporting praise rather than punishment has resulted in "a widespread increase in children's liking for schoolwork and in their achievement" (Sears, 1975, p. 67). Unsurprisingly, no evidence for this is cited. Again, Sears goes the length of supporting "categorically" the idea that a child deprived of parental affection between six months and two years will grow up to be irreversibly harmed in some way as an adult (p. 68). Unfortunately for most developmental psychologists, this idea that nourished them for many years is now known to be untrue (Clarke & Clarke, 1976). To people who argue that parents have brought up children very well in the past without any help from psychologists, the counter has always been that things would have gone better with a dash of the science of developmental psychology. Such a riposte is no longer possible.

Affective Psychology

The bewildering number of personality theories and concepts and the unstable findings place this topic beyond the range of a scientific sub-discipline (Phares & Lamiell, 1977, p. 133). Psychotherapy is in a similar situation (Drummond, 1980); its one stable finding, the significant and sizeable relationship between social class and mental illness of all sorts,

has been ignored by most salient theorists. Behavior therapies no longer enjoy their favorable press (Lazarus, 1976). Certainly behavior modification therapy has been much more successful than any of its rivals. However, no one knows why it is successful; is it simply the administration of the reinforcer, the person's expectation of reinforcement (Bandura, 1977), or the reinforcing model, usually described in non-Skinnerian terms?

Indeed, the whole concept of "mental illness" is suspect. The "medical model" of this illness has been indicted by Sarbin (1967) because of the fallacious logic on which it is based. Amusing examples have been recorded of the conceptually fragile but politically potent diagnostic repertoire of clinicians. For example, Rosenhan (1973) reports some observations made by eight normal people who gained admittance to 12 psychiatric hospitals of various sorts by pretending to hear noises. Diagnosed as schizophrenic, they behaved normally in the hospitals for an average of 19 days. They discovered that their fellow patients were better able to detect their sanity than the staff whose perception of the pseudopatients' behavior was shaped by the diagnostic label. They also found that, on discharge, the label persisted and they were declared to be "in remission" of their schizophrenia. Informed of these findings, the staff members of another hospital doubted whether Rosenhan could slip through more pseudopatients. A significant decrease in admissions to that hospital followed, although he never repeated the stunt. Since "mental illness" is declared to be present when behavior is seen to deviate from certain psychosocial, ethical, or legal norms, it is reasonable to assume that much of the work of clinical staff is to "warehouse" people who are a nuisance to the existing social order (Hurvitz, 1973, p. 235).

Educational Psychology and Research

Educational psychology and research, long regarded in North America as the vehicle for social change (Ladd, 1894, p. 19), comprise a disaster area. Grinder (1978) remarks that "educational psychology . . . is a reality of irreconcilable differences" (1978, p. 289), and Jackson (1978) is as upset about educational research. In fact educational policy is never established or changed as a result of psychological contributions or any other empirical research, because policy in the form of ideas consists of selected information "given meaning and charged with social purpose by larger ideas, assumptions and political judgements which [are] not empirical in nature" (Cohen & Garet, p. 21).

DEFENCE MECHANISMS

Why, then, do Koch's (1974, p. 26) "sub-cartels of knowledge" not merely survive but actually thrive? A number of reasons, not usually mentioned, are worth considering.

Institutional Forces

First, then, consider the momentum of a large organized group of conventionally trained psychologists who are not about to admit that their training is theoretically or professionally pointless. Their number increased greatly after the Second World War when they accepted the role of social technocrats, potentially capable of solving social problems as successfully as scientists and technologists dealt with inanimate matter (Hodgson, 1976, p. 76). The sudden flow of research grants — Koch (1974, p. 21) puts the 1967 figure at \$326 million — had unfortunate consequences, principal among which is the fact that once a body of social knowledge has been established, falsification, however justified, becomes difficult because editors and their consultants often turn aside negative results in order to protect bodies of findings and interpretations in which they have a vested interest (Cartwright, 1973, p. 229; Entwhistle, 1972, p. 385). Another irritant is that researchers choose the sample, the data, and the conditions of the experiment so that they are spared the embarrassment of reporting negative results (McGuire, 1973, p. 449).

The Empathic Ethos

A second reason why psychologists still thrive is that they have ridden to eminence on the back of what Sears (1975, pp. 5-6) labels "the empathic ethos" directed towards helping "unprivileged others." After the demise of oppressive feudal institutions, the salience of exploited groups such as slaves, peasants, the poor, and (rather later) women touched off this "empathic ethos," most obviously in Marx and Engels, who had very definite revolutionary plans for destroying the political and economic conditions which they considered responsible for the presence of these groups. In America, however, these plans were forestalled by allowing the ethos to operate fully in the harmless arena of educational and therapeutic institutions in which a child's "potential" or "growth" could be developed. Dewey's account of this theme is described by Hofstadter (1962, p. 363).

The ethos represents an unexamined stance (Crawford, 1979), desirable in the abstract (few want unjustified discrimination by reason of sex, color, class, or creed) but awkward to implement concretely because such discrimination is embedded in social institutions which control people more powerfully than do schools.

Authoritative Voices

A third reason for the continued existence of subcartels of psychological knowledge is the presence of authoritative voices reminding psychologists that they are committed to the establishment of a "pure science" of behavior providing laws from which they can derive explanations of

extra-laboratory phenomena. For example, Atkinson (1977) concentrates on "our role as scientists. . . . The psychologist's job as a scientist is to search for data, principles, and laws that enlarge our understanding of psychological phenomena" (Atkinson, 1977, p. 207). The problem is: Whose data, whose principles, whose laws? Atkinson and Hebb (1974) are reduced to producing lists of findings and theories which are very different but equally unconvincing. McLeish's (1963) plan is to award favorite psychologists the same status as that occupied by eminent natural scientists. In his salad days he considered Pavlov "one of the great figures of world science" (McLeish, 1963, p. 185), on a plane with Newton and Darwin (p. 185). He regarded it as highly "unlikely that a crucial experiment will falsify such a theory" (p. 194), forgetting the work of Zener (1937), who showed that a conditioned dog will, if unharnessed, cease to salivate on the presentation of the conditioned stimulus and will move towards the pan in which its food was normally kept. There is also the fact that Pavlov's theory of brain functioning is incorrect — indeed was at odds with what was known of neurophysiology in Pavlov's own day (Gray, 1979, pp. 102-103). What appears to have led McLeish astray is his perception of psychology as a science that would incorporate economically "thousands of [presumably stable] empirical observations about the behavior of animals and human beings" (McLeish, p. 194), rather than as a way of understanding and possibly helping people.

Not surprisingly, McLeish's contemporary hero is Skinner, "the most eminent psychologist at the present time" (McLeish, 1976, p. 70) and on a par with Newton (p. 69). According to McLeish, Skinner "seeks to improve human action by a revised structure of society based on a rational system of rewards and punishment" (p. 81). However, Platt's (1973) much modified Skinnerian plan to reach that goal is not even plausible on paper because of his refusal to take into account the barrier of social (mainly economic) forces. If, with Petrinovich (1979), we see much of psychology as "behavior in a context" (p. 373), then an account must be given of the significant variables in that context; Skinner and his associates fail to do that. McLeish's dilemma is understandable. He contends that in the long run "the scientific picture of the universe is simpler, easier to understand, is more general in its content — than any other picture" (1965, p. 5). This is true, but it does not mean that the study of people, whose predilection for making value-judgments makes them substantially different from the material "universe," will become part of this "scientific picture," Science₁.

SCIENCE₂: THE HUMAN SCIENCE

Exponents of psychology as Science₂, Harré and Secord (1972) in Britain and Giorgi (1970) in the United States, differ in many ways but they join in disliking the application of the methods and approaches of the

natural sciences to human behavior or "experience-behavior dialectics" (Giorgi, 1970, pp. 194-195). For these authors, Science₁ (and their Science₂ counterparts using experimental methods, especially in a laboratory or quasi-laboratory situation) are tarred with the same brush. Both are testing hypotheses by the systematic manipulation of variables in a setting in which the subjects are restricted or controlled in some sense. In Milgram's (1974) studies of obedience to authority, for example, the overt meaning of the experiment was controlled by Milgram's deceitful instructions which appear to have the same effect as Pavlov's apparatus on his dogs (Osgood, 1953, p. 405). Although Science₂ psychologists never accept the behaviorists' position that the organism is a passive respondent to the stimulus, it has been argued by Argyris (1975) that their common use of the experimental method drives them into the same position of a unidirectional approach to causality and, by implication, into accepting the responses of the organism as relatively restricted. "In fact, the very capacities to behave in certain ways may be created or abrogated by the laboratory situation" (Harré & Secord, p. 48).

Furthermore, since Science₃ protagonists dislike the "reductionist" approach of the natural science they are forced to describe "human science" as some understanding of the meaning of the total human "experiential behavior dialectics." Here the two groups part company. The British group describes these dialectics in terms of a list of variables: the presence of many "social selves" directing and controlling behaviors (p. 93) and the "open souls doctrine" which means that the "phenomena which people purport to report both *really* exist and *are relevant*" (p. 105), an idea that would have irritated Freud. Finally, they describe their approach to people in a simpler and less specific way than Giorgi. "We should treat people, for scientific purposes, *as if they were human beings*, as we know and understand them in everyday life" (p. 87).

Although Goirgi (1970) admits that he has no precise definition of human nature (p. 201), he knows what the "human sciences" are supposed to be studying. His list of terms includes

meaning, description, qualitative differences, the process of explication, investigating intentional relations, dealing with human phenomena in a human sense and in a human way, articulating the phenomena of consciousness and behavior, within the context of a broadened conception of nature, and assuming the privileged position of the life-world, the primacy of relations and the presence of an involved scientist. (p. 205)

The problem here is that since the proponents of Science₃ can describe their "person-oriented" but theoretically unrealized approach to people as a "science," a much preferred honorific label, there is no clear criterion for distinguishing between what should be evaluated in terms of Science₁, Science₂, and Science₃ and what should be relegated to the realm of unreachable private events. This dilemma is complicated by the fact that people with a limited capacity for processing, storing, and retrieving

information desire explanations and remedies. Since these should be more professional and shrewd than those of popular columnists, what is the "crass psychologist" (Bruner, 1979, p. 1590) to do?

HUMBLE SOLUTIONS

First, it is important not to abandon Science₁, especially in areas like psychophysiology. The brain is a physical system and the study of its psychological correlates should be encouraged despite the psychological (Gregory, 1962) and philosophical (Hearnshaw, 1979, pp. 378-379) problems entailed in the study of brain-behavior relationships. This bafflement may be only temporary: one never knows what will be round the corner. Likewise, the successes of behavior modification should encourage a willingness to contemplate the occasional value of Skinner's non-algebraic laws in understanding people.

Second, although the findings stemming from Science₂ experiments and correlational analyses are practically valueless, they are better than no findings at all. The trouble begins when the laws are given a strong psychological interpretation which so disregards environmental variables that negative results are inexplicable. Head Start, for example, was backed by psychological research, especially Hunt's (1961) contention that "intellectual capacity" (p. 362) could be improved during the early years of childhood with an accompanying "substantially higher level of intellectual capacity" (p. 363). Since Head Start has failed to bring about this desired change, Hunt must now recite his old research and conclude that the Head Start programs were insufficiently "enriched" (Pines, 1979, p. 67) and that poor mothers could develop the "intellectual capacity" of their child if they started "right at the lying-in hospital" (p. 64).

The difficulty has been pointed out by McGuire (p. 449); the author is convinced of the rightness of his thesis, regardless of a variable like the anti-educational experience of the underprivileged child. If, then, the thesis is not corroborated, there must be something wrong with the conditions affecting the accuracy of the thesis. Head Start programs must be made more appropriate; the mother must start the enrichment process from the first day of the child's life and carry on until some highly educational day-care service takes over. No thought is given to the unrealistic nature of the nostrums, particularly to questions like how a poor mother is going to start the enriched program, who is going to find the day-care centres and how the encroaching anti-educational experience will be fended off.

This impasse could have been avoided if psychologists preaching the value of one-shot inoculation against the milieu had been ignored. An example of this tactic is the work of Smith and Jones (1975), who tried to find out whether school performance could be improved by a variety

of preschool programs, parental involvement, and the development of a follow-up program. They discovered that when one language enrichment program had been carried on for an entire year of preschool education and then removed, the performance of the experimental group collapsed to that of the control group within two years. They conclude that "the disadvantaged are those who have lost out in the struggle for resources" (p. 237) and that this condition will militate against adequate school performance unless — and this is an unlikely, costly event — the enriched program is carried out throughout the school years. That is, parents who are under pressure to cope with housing, unemployment, and medical problems are unlikely to have any time and energy left over to worry about their children's attainment in school. The rule must be that if education is going to improve the lot of slum children, then social and economic conditions ought to be improved first.

Third, Canadian educational researchers should ignore psychology in favor of becoming specific with regard to the educational aims accepted by the authorities in question, to the educational programs likely to realize these aims, and to the measurement criteria regarded generally as appropriate. For example, far too much time has been spent on the American fad of discovering the earliest time when a concept can be learned by a child; this feat may bear only a negligible relationship to the total learning of the older pupil. Researchers and thinkers should strive to avoid the errors and contradictions embedded in easily forgotten provincial documents, to ignore vacuous questions like "What is the optimum class size?" and to be enterprising enough to try out and advance new ideas which pay attention to pupils as not altogether man-machine systems. The new minicomputers will certainly alter significantly much professional training, but it is far too soon to announce that future teachers will be merely tenders of educational hardware because not all students will necessarily be any more keen to learn from hardware than from teachers. If past experience is a useful guide, the failure of this updated electronic gadgetry to improve school performance is predictable.

Finally, what can be done about Sciences? It would be easy to dismiss it for what it is, an untested (and probably untestable) moving of words around until they make people, especially its users, happy. The question rising early in the friendliest mind must point to its unrealistic nature as a series of propositions about people, now or in the foreseeable future. What does the word "human" mean embedded in phrases like "treating people as if they were human beings" and "dealing with human phenomena in a human sense and in a human way"? What of people living at or below the poverty line (Adams, 1971)? What about the treatment meted out to coal-miners (Candill, 1977), to prisoners (Abbott, 1980), and to those unfortunate enough to be locked up in mental hospitals (Rosenhan, 1973, pp. 254-257)? What of the many people whose skills are being automated out of existence? How would the "human scientists"

deal with Bettelheim's (1961) contention that living in a metropolis resembles living in a concentration camp, or Jacoby's (1975) conclusion that "human relations" have yet to be realized for many people in contemporary existence?

However, this is brutal and unfair to Science₃ psychologists because their Science₁ and Science₂ colleagues, who rarely use the word "human" except in an abstract way, have never even faced these questions. More to the point, some psychologists are enthusiastic about Science₃, while Science₁ and Science₂ command respect often because of their institutional support. A decent compromise would be to consider Science₂ and Science₃ as belief systems, any combination of which might be informative and useful on certain occasions. In the realm of theory, no author should lay claim to a greater scientific status than others, but the belief system must be capable of being replicated by other competent researchers. Again, since most psychologists have made perceptive observations about behavior which have a circumscribed validity (Rapoport, 1978/79, p. 660), there is good reason why different theories as belief systems should coexist in the repertoire of the helping professions. For example, behavior modification, useful for dealing with those who have no degrees of freedom (mental retardates, seniles, and so on), is of limited value when teachers have to face awkward troublemakers who are hostile to learning in school and reluctant to respect even the best-intentioned authority (Ayllon & Roberts, 1974).

An additional advantage of seeing contemporary psychology not as a science but in terms of belief systems is its consistency with the idea that the model, rather than the theory, is the agency of any change that may take place in the interpersonal situation (Drummond, 1978). Correlatively, the education of teachers might well become largely the selection of the right kind of teachers in the first place, rather than the giving of quickly forgotten lectures.

For far too long psychologists have studied at length statistics, physiology, computer programming and anything that looks scientific to the unquestioning eye, despite the fact that they have produced nothing of consequence and are derided by natural scientists. It is time to restore this imbalance by encouraging psychologists to be widely read, knowledgeable and literate students of humane and cultural studies. This is not to deny that there is some professional value in having a nodding acquaintance with statistics, etc. Rather it is to assert that substantial contributions to the understanding of people can be made without the student or researcher being bogged down by perennially attractive methodological and conceptual morasses (Koch, 1978, p. 638; MacRae, 1974, p. 84).

Of course, a dedication to belief systems requires Science₃ psychologists to take a more benignly critical approach to their Science₁ and Science₂ counterparts who are not evil or misguided people. In a belief systems approach to people, there is no room for Harré and Secord's (1972, p.

78) castigation of Science₁ as a "mechanical model" inappropriate to the study of human beings. Many psychologists like that model and find that, for some reason, its therapeutic offshoot works. Why then compare the "mechanical model" as less appropriate to the human situation than Harré and Secord's "anthropological model" or Giorgi's "human science" approach? Why not just accept the two as different belief systems to be discussed and disagreed about? Any suggestion that this may lead to chaos can be offset by pointing to the more inflamed chaos that currently exists, together with the cynicism, nicely expressed by McGuire (1973), about the value of psychology as a way of understanding people.

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