

Political and Cognitive Structures Underlying Scientific Inquiry in the University: The Challenge to Educational Researchers

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ABSTRACT

There is a disabling avalanche of scientific production which has overtaken most students of the behavioral sciences. Though science is advanced by this production, much of it is seen to be of marginal value. This has caused some disenchantment among university students with psychology-based research. To understand the sources of this problem, several phenomena are re-examined: (a) the functional autonomy of research paradigms and their assumptive justifications, (b) the failure to discard them when their dysfunction interferes with inquiry directed to solving pressing social problems, and (c) the intersection of politics, academic policies, and the reward structures woven into publication and research networks. The challenge to university researchers, among others, that these conditions impose, are assessed, and suggestions for countering them are presented.

RÉSUMÉ

L'hypertrophie de la production scientifique dans les sciences du comportement semble avoir créé un climat de désenchantement et d'inutilité par rapport à la recherche. Pour comprendre cette situation, les phénomènes suivants sont analysés: (a) l'autonomie fonctionnelle et les postulats explicatifs des schèmes de recherche, (b) la résistance et la durabilité des modèles de recherche, (c) les contraintes politiques et socio-économiques de la recherche et de la publication. Les implications de cette situation en particulier dans les milieux académiques sont abordées. Des propositions et des suggestions susceptibles d'améliorer un développement fidèle et valide de la recherche sont présentées.

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There has been a great deal of disenchantment in the past decade among some psychologists about the troubled state of investigation in their respective sub-disciplines. Much research gives the impression of meticulous planning and execution but conceals a poverty of meaning and conceptual rigour – (e.g., Goldman 1976; 1979; Koch, 1981; Resnikoff, Tinsley, Sperry, & Schmidt, 1978; Wachtel, 1980). If one restricts oneself to a consideration of the applied domains, the malaise is even more acute. For example, the fundamental question of which psychotherapies, if any, do any good by virtue of the modalities specific to their treatment approach still haunts that domain. “After decades of research, the amount of well-established, clinically relevant knowledge about psychotherapeutic outcome still remains disappointingly meagre” (Frank, 1982, p. 281). This example, from one applied field of psychology, is not an isolated one. And we suspect that the deficits, distortions, and lacunae inherent to this field are related to an underpinning research ethic and are characteristic of the larger canvas of investigative and scientific activity in the behavioural sciences.

It is not that insufficient research is being done – it can be argued that too much is being done. Rather, it is that the wrong kind of research is being done. The researchers’ plaint that their findings “have not impacted sufficiently on the practitioner or on the policy maker” (Parloff, 1979) should be expressed with relief rather than regret, for we have little certitude that the floods of research directed towards the public, but rarely reaching it, truly speak to society’s needs and well-being. They may simply speak to the apparent futility of meeting those needs with the paradigms at our disposal. Rather than pursue the ever-receding goals of a troubled society with “more-of-the-same” research, it would be to our advantage to examine the cognitive and social structures that have locked us into futile patterns of investigation.

Secondary Schools in North America

The arguments developed in the rest of this paper can be applied to numerous, in some cases all, scientific disciplines, but we will restrict our focus for the purposes of this paper to the field of educational psychology. And as a backdrop for an examination of the constraints which operate there, it may be useful, by way of example, to look at one area of major concern – our North American secondary-school systems – and to explicate some of the presuppositions that shape our problem-solving there.

The evolution of mankind over millions of years has left it, heretofore, admirably equipped to extract a living from the crust of this planet and to protect itself from the assault of the elements as well as from its natural enemies. The human organism with its adaptive intellective and psychomotoric capabilities has indeed thrived as it has asserted its mastery over the earth and all infra-human species (with some notable exceptions like the cockroach, for example, and a host of microorganisms).

If we simply reflect on the history of human beings in North America, for example, over the past few centuries, we readily conclude that the qualities that were needed to forge the modern states of Canada and the United States are not those needed in 1983. The stamina, the muscular robustness, the physical vigor, the psychomotor and sensory resources, (as well as an informed pragmatism) necessary to build transcontinental railroads, brings forestlands under cultivation, build cities, highways, dams, and levees, and the massive infrastructure of an industrial society are no longer what is needed. The adolescent grandchildren of the people who accomplished these feats are now sitting in classrooms, relatively immobilized for hours on end, pushing pencils, turning pages, and manipulating nothing more resistant than the keys of a personal computer. History has played a nasty trick on legions of these children, many of them stigmatized in this sedentary environment as temperamentally difficult and "hyperactive". Are they not youths who would have been the restless, tireless pioneers, trailblazers, voyageurs, sodbusters, and cattlemen of yesteryear? Those near-adults seem the natural roleplayers for the legends and sagas which are part of the historical fabric of our societies. They now sit during the vibrant, expansive, exuberant years of their adolescence, dysphoric, counting the minutes until they can leave the confinement of their classrooms and run out onto the ballfield – or do anything that allows them to use the resources of their bodies in a more balanced mix with the resources of their minds. This is the backdrop for one of the tragedies of contemporary education, tragedy by virtue of its debilitating impact on the psychosocial development of the adolescent, and by virtue of the large numbers afflicted.

The assumption that universal secondary education is not only an ideological desideratum but that cognitive access to anything taught in our high schools is a real possibility for anyone who wishes it, is regnant and inviolable. That double assumption needs to be re-examined as does the "associationist" learning paradigm which is its bulwark. We have convinced our adolescents (and their parents) that success in life is predicated on a good high school education. But for innumerable reasons that have nothing to do with teaching methods, we are still not sure that the minimum intellectual demands fall within the capability of many of them at the time that they are exposed to it. Hundreds of thousands of youngsters are dropouts from high schools, not only because that institutional ambience is not temperamentally congenial to them, but because, given their individual developmental schedules, they do not have the cognitive ability to succeed *there*. There is a remarkable asynchrony between the cognitive tasks they are asked to perform and their cognitive readiness to address themselves to such tasks.

Karplus and Peterson (1970) did a remarkable study which involved testing 727 students from suburban and inner-city schools on their use of proportional reasoning in arithmetic. They found that 94% of the suburban children they tested did not reason proportionally in grade six; 68% did not in grades eight through ten; and 20% did not in grades eleven and twelve. The corresponding figures for inner-city areas were more disturbing: in grades eight through ten 95% did not

reason with proportions, and 91% did not do so in their last two years of high school. The investigators arrived at these results by giving the children a problem in proportional thinking (using buttons and paper clips for measuring stick figures) that assessed skills characteristic of those required in certain curricula of the secondary school. These findings are by no means unique (e.g., Wollman & Karplus, 1974; Chapman, 1975).

This is an important matter since the ability to use such reasoning is critical to success not only in mathematics but in other scientific disciplines. And given its cognitive relatedness to analogic thinking, there are few areas in the typical high school curriculum that do not require rather sophisticated levels of this ability for success. There are, of course, other formal operations such as Linnaean-style classifications, the construction of hypotheses, combinatorial strategies, and innumerable logical principles which, if one cannot use them, at least in an inchoate way, will ensure that high school proves to be a series of failure experiences and a torment without end.

The reasons for deficits in these areas are complex (Chapman, 1975); they are maturational, socio-economic, genetic, nutritional-medical. But no matter what the cause, a massive social operation such as a secondary school education system that forces children into our procrustean "curriculum and instruction" beds can only be the cause of significant levels of failure. The interesting and disturbing feature in this is less in the fact that hundreds of thousands, if not millions, of adolescent children in North America lack the essential cognitive skills to understand the material delivered in their classes; it is more in the fact that they are spending years of their young lives "learning" that they cannot learn, that they are hopelessly incompetent in the domains that a dominant culture has convinced them are absolutely necessary to success. The subjective experience of spending hundreds of hours in classes where the content of the instruction is literally incomprehensible to them can, over the years, only be damaging to them. They while away those precious hours ruminating of ways to salvage some few shreds of self-respect. Even dropping out and pumping gas can be an appealing, if not irresistible, prospect by comparison with the numbing experience of incessant, unrelieved failure and boredom.

This has long had the earmarks of a tragedy of epidemic and vast proportions, one of our own making and, given our assumptions, a tragedy, we believe, without solution. Given a political credo which affirms not simply that everyone has the right to a secondary school education such as it exists, but that everyone has potential cognitive access to every discipline regardless of the cognitive processes that underpin its exercise, a question needs to be answered: what provision are we making for the millions of adolescents who do not fit our assumptions?

There are many and complex reasons for explaining the stability of educational and other social institutions that have reached critical levels of dysfunction. The assumptions and paradigms which underpin them remain unshaken even when the anomalies and counterindications multiply at an accelerating rate. The paradoxical aspect of this is that scientific research more often than not advances the

misconceptions which promote the continuation of our problems. Some of the reasons for this are principally psychological, rooted in the inherent conservatism of all disciplines. Others are exquisitely political, determined by more or less stable institutional structures, regional as well as national, representing entrenched ideologies. The following sections of this paper provide a re-examination of these issues and the underpinning paradigms which frustrate their resolution. Let us look first at perseveration in superannuated paradigms.

Disciplines and Functional Autonomy

Feyerabend (1970) has promoted the notion that a theory should be retained "even if there are data which are inconsistent with it" (p. 203). He has called this notion the *principle of tenacity*. Theories, he affirms, are capable of development. They can be improved, and "they can eventually be able to accommodate the very same difficulties they were incapable of explaining" (p. 204). This, it seems, is the perennial hope of every theoretician and scientist whose professional life is deeply involved with a theory in 'Big Trouble'.

The human animal, not excluding the scientist, is an inveterate proselytizer. He is not content simply to discover the "truth". He feels a compulsion to disseminate it (else why must he cry out his "Eurekas" to the world). Further, he seems distressed when others do not share his belief in it. He is not less distressed, it seems, when he is propagating, not a scientific discovery, but a theoretical paradigm. François Jacob (cited in Monod, 1971, p. 20), a Nobel laureate in biology, said, "The dream of every cell is to become two cells". The dream of every researcher, we theorize, is to recruit, to become two, four, eight of himself or herself. It is a cloning propensity that is given ample scope for actualization in educational psychology as in other scientific fields.

Once a major theory has become entrenched and has a committed cohort of adherents, it is only able to be disestablished with great difficulty, if at all. When a scientist or a professional has invested 10 or 15 years of his life in a belief system, and he has, in effect, come to regard the assumptions of that system as laws of nature, he cannot easily be convinced that he has been in error (Boring, 1964). In matters of little importance if adaptive responses of individuals continue long after they have ceased serving a useful purpose, they are simply considered quirks. But it is not a laughing matter when professions founded on science perpetuate discredited notions. Perseverating in obsolete practices is known to have occurred, and continues to be evident, in the helping professions – with great harm to those who have sought help as clients, patients, students (cf. Koch, 1980).

Relative to this issue, Max Planck (1949) stated that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it" (pp. 33-34). A corollary of this is that proponents of an old doctrine do not abandon it by admitting the validity of their adversaries' criticisms. Depending on their level of commitment to it – and in any case there are wide individual

differences in the cognitive flexibility of everyone involved – they either entrench themselves more deeply or they begin to make gradual, almost imperceptible shifts toward the position of their rivals (Birk & Brinkley-Birk, 1974).

The field of medicine provides many examples of the above, and some with tragic consequences. One of the most instructive for all professionals revolves about the 19th century figure of Ignaz Semmelweiss, patron saint of modern infection control practitioners. Semmelweiss, a Viennese physician, is renowned for his investigations of the high levels of infant and maternal mortality that prevailed in the hospitals of that city. He observed that parturient women were getting infected in the hospital and dying at rates that stupefied him. His research in the matter led him to the conviction that the incidence of these puerperal diseases was correlated with the practice in which physicians went directly from autopsy rooms and other centers of contagion into the maternity wards, without washing their hands. He was fiercely attacked by his fellow-physicians for propagating these findings. This pioneer in antiseptic procedure, tormented by the continuing high deathrates of childbearing women, slid into mental illness from which he never recovered. It was only after a prolonged struggle within the medical establishment and the compelling studies of Lister that basic antiseptic procedure became accepted, and the norm.

Rachman and Wilson (1980) have recounted for us the “cautionary tale of the rise and fall of insulin therapy” (pp. 13-16). This “misguided therapy” disappeared from the psychiatric scene in the ‘60s not, apparently, because it was admitted to be ineffective, which it evidently is, but because a generation of powerful psychotropic drugs – principally the tranquilizers – were seen to be more effective.

The disquieting feature of this history is that insulin coma “therapy”, per se, was never adequately demonstrated to be of help in the treatment of schizophrenia (Kalinowski & Hippus, 1969), although it had been demonstrated to have deleterious side effects. Nevertheless, it was used by medical practitioners and promoted by scholars, all of whom, presumably, had undergone rigorous and scientific training. The failure to apply the principles of scientific evaluation to our own remedies, on the one hand, and on the other the refusal to acknowledge the cogent and powerful criticisms of those who have tested those remedies, demonstrated once again how difficult it is to separate indoctrinated scientists and professionals from their mindsets and habitual practices. The praxis and theory of professionals form belief systems and as such, even when they become superannuated they do not die, they just fade away.

Relative to the principle of tenacity elaborated by Feyerabend, neither educators nor psychologists need to be exhorted to embrace this principle. This is analogous to training monkeys to climb trees. There has always been altogether too much of this going on. As Lakatos (1970) put it, “Given sufficient imagination, any theory ... can be permanently saved from refutation by some suitable adjustment in the background knowledge in which it is embedded” (p. 184). This is another expression of the Duhem-Quine thesis: in the face of incontrovertible evidence that

appears to discredit a system one can (and is inclined to) continue to make adjustments in it, at least to the extent necessary to save it from absurdity.

A similar observation has been provided by Watzlawick (1976, pp. 49-51). In discussing a noncontingent-reward experiment conducted by Bavelas, he concluded "that once a tentative explanation has taken hold of our minds, information to the contrary may produce, not corrections" but a more and more complex and elaborated explanation. The explanation becomes self-sealing and irrefutable no matter how groundless it is. "Intellectual honesty does not consist in trying to entrench or establish one's position by proving (or probabilifying) it...intellectual honesty consists rather in specifying precisely the conditions under which one is willing to give up one's position" (Lakatos, 1970, p. 92) and try something different. If, for example, Freud and orthodox Freudians had spent less time excommunicating dissidents and otherwise protecting their system from change, more time, in short, trying to disconfirm their tenets, the psychotherapeutic enterprise might be more advanced than it is at present.

Research normally arises in the context of perceived contemporary needs. As needs change, pressure correspondingly builds up to change research priorities. Unfortunately scientists, still less practitioners and educators, do not respond with alacrity except within the constraints of their own ideological premises (e.g., Mahoney, 1976, pp. 116-117). The shifts in research resources necessitated by need and problem shifts entail organizational and logistical difficulties which are barely manageable in modern democratic societies. But as Shadish (1984) has cogently argued, research which is not in synchrony with contemporary political and ideological trends still needs to be done if only so that broadscale innovations can be implemented should a favorable and unanticipated opportunity present itself. The alternative to this is that one is "constrained to work within the flaws of existing structures and ideologies, searching mostly for incremental, technical changes (p. 735)". This point leads to a consideration of some political realities and the kinds of research it promotes.

The Politics of Research

The notion that most scientific research is a highly politicized endeavor is widely accepted. Among the more modern and cogent statements of that principle is that of Karl Mannheim (1936) who argued that no idea is impervious to the ideological currents prevalent in the society in which it has arisen. A good contemporary but tendentious treatment of the problems arising from the socio-political matrix of science is that of Knorr-Cetina (1981). She rehearsed the argument that the discovery of principles in science is distinct from the process of validating those principles. The community of our peers is, alone, the entity that can turn the most brilliant of discoveries into acceptable ones, if not resounding ones. "If we look at the process of knowledge production in sufficient detail", she states, "it turns out that scientists constantly relate their decisions...to the expected response of specific members of this community of validators or to the dictates of the journal in which they wish to publish" (p. 7). However, the scholars who innovate are among

the scholars who validate. Since they form communities whose principal public behavior is the communication of their own ideas, one can sense how complex and political is this influencing process. It resembles vast, fluid, constantly shifting magnetic fields.

Let us consider for a moment, on a macro-level, the political implications of the choice of a research problem, a choice which, far more than ability or native intelligence, would appear to separate out the unsuccessful researchers from the successful ones. Although the political engines of research are always turning, it is in times of social and political turmoil that they take on visibility and high relief. We are working in such times. A perusal of issues of the American Psychological Association's (APA) *Monitor* of recent years alone can demonstrate the preoccupation of the psychological community with sources of funding, especially federal funding. One finds innumerable allusions to the changing priorities of the public agencies that support private research, as well as the priorities of the ideologies that prevail in those agencies.

Without excessively laboring this point, let us draw attention to an article by Richard Weinstein in the January, 1982 issue of the *APA Monitor*. His reportage bore on presentations made to an annual meeting of the Psychonomic Society by representatives of eight federal agencies that provide research funding in the behavioral sciences. The crux of the problem addressed was that agency budgets for funding are in part determined by the amount of money applied for in previous years. But if the perception of social scientists, for example, is that an incumbent administration does not favor the kinds of social programs that their studies in reality are promoting, they may be loath to invest time applying for funding for such studies. The likelihood of their labors being rewarded by the granting of funds will be perceived to have declined to the point where the investment seems to be a poor one. The upshot of this is that the number of research proposals submitted will decline, justifying a future retrenchment of available funds. This, in turn, further reduces interest in applying for funds.

James Scheier of the (U.S.) National Institute of Mental Health warned that inasmuch as research proposals addressed to that agency had declined recently by two-thirds, that would entail, in all probability, deeper cuts in the allocation of funds in the future (Weinstein, 1982). A similar trend was made evident in other agencies. A recommendation, therefore, that emanated from this sombre backdrop was that a continuing stream of applications should be directed to any agencies whose public policy objectives were roughly consistent with the research interests of the applicants. It was further recommended that one send the same proposal, suitably "adjusted", to several agencies more or less interested in one's field of study.

What are the implications of this advice to chase dwindling funds, to be granted on the basis of rapidly shifting criteria of social need, with more numerous and heftier applications? From an actuarial point of view, the probability rises that a much larger absolute number of applications will be turned down. And the probability also rises that those who see themselves as junior or "second-line"

rather than senior or "first-line" researchers, and less well connected than some of their colleagues, will not invest several months of hard work in developing proposals whose underlying rationale is, in part, to enhance future funding that will be garnered in great likelihood by better established scholars and researchers. As one wag has put it, this is the academic analogue of Amway. At the bottom of a pyramid of proposal generators is a host of junior academics "beaver away" on proposals that will have as their principal function increasing funding for better established centers and researchers.

The competition for research funding exacts a heavy toll from grant proposal developers. First, they may need, at times, to modify their interests so that they fall under the rubric, "hot research subjects". If considerations of professional advancement override the meanderings of their scientific curiosity, they may have to change their tack, say, to defense related subjects or to bilingual education. In many cases this might involve more than minor adaptations in their research trajectories. They will be tempted, further, to design a multi-purpose proposal such that, should it fail to gain funding, a monograph or several review articles can be chucked out of it.

Second, publication strategies and the ideological preferences of the research network that may vet one's manuscripts and proposals are potent factors in determining the shape of the underpinning research objectives and designs. Many of our colleagues are not disposed to act favorably on scholarship that questions the conventional wisdom of their field. And government agencies on the other hand, resist underwriting hypotheses that challenge, even implicitly, the convictions and values of their constituencies. The recent turbulence we witnessed relative to a new managerial style in the (U.S.) National Institute of Education and the revamping of its research agendas (Mervis, 1982) clearly illustrate what has always been the case: science and research are inherently and pervasively ideologized. Although we can complain that traditional peer review procedures have been drastically altered, and that agendas have been distorted, the historical reality is that politics and social context have always, directly or indirectly, been the prepotent determinants of the kinds of science that we busy ourselves with, and these are in constant change.

Third, the development of research proposals has become an increasingly demanding art for a number of reasons. Knorr-Cetina (1981) states that research teams learn the art of dissimulation, for they often sense the need to conceal their ideas from peer reviewers, particularly those who are their "most dangerous competitors in the area" (p. 87). She adduces evidence to support a graver concern: a competitor who is reviewing another's proposal can delay forwarding his assessment in order to have had the time to cull out useful ideas for himself and gain some time in advancing a more competitive proposal of his own. Aside from the consideration that this tactic is less than virtuous and militates against some treasured canons of scientific inquiry, grant proposal developers must use language that is sufficiently explicit and concrete so that their projects are comprehensible and compelling should they be sent by chance to reviewers who

are less familiar with the subspecialization in question than they are.

Beyond that, this process can be very time-consuming. Academics can allocate more time to the development of proposals, steering them in and through the granting machinery, and renewing them yearly, than they spend on the research itself. Comparing this allocation of time and energy to expenditures on actual research, class preparation, and other non-grant seeking activities illuminates the distortions in the distribution of resources that can compromise the educative functions of the university.

Toward the micro-end of a continuum of analysis, it is interesting to look at the university context within which some scientists work. Academics are no longer simply salaried employees of a university. Increasingly, they have been given to understand that they have entrepreneurial responsibilities of a major magnitude. Not only must they do science, they must garner their own funding for it. If 30% to 60% of each grant dollar is sequestered by the university comptroller, it is evident that the university is not just recouping the direct and indirect costs to it of having research done within its premises. Those funds like general revenues are used in part to defray the normal operational expenses of running such an institution.

The implicit job description of contemporary academic staff includes, as a major component, fund-raising. And fund-raising, largely through promotional work involved in grant procurement, requires levels of political skill and salesmanship, entrepreneurial drive, and administrative acumen, that lie far beyond the competencies, and usually the interests, of academics. "Hustling the research dollar" can be extremely time-consuming. For many it is a potent distractor from their research and teaching responsibilities.

This complex of discordant responsibilities so acutely felt by many academics does not figure to the same extent in the professional lives of researchers working for private laboratories or for government departments. A comparative study of the parallel and divergent responsibilities of researchers working in these various settings would be useful. It would illumine, we would think, the relative efficiency with which each of these organizational models promotes creative and truly productive research.

The Molecularisation and Hypertrophy of Knowledge

The reward structures and constraints described in the previous pages have provoked their own peculiar anomalies in the massive research production of the past few decades. Countless researchers spend their lives chipping away at tiny problems which have meaning only within the dominant paradigms of their disciplines. In the numerous domains that constitute psychology there are few paradigms that find general acceptance. Much of the problem-solving, therefore, is of an *ad hoc* and even whimsical nature, unrelated in a coherent fashion with any paradigm that can give it meaning as science. There may be nothing intrinsically wrong with any one of these studies. The problem develops when a field is top-heavy with such studies and their authors are not asking the broader questions that give meaning to them all.

A different research attitude consists in striving to gain a broad perspective on a larger field of inquiry, one that can eliminate thousands of studies of a patchwork, trial-and-error, evolutionary kind. An exemplary model for that broader approach is Subrahayam Chandrasekhar who has labored a lifetime in the area of astrophysics. He wrote a tract in the '60s on ellipsoids (1969), a rather unfashionable subject in his field at that time. He has told us that the reason for writing that tract was that previous research had left the subject "with many gaps and omissions and some plain errors and misconceptions. It seemed a pity", he stated, "that it should be allowed to remain in this destitute state" (Tierney, 1982). Chandrasekhar systematically analyzed the total problem of the rotating ellipsoid and brought order to a domain of inquiry that was heretofore filled with uncoordinated and faulty answers to a host of uncoordinated questions.

This research effort of several years did not yield a product that could easily be contained in a 10-page journal article. It had the further liability that it did not seem to have any immediate relevance to the major questions the field was addressing itself to. Despite its apparent marginality, the tract is beginning to be seen as an extremely valuable synthesis of a field, having applicability that was not imagined a few short years ago.

"If you make a sculpture", says Chandrasekhar, "you don't want to go on chipping it here and there". This research philosophy seems to be needed in our own domains. We have been bringing single bricks to research edifices which are still in a pre-paradigmatic stage. We suppose, as Goldman (1976) has suggested, that the well-executed survey article can serve to give coherence and direction to our work. But it is inadequate for providing the coordination of a well-executed paradigm. Nor can it, except in rare instances, make sense out of a pattern of research activity that was conducted in molecular fashion and which lacked the direction and vision which has characterized the work of, say, Bandura, or Super, or Thomas and Chess, or Maccoby. The highly molecularized nature of our work cannot be explained, as it can in physics and other hard sciences, by the existence of widely accepted paradigms – for we have few. We engage in bits-and-pieces operations because there are lacking the overarching directive principles which might give meaning and coherence to longer-term projects.

There are structural constraints other than those noted above that have provoked their own peculiar anomalies in the massive "research" production of the past few decades. The reward structures in contemporary North American academia are not the least of these influences. The exigencies of promotion and tenure regulations have impelled legions of professor-researchers to publish several articles a year (each of which may be hardly distinguishable from the others) without regard to their alleged intrinsic importance. This has contributed to the proliferation of tens of thousands of snippets of research which are non-programmatic or otherwise uncoordinated in character.

We are not, on the other hand, making a plea for psychological and educational researchers to write the periodic book. Indeed, books are becoming increasingly an anachronism in this scientific age. As Kuhn (1969) pointed out over 20 years

ago, books are largely either “texts or retrospective reflections” (p. 20). And we would think that their general usefulness declines in rough proportion to the decline in the half-life of the discipline of which they are an exposition. On the other hand, myriad journal articles pass through our intellectual universe like so many unconnected asteroids. We cannot keep track of them, or even for that matter take note of most of them, relevant as they may be to our work. Unless they have a seminal character they become immemorable. We are reaching the human limits of what we can be aware exists, let alone speak of assimilating it. When one recognizes the tens of thousands of articles that are published each year in one or another social science, the problem looks like a terminal disease.

The eminent historian of science, Derek DeSolla Price (1982), said that “in science we are at the stage where it is often easier to do an experiment over again than try to find it in the published literature”. As science has continued its exponential growth we have tended increasingly to miss things and to lose things. “The structure of science has been threatened with a sort of dry rot from within, owing to sheer human limitations” (p. 96). Knorr-Cetina (1981) quotes a scientist who gives a similar perspective on the same difficulty. She states that “there is a certain...high percentage...maybe...40% of (the published material) I ask for which I never get. ...The authors don’t send you a reprint, the library can’t get it – for one reason or another, I don’t receive it” (p. 37). This researcher may not have reflected on it but she has been unfortunate, further, in that she has lacked countless other references of greater value, perhaps, than the one she couldn’t track down.

This situation poignantly presents us with the Baconian challenge: “Truth emerges more readily from error than from confusion”. With the hypertrophy of knowledge and data-bases that even the most specialized of social sciences is presented, it becomes increasingly difficult to distinguish errors and irrelevancies from their opposites. The Babel of subdisciplines creates an ambience at least as confusing as attending a congress whose principal language one doesn’t understand (Knorr-Cetina, 1981, p. 37). The result of the absence of broader, more directive paradigms for giving us perspective on our field is to leave us with warehouses full of odds-and-ends. The chances of any one of them being used is diminishing, for even when computer searches could ferret it out, it may hardly occur to anyone that it exists, or believing it to exist that one has the time to examine it.

Curvilinearity and Common Sense

Yalom (1980) has stated that “paradigms are self-created, wafer-thin barriers against the pain of uncertainty” (p. 26). That may be so, but they afford us the luxury of being in error rather than floundering in chaos. And to evoke the dictum of Francis Bacon once more, it is easier to restructure a paradigm than correct myriads of answers to so many uncoordinated questions. Moving from confusion to error is no small step in moving toward the truth, “scientific” or other.

We suggested above that there are some potent political and institutional constraints (as well as more purely psychological ones) within which our problem-solving is habitually done. The state of our public education systems reflects the baneful influence of these constraints as well as the need to take a metaperspective on their components in the manner (if not with the virtuosity) of a Chandrasekhar, or an Alfred Adler, or a Bandura.

We began this article with an example of a largescale institutional problem which is highly resistant to the remedies we have at hand. It may be useful to close this presentation with a look at a smaller, theoretical, more manageable domain – personology, though others could serve our purpose. Consistent with what has been said above, we suggest the need to formulate personological principles in more holistic, paradigmatic terms.

An example. The celebrated Yerkes-Dodson Law was formulated years ago to define the relationship that exists between performance levels on discrimination learning tasks of varying degrees of difficulty and, on the other hand, anxiety or drive state. The data curves describing the relationships are U-shaped. We have always been puzzled by the importance accorded in textbooks to the nonlinear character of this particular curve. And we have been surprised, but less often, by the surprise of researchers who proclaim in “Eureka-tones” that they have found yet another J-curve or U-curve describing the relationship between some personological variable and a certain behavior.

We have a hunch that it is the rare personological variable that is not related in a non-linear way with performance variables with which it has an inherent relationship. As long as one is dealing with organisms which admit of only narrow bands of tolerance for stimulation, whether intro- or exteroceptive, one will get optimal performance only within that band. Outside of it, depending how conservatively it is defined, one gets more or less rapid falling off of performance. That is as true of such a simple behavior as breathing air, too much or too little of which is noxious for the breather, as the influence of such a complex variable as anxiety on puzzle-solving.

In discussing this with colleagues, we asked them to enumerate some personological variables which we could play with. “Curiosity”, “assertiveness”, “KAE levels”, “self-esteem”, “perceptions of locus of control”, and “conceptual level” came to mind. We began with “curiosity” and looked at its relationship with “harmony in interpersonal relations”. It was a rather easy construct to push operationally into zones of absurdity. Little expressed curiosity about people in general or persons in particular, or extreme curiosity at the other end of that continuum, would precipitate declining levels of interpersonal harmony. In the former case, a sufficient interest in others could not be sustained to assure more than a transient contact with them. In the latter case, excessively curious behavior would generate a sense of invasiveness and personal prying in response to which one would become defensive. All of this would be modified interactively, no doubt, by the environmental matrix from which it emerges, as well as by a host of idiosyncratic variables.

Piaget, Gesell, and other developmentalists have pointed out that faster and sooner are not always better. If they are better, the range within which that may be true is limited. And Bruner (1964) has suggested that, in terms of lifestyle, maximum and optimal are conceptually quite different (p. 292). The broad principle which bears constant study in our view is that all human variables that are in an essential capacitating-performance relationship with each other need moderate levels of the "capacitator". This is just another way of saying that Aristotle's principle and explication of the Golden Mean embody a world of educational and psychological theory, and that much of the molecular work done in personality research can be simplified as much by this principle as the Galilean formula, $S = 1/2 gt^2$, simplified physical dynamics in the 16th century.

Each educational researcher in the land, we can reasonably hypothesize, going through the personality and ethnographic literature, could pick variables at random, study their interactions with one or more relevant behaviors, and ultimately derive a law affirming the curvilinearity of their relationships. The usefulness of that is not completely evident. Rather, the first task of such researchers would seem to be not "to go on chipping...here and there" but to ask themselves what the overarching, paradigmatic principles of their specialty are, and what any research endeavor they are engaged in means in that conceptual framework. Not only might that reduce the flow of scholarly studies to a modest stream, but it might redirect our substantial intellectual resources into effecting reforms where they are needed.

Ratchet Effects: Asserting a Moral as a Conclusion

Karl Popper (1962) wrote in the preface of his book, *Conjectures and Refutations*, that the fundamental thesis of that book was that we can learn from our mistakes. Increasingly, the problem for society is not so much learning from our mistakes as *undoing* them, for, in most realms of science, once basic findings have been applied, certain long term consequences result which are very difficult, if not virtually impossible, to undo. We are condemned to build upon our past errors (Ellul, 1967), doing our best to generate remedies that will be less damaging than the problems we are trying to eliminate.

The ratchet effects (often technological in origin) that are most to be feared are those which build on political and social realities which cannot be undone without undoing an entire social order. We think of agricultural revolutions provoked in part by the widespread and intensive use of insecticides and herbicides which have made possible vast increases in the population of this planet, of which we have only seen the first surges. Building a 2-billion dollar airport too far from the wrong city or putting a wrecking ball to a priceless architectural monument are other examples. Developing an educational system, such as we discussed at the beginning of this presentation, with the serious faults that ramify through its very foundation, is still another.

It is difficult to do meaningful research if one does not see its relation to the larger issues which it will influence. For example, if we do not continually challenge the assumptions (whatever they may be or may become) of our thoroughly ideologized public school system, our efforts will not be directed to making structural changes but to compensating for fundamental errors. If, to be more specific, the educational researcher does not relate curriculum research and development in an enlarging cognitive universe to the latencies and surges of human cognitive development, not to mention the problem posed by individual differences, then all his work becomes simple tinkering, and his professional life a service to his political masters.

Travel brochures are wont to tell us that "getting to your destination is half the fun". It may be that in much of scientific research, all of the fun is in getting there, for we have committed ourselves to the quest for continually improved means to reach carelessly examined ends (Merton, 1964, p. vi). It may be useful to reflect periodically on the idea that it has been the questioning of long-sought goals and the perennial testing of our most fundamental and long-accepted convictions that have permitted the most significant advances in human welfare and scientific knowledge. It takes courage as well as an intelligent vision to engage in a meaningful programmatic shift which is alien to one's personal professional history. In any event, it seems like a salubrious psychological exercise to examine the possibility and the need to do this once or twice in one's lifetime.

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