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Towards a Sociology of Objectivity

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The concept 'objectivity' has been described as 'slippery'¹ and 'heavily burdened with a heritage of contradictory usages and of inconclusive and interminable discussions'.² Gouldner³ analyzes objectivity as a value and an ideology that manifests detachment and alienation from self and society. And Friedrichs⁴ notes an increasing preference among philosophers and scientists for the term 'empirical' over 'objective'. But objectivity as a value or ideology, and as a troublesome philosophical concept, should not be confused with objectivity as the affirmation of 'objective reality'. This affirmation is based on the fact that human beings do not and cannot know the nature of reality *a priori*; they must exert mental, physical, and social effort to obtain knowledge. In this sense, objectivity is generally viewed as the product of a social process, widely referred to as 'intersubjective testing'.⁵ It is also conceived in terms of 'the "public" nature of scientific evidence', and the 'communal enterprise' of science.⁶ The generally accepted social theory of objectivity rests on the assumption that communication and exchange in a public forum or community of scientists are necessary and effective means for insuring that we admit to science only statements which are valid approximations to objective reality and not products of abnormal perceptions, selective and unique subjective cognitions, or 'uncontrollable and unverifiable introspection'.⁷ The problem with this theory is that it treats the psychological level of scientific activity as problematic, but not the social level. In a recent review article, for example, Joseph Ben-David⁸ concludes that public test, logic, and experiments, or empirical

¹ G. Bergmann, 'The Logic of Quanta', in H. Feigl and M. Bodbeck (eds.), *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953. Originally published in 1947), 477.

² K. R. Popper, *Then Logic of Scientific Discovery* (New York: Science Editions, Inc., 1961. Originally published in 1959), 44.

³ A.W. Gouldner, *The Coming Crisis of Western Sociology* (New York: Basic Books, 1970), 102-5.

⁴ R. W. Friedrichs, *A Sociology of Sociology* (New York: The Free Press, 1970), 207.

⁵ H. Feigl, 'Naturalism and Humanism', *American Quarterly*, 1, No. 2 (Summer 1949), 135-48; A. Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), 128.

⁶ Friedrichs, *op. cit.*, 209.

⁷ A. Schutz, 'concept and Theory Formation in the Social Sciences', in M. Natanson (ed.), *Philosophy of the Social Sciences* (New York: Random House, 1963. Originally published in 1954), 231-49; K. Pearson, *The Grammar of Science* (New York: Meridian books, 1957. Originally published in 1911), 53-54.

⁸ Joseph Ben-David, "The State of Sociological Theory and the Sociological Community: A Review Article". *Comparative Studies in Society and History*, 15, No. 4 (October 1973), 448-72.

observation, gradually eliminate *personal* bias and mistakes. He does not consider the identification and elimination of *social* biases and mistakes to be of at least equal concern. My objective in this paper is to suggest some bases for the construction of a sociological theory of objectivity which considers the facilitative and obstructive effects of social organization on scientific activity.

OBJECTIVITY AS A SOCIAL FACT

In his *Critique of Pure Reason*, Kant used the term ‘objective’ to refer to knowledge that could be justified independently of any individual’s whim. If a justification can, in principle, be tested and understood by anyone, it is objective. Karl Popper⁹ followed Kant in noting that the objectivity of a scientific statement is based on the fact that it can be intersubjectively tested. Lundberg¹⁰ noted that objectivity is not a product of universal human consensus. Practically, we tend to rely on corroboration by a limited number of persons, invested with the authority to establish ‘truth’ by virtue of their ‘qualifications’.

The extent to which a given definition of objectivity expresses its social nature varies from phrases such as ‘universal agreement’,¹¹ and ‘co-operative nature of scientific research’,¹² to Popper’s¹³ conceptions of ‘social institutions’ (e.g., laboratories, scientific periodicals, and congresses) as the collective bases for generating scientific (objective) statements. Popper¹⁴ argues that an individual cannot simply decide to be ‘objective’; objectivity is a product of cooperation among scientists. Assume, Popper proposes, that an individual, trained in science but now alone and isolated from communication with others, success in building laboratories and observatories. This Robinson Crusoe writes numerous papers based on his experiments and observations. He has unlimited time, and ultimately succeeds in developing scientific systems which coincide with those accepted by ‘our own scientists’. Such a situation, Popper argues, would be nearly as accidental and miraculous as the case of science revealed to a clairvoyant. The reasons are:

1. **There is no one to check** this Crusoe’s results.
2. **There is no one to correct** the prejudices which unavoidably result from his peculiar experiences.

⁹ Popper, op. cit.,46.

¹⁰ G. Lundber, “The Postulates of Science and their Implications for Sociology”, in M. Natnason (ed.), *Philosophy of the Social Sciences* (New York: Random House, 1963. Originally published in 1939), 33-72.

¹¹ N. Campbell, *Foundations of Science* (New York: Dover, 1957. Originally published in 1919), 21.

¹² I.M. Copi, *Introduction to Logic* (New York: Macmillan, 1961), 2nd ed.,, 449.

¹³ K.R. Popper, *Then Open Society and its Enemies* (New Jersey: Princeton University Press, 1950), 404.

¹⁴ *Ibid.*, 405-7.

3. **No one can help him exploit** the inherent possibilities of his results because such possibilities are often recognized in the course of taking ‘comparatively irrelevant approaches’ to the results.
4. **Having no one to explain his work to**, he is unable to develop the ability to communicate clear and reasoned results; this is a discipline that one learns only by having to explain one’s work to others who have not done that work.
5. **He can only discover** his ‘personal equation’ in a revealed way, by discovering changes in his reaction time and developing means for compensating; in ‘public’ or ‘objective’ science, reaction time is discovered when the contradictions among the results obtained by various observers are analyzed.

Popper concludes that objectivity is a social product, and not a product of an individual’s impartiality:

...and the individual scientist’s impartiality is, so far as it exists, not the source but rather the result of this social or institutionally organized objectivity of science.

Scientific criticism and scientific progress, according to Popper, depend on cooperation, intersubjectivity, and public method.

Norman Campbell,¹⁵ in a philosophical exercise similar to Popper’s, concluded that a Crusoe could develop science even though the criterion of universal assent could not be applied. A scientific Crusoe could replace the intersubjective criterion with ‘the criterion of the satisfactoriness and coherence of the laws which can be derived from the subject matter’. This idea deserves serious attention. If it is meaningful to consider social factors which facilitate the production of objective statements, then a similar search could be undertaken to identify psychological conditions which facilitate the production of objective statements. But Campbell’s Crusoe would have to be socialized in some form of ‘scientific community’ in order to later carry out his work in isolation. And it is with the nature of such a ‘community’ that the sociology of objectivity is concerned.

Having recognized that objectivity is a social fact, some students of science have gone on to ask what it is about the organization and values of science that accounts for its capacity to progressively generate objective statements. One response to this query has been to view science as an adventure in rugged individualism. Michael Polanyi has been among the most articulate spokesmen for this *laissez-`etudier* position. Polanyi¹⁶ argues that there is an ‘invisible hand’ that coordinates the independent activities of individual scientists and leads to ‘unpremeditated’ discoveries in science. Goodall’s¹⁷ view of science as a ‘genuinely democratic’ system with

¹⁵ Campbell, op. cit.

¹⁶ M. Polanyi, *The Republic of Science: Its Political and Economic Theory* (Chicago: Roosevelt University, 1962), 7-8.

¹⁷ M.C. Goodall, *Science, Logic and Political Action* (Cambridge, Massachusetts: Schenkman, 1970), 78.

‘largely built’ in guarantees that it is not going to get poilt’ is another example of the *laissez-faire* conception of science. Stated in its crudest and most sociologically vulnerable form this position requires scientists to do nothing but act in terms of what they consider their self-interests; the ‘invisible hand’ is responsible for the beneficial societal outcomes of these independent acts of self-interest. But *laissez-etudier* sometimes gets linked to altruism and humanism, and scientists are portrayed as individuals whose self-interests happen to be ‘broadly humanistic’.¹⁸

In a more sophisticated approach to the problem of scientific progress, Kuhn¹⁹ argues that normal science is educationally narrow, rigid, and ill-designed to produce creative scientists. But he optimistically adds that individual rigidity is compatible with scientific progress. He does not consider whether rigidity is a social as well as an individual fact. Is the supply of scientific innovators—young scientists new to their fields – independent of social conditions within and outside of science? Can youth and newness become increasingly unlikely and ultimately impossible as individuals become more and more standardized, and as deviation becomes not merely less likely, but more intolerable and more at the mercy of agents and agencies of social control? Even if we assume the validity of Kuhn’s model, certain ‘damping’ effects on the cycles of scientific revolution and normal science can be hypothesized. The rigidifying effects of processes such as bureaucratization may lengthen the period between revolutionary peaks, lessen the intensity of revolutions, progressively decrease periods of conceptual crisis in science, and progressively decrease the probabilities that:

1. **An individual scientist** will conceptualize a revolutionary idea, and
2. **Such an idea will be recognized** and precipitate a crisis,

A second damping source is the ‘cost’ associated with each revolution. Boulding²⁰ suggests that the dialectical processes accompanying scientific revolutions are costs, representing the ‘heat of crystallization in a process of essentially continuous change’. The costs concept can be extended to include ‘cumulation’ of costs, and thus the possibility that social systems, like biological systems, can – under certain conditions – progressively lose their capacity to ‘recover’ and to continue to ‘progress’ having incurred certain costs.²¹

Science cannot be comprehended if social facts are ignored, treated naively, or approached with and optimism that obscures or denies their problematic nature. The full implications of the sociology of science must be recognized if science is to be genuinely comprehended as a social fact.

¹⁸ G.T. Seabor, ‘A Scientific Society – The Beginnings’, in S. Rapport and H. Wright (eds.), *Science: Method and Meaning* (New York: Washington Square Press, 1964), 218-32.

¹⁹ T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970), 2nd enlarged ed., 166.

²⁰ K.E. Boulding, *A Primer on Social Dynamics* (New York: The Free Press, 1970), 60-61.

²¹ S.P. Restivo and C.K. Vanderpool, ‘Science: Social Activity and Social Process’, in S.P. Restivo and C.K. Vanderpool (eds.), *Comparative Studies in Science and Society* (Columbus, Ohio: Charles E. Merrill, 1974), 9-10.

THE SOCIOLOGY OF KNOWLEDGE

One of the basic objectives of sociologists of knowledge is establishing relationships between types of social structures and types of knowledge. This idea had occurred to Francis Bacon.²²

That monarchies incline wits to profit and pleasure, and commonwealths to glory and vanity. That universities incline wits to sophistry and affectation, cloisters to fables and unprofitable subtilty, study at large to variety; and that it is hard to say, whether mixture of contemplations with an active life, or returning wholly to contemplation, do disable and hinder the mind more.

The systematic development of the sociology of knowledge in the late nineteenth and early twentieth centuries is associated with names such as Marx, Mannheim, and Scheler. Scheler, for example, associated Plato's theory of ideas with the organization of the Platonic academy; he followed Troeltsch in arguing that Protestant beliefs determined an could only exist in the form of organization of the Protestant churches and sects; and he argued that *Gemeinschaft* societies generate a traditional, conclusive fund of knowledge rather than form of knowledge which is continuously subject to discoveries and extensions.²³ The generalization of these types of hypotheses led to an intolerable relativism in the sociology of knowledge. For, it was argued, if 'scientific theories' are rooted in social milieu, then the prospect of obtaining warranted knowledge appears utterly futile. Objectivity then appears to be 'an arbitrary emulsion of social conditions, regarded as proper in its time as soothsaying was by the Greeks who once stood before the gleaming towers of Troy'.²⁴ Indeed, if we accept this perspective, what warrant is there for the sociology of knowledge, which must itself be 'nothing but' a product of its particular social milieu?

The problem can be resolved as follows. First, as Mannheim recognized, the sociology of knowledge can trace the emergence of different types of knowledge to different social milieu, but it cannot judge the truth-value of these systems. Secondly, if types of knowledge are rooted in types of social milieu, we can set ourselves the task of discovering the social conditions under which 'scientific' or 'objective' knowledge is generated. The literature on science and society illustrates a number of approaches to this task. The facilitative relationship between science and democracy has been commented on by Tocqueville, Sigerist, and Merton among others. Merton²⁵ sees some basis for the provisional assumption that science flourishes in democratic milieu. He notes that science in some form has existed in all kinds of societies; but the crucial question is which societal type(s) facilitate(s) the 'fullest measure of development' in science. The emphasis in these and related inquiries is on external 'social forces' that facilitate or obstruct scientific activity and scientific progress. Internal social forces that affect science as a social

²² J. Spedding, R. L. Ellis and D.D. Heath (eds.) *The Works of Francis Bacon*, Vol. 6 (Boston: Houghton, Mifflin and Company, 1863), 76.

²³ R. K. Merton, in N. Storer (ed.), *The Sociology of Science* (Chicago: University of Chicago Press, 1973), 24.

²⁴ B. Walter, 'The Sociology of Knowledge and the Problem of Objectivity', in L. Gross (ed.), *Sociological Theory: Inquiries and Paradigms* (New York: Harper and Row, 1967), 340.

²⁵ Merton, *op. cit.*, 269.

activity, process, organization, or institution are treated incidentally if at all. To fully comprehend science as a social fact, we must attend to internal social forces. Professionalization and bureaucratization are examples of such force. Both processes have been associated with the emergence of science as an autonomous, progressive social activity. Their continuing impact on science has stimulated some concern about dysfunctional consequences.

Richard LaPiere²⁶ argues that the adaptivity of the universities and of the professions has decreased as they have become increasingly bureaucratized:

1. **Rewards for bureaucratic conformity** tend to be higher than rewards for innovative behavior.
2. **Bureaucracies tend to exercise some control**, directly or indirectly, over nonmembers; thus scientists often have to design their work in accordance with the 'rules and prejudices' of bureaucratic organizations; and
3. **Established or 'mature' bureaucracies tend to resist adaptation** to changes in external conditions and to resist adopting available innovations.²⁷

Bureaucratization, according to LaPiere,²⁸ tends to subordinate individual to collective decision-making, dividing responsibility for a given decision. This can easily lead to the negation of responsibility, and then to a failure to act effectively with regard to internal organizational problems, or broader 'external' societal problems.

The dysfunctions of bureaucratization are reinforced by and reinforce the dysfunctions of professionalization. The two processes are linked at least to the extent that they are concomitant in the modern history of industrializing nations. Professionalization has been associated with the increasing specialization in the division of labour, the knowledge explosion, and the increasing demand for management expertise in highly technical and bureaucratized societies.²⁹

In the process of professionalization, an occupation becomes 'relatively colleague-oriented', with practitioners seeking exclusive rights over naming and judging their mistakes.³⁰ The goals of professionalization include standardizing, specializing, gaining status for occupational roles and services to society, and 'objectivizing', i.e., limiting the impact of subjective elements on performance and service. One of the first, and among the foremost, students of professionalization, Carr-Saunders³¹ concluded that

²⁶ Richard LaPiere, *Social Change* (New York: McGraw-Hill, 1965), 437.

²⁷ *Ibid.*, 410-11.

²⁸ *Ibid.*, 413.

²⁹ C. Jencks and D. Reisman, *The Academic Revolution* (New York: Doubleday-Anchor, 1967), 202.

³⁰ *Ibid.*, 201-2.

³¹ A. M. Carr-Saunders, *Professions: Their Organization and Place in Society* (Oxford: The Clarendon Press, 1928), 30-31.

....taking all in all the growth of professionalism is one of the hopeful features of the time.

The dysfunctions of professionalization, however, arise precisely from the 'hopeful' tendency toward occupational demarcation. This creates a volatile potential for subordinating reason to dogma:

...once given its special status, the profession quite naturally forms a perspective of its own, a perspective all the more distorted and narrow by its source in a status answerable to no one but itself.³²

In his analysis of the medical profession, Friedson³³ argues that while professional autonomy may have facilitated significant increments in knowledge about disease and treatment, it 'seems to have impeded the improvement of the social modes of applying that knowledge'. Horowitz³⁴ affirms this aspect of professionalization:

The professional can, by virtue of his professionalism, exempt himself as a scientist from responsibility for the ends to which his scientific findings are put.

The negation of responsibility, as I noted earlier, has also been associated by LaPiere with bureaucratization.

The literature on professionals and complex organizations has traditionally stressed the conflicts inherent in linking the roles 'professional' and 'bureaucrat' based on differences between 'professions' and 'bureaucracies'.³⁵ This research focuses on the independent professional's resistance to bureaucratic standards, and his conditional loyalty to the bureaucracy. But Scott³⁶ notes an increasing convergence of bureaucracies and professions, as bureaucrats become professionalized and professionals become bureaucratized. In this convergence, the dysfunctions of the two processes are likely to reinforce one another. Bureaucratization, for example, may reinforce tendencies in professionalization toward occupational closure and dogma with its demands for 'reliability of response and strict devotion to regulation'.³⁷ To the extent that the dysfunctions of bureaucratization and professionalization become increasingly salient and converge, we can expect a tendency toward occupational closure, and ethnocentrism of work, and a decrease in the capacity of individuals and

³² E. Friedson, *Profession of Medicine* (New York: Dodd, Mead and Company, 1970), 370.

³³ *Ibid.*, 371.

³⁴ I.L. Horowitz, 'Mainliners and Marginals: The Human Shape of Sociological Theory', in L.T. and J. M. REgynolds (eds.), *The Sociology of Sociology* (New York: David MacKay Company, 1970. Originally published in 1967), 345.

³⁵ Eg., T. Parsons, *Essays in Sociological Theory* (New York: The Free Press, 1954), revised ed., 34-49; R. G. Francis and R. C. Stone, *Service and Procedure in Bureaucracy: A Case Study* (Minneapolis: University of Minnesota Press, 1956), 153-57; P.M. Blau and W.R. Scott, *Formal Organizations: A Comparative Approach* (San Francisco: Chandler, 1962), 60-63

³⁶ W.R. Scott, 'Professionals in Bureaucracy – Areas of Conflict', in H. M. Vollmer and D.L. Mills (eds.), *Professionalization* (Englwood Cliffs, New Jersey: Prentice-Hall, 1966), 266-67.

³⁷ R.K. Merton, *Social Theory and Social Structure* (New York): The Free Press, 1957), revised and enlarged, ed., 200.

organizations to respond to problems in critical and creative ways. Horowitz³⁸ has remarked that the ultimate consequence of this process may be the ‘loss of objectivity’.

The important point to consider in reviewing the literature on the dysfunctions of professionalization and bureaucratization is not so much what it reflects about particular empirical realities, but the fact that it illustrates the mutability of social facts and the potential that exists in all social phenomena for dysfunctional or pathological transformation. Philosophers and other students of science have acknowledged the potential for evolutionary change in science, but they have not given adequate attention to the potential for devolutionary change inherent in science as social phenomenon.

I have selected professionalization and bureaucratization to illustrate the problematic aspects of social facts because these processes have received significant attention from students of ‘the crisis in science’.

THE CRISIS IN SCIENCE

Harvey Brooks³⁹ recently asked readers of *Science* to consider whether:

...the conditions of modern society are generating a cultural climate which is not longer hospitable to the cultivation of a “true science”...

In the same volume, Arnold Thackray confronted *Science* readers with his reflections on the decline of science in America; the title of his paper paraphrased Charles Babbage’s 1830 *Reflections on the Decline of Science in England and on Some of its Causes*. Thackray⁴⁰ concluded that:

...the broadening social cost and social implications of science raise the demand for the new organizational forms, just as earlier social changes led science from generalist to specialist societies.

J.D. Bernal⁴¹ produced the first comprehensive report on the modern crisis in science. He wrote that the contemporary view of the ‘fruits of science’ was dominated by images of war, economic chaos, the willful destruction of needed goods, and the fear of more and more terrible wars. Twenty-five years later, Bernal⁴² wrote that the potential of science for serving humanity was buried beneath ‘the actuality of a divided world with greater poverty, stupidity, and cruelty than it has ever known’.

³⁸ Horowitz, op. cit., 347.

³⁹ Harvey Brooks, ‘Can Science Survive in the Modern Age?’, *Science*, 174 (1 October 1971), 21

⁴⁰ A Thackray, ‘Reflections on the Decline of Science in America and on Some of its Causes’, *Science*, 173 (2 July 1971), 31.

⁴¹ J. D. Bernal, *The Social Function of Science* (New York: Macmillan, 1939), 31.

⁴² J. D. Bernal, ‘After Twenty –five Years’, in M. Goldsmith and A. Mackay, (eds.), *Society and Science* (New York: Simon and Schuster, 1964), 211.

In the 1930's, Pitirim Sorokin (described by Merton as a 'sociological Jeremiah') regularly lectured on the decline of science. He warned prophetically that one day scientists would make it possible to destroy all life on earth, and then some of them would be curious to see what happens when the button is pressed.⁴³ This is reminiscent of Fillipo Buonarrot's concern during the French Revolution that scientists would derive from their successes 'real and supposed claims to distinctions, to superiority, and to exemptions from the common burden'; they might then indulge 'in disastrous enterprises against the rights of simple and less informed persons'.⁴⁴

More than thirty years ago, George Santayana remarked on the increasing timidity of the mind in the wake of the increasingly abstract formulations of modern science: he noted that 'natural man' can tolerate imagination only insofar as it 'poses for truth'; and, more troubled by the thought of deception than by the fact of boredom, 'he would wish to escape imagination altogether'.⁴⁵ Francis Bacon, Thoreau, and Thomas Huxley are among others who have echoed this concern. It was Huxley, who, as a young man, wrote in one of his letters,

You have no idea of the intrigues that go on in this blessed world of science, Science is, I fear, no poorer than any other region of human activity; though it should be.⁴⁶

The root of crises in science is the trivial fact that scientists are human beings. More to the point is the fact that science is a social activity and social process. The biologist Loren Eiseley has expressed the implications of this fact better than many of his colleagues in sociology. Science, he notes, is an institution, and like all institutions 'is apt to reveal certain behavioral rigidities and conformities that increase with age'; under the impact of professionalization and bureaucratization, standards in science can lose their function as expressions and guarantors of excellence and become 'an excuse for stifling original thought, or constricting much latent creativity within traditional models'.⁴⁷

Increasing attention is being given by some social scientists to the crisis in science.⁴⁸ A critical perspective on science is increasingly evident among physical scientists. John Ziman,⁴⁹ for example, warns of 'closure' and 'ecclesiasticism' in modern science, arising as a consequence of the increasing control of certification in science by an 'establishment'. In science as in other social activities, professionalization and bureaucratization have tended to increase specialization to the point of overspecialization, and stimulated the development of excessive competition and a conflictful division of labour. This has led some scholars to speculate about

⁴³ Merton, (1973), op. cit., 168.

⁴⁴ L. Feuer, *The Scientific Intellectual* (New York: Basic Books, 1963), 279.

⁴⁵ L. Eiseley, 'The Illusion of the Two Cultures', *The American Scholar*, 33, No. 3 (Summer 1964), 388

⁴⁶ Meron (1973), op. cit., 493.

⁴⁷ Eiseley, op. cit., 392.

⁴⁸ E.g., J. Haberer, *Politics and the Community of Science* (New York: Van Nostrand Reinhold, 1969); H. Rose, *Science and Society* (Baltimore: Penquin Books, 1970); M. Blisesett, *Politics in Science* (Boston: Little, Brown, 1972); S. S. Blume, *Toward a Political Sociology of Science* (New York: The Free Press, 1987).

⁴⁹ J. Ziman, *Public Knowledge* (London: Cambridge University Press, 1968), 65

possible decreases in the evolutionary potential of science. The resolution of the crisis in science—and the broader societal crisis to which it is related – is not guaranteed. A necessary condition, however, for resolving the crisis is rethinking the nature of science, and its relationship to values and social organization. In the next section, I consider the problem of value orientations and the generation of objective knowledge.

SCIENCE AND VALUES

The goal of scientific activity is the comprehension (knowledge, understanding, explanation, and appreciation) of human experience. It assumes a comprehensible reality. The form of this assumption varies. Norman Storer,⁵⁰ for example, conceives “but a single physical reality “out there” which can be discovered through the creative application of observation, intuition, logic and experiment’. Bohm,⁵¹ by contrast views nature as:

Scientific research does not and cannot lead to a knowledge of nature that is completely free from error. Rather it leads and is able to lead only to an unending process in which the degree of truth in our knowledge is continually increasing.⁵²

Any given kind of thing is in principle “knowable”, but ‘complete, perfect, and unconditional knowledge of reality as a whole’ is impossible.⁵³

The truth of any given theory can only be approximate, conditioned, and relative. But this does not mean that there is no ‘objective reality’. Scientific laws are not dependent on our ‘tastes’ or ‘wills’. We must be guided by ‘correct conceptions of these laws if they are to lead to the results that we aim for’.⁵⁴ This experience shows that scientific laws, though tentative, have some objective content. Basic reality is ‘the infinite totality of matter in becoming...’⁵⁵ Our objective in science is to find more and more of the things of which matter in becoming is composed, to study the relationships among these things in better and better approximations, and to discover the conditions under which specific concepts and laws are applicable in greater and greater detail. Science approaches the absolute “by studying the relative, in its inexhaustible multiplicity and diversity’.⁵⁶

The question of the nature of objective reality can be examined in a broader historical context by distinguishing between the hypotheses of Parmenides and Heraclitus. The Parmenidian hypothesis states that for the world to be knowable, reality must be eternally

⁵⁰ N. Storer, ‘The Internationality of Science and Nationality of Scientists’, *International Social Science Journal*, 22, No. 1 (1970), 80.

⁵¹ D. C. Bohm, *Casualty and Change in Modern Physics* (Philadelphia: University of Pennsylvania Press, 1971. Originally published in 1957), 164.

⁵² *Ibid.*, 166.

⁵³ *Ibid.*, 168.

⁵⁴ *Ibid.*, 165.

⁵⁵ *Ibid.*, 170.

⁵⁶ *Ibid.*, 170.

immutable. The Heraclitian hypothesis states that the Parmenidian hypothesis is formally true; but it offers no imperative for humans since reality is in constant flux and therefore unknowable. Toulmin,⁵⁷ commenting on these views of reality in terms of the distinction between absolutist and relativist perspectives on acquiring knowledge, notes that this position implies a process of ‘fixed minds’ gaining control over ‘fixed nature’ using ‘fixed principles’. But it is clear that nature and principles have changed. This experience yields not the Heraclitian hypothesis, but the Bohmian hypothesis, in Toulmin’s terms, of variable minds commanding (or, preferably, comprehending) variable nature using variable principles.⁵⁸

I accept the Bohmian view of reality. The concept of nature as an infinite diversity of things expresses more clearly than relatively static alternative conceptions of nature the need to conceptualize science and the ‘search for truth’ as an endless process. The question then arises, what values must direct our activities if we are to engage in science, a cumulative and effectively endless process?

In his pioneering work on the ‘norms of science’ Merton attempted to drive the ‘mores’ of science from the goals and methods science.⁵⁹ Universalism, communism, disinterestedness, and organized skepticism were identified as ‘scientific ethos’. Other notable contributions to the study of the norms of science were made by Parsons,⁶⁰ Barber,⁶¹ and Storer.⁶² These contributions have been criticized on several points:

1. **They encourage a view of the norms** as those which do and should (logically) prevail in science; the relationship between ideal and actual behavior and orientation is obscured, and no provision is made for potential or actual changes in the norms due to changes in the organization and goals of science, or to changes in conceptions of the goals and methods of science;
2. **The identification of the norms of science** is based on systematic, continuous, and cumulative empirical and theoretical analyses.⁶³

The virtues of the norm studies are that they:

⁵⁷ S. Toulmin, *Human Understanding*, Vol. 1 (New Jersey: Princeton University Press, 1972).

⁵⁸ D. L. Hull, ‘A Populational Approach to Scientific Change’, *Science*, 182, (14December 1973), 1124. Book review.

⁵⁹ Merton (1973), op. cit., 270.

⁶⁰ T. Parson, *The Social System* (New York: The Free Press, 1951), 335-59.

⁶¹ B. Barber, *Science and the Social Order* (New York: The Free Press, 1952), 78-80.

⁶² N. Storer, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966), 78-80.

⁶³ E.g., N. Kaplan, ‘Sociology of Science’, in R.E.L. Faris (ed.), *The Handbook of Modern Sociology* (Chicago: Rand McNally, 1964), 857; Blissett, op. cit., 65-86; Restivo and Vanderpool, op. cit., 4-6; S.P. Restivo, ‘The Ideology of Basic Science’, in S. P. Restivo and C. K. Vanderpool (eds.), *Comparative Studies in Science and Society* (Columbus, Ohio: Charles E. Merrill, 1974), 334-51; Blume, op. cit., 167.

1. **Contribute to the development** of a model of science and its logically associated values, and
2. **Do, in fact, identify** a number of orientations which must direct the pursuit of objective knowledge.

Due consideration, however, has not been given to the humanistic dimensions of the scientific ethos, and to whether this ethos is thriving or threatened by the external and internal social relations of science. Merton, for one, does exhibit an awareness of these problems; but he does not pursue the humanistic and radical implications of science, nor dysfunctional changes (actual and potential) in science with sufficient vigour.⁶⁴

The idea that scientific activity implies a certain set of values has been lucidly expressed by Jacob Bronowski. If, he argues, the goal of scientific activity is ‘to explore truth’, then scientists must be individually independent and collectively tolerant. These two ‘prime values’ are the foundation for a set of values: ‘dissent, freedom of thought and speech, justice, honour, human dignity and self-respect’. These values manifest ‘the inescapable conditions’ of scientific activity. They are not derived from the virtues of scientists, ‘nor from the finger wagging codes of conduct by which every profession reminds itself to good’.⁶⁵

Ravetz⁶⁶ concludes that neither the objects of scientific inquiry, nor the social aspects of scientific work can guarantee ‘the health and vitality of scientific inquiry’. There must be, he argues, an ‘effective ethic’, something more refined than a ‘professional ethic’. The source of such an ethic, he suggests, lies in a sophisticated humanitarian commitment and not in religion, philosophy, or elitist codes of conduct. The specification of the values which should comprise a humanitarian commitment is a relatively new task being carried forward by humanistic social scientists and philosophers. Maslow’s work in this area stands out and is especially interesting on two counts: first, because it is congruent with Bohm’s ‘in-becoming’ conception of nature; second, because it encompasses many, if not all, of the values associated by students of science from Merton to Bronowski with the scientific ethos. Maslow assumes the intrinsic value of truth, and views inquiry as a basic defining activity of human life. Among the being-values Maslow⁶⁷ associates with the ‘good person’ and the ‘good society’ are truth, goodness, beauty, wholeness, dichotomy-transcendence, aliveness, uniqueness, perfection, necessity, completion, justice, order, simplicity, richness, effortlessness, playfulness, and self-sufficiency.

Then search for a humanistic ethos has often overlapped with some variety of ‘radicalism’. The association of science with radicalism can be considered curious only by those who have not thought seriously about the nature and history of inquiry. The scientific ethos tends

⁶⁴ Merton (1973), op. cit., 254-66.

⁶⁵ J. Bronowski, *Science and Human Values* (New York: Harper Torchbooks, 1965), revised and enlarged ed., 60-62.

⁶⁶ J. R. Ravetz, *Scientific Knowledge and its Social Problems* (Oxford: The Clarendon Press, 1971), 313.

⁶⁷ A.H. Maslow, *The Farther Reaches of Human Nature* (New York: The Viking Press, 1971), 133-35.

to conflict with the ethos of other social institutions.⁶⁸ Ideally, only science is associated with a full, uncompromising, unfettered commitment to pursuing knowledge. Scientific inquiry must be constantly pressed forward, driven by skepticism and the idea that even fundamental assumptions are ultimately subject to criticism and change. Nothing is protected from the basic query, Why? No other social activity – and in reality not even scientific activity itself – operates fully according to this imperative to inquiry. Scientific activity – ‘true science’ – must inevitably be perceived as a radical activity relative to the other social activities in a society. When Tom Hayden⁶⁹ defined the ‘radical style’ some ten years ago, he came intriguingly close to a definition of science:

Radicalism as a style involves penetration of a social problem to its roots, to its real causes. Radicalism presumes a willingness to continually press forward the query: Why? Radicalism finds no rest in conclusions; answers are seen as provisional, to be discarded in the face of new evidence or changed conditions.

In addition to humanistic and radical commitments, a related value orientation that must be considered in constructing a scientific ethos is ‘reflexion’. Reflexive Sociology, according to Gouldner,⁷⁰

...sees all men as profoundly shaped by their shared past, by their evolving culture and social systems. Yet it does not see men either as the helpless agents of some inexorable social force to which they must bow, or as the omni-present overlords of an historical process that they can neatly engineer. A Reflexive sociology believes that there is an inevitable ‘slippage’ between man and society.

Reflexion can be generalized as follows: a reflexive life is one in which the ‘things’ of experience are all and always, at least in part, turned inward, and incorporated in our increasing awareness of who and what we are. Physics can be learned reflexively by analyzing ourselves as physical systems. Astronomy and geology can be studied in terms of their meaning relative to our existence in and relationship to the universe – past, present, and future. The most abstract human endeavours have reflexive potential. Mathematics and logic, for example, can be explored as themselves explorations in the structure and processes of thinking. In this way, it becomes possible to realize that the self, as Alan Watts⁷¹ writes, resides ‘in the whole surge of energy which ranges from the galaxies to the nuclear fields in my body’. Reflexion is not a one way process. Its relevance for a scientific ethos lies in the fact that increased awareness is a condition of new perceptions and ultimately new conceptualizations and comprehensions.

Humanistic, radical, and reflexive commitments are bases for the construction of scientific ethos. In a sense, they are the specification and elaboration of values apparent in ideal science, and in ‘good’ scientific research. This complex of values emphasizes open-endedness, process, and change; it is in this sense consistent with Bohmian reality. It is also consistent with

⁶⁸ Merton (1973), 266.

⁶⁹ T. Hayden, ‘A Letter to the New (Young) Left’, in M. Cohen and D. Hale (eds.), *The New Student Left* (Boston: Beacon Press, 1967. Originally published in 1961, revised and enlarged ed., 6.

⁷⁰ Gouldner, *op. cit.*, 507.

⁷¹ A. Watts, *The Book*, (New York: Collier Books, 1967), 10.

– and in part reflects – certain developments in the psychology of science and the theory of inquiry.

THE PSYCHOLOGY OF SCIENCE AND THE THEORY OF INQUIRY

Abraham Maslow⁷² pioneered in the development of a psychology of science that reflects the realities of human psychology and of scientific activity. He argued⁷³ as follows:

1. **Scientific activity** can be an anxiety –avoiding, anxiety-controlling mechanism.
2. **Science can be ‘neuroticized’**; it then becomes more a defense and less a growth-motivated activity; the growth-motivated scientist is ‘not compulsive, rigid, and uncontrollable’, nor is he anxious when the rewards of his scientific activities have to be postponed: ‘It is possible for healthy scientists to enjoy not only the beauties of precision but also the pleasure of sloppiness, casualness, and ambiguity’.⁷⁴
3. **The education of scientists** must expose them to techniques of caution *and* boldness.

Objectivity, according to Maslow,⁷⁵ means seeing things ‘as they really are’. He distinguishes between ‘not-caring’, ‘laissez-faire’ objectivity, and ‘caring’ objectivity.⁷⁶ The former allowed scientists to assert their freedom from a priori truths established by the church or state. ‘Caring’ objectivity arises in situations where ‘not caring’ is difficult or impossible. Such situations are not unknown in the scientist’s relationship to physical phenomena; but they emerge most clearly with the development of the human and social sciences. In these sciences, the application of traditional canons of objectivity results in scientists trying to be objective about people, values, and themselves, things that they love and hate.

The basic thesis of ‘caring’ objectivity is that loving someone or something enough means you will not want to interfere with it. By not interfering, you will perceive what you love as it is, ‘uncontaminated by your selfish wishes, hopes, demands, anxieties, or preconceptions’.⁷⁷ Such ‘contamination’ can never be entirely avoided; but it is certainly possible to reduce the amount of physical manipulation used in exploring things. This aspect of science must be stressed because it has been subordinated to literal

⁷² A. H. Maslow, *The Psychology of Science* (Chicago: Henry Regnery Company, 1969), 114-18.

⁷³ *Ibid.*, 20-32.

⁷⁴ *Ibid.*, 30-31.

⁷⁵ *Ibid.*, 115.

⁷⁶ *Ibid.*, 114-18.

⁷⁷ *Ibid.*, 114-18.

and theoretical ‘analyses into parts’ for so long. Both approaches are necessary ingredients of science.

In general, according to Maslow,⁷⁸ objectivity is psychologically the product of a learned capacity to be ‘either controlled and/or uncontrolled, tight and/or loose, sensible and/or crazy, sober and/or playful...’; these are the characteristics of psychological health and of scientific creativity.

Maslow⁷⁹ identifies two polar activities in science: (1) ‘experiencing and comprehending concreteness’, and (2) ‘organizing the welter of concreteness into graspable abstractions’. The former refers to the Taoistic conception of non-intrusive, receptive contemplation. Thus, in addition to controlled experiments and quasi-experiments, comprehension of ‘objective reality’ in its totality requires a second mode of inquiry: receptive, contemplative, ‘nonactive, noninterfering witnessing and savoring.’⁸⁰

R.G.H. Siu⁸¹ elaborates Maslow’s conception of Tao in science. He distinguishes three fundamental approaches to reality:

1. **Rationality**, which is open to ‘patterned discourse’, and capable of being logically reconstructed.
2. **Intuition**, which cannot be reconstructed, or systematized in the way that rational approaches can; and
3. **Sage** – or no-knowledge (Tao).

Scientists and philosophers have readily admitted intuition into the scientific process. But since intuition cannot, by definition, be incorporated into a paradigm it has been accorded only cursory and anecdotal attention in the methodology of science. But the rational-intuitive process has limits. Beyond these limits, Siu⁸² argues, ‘Our faltering mind must then seek repose and cure in what it cannot know’; sage or no-knowledge

...transcends events and qualities; it has no shape or time. As a result it cannot be the object of ordinary knowledge. At the highest level of cognizance, the sage forgets distinctions between things. He lives in the silence of what remains in the undifferentiable whole.

If science is the process of comprehending an ‘infinite variety of things’, then the totality of human creative and critical intelligence must be tapped in order to deal with the infinite variety. Maslow and Siu have contributed to the identification of the different modes human

⁷⁸ Ibid., 31.

⁷⁹ Ibid., 101.

⁸⁰ Ibid., 101.

⁸¹ R. G. H. Siu, *The Tao of Science* (Cambridge, Massachusetts: M. I. T. Press, 1964).

⁸² Ibid., 75.

beings have used for comprehending reality and have provided a rationale for viewing these different modes as complimentary, and as integrally related.

It is possible to conceive the scientific process represented by the concepts rationality, intuition, and no-knowledge (R-I-N) as one in which the R-I-N comprehension of reality at any given point in space-time ultimately becomes assimilated into a rational structure, and gives rise to a new R-I-N comprehension. This idea is at least implicit in the theory of inquiry proposed by TenHouten and Kaplan. They hypothesize the existence of a general class of nonscientific inquiries, called 'synthetic'. Their thesis⁸³ is that science involves perception but is 'primarily rooted in language and the clarification thereof'; synthetic inquiries involve language, but they are primarily perceptual. This distinction has a neurological basis, according to TenHouten and Kaplan:

1. **Propositional (scientific, analytical)** and appositional (synthetic) modes of thought are lateralized in the brain; '...in most persons capacity for abstract logical thought is associated with the left side of the brain and capacity for perceptual and spatial thought with the right side of the brain'.⁸⁴
2. '**...although...the products** of science are founded on rationalities that are primarily propositional, science as a practice is not confined to these rationalities'.⁸⁵
3. '**...the act of constructing a theory...**may involve appositional thought integrated with propositional thought. In science, the most creative endeavours may be related to levels of thought that transcend the functions of either cortex'.⁸⁶
4. **2 and 3 are psychological manifestations** of the bilateral cognitive functions.
5. **Synthetic inquiries such as the Tarot and I Ching** are mirror-images of scientific inquiries; this manifests the mirror-image relationship between the hemispheric functions (a neurological hypothesis).
6. **Four synthetic rationalities parallel** the four rationalities of scientific method identified by Garfinkel.⁸⁷ The synthetic (nonscientific) rationalities

⁸³ W. TenHouten and C. D. Kaplan, *Science and its Mirror Image* (New York: Harper and Row, 1973), xii.

⁸⁴ *Ibid.*, 4.

⁸⁵ *Ibid.*, 26.

⁸⁶ *Ibid.*, 26.

⁸⁷ H. Garfinkel, *Studies in Ethnomethodology* (Englewood Cliffs, New Jersey: Prentice-Hall, 1967), 262-68.

are present in the Tarot, I Ching, and the Teaching of Don Juan,⁸⁸ an example of ‘primitive’ inquiry.⁸⁹

TenHouten and Kaplan⁹⁰ propose the following ‘transformation’ in which Garfinkel’s inventory or scientific rationalities is ‘mapped’, or ‘mirrored’ against an inventory of nonscientific rationalities:

Transformation A:

(1) Compatibility of ends-means relationships with principles of formal logic.

(1’) Compatibility of means-ends relationships with layers of structural perception.

Transformation B:

(2) Semantic clarity and distinctness.

(2’) Semantic veiledness and complexity.

Transformation C:

(3) Clarity and distinctness ‘for their own sakes’.

(3’) Veiledness and complexity ‘for their own sakes’.

Transformation D:

(4) Compatibility of the definitions of a situation with scientific knowledge.

(4’) Compatibility of the perception of a situation with synthetic knowledge.

TenHouten and Kaplan⁹¹ conclude that scientific theory construction, concept formation, and methodology involve essentially subjective synthetic rationalities. Objective rationalities are employed primarily in linguistic formulations. TenHouten and Kaplan affirm the analytic duality of objectivity-subjectivity (manifesting the hemispheric duality of the brain), and the

⁸⁸ C. Castaneda, *The Teachings of Don Juan* (New York: Ballantine Books, 1968); C. Castaneda, *A Separate Reality* (New York: Simon and Schuster, 1971); C. Castaneda, *Journey to Ixtlan* (New York: Simon and Schuster, 1972).

⁸⁹ TenHouten and Kaplan, *op. cit.*, 140.

⁹⁰ *Ibid.*, 115.

⁹¹ *Ibid.*, 222.

transcendence of this duality in total brain functions (physically rooted, perhaps, in the corpus callosum which links the hemispheres?). Science represents a dialectical unification of objective and subjective rationalities; TenHouten and Kaplan's view of this process is similar to the view I outlined earlier of a rational, intuitive, no-knowledge dialectic. Indeed, it may be that TenHouten and Kaplan have given us a clue as to how intuition and no-knowledge can be rationalized. Instead of passively 'waiting' for moments of insight or flashes of intuition, 'paradigms' like the I Ching may be turned to when rational paradigms are exhausted. Such exercises might lead to the systematization, modification, and eventual rationalization in 'objective' terms of the synthetic modes. This would in turn lead to the emergence of a new R-I-N, or, in TenHouten and Kaplan's terms, propositional-appositional framework. Rationalization or objectivization would provide 'closer approximations' to reality, but would also create new frontiers for intuition and no-knowledge, or for appositional modes.

The crisis in science reflects in part the fact that the prevailing objectivity-subjectivity dichotomy is increasingly an obstruction to scientific inquiry. This reflects novel problems of the human and social sciences, and the emerging ecological-evolutionary challenges confronting the human species. These emerging problems demand a new set of problem-solving values. Two things seem to be necessary for solving these problems in ways congruent with the enhancement of human life. One is a broader and at the same time more sophisticated conception of science, such as suggested in the works of Maslow, Siu, and TenHouten and Kaplan. The second is a generalization of this conception of science to other societal activities. The congruencies I have discussed between the scientific ethos and humanistic, radical, and reflexive values should be examined carefully. Such an examination may support current speculations on the essential oneness of a scientific ethos emphasizing wisdom rather than simple technical power and efficiency, and a life-enhancing ethos.⁹²

Having sketched an approach to the problem of values, the next task is to examine bases for constructing organizations consistent with the values of science.

THE SOCIAL ORGANIZATION OF SCIENCE

Proposals for resolving the crisis in science emphasize the necessity of achieving a closer realization of the ideal communal organization of science. These organizational proposals are usually linked, implicitly if not explicitly, to values which fit into the complex of humanistic, radical, and reflexive values discussed above. Examples of such proposals include Bernal's⁹³ advocacy of 'science as communism', Husserl's⁹⁴ association of 'scientific culture' with thoughtfulness and enlightenment, and Jaspers'⁹⁵ conception of science as a basis for world-

⁹² Cf. J. Salk, *The Survival of the Wisest* (New York: Harper and Row, 1973).

⁹³ Bernal (1939), *op. cit.*

⁹⁴ E. Husserl, *The Crisis of European Sciences and Transcendental Phenomenology* (Evanston, Illinois: Northwestern University Press, 1970. Originally published in 1950).

⁹⁵ K. Jaspers, *The Future of Mankind* (Chicago: University of Chicago Press, 1963), 200.

unity, friendship, and trust where the fundamental drive of science ‘binds men existentially – where their common work makes them friends’.

The ‘communistic’ or ‘communal’ theme is not radical in science; it has been identified as one of the norms of science. Merton⁹⁶ used ‘communism’ in ‘the nontechnical and extended sense of common ownership of goods’, and referred to it as an ‘integral element of the scientific ethos’. Writing in 1942, Merton⁹⁷ noted that concerns about the ‘frustration of science’ reflected the conflict between the communistic ethos and ‘the definition of technology as “private property” in a capitalistic economy’. He noted a variety of responses to the conflict: defensively patenting scientific work to ensure its public availability; urging the promotion of new businesses by scientists; and advocating socialism.

Wartofsky⁹⁸ has recently outlined a rationale for socializing science. He argues⁹⁹ that science is reason, that scientific rationality is the most highly adapted and most advanced form of cognition which our species has evolved; and yet, that it stands in danger, for the first time, of becoming dysfunctional or maladaptive’. Wartofsky’s vantage point is global ecological and evolutionary history. The question he raises is whether ‘rationality, like some other growth-mechanism, has become dysfunctional and destructive to the future survival of the species’.¹⁰⁰ The dysfunctions of science, he says, are the result of reason being used as ‘the instrumentality of conflicting wills’; this ‘flaws rationality itself...and transforms even its liberating features into repressive ones’. His conclusion is consistent with advocacy of the communal ethos in science except in its emphasis on the sense of responsibility the ethos implies: ‘The alternative is a socialized reason – more concretely, a socialized science, or one which takes its rational imperative to be its own responsibility for human welfare’. This is not simply an “ethical imperative”, but ‘an imperative of reason itself’.¹⁰¹

When science is conceived as a social system unto itself, isolated from other human enterprises and from the psychological and sociological realities of those enterprises, the communal ethos is also isolated; its relevance to broader socio-cultural concerns is obscured. An awareness of the reciprocal relations between science and society, and of the fact that science is a social activity and social process, leads inevitably to a generalization of the communal ethos. Just as this ethos was linked to the survival of science in isolation, it now becomes linked to the survival of the species. The fact that communality is advocated in the traditional-normative conception of science, and also in radical conceptions of socialized science is a strong rationale for considering communality a basic organizational imperative in science.

⁹⁶ Merton (1973), op. cit., 273.

⁹⁷ Ibid., 275.

⁹⁸ M. Wartofsky, ‘Is Science Rational?’, in W.H. Truitt and T. W. G. Solomons (eds.), *Science, Technology and Freedom* (Boston, Mifflin and Company, 1974), 202-10.

⁹⁹ Ibid., 203.

¹⁰⁰ Ibid., 208.

¹⁰¹ Ibid., 209.

Progress is a second organizational imperative. Progress is not a function of optimism but a logical necessity in an infinitely diverse universe. In order to comprehend reality we must organize in such a way that we maximize actualization of our potential for achieving closer and closer approximations to reality. More generally, if science is conceived to encompass all activities which contribute to human progress and evolution, then the question we must answer is: How must human beings organize in order to adapt, progress, and evolve?

Following Lenski,¹⁰² I define progress as the process by which human beings raise the upper limit of their capacity for perceiving, conceptualizing, accumulating, processing, and utilizing information and energy in the adaptive-evolutionary process. The relationship between adaptation and evolution is a paradoxical one. On the one hand, survival depends on the capacity to adapt to surroundings; on the other hand, adaptation involves increasing specialization and decreasing evolutionary potential. Adaptation is a dead end. As a given entity adapts to a given set of conditions, it specializes to the point that it begins to lose any capacity for adapting to significant changes in those conditions. Sahlins and Service¹⁰³ summarize these ideas as follows:

1. **Principle of Stabilization:** specific evolution (the increase in adaptive specialization by a given system) is ultimately self-limiting.
2. **General evolution** (progressive advance measured in absolute terms rather than in terms of degrees of adaptation in particular environments) occurs because of the emergence of new, relatively unspecialized forms.
3. **Law of Evolutionary Potential:** 'The more specialized and adapted a form in a given evolutionary stage, the smaller is its potential for passing on the next stage'.

Sahlins and Service discuss the applicability of these principles to socio-cultural change. This is not a new concern in sociology. What is noteworthy is that the revival of interest in evolutionary theory among sociologists is associated with an increasing interest in the sociological relevance of ecology (which by itself is also not a new concern among sociologists). Lenski's *Human Societies*¹⁰⁴ outlined an evolutionary-ecological approach that represented a radical departure from mainstream introductions to sociology. Leggett¹⁰⁵ has recently outlined an evolutionary approach to political sociology. The attraction of evolutionary and ecological theories is that they are relatively more sophisticated than general sociological theories, and that they tend to converge. In both perspectives, for example, viability is associated with complexity, flexibility, and diversity.¹⁰⁶

¹⁰² G. Lenski, *Human Societies* (New York: McGraw-Hill, 1970), 59.

¹⁰³ M. D. Sahlins and E.R. Service, *Evolution and Culture* (Ann Arbor: University of Michigan Press, 1960), 95-97.

¹⁰⁴ Lenski, *op. cit.*

¹⁰⁵ J. Leggett, 'From the Bottom: An Evolutionary View of Underclass Challenge', in J. Leggett (ed.), *Taking State Power* (New York: Harper and Row, 1973), 1-15.

¹⁰⁶ Cf. G. Bateson, *Steps to an Ecology of Mind* (New York: Ballantine Books, 1972).

In some cases, sociological research seems to have uncovered the operation of principles of social organization that parallel evolutionary and ecological principles.¹⁰⁷ All of this suggests the possibility of a general approach to systems and change that encompasses ecology, evolution, and social organization. In particular, given the theme of this paper, an approach that manifested or otherwise allowed for the imperatives of community (as ecology might) and progress (as evolutionary theory might) in science would be ideal. One approach which recommends itself because of its generality and congruence with the open-endedness of reality, the Maslowian psychology of science, the TenHouten and Kaplan theory of inquiry, and the humanistic-radical-reflexive value complex, is dialectical sociology.

DIALECTICAL SOCIOLOGY

Friedrichs¹⁰⁸ suggested a few years ago that with a change in the self-image of social scientists as scientific agents, a paradigm shift to dialectical sociology would become ‘increasingly tenable’ for sociologists. But the pervasiveness of dialectical thinking (e.g., in Bohm, TenHouten and Kaplan, Maslow, and others) suggests the possibility that a dialectical perspective may be emerging in a scientific world-view shift. My objective in the following discussion is the modest one of proposing a dialectical strategy for designing progressive scientific communities.

If we begin by assuming a Bohmian reality, Maslow, and TenHouten and Kaplan recommend themselves because they are consistent with the assumed nature of the reality their ideas are part of, and because their ideas are conditions for comprehending that reality. Their ideas, and the value orientations I considered, have a dialectical quality similar to Bohm’s view of reality. It is reasonable to consider the hypothesis that Bohemian reality encompasses social reality. Engels¹⁰⁹ outlined such a dialectical perspective on physical, natural, and social reality. According to Engels, the laws of dialectics are abstracted from natural and social history. Following Hegel, Engels¹¹⁰ notes that these laws are simply ‘the most general laws of the two aspects of historical development as well as thought itself. And indeed they can be reduced in the main to three: the law of the transformation of quantity into quality and vice versa; the law of the interpenetration of opposites; the law of the negation of the negation’.

¹⁰⁷ E.g., LaPiere, *op. cit.*; cf. K. E. Boulding, *The Organizational Revolution* (Chicago: Quadrangle Books, 1968. Originally published in 1953), 78-80.

¹⁰⁸ Friedrichs, *op. cit.*, 297.

¹⁰⁹ F. Engels, *Dialectics of Nature* (New York: International Publishers, 1940. Originally published posthumously in 1927 (Riazanov, ed.), and 1935 (Adoratski, ed.)).

¹¹⁰ *Ibid.*, 26.

Gurvitch's view of social reality parallels Bohm's view of reality. He paints a picture of social reality in constant motion, filled with tension, 'fluctuating, renewing, threatened by revolution...'.¹¹¹ Gurvitch identifies five operational procedures of the dialectic:¹¹²

1. **Complimentarity** (contradictory alternatives turn out to be complementary; two polarities are connected by a continuum; polar points pull together, i.e., go in the same direction; and they pull apart, i.e., as a compensatory action).
2. **Mutual implication** (things that are heterogeneous or opposite exhibit mutuality and interdependency; they turn out to be imminent, at least partially in one another).
3. **Ambiguity** (ambiguity can eventually lead to ambivalence, and then to polarization).
4. **Polarization** ('The tensions between the several factors or aspects [of social reality] are relative; they are observable in different degrees of intensity. Sometimes they reach the proportion of polarization; other times they exist as ambiguities or mutual implication or evidences of complimentarity. The paradoxical often resists the dialectical process of polarity').¹¹³
5. **Reciprocity of perspectives** (total identification and separation are denied; mutual immanence, parallelism, and symmetry).

The affinity between the dialectical view of social reality and the dialectical view of Bohm, Maslow, and TenHouten and Kaplan makes it reasonable to allow dialectical assumptions to guide us, at least in part, in the construction of scientific organizations. Following Gurvitch's schema, for example, the organizational imperative would be to coordinate his operational procedures with organizational structures and processes. One possible starting point for such coordination is the research on 'creative organizations'. This makes sense given the fact that science is paradigmatically creative and innovative. Steiner's¹¹⁴ summary of the characteristics of creative organizations shows several points of coordination with Gurvitch's operational procedures, and the more general dialectical laws summarized by Engels. The use of ad hoc devices and approaches, contact with outside sources, a heterogeneous personnel policy, the inclusion of marginal and unusual individuals, assignments for non-specialists, allowances for eccentricity, experimentation, decentralization, a risk-taking ethos, cooperation between stable 'philistines' and roaming 'creators' are all characteristics which allow for the creation of and interplay between and among polarities, contradictory alternatives, ambiguities and ambivalencies, and reciprocal perspectives. Creative organizations, in short, seem to be structurally and processually dialectical.

¹¹¹ P. Bosserman, *Dialectical Sociology* (Boston: Porter Sargent, 1968), 227.

¹¹² *Ibid.*, 232-42.

¹¹³ *Ibid.*, 237.

¹¹⁴ G. Steiner (ed.), *The Creative Organization* (Chicago: University of Chicago Press, 1965), 16-18.

CONCLUSION

Objectivity is a social fact. The achievement of closer, more detailed, and more exact approximations to objective reality in a universe of infinite diversity cannot be taken for granted. This has been recognized by students of the crisis in science. The crisis in science is a species-level crisis, one that reflects the emergence of ecological and evolutionary challenges that are new in type and scale. I do not believe that a new biological response is necessary for dealing with these challenges. New value orientations and new forms of social organization are necessary in order to survive in ways that enhance the lives of individuals and communities. New levels of consciousness concerning the physical, natural, and social worlds must be achieved. This applies to our consciousness of science as a social reality. Our conception of science must be broadened to include all inquiry pursued indefatigable, identifying and encompassing more and more of objective reality. The facilitation of science – or, inquiry – depends on developing an integrated perspective on (and, ultimately, theory of) the nature of reality, the psychology of inquiry, and the relationship between inquiry, values, and social organization. The primary concern of the sociologist of science in this endeavour is with social organization and how it facilitates and/or obstructs inquiry.

Among the tasks that lie immediately ahead are comparative studies of research organizations, theoretical studies of social organizations, and experiments in the design of scientific organizations. Following the strategy I have proposed, these studies would be guided by a dialectical paradigm. Other promising paradigms, such as general systems theory,¹¹⁵ should not be ignored.¹¹⁶ These studies must be guided by a sense of current and emerging ecological-evolutionary challenges, and an unwavering commitment to raising the probability of enhanced human living on this planet.

The present abounds in evolutionary and devolutionary tendencies. To encounter these tendencies without care and passion for one's self and one's community, to meet them technocratically and scientistically, or to sit back and rely on Providential goodwill can only court disaster. A sociology of objectivity cannot insure that we will continue to do science, or participate indefinitely in the evolutionary process. It can, however, provide us with a better sense of what has to be done, and what (in terms of available resources) can be done. It can help us identify conditions of evolution and devolution, progression and retrogression. Only our

¹¹⁵ W. Buckley, *Sociology and Modern Systems Theory* (Englewood Cliffs, New Jersey: Prentice-Hall, 1967); A. L. Bertrand, *Social Organization* (Philadelphia: F. A. Davis Company, 1972); A. S. Iberall, *Toward a General Science of Viable Systems* (New York: McGraw-Hill, 1972); F.K. Berrien, *General and Social Systems* (New Brunswick, New Jersey: Rutgers University Press, 1968).

¹¹⁶ O. E. Klapp, *Models of Social Order* (Palo Alto, California: National Press Books, 1973), 286-305.

participation in this process of uncertainties can determine whether ‘wisdom’¹¹⁷ a ‘higher sanity’,¹¹⁸ and ‘life and liberation’¹¹⁹ can take root in this world.

¹¹⁷ Salk, *op. cit.*

¹¹⁸ T. Roszak, *Where the Wasteland Ends* (New York: Doubleday-Anchor, 1972), 426.

¹¹⁹ B. Easlea, *Liberation and the Aims of Science* (Totowa, New Jersey: Rowman and Littlefield, 1973), 341.