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The journal of Marxist philosophy for natural scientists

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Science and Nature is devoted to the philosophical, historical and sociological problems of the physical, biological and formal (mathematical and logical) sciences. The primary editorial purpose is to demonstrate the usefulness of the Marxist world view in the practice of science, and help further the development of dialectical materialism and the Marxist theory of knowledge in their application to these areas.

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A Puzzler: Who's Who at the Computing Center ———

The following facts are known about four men who work together:

1. Mr. Blackburn is a confirmed bachelor.
2. The junior programmer has blue eyes.
3. The mathematician's wife has her own car.
4. Mr. Wilson likes to play golf with Dave.
5. Mr. Smith went to Yale.
6. The analyst has been having engine trouble with his Corvair.
7. John drives a Pontiac.
8. Terry, who drives a Ford, hopes to be promoted next week to Bill's level.
9. The senior associate engineer drives a green car which he parked next to Dave's Buick today.
10. Mr. O'Brian's convertible is red and the associate's coupe is white although his wife wanted a blue one.
11. The man at staff level drives a black car which has always given good performance.

What is the full name and title of each man, and what is the color and make of his car? Also, what if any principle of dialectical logic is involved in solving this oldie but goodie? Send your answers (for both questions) to Editor. (And send a subscription so you won't miss the feedback in next issue.)

LETTERS TO THE EDITOR

In Defense of Dialectical Logic

I enjoyed Irving Adler's presentation, "Basic Concepts of Dialectical Materialism" (*Science and Nature* No. 3). However, the presentation is defective because it omits the heart and soul of dialectical materialism—the unity and struggle of opposites.

Fortunately, the very same issue of your journal more than compensates for Adler's omission; I refer to the papers "Dialectical Materialism in Modern Biology" by Garland E. Allen and "Mathematics: Its Essential Nature; Its Objective Laws of Development" by A.D. Aleksandrov. Both of the above furnish overwhelming evidence for the fundamental importance of the law of the unity and 'struggle' of opposites.

I would urge materialists who have questions about the usefulness of dialectics, including its law of contradiction, to read carefully the two articles mentioned here.

Saul Birnbaum
Bronx Community College

In Irving Adler's enumeration (page 58) there is no mention of the Law of Contradiction, of the unity and interpenetration of opposites. In the same issue, Garland Allen's table (page 45) mentions contradiction only peripherally. But there has been lively discussion in *Science & Society* about the difference between dialectical and Aristotelian contradiction; the issue is still seemingly unresolved. I want to know what happened to "contradiction"? And how about negation of the negation?

R.W. Castown
2122 Valentine Ave. #2E,
Bronx NY 10457

Editor comments: First, I would say, from the evidence of these letters, that dialectical logic is alive and well and living in the Bronx.

Second, I think Rudy Castown has reason to be disturbed over the indignities that Marxist philosophy has suffered at times in the pages of *Science & Society,* especially from the formalist attacks of Swedish physicist M. Mark Mussachia that have created much confusion. We are accustomed to the idealist attacks on Frederick Engels which reject the concept of dialectical processes taking place in nature, i.e., independent of human mind and human society. But Mussachia comes from a different direction. Taking a mechanist position that the human mind works according to the laws of Aristotelian (formal) logic, he argues that dialectical contradiction cannot exist in thought processes. Mussachia's arguments are persuasive (and confusing) only so long as his premises are accepted uncritically. For a critical comment on Mussachia, the reader may turn to the section of Bibliographic Briefs, this issue. □

On the Philosphical Fronts

KARL POPPER AND CREATIONISM

History, it seems, caught up with Sir Karl. The creationist attorney Richard K. Turner, in his successful suit to influence science teaching in California's public schools, made good use of Popper's longtime positivist claim that evolutionary theory cannot be falsified and is therefore metaphysical rather than scientific (William J. Broad, *Science* 211: 1331f; 1981). The use of Popper's formulations to justify creationist arguments is nothing new: see the letter from creationist theoretician Robert E. Kofahl (*Sci Amer* July 1976 p. 6) and the paperback tract *Darwin Retried* by another creationist attorney Norman Macbeth (Dell 1971).

Another factor contributing to the legal defeat in California may have been the Popperian mode of thought in scientists testifying in court , as W.D. Russell-Hunter indicates: "Credible and successful scientific defense in future creationist trials could require that scientists avoid making narrowly restrictive statements about their scientific methodology" (*Science* 212:281; 1981). Popper's debilitating influence on the scientific community also shows in a *Nature* editorial (290:75; 1981) which concedes evolutionary theory to be metaphysical according to definitions so "helpfully constructed" by Popper, but argues that "metaphysical theories are not necessarily bad theories." Though expressing justifiable concern over a trend to "agnosticism" among evolutionists, the editors seem unable (or unwilling) to acknowledge that Popper himself is a major source of agnosticism toward science. Commenting on the editorial, Arthur L. Caplan (*Nature* 290: 623f; 1981) says: "The real question that should concern scientists is whether they know enough about current thinking in the history and philosophy of science to know a sound theory when it stares them in the face" (cf. also R.W. Lewis, *Nature* 291: 448; 1981).

Popper sought to make a "recantation" of his damaging views (*Dialectica* 32: 344; 1978, cf. also Hans Zeisel, *Science* 212: 873; 1981). but it was only a narrow empiricist concession: "historical sciences have in my opinion scientific character, their hypotheses can in many cases be *tested*" (*New Scientist* 21 Aug 1980 p 611, emphasis in original). Kofahl was quick to point out tauntingly that Popper had neither changed nor denied his characterization of evolutionary theory as metaphysical (*Science* 212: 873; 1981).

Popper's criticism of evolutionary theory has always been closely linked with his rejection of Marxism. His well known hostility to revolutionary movements led him to reject the idea that *any* historical development could be governed by laws (cf. his *Conjectures and Refutations*, New York 1968, esp. p. 340). From an opposite viewpoint, Marx in 1861 also recognized the analogy between the then new evolutionary and revolutionary theories: "Darwin's book is very important and it suits me well that it supports class struggle in history from the point of view of natural science" (Marx-Engels *Selected Correspondence*, Moscow 1975 p 115).

We are waiting to learn whether Popper includes political economy among the historical sciences on which he recanted. (Has Sir Karl yet caught up with history?) In the meanwhile, the sobering fact that Richard K. Turner was a legal aid to Ronald Reagan (when the latter was governor of California) makes it urgent that the scientific community get its philosophical act together in order to expose the slick sophistry of the new "scientific" creationists.

New insights on the origins and social role of Kuhnian concepts

LUDWIK FLECK
*Genesis and Development of a Scientific Fact**

Fleck uses history of the medical concept of syphilis to show the historically-conditioned nature of all knowledge. The figure is a syphillitc nobleman, by Albrecht Duhrer.

A review essay [1]
DIETER WITTICH
University of Leipzig (GDR)
Translated by Henry F. Mins†

In 1935 the Basel publishing company and bookselling firm of Benno Schwabe and Co. published a 150-page book with the title, *Genesis and Development of a Scientific Fact: Introduction to the Theory of Thought Style and Thought Collectives* [Denkstil und Denkkollektiv]. The book had been completed the previous summer. Its author, Ludwik Fleck, was a Polish physician and scientist who headed the bacteriological laboratory of the sickness fund in Lvov (Lemberg) during the 1930s and also had the experience from "several years of working in the venereal disease section of a large city hospital" [2]. In addition to this book, Fleck published a number of papers on bacteriology and immunology in important medical journals [3]. Thus he was not only an experienced physician but also a scientist active in several fields and with a strong interest in theory.

Fleck's medical investigations finally led to a concern with questions of scientific theory and the theory of knowledge, with which he first became acquainted in their logical positivist versions. Positivism was influential in the university of his city in the 1930s when many internationally-recognized logic studies of the Warsaw-Lemberg school were closely related philosophically with the Vienna Circle current of thought. Even before then, however, Fleck must have had

* Introduction by Thomas S. Kuhn. Edited by Robert K. Merton and Thaddeus J. Trenn. Translated from German by Fred Bradley and Trenn. University of Chicago Press 1979. 191 pages + index. Hardcover $17.50.

†This essay, based on the original 1935 Basel edition of the Fleck book, has been slightly abridged from *D.Z.f. Philosophie* 26(1): 15-113; 1978. But page numbers here refer to the 1979 English-language edition.

more than a superficial knowledge of the positivist orientation in scientific theory and the theory of knowledge since he came out as an opponent of it not later than 1930.

His first main objection to the positivism of the time was that it ignored the history of human thought, an ignorance that stood in profound contradiction to Fleck's knowledge and experience as a scientist. "Biology taught me," he wrote, "that a field undergoing development should be investigated always from the viewpoint of its past development. Who today would study anatomy without embryology?" [pp. 20-21]. Precisely this mode of procedure is required in order to understand today's science objectively. "It is nonsense to think that the history of cognition has as little to do with science as, for example, the history of the telephone with telephone conversations. At least three-quarters if not the entire content of science is conditioned by the history of ideas, psychology, and the sociology of ideas and is thus explicable in these terms" [p. 21]. For Fleck, "Concepts are not spontaneously created but are determined by their 'ancestors,'" and, from the historical nature of knowledge, he would "argue that there is probably no such thing as complete error or complete truth" [p. 20]. For these and many other reasons an "epistemology without historical and comparative investigations is no more than an empty play on words or an epistemology of the imagination" [p.21].

Fleck's second main argument against the positivism of the time was its gross disregard for the historical, collective nature of human knowledge. Even for reasons of language alone, cognition "is the result of a social activity" rather than "an individual process of any theoretical 'particular consciousness' . . . since the existing stock of knowledge exceeds the range available to any one individual" [p. 38]. Individuals can perform the act of knowing only within a specific cultural milieu, within the framework of a specific "thought collective." The social character of any knowledge also appears in the fact that "scientific activities" always have a recognizable "social structure" [p. 42], and in many other ways. Thus Fleck arrives at the conclusion: "Every epistemological theory is trivial that does not take this sociological dependence of all cognition into account. . ." [p.43].

Thirdly, Fleck's criticism of the positivism of the time relates to its shallow empiricism. In opposing this, he constantly stressed how the theoretical reacts on the empirical and the observed, seeking to substantiate this concept with many facts. "Consequently," he observed, "it is all but impossible to make any protocol statements [*Proktokollsätze*] based on direct observation and from which the results should follow as logical conclusions" [p. 89].

Fleck's insight, that the fundamental presuppositions and assertions of the positivism of his time are incompatible with actual scientific research and its history, enabled him to develop a number of hypotheses for understanding the history of science and the nature of scientific research. The most important are:

1) A scientific collective is characterized above all by an historically determined "thought style" which is common to all its members.

2) The thought style specific and characteristic for a scientific collective at any time puts its stamp in a basic way on all of its research activity.

3) Once formed, thought styles and the theories corresponding to them have a "tendency to persist." Consequently, every scientific thought collective passes historically through two phases: a) one in which the thought style, once formed, leaves its imprint on all research activity (because of its "tendency to persist"); and b) one in which the thought style gradually breaks down and is replaced by a new one.

4) Since any scientific investigation practices a definite thought style, training for science consists necessarily in training for the thought style obligatory in the scientific community concerned.

We now proceed to a closer examination of Fleck's fundamental concepts: "thought style," "tendency of theories to persist," and "training for science," the objects corresponding to these concepts, and the way in which they operate as Fleck saw it.

Thought style, for Fleck, is always a collectively formed "intellectual mood" or attitude which manifests itself "as the readiness for directed perception and appropriate assimilation of what has been perceived" [p. 142 and cf. p. 104]. The direction of the perception is essentially determined by the ideas dominant in a thought collective, corresponding to its mental attitude. Fleck sought to prove this assertion in many ways, in particular from the history of medical ideas on syphillis. Here, as in scientific work [sic], he concludes: "The dependence of any scientific fact upon thought style is therefore evident" [p. 64]. For the thought style followed at a given time dominates the "active elements" of knowing, which Fleck distinguishes from the "passive" (objective) elements. The two elements cooperate so closely in the process of knowing that they "cannot be separated from each other completely either logically or historically" [p. 95]. Even though the formation of individual results of thought already bear the imprint of the thought style, their distribution within the thought collective is again subject to the action of the thought style. For example, he says: "Words which formerly were simple terms become slogans; sentences which once were simple statements become calls to battle" and thus attain the "socio-cognitive value" specific for the style of the given thought collective [p. 43]. The domination of individual thinking by a collective thought style is such that the individual "is never, or hardly ever, conscious of the prevailing thought style," although that style "almost always exerts an absolutely compulsive force upon his thinking and with which it is not possible to be at variance" [p. 41].

According to Fleck, the *tendency to persist* possessed by scientific theories and by "systems of opinion" in general, which are arrived at by a definite thought style, is manifested in their enduring immunity to any deviant assertions: "Once a structurally complete and closed system of opinions consisting of many details and relations has been formed, it offers constant resistance to anything that contradicts it" [p.27]. This "is not so much simple passivity or mistrust of new ideas as an active approach which can be divided into several stages: (1) A contradiction to the system appears unthinkable. (2) What does not fit into the system remains unseen; (3) alternatively, if it is noticed, either it is kept secret, or (4) laborious efforts are made to explain an exception in terms that do not contradict the system" [p. 27]. Deviations are seen by adherents of the classical school "as technical mistakes to be simply passed over in silence or rejected" [p. 93]. To a certain extent the collective thought style is always imbued with a "harmony of illusions" [p. 28] for, he says: "What we feel to be an impossibility is actually mere incongruence with our habitual thought style" [p. 48]. Conversely: "Good work, done according to style, instantly awakens a corresponding mood of solidarity in the reader. It is this mood which, after a few sentences, compels him to regard the book highly and makes the book effective. Only later does one examine the details to see whether they can be incorporated into a system, that is, whether the realization of the thought style has been consistently achieved and in particular whether procedure has conformed to tradition (= to preparatory training). These determinations legitimatize the work so that it can be added to the stock of scientific knowledge and convert what has been presented into scientific fact" [p. 145]. As a result: "The thought style, developed in this particular way, made possible the perception of many forms as well as the establishment of many applicable facts. But it also rendered the recognition of other forms and other facts impossible" [p. 93]. To this extent, discovery is "inextricably interwoven with what is known as error" [p. 30]. Moreover, "The more developed and detailed a branch of knowledge becomes, the smaller are the differences of opinion" [p. 83]. Fleck concludes that: "Cognition proceeds in this and no other way. Only a classical theory with associated ideas which are plausible (rooted in the given era), closed (limited), and suitable for publication (stylistically relevant) has the strength to advance" [p. 30]. It follows that no style of thought can permanently suppress the matters of fact that are not in agreement with it. "In the end there are often more exceptions than normal instances" [p. 29], and we have the "bursting" of one thought style with the formation of new one. [Editor's note: Wittich here refers to p. 9 of German-language edition; no passage has been found in the English-language edition describing the process by which a transformation of thought style occurs.]

In Fleck's view, *scientific training* has an essential function in the tendency to persist of a thought style and systems of opinion consistent with it. "Any didactic introduction to a field of knowledge passes through a period during which purely dogmatic teaching is dominant. An intellect is prepared for a given field; it is received into a self-controlled world and, as it were, initiated," with the textbook serving as a sort of "catechism" [p. 54]. In this connection, Fleck ascribes an

an important role to popular science and, even 40 years ago, deplored its epistemological deficiencies [cf. pp. 112-116].

It is easy to see that many of the concepts made familiar by Thomas S. Kuhn clearly resemble those put forward decades earlier by Fleck. For example, Kuhn's doctrine of the paradigm has a close parallel in Fleck's concept of thought style; what Kuhn designates as the "theory-charged" nature of observations was similarly stressed by Fleck; Kuhn's distinction between normal and revolutionary phases in the development of science is at least very closely adumbrated by Fleck; both have almost identical views as to the role of scientific training, and of textbooks particularly, in preserving and furthering the practice of an established thought style or paradigm. It is true that these assertions have nothing sensational about them. Kuhn himself, in *The Structure of Scientific Revolutions* [Chicago 1970, p. vii], referred to Fleck as a thinker whose work "anticipates many of my ideas." Further, W. Stegmüller, for decades head boy and keeper of the Grail in Germany for a variant of positivism, has only recently learned (from the example of Ludwig Fleck) that his renunciation of positivism, published in 1973, did not need a Kuhn to bring it about as he had believed.

L. Schäffer [4], reporting the Stegmüller conversion, cites other bourgeois philosophers (besides Fleck) to show that the historical and social dimensions of scientific work had been known and discussed long before Kuhn. Unfortunately, Schäffer himself "overlooks" the authorities most important for his purpose, namely, K. Marx and F. Engels. Many of the ideas found in Fleck and Kuhn on scientific research and its history were expounded by Marx and Engels much earlier and on a sounder philosophical basis; moreover, here the ideas were not a matter of chance but were rooted in the *structure* of their philosophical thought. That any state reached by science can be understood epistemologically only if seen in its historical movement, that the history of a science presents revolutionary and evolutionary stages, that science is social in nature and is in a necessary connection with society as a whole, with the practical-material basis in particular—is there any Marxist who does not know that these insights have been known at least since Engels' *Dialectics of Nature* and that they are supported there by copious materal? [5]. Furthermore, Marx and Engels developed these findings philosophically on a much more solid basis than Fleck and Kuhn. For example, Fleck and Kuhn, while correctly stressing the social character of scientific work, are content with documenting this merely by pointing to striking instances. Yet Karl Marx *gave the basis* for this as early as 1844: "But also when I am active *scientifically* . . . then my activity is *social* because I perform it as a *man*. Not only is the material of my activity given to me as a social product (as is even the language in which the thinker is active), my *own* existence *is* social activity" [6]. And Marx had already derived the fact that his "*own* existence *is* social activity" from the social character of the material production on which the entire process of mankind's life and history is based.

It is not my purpose to start a dispute over priority, over whether Marx, or Engels, or Fleck, or Kuhn, or anybody else was the first to see certain conntions between scientific research and history. I wish rather to point out how much *difference* there is among them in the basis given for properties of science that they all noted, and which hence were *conceived* differently.

A further question of fact should be considered. To some people, just beginning to find their way out of the metaphysics of traditional positivism, a concept such as the " theory-charged nature" of observations may seem an intuition of almost epoch-making significance. At best they will see Karl Popper's so-called searchlight theory [the view that science itself throws new light on things] as a "milestone" on the road to this appreciation. They must certainly have no knowledge of a statement such as: "The *senses* have therefore become directly in their practice *theoreticians*" [7], an insight published by Marx in 1844.

Undoubtedly, thoughts expressed by Marx and Engels, often in quite general terms, sometimes only in the margin of discussion with a different orientation and, of course, within the limits of 19th-century science and its history, could often be elaborated by Fleck and Kuhn in more detail and more comprehensively in the light of the results and the experience of many more decades of scientific research. My concern is the consideration and appreciation of the historical achievement in the contributions of Marx and Engels toward the philosophical understanding of objects in the theory and history of science. I will show, from the case of Fleck and even more so from the case of Kuhn, that superficiality in philosophical thinking and ignorance of the history of philosophy, in places where philosophical understanding is required, led each of them to ideological *Weltanschaung* positions that undermine their aims and achievements in the theory of science [8]. This is why we have to take Schäffer's "oversight" so seriously and cannot pass over it as just the usual attitude towards Marxism of bourgeois thinkers.

We return to the views of Fleck and Kuhn concerning historical connections in the theory of science. Having seen how similar their views are in many respects, the question arises: What made it possible for Kuhn to become the center of a much respected and much discussed movement of bourgeois thought, a movement hailed as a "new approach" compared to positivist-oriented traditional theory of science, whereas 30 years earlier Fleck was hardly noticed [9] and soon quite forgotten? To explain this, we must consider the positivist-impregnated state in which Kuhn found the theory of science. This traditional theory of science, generally accepted until the sixties, was labeled by Hilary Putnam in 1960 as the "received view" in an article with the provocative title, "What Theories Are Not" [10]. Later, Stegmüller referred to it as the "statement view" and the "micrological mode of consideration" [11]. Theory of science had lost itself more and more in constructing and reflecting on models of science that gradually revealed, even to their advocates, two basic defects:

1) Not only were these models inadequately oriented to actual scientific research but their sweepingly formal character stood in the way of any penetrating view of the actual content of scientific theories, the origin of such content, its formation and practice. "Positivist theory of science, confined for the most part to study of the *logical* characteristics of theory, hypothesis, scientific law, explanation and prediction, never gets down to the *specific* content and contributes little to our understanding of the phenomenon, science" [12]. Accordingly, the practicing researcher and even more the practicing research collective could hardly find answers to these questions in the received view (or even expect to find them), though such questions of the theory of science presented themselves to scientists with ever greater frequency and urgency in connection with the nature of their specific theoretical tools, the organization and planning of their work, and the practical application of their results. The received view was belittled as "uninteresting" [13], and D. Pears went so far as to call it a "crude theory, interesting only for its footnotes" [14].

2) The received view also suffered from internal difficulties, contradictions, and absurdities which, despite decades of effort, the positivists have been unable to eliminate. Serious defects in the philosophical and theoretical foundations of traditional positivist-oriented theory of science were revealed by the efforts of its own adherents:
—to find a secure basis for all knowlege within the idealization itself (the so-called "protocol sentence" problem);
—to reduce all the concepts and assertions of science to those of physics (the so-called physicalism problem);
—to find especially a criterion making it possible to present any philosophical proposition as non-scientific (the so-called "sense criterion" problem);
—to reduce completely the specific quality of scientific theories to their empirical basis (the so-called empiricism" problem);
—to interpret cumulatively the relation of a more developed theory to its predecessors, i.e., trying to deduce the predecessors logically from the more developed theory (the so-called "cumulation" problem).

Until about 1960, the received view, because of its positivist mode of procedure and the impregnation of its content with positivist ideas, continued to perform an ideological service for bourgeois class interests because it deliberately refrained from theoretical examination of the basic structures of social life in general and capitalism in particular. It was thereby inhibited from considering the social determination of all scientific work, including that under capitalism. The closely related subjective-idealist and relativist tendencies implicit in the received view provided an ideological opening to influence people so that capitalism and its science would appear to them naturally superior, compared to a way of thought with materialist, dialectical and critical content that is damaging to capitalist ideology.

So long as the received view could be considered scientifically attractive or at least theoretically sound, its ideological influence increased. In the end, however, it became hard to conceal the fact that the received view was in a desperate state, its ideological usefulness seriously impaired both theoretically and practically. Hence, the bourgeois ideological interest in this theory of science could only tend to decline, no matter how faithfully and well the received view had served its class ideologically in previous decades.

A further factor was that scientific research, especially in the natural sciences, had become of great practical importance throughout the world, including the capitalist sector, with major economic, political and military impact in the universal struggle waged today between socialism and capitalism for the future of mankind. The bourgeois system, though neither willing nor able to dispense with the ideological influence provided by the received view, nevertheless *objectively* requires that its theory of science supply more energetically the theoretical advances that provide a basis for practical procedures to make capitalist science more effective. This includes the organization and planning of scientific work, as well as the development of bold theories and the training of a new scientific generation capable of thinking and acting creatively for capitalism. Bourgeois thinkers have long since posed this task for the theory of science. For example, L. Krüger, editor of the German edition of Kuhn's book *The Essential Tension*, wrote: "since the survival of mankind depends on it [science] which requires rapidly growing expenditures, it must be at least in part planned. The relation of science to society and politics, and their history, has thus become an inescapable theme" that calls, among other things, for "theoretical ideas of the 'mechanism' by which science develops" [15].

These, more or less, are the conditions under which Kuhn, through his book *The Structure of Scientific Revolutions*, was able to launch, within the bounds of bourgeois theory of science, the well-known movement against the received view. These conditions must be viewed as a whole if we are to understand why it is precisely Kuhn who got and is getting so much attention. The point is that a mere denunciation of the theoretical defects of the received view, which (aside from Marxist criticism of it) Ludwik Fleck had already performed in part, could hardly have sufficed to make positivism abandon one of its favorite offspring.

Kuhn's ideas and initiatives promised three things: a) a theoretical analysis of the distressing condition into which traditional positivist-oriented theory of science had fallen, with the reasons for its fall; b) the gradual erection of a theory of science that meets the needs of capitalist-dominated science today more comprehensively, more consciously, with a better theoretical foundation and greater practicality than the received view could; and c) conservation or even reinforcement of the bourgeois ideological potential inherent in the received view.

As a trained physicist and experienced historian of science, Kuhn vigorously demonstrated the deep discrepancy between the theotical content of the received view, on the one hand, and the objective needs of modern capitalist research, on the other [16]. He was able to point out two main sources, within the framework of bourgeois theory of science, for the impotence of the received view, namely, its general neglect of both the social character and the historical character of scientific work. Kuhn developed a series of hypotheses in the history of science which take into account the social and historical dimensions of scientific research, reflecting closely research procedures as they really occur or have occurred in history. In this way, he provoked methodological discussion of how (under capitalism) a theoretically grounded and practically useful science can, should, or must be conducted [17]. Yet, despite all his theoretical innovations contrary to the received view, he managed not only to conserve the ideological content of this traditional positivist-oriented theory of science but also to increase its actual or potential social influence, since the new approach, with which that content was now linked, grew in scientific standing or interest. Only because Kuhn accomplished the last task was he able to win the eminence in bourgeois thought that he enjoys today.

Much of what has been noted here about Kuhn could, at least in its trend, be said about Fleck as well. There are clear parallels between Fleck and Kuhn in their *Weltanschaung* and ideological conceptions. For this reason, it must be the changed historical situation of capitalism that provides the decisive reason why it was Kuhn and not Fleck who initiated so powerful a movement within bourgeois thought.

Permit me to make a tentative historical comparison as follows: Fleck had the ill fortune of trying to reform the original positivist theory of science at a time when the bourgeoisie was first becoming aware of the great ideological potential in this child of theirs and had hardly begun to exploit it. Ludwik Fleck could not but fail, as Otto Liebman would certainly have failed in 1840 if he had begun then instead of in 1865, with his sensational book *Kant und die Epigonen*, to recast the work of the Königsberg philospher as Neo-Kantianism. Kuhn, on the other hand, had the good fortune of being a "Liebmann" for the traditional positivist-oriented theory of science because historically he operated at a time when the bourgeois system had the objective need for such a reformer.

NOTES

1. This essay is revised from a lecture given 11 June 1977 at an international colloquium on "Philosophy—Science—Weltenschaung: Conditions and process of formation of scienti. c and world-view generalizations," on the occasion of the 450th anniversary of the Phillip University in Marburg (West Germany).
[*Editor's note:* Since Wittich's essay is based on the 1935 Basel edition of *Enstehung und Entwicklung einen wissenschaftlichen Tatsäche, Enfürung in die Lehre vom Denkstil und Denkkollektiv*, new material included in the 1979 Chicago edition will be mentioned below in bracketed comments, appended to the author's notes.]

2. Much effort has not enabled me to get much biographical information on Ludwik Fleck. What is given here was taken from the Fleck book [p. 22] and from L. Fleck and O. Elster: "On the Variability of Streptococci," *Z.f. Bakteriologie, Parasitenkunde und Infectionskrankheiten*, 125(¾): 180; 1932. [See also the Biographical Sketch appended to the English-language edition.]

3. Cf. Note 2 (above) and L. Fleck, "On Reactions, Pseudoreactions and Complementary Protection Procedures," *Z.f. Immunologie*, vol 94, 1938.

4. Cf. L. Schäffer, "Theories—Dynamic Complements: Remarks on Kuhn, Steeg, Stegmüller," *Z.f. philosophische Forschung*, 31(1); 1977. Here Schäffer criticizes, with reason, the impression produced by W. Stegmüller in *Theorie und Erfahrung* (West Berlin/Heidelberg/New York 1973) to the effect that it was only through Kuhn that he became aware that his previous (positivist) position had serious defects such as neglecting the historical development of theories. Stegmüller has since become an adherent of Kuhn and seeks to improve on Kuhn's ideas, making such statements as ". . . analysis of the structure of the sciences also includes analysis of the structure of development" [p. 6].

5. Cf. H. Bernhardt, "Friedrich Engels' *Dialectics of Nature* and Its Significance for the History of Natural Science," in *NTM. Schriftenreihe f. Geschichte der Naturwissenschaften Technik und Medizin*, 1; 1977.

6. Karl Mark: "Economic and Philosophic Manuscripts of 1844" in *Marx-Engels Collected Works*, New York 3: 298; 1975.

7. *Ibid.*, p. 300.

8. An article on this topic will appear in an early issue of this journal. [Cf. Wittich, "Hobbled Dialectics," *D.Z.f. Philosophie* 26: 785-797; 1978.]

9. So far as I know, reviews of Fleck's book appeared in Germany only in two publications, one not a philosophical journal (Carl Haeberlin, *Deutsche medizinische Wochenschrift* 63: 244; 1937) and the other preaching a Nazi-vulgarized monism (Prof. Dr. M.H. Baege, in *Monatshefte f. Wissenschaft, Weltenschaung und Lebensgestalt* (founded by H. Schmidt, Jena) 12: 380f.; 1937). Both reviews spoke favorably of Fleck's work, stressing particularly his notion of the collective nature of scientific work. However, neither review was capable of even indicating Fleck's concepts in theory of science, philosophy and the history of philosophy. I have found only one review of Fleck's book in a professional philosophical journal: H.M. Féret, in *Revue des sciences philosophiqes et théologiques* (Paris 1937, No. 26). [For a larger bibliography of reviews, see pp. 163-165, 191 of English-language edition.]

10. Cf. H. Putnam, "What Theories Are Not," in *Logic, Methodology and Philosophy of Science: Proceedings of 1960 International Congress*. E. Nagel, F. Suppe, A. Tarski, eds., Stanford 1962.

11. Cf. W. Stegmüller ref. 4 (p. 2).

12. W. Beyer, "On New Trends in Modern Bourgeois Theory of Science," in *Protokollband der 6. Arbeitstagung zu Fragen der marxistisch-leninistischen Erkenntnistheorie*, Leipzig 1977.

13. Stegmüller, ref 4, p. 3.

14. D. Pears, *Ludwig Wittgenstein*, Munich 1971, p. 36.

15. In the foreword of T.S. Kuhn, *Die Entstehung des Neuen*, L. Krüger, ed. (Frankfurt a. M. 1977, pp. 11f.).

16. Cf. T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago 1970), esp. pp. 1, 137 ff, 202; T.S. Kuhn in *Criticism and the Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge Univ. Press 1970), pp. 1, 231; and T.S. Kuhn, *The Essential Tensions* (Chicago 1977), esp. pp. 3, 105, 127, 165.

17. Cf. also W. Lefèvre, "On the Kuhn Controversy," in *SOPO. Sozialistische Politik*. 40: 62 f.; June 1977.

Editor's Comment: On Fleck Versus Kuhn

There is another historical aspect that may help explain why Fleck was ignored while Kuhn gets attention from the establishment. In the 1979 foreword, Kuhn seeks to put distance between himself and Fleck by rejecting the concept of "thought collective," pleading instead for the formulation of thought processes strictly in terms of "individual psychology" [pp. x-xi]. Following the same philosophical bent, Kuhn in 1965 went out of his way to agree with Karl Popper in denying the scientific content of Marxist historiography, at the same time claiming that his historical approach (by which he lumped Marxism with psychoanalysis and, by implication, with astrology) was superior to Sir Karl's approach [*Criticism and the Growth of Knowledge* (Note 16), pp. 7-8]. Note how far this separates Kuhn politically from Fleck who, in the repressive Poland in the 1930s, for an example of collective thinking chose to compare "the effects of terms such as 'materialism' or 'atheism,' which in some countries at once discredit their proponents but in others function as essential passwords of respectability" [p. 43]. (This book could not be published in Poland because Fleck was a Jew, and the year it came out he was dismissed "as an anti-Jewish measure" from the bacteriological laboratory he had headed [p. 150]). Mark Kac, who, as a graduate student in Poland during the 1930s, met Fleck at university gatherings, reports that Fleck was much respected by a Marxist-oriented faculty member (private communication).

A philosophical gulf between the two is also revealed in the different ways they treat the category of truth. Kuhn, in his effort to refute the charge of relativism, struggles vainly to escape from the confusion of a positivist formal approach in which a theory or a paradigm is (logically) either true or false (cf. *Structure of Scientific Revolutions*, pp. 168-173]. Fleck, on the other hand, matter-of-factly asserts its relative nature: "there is probably no such thing as complete error or complete truth" [p. 20]. The difference in the materialism of their philosophical outlooks may reflect the fact that Fleck was a hands-on practitioner of science while Kuhn switched to history of science in graduate school.

From such evidence, one may question the extent to which Fleck belongs in the same category as Kuhn, for whom the characterization as reformist seems more apt since his whole thrust has been to save the idealist formulations while seeking to remedy the methodology. To see Kuhn as a reformer, however, does not imply a simplistic rejection of his historically-based concepts such as paradigm and scientific community, since these have already proved useful to practitioners of science. What is needed is materialist interpretation of what is valid in Kuhn's methodology, together with criticism of his idealist formulations from the standpoint of Marxist theory of knowledge.

For the purpose of critizing Kuhn, it is very worthwhile to read Fleck who, without benefit of Marxist terminology, provides nevertheless an excellent description of science as a special form of consciousness.

And This Comment From Irving Adler

Wittich begins well by showing that some of Fleck's concepts anticipated Kuhn, that Marx and Engels anticipated Fleck, and that Marx and Engels put these concepts into a unified setting on a strong philosophical basis. However, I find the rest of the essay feeble. Here Wittich raises the interesting question: Why did Kuhn become the center of an influential movement while Fleck was hardly noticed and soon forgotten? However, Wittich's attempted answer, that Kuhn wrote at a time when the bourgeoisie needed his theory, has these serious weaknesses:

1) It makes it sound as though a bourgeois executive committee chose a theory that would be useful to it.
2) It is unsupported by any significant evidence.
3) It ignores the fact that most scientists have little or no interest in philosophical questions and are completely uninvolved in the discussion of Kuhn's theories.
4) It ignores this question: How much of the attention given to Kuhn is a consequence of the *accident* that Harvard had as president a chemist (Conant) who, because of personal interest, strongly encouraged the study of history of science at the university.
5) It ignores the fact that Marxists, especially in England, prepared the ground for increasingly serious attention to history of science: Did not Kuhn's views, just as Popper's, develop in relation to and as a reaction to the views of Bernal, Hogben, Haldane, etc.? □

History, the Matrix of Logic

The concept "historical" means objective reality in a state of motion and development. The concept "logical" means the necessary connection of thoughts reflecting surrounding reality in man's consciousness.

The historical [in knowledge] is primary to the logical, which reflects the former . . . The logical does not fully coincide with the historical . . . The logical must not and cannot reproduce all [the] zigzags of history. Its sole objective is to reflect the necessary changes, the necessary tendency to pass from one qualitative state to another . . . The logical thus reproduces the historical that is free of fortuity. Engels stressed the agreement between the logical and the historical when proceeding from the abstract to the concrete. He wrote: "The chain of thought must begin with the same thing with which this history begins, and its further course will be nothing else but the reflection of the historical course in abstract and theoretically consistent form; a corrected reflection but corrected according to laws furnished by the real course of history itself . . ." – A.P. Sheptulin, *Marxist-Leninist Philosophy*. Moscow, Progress 1978, pp. 168-70.

Practice as the Cure for Mysticism

Social life is essentially *practical*. All mysteries which mislead into mysticism find their rational solution in human practice and in the comprehension of this practice. – Karl Marx, Theses on Feuerbach No. 8. In Marx, Engels, Lenin, *On Dialectical Materialism*. Progress, Moscow 1977, p. 31.

A Matter That Concerns the Future of Science

MATHEMATICS EDUCATION: *The Fraud of "Back to Basics" and the Socialist Counterexample**

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Conferences that deal with the shortcomings of our educational system rarely consider the subject of mathematics. Perhaps the participants in such conferences are themselves victims of the abysmally poor teaching that is common in the United States, particularly on the elementary level. They may be suffering from the recently-publicized syndrome known as "mathophobia" or "math anxiety," and may prefer to avoid the subject altogether.

Yet it is the content and methodology of the mathematics curriculum that provides one of the most effective means for the rulers of our society to maintain class divisions. The "back to basics" movement in mathematics education is the current version of this sorting-out process. By "back to basics" is meant a return to rote memorization of arithmetic facts and to drill in computation, the kind of math that parents recall from the "good old days." Legislators, on the other hand, are pushing "back to basics" in order to cut education budgets and to perpetuate class inequities.

Mathematics has an all-pervasive influence in our technological society. Without an understanding of numbers, one cannot even comprehend much less analyze the reliability of stories in the media about unemployment or about the military strength of the U.S. versus that of the USSR. How can people make intelligent decisions about nuclear power, environmental protection, the use of computers, or the national budget, without knowledge of mathematics?

As computers take over in industry, skilled workers either lose their

* Abridged version of paper delivered at First Annual Eastern Marxist Scholars Conference, 17-19 Oct 19890, Hostos Community College, Bronx NY.

jobs or become low-level cogs in the production process. Unions must understand these developments and devise methods of protecting their members. Many careers are closed to those who are inadequately prepared in mathematics, particularly women, minorities, and the poor.

The vital role of mathematics is well recognized by socialist societies, even the least developed among them. Mozambique, with an illiteracy rate of 93% in 1975, has adopted the slogan: "Let us make mathematics a weapon in the building of socialism" (Gerdes 1980).

It is instructive to compare mathematics education in the United States with that in socialist countries, from the point of view of goals, research and planning, curriculum content, teaching methodology, and teacher training. I shall refer in particular to the Soviet Union, Hungary, Cuba, and Mozambique.

Why "Back to Basics" Is a Fraud

People associate "back to basics" with the return to good solid earth after a flight into Never Never Land. In reality, the slogan hides a plan for miseducation imposed on the educational system by the dominant capitalist class.

In relation to mathematics education at the elementary level, "back to basics" implies the following (cf. Hilton 1980):

ROTE LEARNING. Arithmetic facts taught by drill in the form of dull, meaningless repetition, with emphasis on getting right answers to simple problems rather than on the process of solution. Formulas and algorithms (procedures) memorized without understanding of the underlying arithmetic concepts, so students acquire no way to distinguish between correct and incorrect procedures. Children grow to hate the subject.

NARROW CONTENT. Few applications to real life or to the environment of the students; no opportunity to grapple with complex relationships.

BEHAVIORIST APPROACH. Psychology of learning based on narrow behaviorist objectives exemplified in current textbooks and publishers' Scope and Sequence charts. Attendant emphasis on authority of teacher rather than on developing reasoning power of students. Classroom atmosphere of individualism and competition.

POOR EVALUATION. Standardized "achievement" tests do not help children or teachers discover sources of error, as Ginsburg (1977) shows with great insight.

This type of miseducation is probably more damaging in mathematics than in other subjects, and its effect more pervasive. Having no chance in school to learn to think independently, these students tend to be incapable of dealing with complexities of society, become easy prey for demagogues and fads, yet blame themseves for their failure.

If "back to basics" is indeed so harmful, why is there a demand for this type of schooling?

One immediate objective is to save money. As the military budget expands and money for education and other social services disappears, "back to basics" offers a solution to the lack of funds. Elementary school teachers, poorly trained in mathematics, are expected to handle classes so large that real teaching is impossible. A cheap and easy way out is to have the teacher hand out workbooks and worksheets, and keep children busy with dull, repetitious drill.

The long-range objectives of "back to basics" is social control, carried out through "a method of pedagogy that fosters both an uncritical mode of thinking and a sorting into job slots which inevitably maintains the class structure of the American society. Memorization as a method of pedagogy fits both criteria" (Washington and Taylor 1980).

In criticism of the underlying behaviorist psychology, mathematician Peter Hilton wrote (1973): "Genuine education teaches people to think and to question, whereas behavioral objectives, which are rewarded by the managers, are those of acceptance, loyalty, and efficiency."

Scores on standardized achievement tests are used to track children into differentiated classes, as well as to provide "the ceiling on the amount of learning children are expected to achieve. If the scores are low, they are not expected to learn much and therefore no great effort is made to teach them" (Pollack 1980). As they advance in school, their scores become progressively lower, and they lose all confidence in their scholastic abilities (Rodriguez 1974; John 1974). No wonder they tune out!

Who are the victims of "back to basics"? They are the poor, the minorities, those destined to fill jobs requiring little skill. Increasingly, they are the future unemployed, people who may never hold a regular job during their entire lifetimes. Some say that our schools really act as filters to separate out those who will become highly paid professionals from those who will end up in low-paid unskilled work. Commenting on this process of producing "serfs," the author of an excellent elementary math program, Robert Davis (1975: 45), points to recent studies showing that women typically avoid the fields requiring mathematical ability, and that there is a sizable positive correlation between the amount of math required and how well a field pays, thus forcing women into "serfdom." Davis concludes on this hopeful note:

> We have never done much to help the serfs, unless and until they became organized and compelled us to see their plight with new eyes. The day is fast approaching when neither blacks, nor the poor, nor women, nor minorities can be wholesaled into serfdom.

The introduction of "new math" marked an attempt to upgrade the content and methodology of mathematics education. The attempt was doomed to failure in the United States. Let us see why.

Mathematics Education in the U.S. Since 1955

Few acdemic disciplines are so poorly understood in the U.S. as mathematics, and the debates in the media about "new math" versus "old math" have confused the public even more. What is the controversy all about?

I shall review the highlights of the history of "new math." For a fuller discussion see NACOME (1975), Hill (1976), and Fey (1978, 1979).

In the 1950s educators and mathematicians throughout the world began to examine the mathematics curriculum. The old rote memorization methods and outdated content were no longer adequate to the expanding economy's demand for more and better-trained technical workers. Advanced courses in mathematical methods were becoming a prerequisite for specialization in the social sciences and business management.

In 1957 Sputnik astounded the world. Alarmed by the threat of Soviet technological superiority, Congress and private foundations appropriated funds on an unprecedented scale for the design of modern mathematics programs. Mathematicians from universities and industry gathered at conference sites, and the "new math" was born in the United States.

Actually there were several types of modern math programs, all with the common goals of emphasizing the underlying structures and of updating the curriculum. Elementary school mathematics stressed the concepts underlying the procedures in arithmetic, rather than mere rote memorization and drill in facts. Topics from geometry and statistics enriched the curriculum. Abstractions were introduced early. Some programs, influenced by the pscyhology of learning of Bruner and Piaget, encouraged the acquisition of concepts by "guided discovery" methods, aided by the manipulation of concrete materials (Adler 1963, 1966, 1972).

Unlike most countries of the world, the United States has no national institute of education, no central planning or funding except for specific curricula or educational populations, no inspection, no uniformity of programs or standards. Change in education is created through patchwork short-term responses to new stresses, which, in turn, create different stresses and different responses.

The commercial textbcok industry plays a critical role as an agent of change (Fey 1978). Publishers adopted some of the newly-developed programs, while others of equal or greater merit were virtually ignored. In the long run it was the textbook salesmen, representing Big Business, who established the curriculum in the states and the local school districts.

Innovations were introduced first at the college level, rather than laying a foundation in the early grades. New college courses, designed to prepare an elite of professionals and managers, were followed several years later by revisions in the secondary school curriculum. Not until the late '60s and early '70s did "new math" enter the elementary school.

The most popular elementary textbook series gave little consideration to the needs of children, nor were teachers trained in the new content or methodology. The National Science Foundation, which provided courses for secondary and college faculty, was specifically prohibited from serving elementary teachers (Fey 1978).

Parents, too, remained ignorant of the goals and content of "new

math." No longer able to help their children with homework, they became confused, alienated, and angry.

Designed for college-bound students of white middle and upper class origin, the programs ignored the cultural diversity of our population. Mathematical concepts were expressed in unfamiliar language. There was little reference to children's own experiences. In fact, these very same programs were exported to developing countries, with no regard to the culture, language, or environment of these regions, often with disastrous results (Begle 1969). No wonder the Nigerian government, one of the beneficiaries of American expertise, abolished "new math" in the primary schools in 1977 (Ohuche 1978).

By the mid-seventies alarm over falling scores on standardized tests, the popular cry for "back to basics," and criticism from colleagues, the media, and the public forced the leaders in mathematics education to take a close look for the first time at the mathematics curriculum (NACOME 1975). They found:

– Test scores had declined in all subjects, not just mathematics.

– Many states (among them New York) do not require courses in mathematics content or methods as part of the preservice training of elementary school teachers.

– Elementary teachers could not cope with abrupt changes in curriculum, and few had implemented the new programs. Most concentrated on drill in arithmetic computation, the topics emphasized on standardized achievement tests. Some required their students to memorize definitions and abstractions, without regard to their meaning or to applications. Commercially-produced manipulative materials lay unused in classroom closets.

– Children could compute efficiently, but were weak in problem-solving (Carpenter et al, 1980).

– The potential of computers and hand-held calculators was ignored.

– Two curricula had emerged in secondary schools. College-bound students learned concepts and problem-solving, while minority students and children of the poor were steered into courses for "consumers," where they were taught more computation and trivial applications to everyday life. New York requires only a ninth grade math course, of any level, for high school graduation, while New Jersey has no requirement whatsoever. Basic competency tests, mandated for high school graduation in many states, emphasize computation.

Have the well-intentioned innovations of "new math" been discarded? Fey comments (1978): "Especially in the United States, education seems easily swept by superficial fads which consume enormous energy of innovators but burn out quickly when they fail to yield quick and permanent solutions to deep and longstanding problems."

And so the country jumped on a new bandwagon, "back to basics." As money for education and human services dwindled, local school boards have had to cut their budgets, resulting in larger classes, elimination of specialists and aides, and emphasis on the fulfillment of narrow behavioral objectives. While some textbooks retain topics other than computation, the workbooks and worksheets that accompany the texts are devoted almost entirely to drill, and frequently constitute children's entire experience with school math.

Fred M. Hechinger comments (9/26/80): "The widespread demand for cheaper, no-frill schooling threatens to produce a two-tier system offering austerity for the poor and much of the middle class, and all the perks, including art, music and playing fields, for those who can afford them." (See also Hechinger 4/1/80).

This two-tier system is obvious in mathematics education. Mathematics teaching in inner-city schools is geared to the expectation of low achievement, and children are promoted solely on the basis of good conduct (Goldman 1980; Hechinger 4/29/80). In wealthy districts, on the other hand, public schools can offer up-to-date curricula, well-trained teachers, small classes, and computer education for every student, as exemplified by the five-year project with Teachers College, Columbia University, to "integrate computing into the entire Scarsdale (New York) curriculum, kindergarten through twelfth grade" (*TC Today* 8, Fall 1979). Federally-funded "programs for the gifted" enable predominantly middle class and wealthy children to enjoy small classes, teacher aides, and the creative programs to which all children should have access. Secondary school mathematics departments are begging for teachers, while most of the qualified young people prefer to take more lucrative and less stressful jobs in industry (Maeroff 9/29/80).

A Socialist Alternative: Mathematical Literacy for All

The following incident, which took place during the Allende regime, illustrates how socialism at last gave Chilean workers the opportunity to use their almost forgotten school arithmetic. A group of metallurgical workers, having taken over their plant, were learning the theoretical and technical aspects of production from a visiting physicist. After a lengthy discussion about the actual size of a post represented on a blueprint with the notation "Scale 1:5," they agreed on a solution. One worker smiled as he said slowly, "So that is what we can use the multiplication table for!" (Zaslavsky 1975).

Each society determines the system of education that best serves its needs. Socialist education has as its goal the creation of a well-rounded person who can participate actively and conscientiously in the construction of socialism, one who has a scientific dialectical-materialist concept of the world.

Three attributes give socialist education its particular character (Swetz 1978: 14-15):

—A strong central authority coordinates all aspects of education such as research, teacher training, and curriculum development. Funding for education is a high priority.

—Education is related to production, resulting in "a well-defined relevance between societal needs, school curricula, and student expectations."

—Education in its broadest sense is a priority of society, and includes not merely school instruction but extracurricular activities, communications media, and youth organizations. Principles of egalitarianism and collective living are stressed.

"Mathematical literacy for all in a technological society" is one the goals of socialist education. How well the Soviet Union has fulfilled these goals is attested by University of Chicago professor Izaak Wirszup, an authority on mathematics education in Eastern Europe and the USSR:

> For the 98% of the school age population that now completes secondary school or its equivalent, the Soviets have introduced science and mathematics curricula whose content and scope place them far ahead of every other nation, including the United States. . . . In only ten years, the Soviet compulsory program for all students covers the equivalent of at least 13 years of American schooling in arithmetic, algebra, and calculus, and does so much more thoroughly and efficiently (Wirszup 1981).

In 1965 Cuba took steps toward the introduction of a mathematics program based on "modern views of mathematics and new ideas of active learning. . . Mathematics must be considered of primary importance, both because of the close bond that exists between it and the other sciences it serves as an instrument, and for its own application in many varied branches of knowledge" (Vilella 1975).

Recently-independent Mozambique seeks "to stimulate the broad masses to take an interest and delight in mathematical creation, combatting the elitist idea that mathematics is not for everyone, and to stimulate the type of teaching which, linking theory with practice, places the applications of mathematics within reach of the worker and peasant masses" (Gerdes 1980: 11).

In the socialist countries every student studies the modern mathematics that has been rejected in the U.S. as too difficult and abstract for all but the elite (Swetz 1978; Davis et al 1979). The Soviet program is "modern in content, innovative in approach, well-integrated and highly sophisticated. It gives strong emphasis to theoretical foundations and logical rigor as well as to applications. Moreover, advanced Soviet reserach in the psychology and methods of learning and teaching mathematics has been applied in the new curriculum, which now surpasses in quality, scope, and range of implementation that of any other country" (Wirszup 1981).

All graduates of the Soviet general polytechnic school have received training in mathematics matched only by the elite among American high school graduates, an elite in which women and minority students are vastly underrepresented (Walsh, 1980). Socialist educational systems do not track students into "easy" or "hard" courses at the pre-university level. Every normal child throughout the country studies the same math—both boys and girls, whatever their ethnic background.

Young people with special interest in mathematics can take elective courses, or join interest circles in the Pioneer Palaces, where they work with professionals in research, computers, etc.

The top primary school graduates in Cuba may attend special math-science schools, such as the 4500-student Lenin Vocational School in Havana, for grades seven to twelve. Beginning in the ninth grade, groups of 15 students form interest circles, where they work with professionals in such fields as applied mathematics, information theory, computers, and nuclear physics (Kozol 1978). Girls constitute about half the student body, and seemed surprised at the suggestion that "girls don't do well in math and science" (Wald 1978: 362).

Planning and Research under Socialism

Curriculum revisions are carefully planned, and are preceded by a lengthy period of examination. Mathematicians, psychologists, educators, and sociologists—all are involved in designing new programs.

> In the USSR . . . educational planning follows a clear, deductive sequence. Official planning begins with one societal objective for schools—to produce good citizens. Given this goal, the organization of educational planning and research follows deductively. The writings of Marx and Lenin are used to outline what it means to be a socialist and the attributes of how to become one. Then a series of principles related to learning and instruction are posited, based on such authors as Vygotsky, Galperin, and Leontiev. These are psychological principles of communist upbringing. Next, the structure of the content to be taught is specified. It is only here that mathematicians and mathematics enter. The need for mathematics must be first justified as socially useful . . . The respnsibility of the mathematician then is to specify the concepts, skills, problems, and methods of mathematics, and to help the psychologists and pedagogical scientists in their translation into lessons (Romberg 1979: 94-95).

Soviet research in the psychology of learning is generally acknowledged to be among the best in the world, and mathematics is considered the ideal subject with which to analyze thinking processes (Davydov 1975; El'konin 1975; El'konin and Davydov 1975; Goldberg 1978; Kantowski 1979; Krutetskii 1976; Rachlin 1979; Romberg 1979; Steffe 1975; Zankov 1977). Soviet and other Eastern European research is guided by a Marxist dialectical-materialist philosophy and by the learning theories of L.S. Vygotsky (1962, 1978) and his followers. Learning is viewed as a dynamic process based on the active participation of the learner under adult direction. Development proceeds both quantitatively and qualitatively, and advances with the learner's own activity in mastering concepts. Children's mastery of knowledge should be judged by the degree of assistance they require to solve new problems based on this knowledge, or, as Vygotsky stated it, the child's "zone of potential development."

New content and methodology are constantly under investigation. Vygotsky proposed a "genetic" method of dynamic research that would deal with qualitative rather than quantitative data. In long-term teaching experiments, using clinical interviews and other observational techniques, the team studies changes in mental processes as children confront new content and methodology. Classroom teachers participate in devising and testing new texts and methods. An outstanding example is the 15-year study by Zankov and his associates, started in one class, and eventually expanded to over one thousand classrooms (Kantowski 1979: 142; Rachlin 1979: 121; Zankov 1977).

Curriculum Revision in Socialist Countries

Curriculum revision begins in the lowest grades, and is gradually extended to the upper grade levels. As a result of experimental studies by Zankov and others, the entire curriculum of the Soviet primary school was restructured, the content upgraded, the methodology revised, and the four-year primary program compressed into three years, starting at age seven.

From about age ten, when Soviet children enter secondary school, they are taught mathematics by specialists. (This is true also in Hungary and Cuba.) Soviet curriculum reform for grades four to ten, implemented over the period 1969 to 1975, involved teacher preparation for the gradual introduction of new textbooks, syllabi, and methodology at successive grade levels (Shabanowitz 1978: 71, 82-87). Although the curriculum had been carefully tested, complaints of overcrowding and too much abstraction, particularly from rural and non-Russian language schools, led to a new round of revisions (Kashin 1977; Maslova et al. 1977).

In 1965 Cuba, impelled by the need for immediate action, adapted the primary school mathematics curriculum of the German Democratic Republic as a transitional program. Tested in seventeen first grade classrooms in 1967-68, and in 2000 urban classrooms in the following year, the program was gradually extended to the upper grades and to rural schools, while teacher training continued with the assistance of specialists from the GDR, television programs, seminars and courses, and study guides. By 1977 all schools throughout the country were using a uniform modern program for the six primary grades, and in 1980 for grades seven through twelve.

In Hungary the Ministry of Education adopted a transitional program in 1974, permitting the gradual introduction of modern mathematics and the retraining of teachers. A program of long-term experimentation, backed by the Hungarian Socialist Workers Party and the Hungarian Academy of Science, and involving the participation of many diverse elements in the society, should result in well-researched reforms by the year 2000 (Suranyi and Halmos 1978). The Hungarians, concerned with public reaction, decided upon a compromise between the new and the traditional as the initital step. They did not want "new math" to be the target of popular jokes, as in America and Western Europe! (Halmos and Varga 1978).

Socialist educators, as well as those from many non-socialist countries, look at our educational system and wonder how we can function amid such chaos, while American educators criticize what they consider excessive regimentation under socialism. Soviet teachers are expected to follow detailed syllabi fairly closely, and to use the recommended methodology, with little leeway for the kind of innovation that some American teachers introduce in their classrooms. On the whole, concludes Romberg, Soviet children come out ahead. Based on his first-hand experience with many American classrooms, he states

". . . it is agonizingly plain that the teaching of mathematics by many elementary school teachers in the U.S. is awful . . . The Soviet experience has led me to question our tradition of allowing teachers wide latitude in instructional decision making. When latitude leads to license to be incompetent, can we afford it?" (Romberg 1979: 93).

Socialist educators continue to search for ways of improving the acquisition of knowledge and the development of the personality. While debate continues on curriculum content and methodology, Soviet psychologists are undertaking research on affective aspects such as creativity, motivation to learn, ability to work independently, influence of the collective on the individual, and formation of the moral aspects of personality (El'konin 1978; Davydov 1978). Under investigation is work of scientists like the Bulgarian Lozanov and his idea of making study a joyful process (Kondakov 1978), as well as novel ways of teaching mathematics so that every student can master the subject and "math anxiety" does not develop (Soloveichik 1979).

An Agenda for Action in the United States

The "back to basics" movement in the U.S. has two main objectives:

– To cut funds for education, thereby freeing resources for an ever greater military buildup. The results are large classes, poorly trained teachers, and rote memorization methods of teaching that actually prevent children from acquiring the necessary understanding of the world in which they live.

– To persuade the poor, the minorities, and females to accept their inferior status in a capitalist society. Minority and poor children are told that success in school will lead to well-paid professional positions. When miseducation virtually cuts them off, right from the start, from achieving these goals, they blame themselves for their failure.

The "back to basics" movement has been decried as simplistic, narrow and contrary to the essential purposes of education by the Organizations for the Essentials of Education, an umbrella group formed in 1977 by eighteen educational associations, including the National Council of Teachers of Mathematics. These groups are calling upon legislators, educators, parents, school boards, etc. to consider what is really basic in education.

The National Council of Teachers of Mathematics has published *An Agenda For Action* (1980), in which it recommends problem-solving as the focus of school mathematics in the 1980s, broadening the definition of "basic skills," updating the curriculum to include the use of calculators and computers, a new look at testing and research, and greater public support for mathematics instruction. Other groups have expressed similar concern (Hechinger 4/21/81).

Ohio State University professor Marilyn Suydam (1979) warns that current elementary school mathematics instruction may be depriving children of career options. Shirley Hill, past president of NCTM, goes further (1980): "Our job must be to prepare our students for the non-routine, for the unforeseen, the unfamiliar, and the uncertain . . .

Our students must learn to learn." Mathematics, perhaps more than any other subject area, affords one of the best mediums for problem-solving, for grappling with the unfamiliar, for learning to learn.

Special efforts must be made with respect to the mathematical education of minorities and women (Rule 1980; NCTM 1980: 18). The Association for Women in Mathematics included in its questionnaire to the candidates for office in the American Mathematics Society in 1980: "What efforts should be made to increase the percentage of mathematicians who are women, black, and Hispanic?" Organizations such as "Women and Mathematics" and "Blacks and Mathematics," operating on a shoestring, urge female and minority students to take high level math courses. Role models visit schools to speak not only to students, but also to the teachers, guidance counselors, and administrators who, in obvious or subtle ways, steer these students away from enrolling in demanding courses.

Parents, teachers, unions, and community people must organize to exert pressure for quality education for all children, starting at the preschool level. Every child is entitled to receive the foundation of mathematical learning essential to fufilling his or her potential as a productive citizen. Only a tremendous increase in commitment and funding can accomplish these goals. Let us fight for education, not guns!

Young people with good mathematical backgrounds are finding that careers as scientists, technicians, and computer experts are increasingly linked to the military. The claim that the U.S. is falling behind the Soviet Union in technological capability is used by the shapers of foreign policy to justify their demand for military expansion and the development of new weapons systems. This claim is also behind their present focus upon our mathematics and science education. Wirszup concludes his statement (1981): ". . . the recent Soviet educational mobilization . . . poses a formidable challenge to the national security of the United States, one that is far more threatening than any in the past and one that will be much more difficult to meet."

We must be loud and clear in rejecting this point of view. Let us set our sights on "keeping up with the Russians," not for war, but for the kind of meaningful education for all our children that will lead to peaceful coexistence of all the nations of the world.

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PRAISE OF LEARNING

Learn the simplest things. For you
whose time has already come
it is never too late!
Learn your ABC's, it is not enough,
but learn them! Do not let it discourage you,
begin! You must know everything!
You must take over the leadership!

Learn, man in the asylum!
Learn, man in prison!
Learn, wife in the kitchen!
Learn, man of sixty!
Seek out the school, you who are homeless!
Sharpen your wits, you who shiver!
Hungry man, reach for the book: it is a weapon.
You must take over the leadership.

Don't be afraid of asking, brother!
Don't be won over,
see for yourself!
What you don't know yourself,
you don't know.
Add up the reckoning.
It's you who must pay it.
Put your finger on each item,
ask: how did this get here?
You must take over the leadership.

Bertolt Becht, *Selected Poems*, tr. by H.R. Hays.
Harcourt Brace Javanovich, New York 1947.

A Tutorial for Newcomers to Marxist Philosophy

DIALECTICAL MATERIALISM

By J.D. Bernal

EDITOR'S FOREWORD. Few have expressed so well as physicist John Desmond Bernal (1901-1970) the connections between natural science and political economy, between natural scientists and their social environment. The following essay, reprinted from *Modern Quarterly* [30(2): 80-101; 1948], provides a brilliant statement of Marxist philosophical principles as they relate to both the inner processes of natural science and the interactions with the "outside world" from which scientists cannot escape.

Some anachronisms will be encountered in this paper, such as references to Stalin that seem naive in the light of present knowledge of the contradictions in Stalin's role. (For Bernal's later position on Stalin, see his *Science in History*, MIT Press 1971.) The general tone of hopefulness, reflecting the historic defeat of Hitler, may also seem strange to a generation born into a world that has not turned out so simple. While such anachronisms could have been deleted by severely abridging the work, they are left here as testimony that contradictions will always be developing behind our consciousness. The world historical process, in which both concepts and institutions become transformed into their opposites, provides the experience by which humankind learns more about how to control its destiny.

Keeping this historical context in mind, the reader should find much that is useful concerning the process of developing new knowledge. The basic content of Bernal's discussion concerns underlying philosophical principles that help to attain fuller consciousness of the historical processes at work within science.

Dialectical Materialism, by J.D. Bernal

In the *Communist Manifesto*, Marx and Engels first put forward what was an entirely unheard-of analysis of society and predicted the fall of a capitalist society that had then not even reached its full development. Marx belongs to our time because he foresaw it. He was enabled to predict because he not only observed the world of his time but analyzed it and struggled to change it. His predictions have come true, not merely because they were well thought out and soundly based in theory, but because his own life and work stood as an example of how to turn that theory into practice. He was the first great philosopher who did as well as talked.

Looked at in retrospect, we can now see dialectical materialism, the philosophy of Marx, as a definite and culminating step in the great tradition of human understanding and mastery of the world. Yet because Marx, in spite of his academic training, worked outside the respectable, academic world of philosophers, economists and historians, his contribution was not appreciated or as much as noticed in the learned world of his own time, even while it was everywhere making its mark on history. It is only now that we can see that the philosophers of the late nineteenth century and of the early twentieth represented the backwaters and dead ends of knowledge and that the main stream of human thought follows the direction that Marx was the first to point out. It the last few years, academic philosophy, buffeted by crises and wars which it failed to predict or explain, and unable to offer any guidance to perplexed humanity, has collapsed as catastrophically as conventional market economies. All that is left is a number of polite but totally ineffectual philosophies taught at universities, a revival of dead religious dogma, and outside them a large mass of non-intellectual or even anti-intellectual beliefs, ranging from fairly harmless astrology and spiritualism to the foulest bestialities of the Nazi race theory, of which unfortunately we have not heard the last.

In these times of intellectual decay the philosophy of Marx stands out firm and flourishing. On account of its origin and character it has been immune from the disintegrating forces that have destroyed other forms of human thought. For it is reasonable and scientific; it is comprehensive; it is a philosophy of change for changing times; it is a philosophy of action and not of contemplation, of hope and not of despair; and last and most important, it is, as we shall explain, the philosophy of the working class.

Rationality

Marx and Engels, Lenin and Stalin have carried on the tradition of a rational and non-mystical approach to all human problems; this is the tradition of Greek philosophy and the founders of modern science. Careful analysis; separation of factors; the following of causes into their effects; reliance on experiment: all are taken over into Marxism and provide it with a hard scientific core. There is nowhere any pandering to special intuitions or spiritual experiences.

Comprehensiveness

This does not mean limiting philosophy to mere natural fact—that was the mistake of the old materialists. On the contrary, the essence of Marxism is that while it remains firmly based on the material universe, it includes the whole range of human experience. It deals with society in its productive relations: with the economic and legal forms which have grown out of these relations: and with the whole ideology of science and art and religion, that forms the superstructure of the productive and economic life of society. By relating all these together and by ceaselessly reviewing their relations throughout the changes which society has undergone, and never more rapidly than now, it has a comprehensiveness which no other philosophy has ever achieved.

A Philosophy of Change

The ages in which great philosophies or religions have appeared have all been ages of intense social change. The India of Buddha, the China of Confucius, the Greek cities before Socrates and the Syria of the first century were all in a transitional change between different social groupings. The great seventeenth century, the age of Descartes and Newton, the beginning of the triumph of capitalism, was just such another period. Nevertheless, until Marx the main stream of philosophical and religious thought conceived an ideal philosophy fitted for an ideal, static civilisation. Men saw the evils of their times and strove to hold them back by an appeal to the better social traditions of a stabler time. Even in the seventeenth century, reformation rather than new creation, the return to reason rather than the achievement of new things, was the dominant note.

The philosophy of Marx was the first to acknowledge explicitly the dependence of social organisation on changing technique: the permanently changing nature of human relations and the way in which that change manifested itself in violent revolutions. Marxism does not ask for a return to any ideal state of the past, but demands that men shall understand enough to build and keep on building new social forms for themselves in the future. It differs from the vaguely progressive liberalism of the nineteenth century by its deeper analysis, which shows that progress cannot be taken for granted. It shows it to be due to the interaction of economic and social forces though operating through consciously directed human wills.

A Philosophy of Action

In this respect also dialectical materialism is new. In the classic phrase of Marx: "The philosophers have only *interpreted* the world in various ways; the point, however, is to change it." (Thesis XI on "Feuerbach," written in 1845.) In Marxism, for the first time, thought and action are revealed as inseparable. Marx showed in his life as much as in his writings that any valid social theory implies positive and conscious action by its adherents. Dialectical materialism is a philosophy of action—not of the interested or deluded mystical action of the fascist—but of the carefully weighed, thought-out and timed action of the scientific socialist.

A Philosophy of Hope

It is this combination of knowledge and action that makes the philosophy of Marx predominantly a philosophy of hope. That hope is not a mystical one, nor one founded on that belief in an automatic deliverance through the operation of an inevitable chain of causes that is so often mistakenly attributed to Marxists. Hope is based on experience; the experience of more than a hundred years of bitter, often defeated, but ever more successful struggles. Marx gave men a new understanding of the relation of social forces. In the light of that understanding, they have known how to work with these forces and not against them and they have acquired an unlimited hope that, acting together, they will pass through the critical and transitional time that marks the passage from capitalism to socialism.

The Philosophy of the Working Class

Marxism is first and foremost the philosophy of that section of society which alone can initiate and carry through the only positive, hopeful and creative changes at the present time. It is the working-class philosophy. In the beginning it was learned from the working class and nurtured in the working-class movement. Its rise to importance in the world coincides with the rise in importance of the organised working class itself.

The open triumph of the proletariat began with the successful revolution and the building of socialism in the Soviet Union, and was assured in the heroic defence of the Union which saved Europe and the world from fascism. Marxism is a working-class philosophy, not in the exclusive but inclusive sense. Those who accept and act it—for the two are synonymous—are automatically themselves part of the working-class movement. For in another and longer view it is not limited to the working class; the state it aims to achieve is the classless state, and it has already shown in the Soviet Union that the philosophy of dialectical materialism is not the philosophy of one section but of the whole people. It inspires them, it holds them together, it gives them an intelligence and a strength, it is a weapon in war and peace more powerful than anything physical science can invent. As the philosophy of the working class, it is the philosophy of the people of the world of the future.

The Content and Method of Marxism

Dialectical materialism has an inner content and method of its own, both of which are well worthy studying, though the method cannot be profitably abstracted from its content but rather must be demonstrated as implicit in it.

The content of Marxism derives from the great liberal tradition of the seventeenth and eighteenth centuries, and later incorporated the sound scientific achievements of the nineteenth. It is a balanced and active knowledge of the totality of the objective world. Physics and sociology are both means of describing one real, self-moving and self-changing world. The unity of the universe, the close and necesary connection between objective and subjective, between life and non-living, between human nature and society, is fundamental. Where other philosophers, particularly scientific philosophers, fell into a dualism and separated mind and matter or facts and values, Marx insisted that such separations were simply a running away from problems that had to be faced and tackled. This "one-ness" was not itself—as it appeared to monistic philosophers—simply another dogma, a flattening out of experience to fit into a preconceived pattern. It was, on the contrary, intricate and complex. It was the totality of the relations binding the whole universe together in space and time. Every part of the universe was at the same time the resultant of all that had happened before and the source of all that was to happen afterwards. The Marxist unity does not deny the differences between the things and the processes that make up the universe; these very differences are themselves part of the unitary process of differentiation. Change is implicit in existence. The Marxist view, however, is equally removed from the pluralistic. The universe is not merely a shifting and changing chaos; it shows a sequence of orders of phenomena, each order derived from the previous one and including its phenomena in itself.

The method of Marxism depends on the discovery that significant dialectical changes in the universe were those which led step by step to the production of fundamental distinctions of order between different parts of it—between stars and animals and human achievements. Marx, long before Darwin, was a firm evolutionist; for him the world was a process and not a mere collection of things; but he was not happy in simply noting the fact of evolution, he wanted everywhere to see the fundamental reasons for innovation and change. He found those reasons precisely where change was most rapid and most easily observable, in the changing social and economic conditions of his own time.

The clue to the understanding he took from Hegel's dialectics; it was the content he gave to that clue, it was the way in which he understood the stages of capitalism and the next stages of its development, that makes Marx rather than Hegel the real philosophical orginator. The clue itself is that a process cannot in a real world continue unchanged in any direction, that it inevitably brings with it counter-processes and that the counter-processes, uniting with the original process, produce the true novelty or next stage in development. This is the central core of the theory of dialectical materialism We now see, thanks to Marx and Engels, the whole of the vast history of the universe as a series of transformations from stage to stage. These stages form a hierarchy or ordering of complexity, each one including all the complexities of the stages that went before and adding to them its specific order of complexity. The laws of chemistry, for example, hold for all the higher stages as in the chemical transformations that go on in living bodies. The individuals in human society are animals for whom all biological laws hold.

Marx saw that in moving from one stage to the next there is always something more. There is more in chemistry than in physics, more in biology than in chemistry. What is that something? That very form of question breeds a deceptive answer. The kind of answer that satisfied early man was that a spirit or breath of life was what distinguished man from clay. This is also essentially the view of the modern academic philosopher, who attributes new forms to an entelechy or principle or to the impact on dead matter of a vital force. With Marx the difference was not a "thing," a new substance, or even an emergent order. The difference was intrinsic, it arose from the very multiplicity and complexity of the earlier stage itself which in the new level of organisation manifested new properties, new modes of behaviour.

Revolutionary Change

There is one other crucial aspect of Marxism whose form is not derived directly from Hegel. This is the sharpness of the conflict and transformations by which new things come into the world. It is not that Marx did not recognise gradual change but that he saw that gradual changes ultimately lead to critical situations where change could no longer be gradual and a definite break has to occur. These breaks he saw in the social field as economic and political revolutions. For example, in England, all through the sixteenth and early seventeenth centuries, the growing bourgeoisie spread their adventures and piled up their profits; but the conflict with the older order of society which this process made inevitable could not occur gradually, it took the form of civil war and revolution.

The reason for the inevitability of un-gradualness is that any state or order in the world must be a self-consistent whole; it must unite economic practices, institutional forms, ideas and feelings. One part cannot be changed without involving the rest. But the whole has rigidity, it cannot bend and must break. Marx saw social change occurring not by gradual transition but through the necessary appearance of new systems inside the old ones. These new systems at first existed as part of the old, building up their own internal constitutions through the creation of new ideologies. The old and the new are never distinct; while in opposition they continually react on one another; but the old does not transform into the new, it is rather that the new breaks apart and shatters the old. Sharp transitions were not confined to human affairs; the whole of organic evolution, with the appearance of new dominant classes such as the mammals and, before that, the distinction of solid, liquid and gas, are examples of the abrupt breaks or nodal points which separate both in time and order of complexity the different parts of the universe. Dialectical materialism, while insisting on wholeness of inter-reaction, equally insists on distinction and abrupt change.

Materialism

The philosophy of contradiction and transformation was the philosophy of Hegel. The difference that Marx made to it was not merely of clothing of its abstractions by concrete examples in society and nature, it was in totally reversing it. Marxism was from the start on a material basis—and this is not quibbling about the nature or the reality of matter. It is a standpoint in philosophy which accepts the apparent universe, the universe we know and use, as the first thing. Our own thoughts and feelings must be derived from that universe. This view rejects the shadow show of idealism in which the universe is a dream and an illusion, which logically ends up with the purely private world of the solipsist to whom even other persons are the creation of his own fantasy. Marx does not try, as Hegel did, to evolve the world logically from one idea. Instead he accepts the world and proceeds to find out how it works and how to work it. In finding out how the world works we do in fact through science discover that animals existed before man and that a lifeless world preceded life. But there was before Marx's time, and there still is outside the range of Marxism, a reluctance to admit that the very thoughts and feelings of man are themselves resultants of biological and ultimately material processes, that "the beginning was the fact" and not the "word."

Materialist dialectics is something, however, as different from older materialism as it is from the idealist dialectics of Hegel. The older materialism was heavily influenced by the early achievements in natural science in establishing rigid and eternal laws. It was perhaps most clearly seen and almost reduced to absurdity by Laplace, who claimed that if we knew at any time the velocity and direction of every particle in the universe, it would be possible not only to determine all their past movements but all their future movements to eternity. Modern quantum mechanics had shown that it is not only mentally but physically impossible to determine the motions of all particles at any moment; but long before, Marx had criticised this viewpoint on the grounds that there was more in the universe than the motions of particles, higher orders of complexities had qualities and laws of their own, and that new complexities and new laws for them to follow were being continually generated. The materialism of Marx is not an automatic determinism. it is a continual re-determination with unlimited and, in detail, unpredictable possibilities.

Dialectical Materialism in Modern Thought

Dialectical materialism first appeared effectively to the academic and so-called intellectual circles outside the Soviet Union hardly more than twenty years ago. From the very start every effort has been made to ignore and ridicule it. Nevertheless, it is now a major issue on the philosophic front and has already generated its own opposition. Although the opposition to Marxism on intellectual grounds is largely due to misconceptions, these are not accidental. It is the desire to reject Marxism on political and economic grounds that consciously or unconsciously takes the form of philosophical criticism.

The two bases of attack on dialectical materialism are, not surprisingly, themselves almost completely contradictory. One is that

Marxism is a mere vague set of conventional aphorisms and is no real philosophy at all, in that it makes no provable or disprovable assertion; and the other is that Marxism is a rigid and dogmatic system which claims to determine once and for all the laws of the universe, to explain its past and to predict its future.

Dialectics and Science

The first objection is, effectively, that Marxism is not scientific. Now this depends on a misunderstanding as to the meaning and scope of science. By defining science narrowly and limiting its field of operation to physical quantities that can be more or less precisely measured and to changes that are cyclical and follow eternal laws, it is possible to exclude from science, not only Marxism, but the whole study of human societies, their history, their economics and their politics; in fact social science in its entirety. Marxism does not pretend to be limited to this narrow definition of science. Marx himself maintained, and Lenin and Stalin have demonstrated, that successful prediction and successful experimentation is not limited to the physical or even the biological sciences. Nevertheless it would be absurd to expect that the precise methods of argument which hold in the first, and to a certain degree in the second of these fields, can apply to the far more complex social phenomena. Dialectical materialism claims to be a mode of thought most suited to dealing with the events in the social field. It was built from observations in this field and has been the basis for successful action in it.

That does not mean that dialectical materialism is simply a philosophical basis for social science and something that stands apart from natural science. Because human society includes in itself the whole biological character of the individual human beings that compose it, in each of whom in turn the physiological processes follow the laws of physics and chemistry, so dialectical materialism does not stand beside natural science, but includes it. Natural science itself has two aspects; it stands in relation to the world of material objects and organisms; it is a summary of the methods of analysing and manipulating them, but on the other side it is also itself a human social enterprise built up by real men and responsive to the economic and political changes of society. Science as we know it today is not an abstract product of applied intelligence, it is an integral part—product and producer—of the achievement of capitalist technology. Capitalism made science possible; science makes capitalism superfluous.

It is this social aspect of natural science that is part of the wider synthesis of dialectical materialism. Dialectical materialism enters natural science in order to analyse its conclusions in relation to its origins, as Engels did, for instance, to nineteenth-century science in the *Dialectics of Nature*. But is also enters science in the field of action by indicating how science needs to be organised and how related to economic and social forces, a process first clearly undertaken in the Soviet Union and which was copied through force of circumstances by all the nations fighting in the late war, including the ultra-individualistic Americans.

The characteristic weakness of natural science as developed in the latter stages of the nineteenth and twentieth centuries—that is, in the period of decaying capitalism—was its inability to integrate with the social movements of the time. Just because the intellectual felt in danger of becoming a mere slave and parasite of capitalism, he tended to withdraw, or at least to pretend to withdraw, from the world; he took refuge in abstractions and overspecialisations; he prided himself on his impartiality and purity and on his very incapacity to deal with practical affairs. This was a very convenient attitude for the intellectual, who thus was able to let the captains of industry carry on without protest, and even to help them. It was also very convenient for the captains of industry.

Dialectical materialism offers the very antithesis of this attitude. While not trespassing on the field of the scientific observation and experiment, it is far from being vague and unprecise in its indications of the general direction of intellectual effort. It is the Marxist who knows what to do and how to set about doing it, while the pure intellectual, once the protective shell of his environment is broken, is utterly at sea and easily falls prey to the most unscientific and mystical extravagances.

The Character of Marxist Prediction

The other criticism of Marxism is that it claims to know everything and to predict everything, that it is in fact a return to the teleological systems in which events are determined by the ends towards which they tend rather than by what has happened before. This criticism is itself a reaction to the bankruptcy of the intellectual in the latter days of capitalism. In these days, while natural science is willing to enough to predict particular phenomena and is deeply involved in practical industry based on experiments, social science has withdrawn more and more into the sphere of abstract study. The historian of today claims that there is no theory of history and that all he has to do is to describe events as actually as possible. The economist dissociates himself from actual financial events such as booms and slumps and discusses the theory of an ideal economics which would hold if it were not for the unfair existence of trusts and trades unions. The philosopher gives up once and for all the search for truth and concerns himself only with precision of language. To all of them the concrete actuality of Marxism, its analysis of world history, its discussion of actual economic events, of crises and wars, its claim that even intellectual fashions were economically determined, is a disturbing and shattering challenge. Such pure intellectuals would prefer not to know anything at all than to have such knowledge, precisely because such knowledge is a call to action.

It was from this background that the attack on Marxism as a dogmatic, ready-made scheme was launched. Now, Marx himself, from *The Communist Manifesto* onwards, made predictions as to what would happen in human society. There we find:

> The advance of industry, whose involuntary promoter is the bourgeoisie, replaces the isolation of the labourers, due to competition, by their revolutionary combination, due to association. The development of modern industry, therefore, cuts from under its feet the very foundation on which the bourgeoisie produces and appropriate products. What the bourgeoisie therefore produces, above all, are its own gravediggers. (*The Communist Manifesto*)

Or, in a more generalised form, in *Capital*:

> Centralisation of the means of production and socialisation of labour at last reach a point where they become incompatible with their capitalist integument. This integument is burst asunder. The knell of capitalist private property sounds. The expropriators are expropriated. (Vol 1, Chapter XXXII.)

True, in his bitter exile in London Marx certainly on more than one occasion hoped for the success of that revolution which he had seen fail in 1848 and was to see fail again so gloriously in 1871. But we must distinguish, as he distinguished, between predicting the outcome of particular events in time and place, and that of a general movement in human affairs. A victory for the revolution such as occured in 1917 was a definitive thing. It gave the first possibility of building up the new stage in human affairs which Marx had predicted. On the other hand, a defeat for the revolution left all the contradictions that capitalism has engendered unaltered or even sharpened, it could never be definitive but only the prelude to further struggles. Lenin understood this well and showed his understanding in his own management both of the unsuccessful revolution in 1905 and the successful one in 1917. In any particular event success cannot be assured, but it is only worth refraining from action when success seems totally impossible or premature. To strike at the right time, or to refrain from striking at the wrong time, is to understand the dialectics of the particular situation.

Here again there is a widespread misapprehension of the meaning of Marxism. This ability to understand a situation, to act in it, are not things historically determined in some general and infallible way; they are abilities of actual men at a particular time, with their individualities, characters, judgment and failings. All important issues must depend on individuals. By all those who have real knowledge of Marxism—and such knowledge is not to be found so much books as in practical political activity—this is fully realised. But it is also realised that the individual is built up by the situation in which he grows and that the greatest individuals, the revolutionary leaders, represent most completely and most consciously the social forces actuating the great mass of their followers. The leaders are not separate from the people or above them; their strength is drawn from them. Nor is this true just for one or two great leaders. The revolutionary situation places individual responsibility in greater or less degree on hundreds, thousands and millions of men, women and children. The events of the past few years give a complete lie to the idea that Marxism deals only with inevitable movements of masses. During the war it was only in the Soviet Union and among the resistance movements that the individual rose to his full stature, and was able to deal by his own initiative and yet in perfect accord with the general plan, with situations far exceeding any older estimations of human capacity.

Dialectical materialism does predict and its predictions have a force which goes beyond the mere abstract accuracy of its analysis. It deals with a human situation in which human beings are agents as well as subjects. The understanding of Marxism, the consciousness of the movement of society, are themselves most powerful forces working towards the achievement of the predicted ends, and they are powerful precisely because they are conscious and consciously directed. The unity of Marxism in action is an organised unity and its very organisation is an expression of the acceptance, even before its full achievement, of conscious and planned human cooperation.

One hundred years have passed since Marx put out the first sketches of his method, just sixty years since he died. In those sixty years the great events of which he wrote had begun to happen. The crisis of the transformation which he predicted is with us at this moment. The circumstances of the world situation are vastly different from the apparently stable and expanding capitalism which filled his time. Nevertheless, so close was Marx to understanding the course of development that his own ideas and methods have suffered far less change in the interval than those current in intellectual circles over the period. Indeed, the other writers of the 'forties and the 'sixties of the last century are now only academic curiosities, while the words of Marx seem to apply not only to the present but still more to the future. What was apparent to Marx one hundred years ago was something which seemed highly paradoxical in his time, but the world has been forced to accept, by the march of events, many of the points that then seemed most remote from reality. Who would then have thought, outside the ranks of the Marxists, that economic stability, political liberty and peace were not blessings that humanity was likely to enjoy in greater and greater measure as time went on—

Till the war-drum throbbed no longer, and the battle flags were furled,
In the parliament of Man, the Federation of the World. (Tennyson)

Marxism and the Scientific Revolution

What is true for economics and politics is equally true for philosophy and science. In ideas, in organisation, and in relation to economic and political factors, the trend of modern science is more and more towards the approximation of Marxism. What is interesting is that this approximation was usually spontaneous—that is, it arose out of the development of knowledge of and control over natural forces and the interplay between this development and that of economic and political forms. If Marxism had been able to penetrate more rapidly into scientific circles, these results would have been obtained more clearly and with less trouble: but coming as they do they provide a remarkable and independent confirmation of the fundamental rightness of the Marxist view. That science should reflect the social and economic atmosphere of the time both in the balance of its interest in different parts of the universe and in the mode of expression of its discoveries is

a view that is now coming to be generally accepted. It is easy for us, for instance, to see in the original formulation of Darwin's *The Origin of Species*, the clear reflection of the free-for-all competition of the nineteenth century. Indeed, Darwin himself always admitted his debt to Malthus in the formulation of the concept of the survival of the fittest. At the time, however, this resemblance came to be used the other way round, and morals drawn from the supposed struggle for existence in nature were used to justify the more antisocial features of early capitalism, a tendency of which Herbert Spencer will remain a permanent caricature, and which was later to be the foundation of the Nazi race theory.

Now science in the last sixty years, and particularly since 1895, has undergone a revolution at least as great as its revolution in the middle of the seventeenth and at the end of the eighteenth centuries—the revolution associated with Galileo and Newton or that associated with Lavoisier and Dalton. Since 1895 the atomic structure of matter has been proved in detail; the quantum and relativity theories were elaborated; chemistry has become part of physics and the progress of biochemistry and genetics has reduced much of biology to chemistry and mathematics. In most ways discoveries of the last half-century have brought us far closer to the practical and reliable knowledge of the behaviour of inorganic and organic systems than all the previous discoveries of science put together.

The Atomic Age

This great revolution of knowledge has already reached a culminating expression in the making and the using of the atom bomb. The bursting of the bomb over Hiroshima, expressed at the same time the enormous new power to control nature which science has given mankind, and the utter and criminal incapacity of the old order to use it for anything but horror and destruction. The use of atomic power, coupled with all the other developments of modern science, represents a step in human control over nature far greater and far more sudden and revolutionary than any in the past history of the planet: greater than fire, greater than agriculture. It demonstrates that the only limit to human capacity is to be found in society and not in nature.

Paradoxically, however, inside science, this increased knowledge, these new and verifiable relations that had been established, far from revealing a more regular and coherent picture, have had the opposite effect of disturbing and breaking up the scientific system which Newton had blocked out and to which the nineteenth-century scientists thought they were putting the final touches. The new advances in science have led to the most searching criticism and revision of the foundations of science, a criticism which is still in full swing.

If we explore the nature of the revision of ideas that have come about, we shall find that most of them are of a fundamental philosophic character. They do not affect the practical predictions of science, but they do affect its original foundation. They all seem to tend in the same direction, which is away from what would have been called in the nineteenth century the common-sense, materialistic view of science. Now this kind of criticism very naturally has led a number of people – and among them a number of eminent scientists – to abandon everything and fall pell-mell into mysticism and superstition. (See Sir James Jeans, *The Mysterious Universe*; Eddington *The Nature of the Physical World*, etc.) Their philosophical world, for all that they would not admit they had one, was built on a dualistic basis: there were two worlds, a world of hard fact in which million of atoms were attached firmly by forces that obeyed Newton's laws; and a world of fancy, religion and morals which either obeyed no laws or took them from the Bible.

The Unity of Science

Modern developments in science have made this position untenable. The hard world turns out to be just that region of experience which has some relation in scale to our immediate bodily experience. We know how a table or a billiard ball behaves because they are about the right size for us. We try to make atoms and nebulae behave in the same way and if they do not we say the universe is becoming unreasonable. In expanding our range of experience, science has shown that each level of magnitude, each level of order or complexity, has its own laws. Our common-sense laws are only laws for a little part of the universe, although this is the part that matters or has mattered most to us.

At the same time, social studies of anthropology, history, economics, but most of all, perhaps, psychology, have shown us that the human or spiritual world is not governed arbitrarily by unalterable human nature or divine institutions, but has its own far more complex laws of development and behaviour. These two worlds are not really separate, but regularly merge into each other. We can take the behaviour of animals, for instance, on one side to illustrate beautifully physico-chemical nervous reactions, and on the other to parallel human emotional and intellectual performances. In this sense of the unity of science, the whole tendency of modern knowledge is in the direction which Marx was one of the first to emphasize. The working scientists of today find the dualistic attitude increasingly difficult to maintain. They see success in their fields dependent on close cooperation and understanding of scientific work in all other fields. They begin to sense the importance of historical and social studies in guarding them against prejudices in their own work and pointing towards possibly fruitful research.

Dialectics in Physics

Other aspects of dialectical materialism find increasing reflection in the internal development of the sciences. The greatest and most difficult breach in the common-sense point of view is found in modern physics. The modern physical world picture is full of antitheses and opposites and is a standing example of the failure of the older logic. A

critical instance is that of the nature of radiation. For many years controversies were waged as to whether light consisted of particles or waves; now we know that not only light, but also electrons and atoms themselves have both waves and particles at the same time, or, rather, they are something that can be a wave or a particle. The difference between these concepts is that a particle is something that is somewhere at a specific time and a wave is something happening over a certain space for a certain time. The distinction between them seems easy enough to common sense but we know now that for radiation we can never be specific enough about a particle's position and that contrariwise the wave can be located. The opposites here completely interpenetrate.

Another illustrative example is what we now call the cooperative phenomena of physics in which a process such as the melting of a solid appears no longer as the property of a particular atom but as the property of a group of atoms in virtue of their common mutual interactions. Beginning with one atom, we can say that its movements disturb its neighbours, but the moment its neighbours are disturbed the constraint of the original atom is released and when the movement is large enough the whole system falls apart or "melts," as we say in ordinary language. Now the interesting thing here is that it is quite arbitrary which atom we start from. The characteristic of melting depends on the general pattern and not on any particular part of it. It is communal property, the property of the system as a system. Cooperative phenomena are an illustration both of the character of qualities which arise from quantitative conglomeration and of the critical changes of quality which occur as the result of steady quantitative change. It is aspects of physics like this that make it much easier now than it was fifty years ago to understand and accept dialectic views.

Historical Elements in Physics

Perhaps the most striking of all is the appearance of the historic element. Physical laws used to be considered, in contrast to those in biology or society, immutable: the material basis, the elements of physics, permanent; they represented the embodiment of the Platonic ideals. But, beginning fifty years ago with the discovery of radioactivity, we have seen physics itself change gradually into just such a relative and evolutionary state to which Darwin, forty years before, had brought biology. In the last few years the studies of nuclear physics, cosmic rays and cosmology, have combined into one grand synthesis in which the nature of the physical world is seen to embody the results of great and really historic transformations of the universe: thousands of millions of years ago, it is true, but still at a definite time in the past. The elements themselves show, by their relative abundance and scarcity on this earth, the characteristics of the enormously concentrated, dense, and hot universe in which they were formed before there were such things as stars and galaxies. These in turn were formed by an explosion which by scattering them prevented all but insignificant change in the atoms, or, as we say, froze their equilibria, and at the same time gave us the expanding universe in which we live today. Modern cosmogony has provided in these sudden transformations between widely different states two or three more stages of dialectical transformation of hierarchical order to add at the beginning of the series which Marx and Engels blocked out. As science progresses we may discover still more at the beginning, and, by our own efforts, add more at the end. What the advance of modern physics has taught us is that laws are not absolute and eternal truths, except in so far as they are tautologies illustrating our incapacity to detect at once two different ways of saying the same thing. They are relative and developing relations, not only in respect of our discovery of them but in respect of actual historical evolution of a material universe.

Dialectics in the Social Sciences

Marxism has its roots in the social sciences and it is there that, not only the intellectual views of Marx and Engels, but even more the actual consequences of the social evolution which they predicted, have influenced and transformed our knowledge. It is in fact becoming more and more evident that there is no social science outside Marxism. The old economics has broken down with the disappearance of the system of free competition which was considered to be the natural order of things, and which Marx showed was just one stage that had come into existence and would pass away. Anthropology, archaeology and history are now tending to merge into one study of human social development where economic determinism is becoming more and more recognised as the guiding clue. Psychology itself is tending to lose its highly individualist character put on it by the practical necessities for dealing with the mental ailments of the idle rich. The influence of social factors in the general moulding process of society on the individual becomes the key to the understanding of the human mind. Here again the fundamental Marxist concept of the importance of historic development on existing forms is quite evident. Our very intellectual and emotional reactions are themselves mental fossils attaching to definite historic events in the past, and transmitted, not by any mysterious group soul, but by the normal mechanism of cultural transmission in the family, the school and the workshop: methods which can be understood and ultimately controlled.

The Value of Marxism in Scientific Research

All these examples can do no more than indicate how the scientist of today finds it almost inevitable to deal in dialectic terms even though he may not be clearly aware that he is doing so, or may repudiate any suggestion of Marxist influence. A convinced anti-Marxist, however, might point out that all this will not take us very far because the great bulk of the scientific discoveries of modern times were not made by Marxists or by those influenced by Marxist thought. This is a statement of fact that is indisputable, but it does little more than show the inevitable tendency of the convergence of human thought under

similar social influences. The fact that it is perfectly possible for a non-Marxist to arrive at a Marxist conclusion may be taken as an independent indication of the usefulness of the Marxist viewpoint, but it can never be proof that it would not be far easier and quicker to arrive at that conclusion by conscious application of Marxist principles.

The scientists of today can no longer afford to ignore Marxism or not to avail themelves of methods of thought which, when fully absorbed by understanding and practice, will lead to a new leap forward in our collective capacity for understanding the world and thus for dealing with our physical and social problems. Until now, outside the Soviet Union, there has only been a handful of scientific workers who have had more than a smattering of Marxist theory, and among those few the Marxist views were only becoming to be appreciated in the years preceding the war. In spite of this, many Marxists, such as Joliot-Curie, Haldane or Gordon Childe, are men of note in their professions.

It is, however, in the Soviet Union that we can see the first results of the application of dialectical materialism in science. The actual scientific work that goes on in the Soviet Union is carried out using the same type of apparatus and the same inner logic of analysis and induction that we find in science in other places and at other times. Dialectical materialism is not a substitute for the rigours of scientific method. It enters into science to point the way towards what is to be discovered and to provide the means for making these discoveries effective. In other words it is more concerned with the strategy than with the tactics of scientific advance. That is not to say that it has nothing to do with the detailed scientific work, but its influence here is indirect. The good Marxist should be able to see more clearly, should be able to avoid the preconceptions and conventional views that prevent people seeing things, even when they are under their noses.

The Planning of Science

Marxism is not and does not claim to be a universal method for making discoveries in detail. The human individual qualities of carefulness, honesty and imagination are still as necessary as ever. Where dialectical materialism is most useful is in the choice of field, the direction of attack in that field and the linking up with other workers in the same or different fields. It is in fact the philosophy of planned scientific advance to supersede advance by numerous individuals each following his own track and supporting one another consciously by adding to the scientific tradition and unconsciously by following socially dictated tendencies. That is not to say that the greater scientists have not from the beginning of science planned their work. Some even, as Pasteur, have been able to build around themselves a group of workers dividing up the field among them. These individual efforts have, however, been isolated and impermanent.

Science as a whole has had its ups and downs. Achievements in separate subjects show even more violent fluctuations. The determining factor for advance of this or that subject has been the relation between what is discovered and what is used.

Social and economic needs direct research in certain fields and those researches in turn produce effects which have economic and social consequences; though often only after an enormous time lag. One hundred and fifty years may elapse between the first scientific impulse and the final full-scale practical result. Thus the beginning of large-scale chemical manufacture of the seventeenth and early eighteenth century promoted lively interest in chemistry. This was pushed with vigour up to the nineteenth century, but only began to give returns to industry in a big way with the growth of the scientific chemical industry, particularly with the dyeing industry at the end of the nineteenth century. This casual process, before the Soviet Union showed that it was not necessary, was considered, like the survival of the fittest or free competition, to be a law of nature, something that must not be interfered with for fear of upsetting its delicate mechanism. We know now from our own experience in wartime that these enormous time lags can be reduced quite simply by proper organisation and planning of science, or by two-way linking of research, development and production, through which production problems can pass back to research and out again through development. But it is quite impossible to plan scientific research without planning it in relation to a definite system of demand and to find that is the equivalent to a social analysis. Only if we start with social analysis can we say what part science has to play and how it can play it. It is in this wider sense that dialectical materialism provides the major directive to scientific advance.

Science in the Soviet Union

It is not only or even mainly with the analysis of past and present science that the value of dialectical materialism makes itself felt, it is rather in its indications for the future. For the science of the future, socially directed planning will be an absolute necessity; the type of that planning we can see already in the Soviet Union. It was only through the conscious application of Marxist theory that it was possible to build, on the narrow foundations of Czarist science, the vast, integrated and and vital organism of modern Soviet science. In a generation a nation of illiterates is becoming a nation of scientists, and this has been proved both in peace and in war. It is not a question of having merely a small, scientific *élite*, of advancing the frontiers of knowledge here and there, but of the establishment of a universal practice of treating all problems of production, agriculture, health and strategy as requiring scientific answers on the basis of controlled experiment and statistical trials.

The various plans of the Academy of Science, culminating in the great post-war five-year plan of 1946, show the form of the relation between fundamental science and the needs of the country. The plan is drawn up by the scientists themselves tracing out the inner needs of the different scientific disciplines and knitting them together in a whole which has a coherence of its own as well as numerous links with industry, agriculture and medicine (see *The Anglo-Soviet Journal* published by the Society for Cultural Relations with the U.S.S.R., Autumn issue, 1947). The contribution of dialectics is to be seen in this analysis. The fruits of Soviet science are already apparent in the practical successes of the Soviet Union, but they are only the first fruits. What has really happened is that a whole peole are learning this new dialectical way of dealing with material and social questions, and that whatever the destruction caused by the Nazis, they have consequently in them not only the will, but the means to produce new knowledge and new achievements.

The organisation of science in the Soviet Union is not a restrictive but a liberating organisation; it employs more people to do more things; it discovers and utilises natural resources; most of all it utilises what we are now coming to understand as the greatest and most powerful of all natural resources, man's own capacities and intelligence. There is a latent possibility in every man or woman of every race and culture to contribute something, little or much, to the advance of human culture.

Marxism and Freedom

What we have seen now for thirty years of struggle and development in the Soviet Union, we are beginning to see in the rest of the world. Since the liberation of Europe from the Nazis and the partial and still uncertain liberation of the colonial countries, there is apparent everywhere a new urge to make use of planned science as the most rapid, as well as the most effective and lasting, way of raising the standard of living and achieving a civilisation free from the insecurity of a selfish and grasping capitalism. Everywhere, even in the capitalist countries themselves, the idea is growing of leading science in an organised way to the solution of human needs. It is being opposed, naturally enough, by reactionary forces, in this field as in others, masquerading under the name of freedom. But the freedom of anarchic capitalism is illusory and self-destroying; it is a freedom for exploitation and not for creation. Through the failure to realise this, the great work of the eighteenth-century liberals broke down.

The freedom appropriate to our stage of development is one of cooperation and not of competition. Men are to be liberated by knowing their own limitations and not by ignoring them: by accepting the necessity of working together and not insisting on "rugged individualism" in an age which has outgrown its value. The framework which will help to guide this cooperative effort is the framework of Marxism. This is not a rigid shell, fixing forever like a written constitution the future progress of human intellectual and practical achivements; it is rather a scaffolding which will be taken away when it has served its purpose. But that time is not yet and much has to be gone through before it is reached. □

IS THERE A FEMINIST SCIENCE?

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Historically, the sciences have been seen as masculine, not simply because the vast majority of scientists have been men, but also because the very characteristics of science have been perceived as sex-linked. The values attributed to science are the values attributed to males; the objectivity said to be characteristic of the production of scientific knowledge is specifically identified as a male way of relating to the world. Science is cold, hard, impersonal, "objective;" women, by contrast, are warm, soft, emotional, "subjective." Even the hierarchy of the sciences is a hierarchy of masculinity: as the language suggests, the "hard" sciences at the top of the hierarchy are seen as more male than the "soft" sciences at the bottom [2]. Because science as a whole is perceived as male, women in science are perceived as unfeminine. J.H. Mozans, who celebrated the achievements of hundreds of scientific women in his historical survey of *Women in Science*, found it necessary to defend the womanhood of his heroines, repeatedly assuring us that these scientific women could be graceful and feminine, good housekeepers and mothers [3]. Laura Bassi was a good example: while Professor of Physics at the University of Bologna, she managed to raise twelve children.

There are several possible responses to this tradition which states that the characteristics of the sexes constitute a natural polarity, that male and female are fundamentally different, and that science is essentially masculine. One is to imply, like Mozans, that women can be both male and female: physicists and mothers. Another is to deny that there are any significant sexual differences and to discount apparent differences as the result either of discrimination or of "socialization." A third possibility is to accept the dichotomy between male and female, to promote female values as an essential aspect of human experience, and thereby seek a new vision of science which incorporates these values in a dialectical unity of the objective and subjective.

*Adapted from a paper given at AAAS Meeting, Toronto, 4 Jan. 1981 [1].

Robert J. Lifton, sociologist, has argued that male and female ways of knowing are distinct: the male's mode of thought is through abstract ideas and symbols far removed from organic function, while the female's pattern of thought is rooted in her "identification with organic life and its perpetuation." Woman has, he stated, the "special capacity to mediate between biology and history." [4]

Some recent interpretations of female culture and its relationship to science stress the dangers of the "masculine" attitude to nature. Susan Griffin's *Women and Nature* [5] and, in a different way, Carolyn Merchant's *The Death of Nature* [6] have played with the identification of scientific and masculine ways of thinking: both are seen as analytic, mechanistic, controlling, exploitive, and ultimately destructive. For Griffin, the abstractions of science are bearers of man's alienation from nature and are an instrument of his alienation of woman; the two sexes simply speak different languages, and it is the women's task to rediscover their own voices, to overcome a history of female silence. For Merchant, the alienation of science dates from the mechanistic materialism of the seventeenth century which expressed the merchant capitalist's relationship to nature; it thus represents an historically specific form of knowledge to be transcended in the future through an alliance of feminism with ecology. These new movements are both concerned with the defense of nature against exploitation, both taking the side of mother nature against her son, the industrial engineer. Female culture is seen as cooperative rather than competitive, nurturing rather than exploitative, and oriented towards communal survival rather than individual self-interest. In a similar way, Russell Means, a major figure in the American Indian movement, has denounced all forms of "European" thought as devoid of spiritual appreciation of the natural world, and as therefore leading merely to different forms of exploitation of the earth and its natural resources [7].

Each of these views accepts and builds on the dichotomies produced by western philosophy between nature and civilization. They find that "civilization," in the guise of scientific and technological development, has been responsible for the rape of nature, and conclude that the whole tradition of modern science now endangers human survival. According to Jean Baker Miller, and other feminist psychologists, the male psyche, as it has been socially created in the western capitalist world, is peculiarly unable to integrate self-creative activity with a primary concern for others, having assigned to women the primary responsibility for affiliative ties and emotional expression [8]. This, she says, contributes to men's inability to organize technology for human ends, and produces a scientific culture which, having cut itself off from human needs, can only be recovered for humanity through a recovery of that part of human experience which has been relegated to the female.

The radical feminist critique of science and technology thus appears to agree that there is something unfeminine about science;

the problem, however, is located not in women, but in the particular character of our production of scientific knowledge. In this view, the problem is not one of making women more scientific, but of making science less "masculine." When masculinity is seen as an incomplete and thus distorted form of humanity, the issue of making science and technology less masculine is also the issue of making it more completely human.

These theories confront us with a specific challenge to the idea of the objectivity of science. The distance between the knowing subject and the object of knowledge is interpreted as a measure of the alienation of man from nature; man's aim of controlling nature is taken as an egotistical and dangerous desire for domination. This view calls into question our methods of creating scientific knowledge and the assumptions on which modern science has developed, as well as the products of that knowlege: indeed, all aspects of scientific production are open to question. If we accept the radical feminist view, science itself must be transformed not simply to permit the acceptance of women, but more importantly, to conceptualize new kinds of relationships between human beings and the natural world, by overcoming an alienation between culture and nature built into our current social experience and thus into our existing forms of knowledge.

The radical feminist view of science is only one of the forms in which the growing popular distrust of scientific institutions and authority is expressed. Antagonism towards established scientific authority is also found in the anti-nuclear and environmental movements, the radical science movements, and alternative technology groups on the one hand, and in fundamentalist religious and creationist organizations on the other [9]. Whether identified with left- or right-wing political groupings, these share an opposition to the perceived elitism and authoritarianism of scientific experts, a resentment of the social power of academic and governmental spokesmen, and a defense of alternate ideologies. All perceive the decision-making processes in science and technology as insulated from popular participation, and perceive scientific authority more as a form of power than as a source of truth. At a philosophical level, the rejection of science as a form of authority has been emphatically stated in the writings of Paul Feyerabend [10].

There is a great deal of substance in these different forms of the rejection of scientific authority, and there is also a danger. Because science has been presented as an objective force above and beyond society, and because it has been seen as a monolithic power, it may appear that the claim of science to be the arbitrator of truth must be accepted or rejected wholesale. If rejected, we seem to be left with complete cultural relativism, where no one form of the production of knowledge could claim truth status over any other. The story of Genesis would then have as much claim to validity as the theory of evolution; the decision between sexist and feminist interpretations of social arrangements would, in the absence of any mutually agreed

criteria of validity, be quite simply a matter of political power [11]. It seems overly optimistic to suppose that a completely free marketplace of competing ideas and theories would result in the desired goal of a more human and more liberating knowledge.

We need not, however, go so far as to reject the whole human effort to comprehend the world in rational terms, nor the idea that forms of knowledge can be subjected to critical evaluation and empirical testing. The concept of creating knowledge through a constant process of practical interaction with nature, the willingness to consider all assumptions and methods as open to question, the expectation that ideas will be tested and refined in practice, and that results and conclusions of research will be subjected to the most unfettered critical evaluation, all these are aspects of scientific objectivity which should be preserved and defended. The hope of learning more about the world and ourselves by such a collective process of knowledge production and testing is not one to be abandoned; the idea of individual creativity subjected to constraints of community validation through a set of recognized procedures preserves the promise of progress.

The radical feminist critique of science and of objectivity, therefore, needs to be developed in ways which will allow us to identify those aspects of scientific activity and ideology which need to be questioned and rejected, without at the same time abandoning the ideal that we can come to an ever more complete understanding of the natural world through a collective and disciplined process of investigation and discovery. "Science" is not monolithic, there is not, in fact, a single "scientific method"; there are many sciences and many scientific methods. The sciences are dynamic, and have each undergone many shifts in their underlying assumptions and procedures; we need not suppose that even the most determined critique of currently existing science or proposals for alternate forms and visions of scientific investigation necessarily imply a rejection of either rationality or progress. The proposition that we must either accept science as it is now, or collapse into mysticism and irrationalism, may be simply a tactic to discourage critical inquiry.

Let us begin with one of the central concepts in the ideology of science, the concept of objectivity. The idea of scientific objectivity is sufficiently vague to carry with it a multitude of meanings; many of these are more closely tied to the ideology of science than to the actual processes of scientific work, and serve mainly to mystify scientific reality. We might see scientific production in a clearer light if these did not impede our view.

The concept of objectivity creates a hierarchy of distances within science, a series of dichotomies and silences. One of the more obvious concerns the relationship between the production of knowledge and its social uses. The idea of objectivity can be used to create a distance between the production of pure science—seen as the pursuit of knowledge for its own sake, an abstract and value-free ideal, involving purely intellectual and technical decisions—and the uses of the science, seen as involving purely political and economic considerations. If the production of knowledge is isolated from the uses to which that knowledge is put, then the scientist is freed from any social or moral responsibility. Even the scientist who accepts funding from military sources is therefore free to insist that the use of his research is outside his control, and not part of his responsibility; the researcher in a corporate laboratory is free to consider his work as purely objective and unfettered by any economic considerations.

If scientists take no responsibility for the uses of science, then it is supposedly up to the general public in a democracy to monitor the social applications of scientific research. The majority, however, know little of the technical work nor of its possible implications; when community groups do become alarmed, as in the case of the recombinant DNA research, they may be readily discounted as uninformed, and even as "hysterical." As in the case of Three Mile Island, the problem may be formulated in terms of popular "anxiety" instead of in terms of social responsibility. The voices of scientific authority are more often called upon to quiet public distress than to articulate the grounds for concern; scientific expertise becomes a shield against the effort to ensure public accountability. In this context, scientists who retreat behind the screen of pure science are passively abandoning their social responsibility; those who choose to become actively involved risk being seen as no longer "objective." Here, the notion of "objectivity" becomes merely a code word for the political passivity of those scientists who have tacitly agreed to accept a privileged social position and freedom of inquiry within the laboratory in return for their silence in not questioning the social uses of science or the power relations which determine its direction.

On a personal level, the claim of "objectivity" may be taken as requiring a divorce between scientific rationality and any emotional or social commitment. Thinking is supposed to be completely divorced from feeling, and feeling is said to be outside the realm of objectivity. This distance between thought and feeling can again be used to insulate the scientist, as scientist, from his social world. His roles as scientist and citizen are distinct, and he need feel socially responsible or emotionally involved only in his role as private citizen.

Popular images emphasize the idea of the scientist as a man who is emotionally detached, even emotionally cold, a purely rational being. (Thus it is still difficult to accept the idea of women scientists; emotional detachment is one of the marks of masculinity.) Here again, we may be dealing with a pervasive and powerful aspect of the mythology of science rather than with the actual conditions of scientific work. Scientists, in reality, are often deeply and emotionally committed to their work, to the solution of a particular problem, or to the elaboration of a specific world view. The style of scientific communication, however, as reproduced in scientific journals, is aimed at eliminating

any traces of emotional or personal involvement: the style is cold, passive, impersonal, a jargon to be learnt, a respectable mask of objective detachment, an elimination of the human subject.

The concept of scientific objectivity, when used to denote the separation of thought and feeling, may be employed to devalue any positions expressed with emotional intensity or conviction; feeling becomes inherently suspicious, the mark of an inferior form of consciousness. Once this hierarchy between thinking and feeling has become internalized, it is axiomatic that those who identify with "thought" can justify their dominance over those identified with "feeling." Women are very used to the separation between thought and feeling, and the ways in which it can be used to reproduce relations of dominance and subordination between the sexes; it is a familiar aspect of intimate relationships. If a man can present his position in an argument as the point of view of rationality and define the woman's position as an emotional one, then we know that she has already lost the struggle to be heard; he has already won. In terms of the politics of science, this power relationship is reproduced on a social scale: the scientific experts are in the male role, while the vast majority of the population is given the female role. Everyone lacking scientific credentials can be made to feel uninformed, unintelligent, and lacking in the skills required for successful debate over matters of public policy. While those with sufficient wealth can afford to hire the scientific expertise needed to give their positions public validation, those without wealth must bow to the superior knowledge of the experts. Knowledge can, in this system, flow in only one direction: from expert to non-expert. There is no dialogue: the voice of the scientific authority is like the male voice-over in commercials, a disembodied knowledge which cannot be questioned, whose author is inaccessible.

The relationship of scientific authority to the population, or expert to non-expert, is one of an immense and protected distance. It parallels the privileged relationship of the producer of knowledge to the object of knowledge: the knowing mind is active, the object of knowledge entirely passive. This relationship of domination has been immensely productive in allowing the manipulation and transformation of natural processes to serve particular human ends; when transformed to the social sciences, it also serves as a justification for the attempted manipulation of human beings as the passive objects of social engineering. Women, who have already been defined as natural objects in relation to man, and who have traditionally been viewed as passive, have special reason to question the political power relation expressed in this epistemological distancing. The subject/object split legitimizes the logic of domination of nature; it can also legitimate the logic of domination of man by man, and woman by man. If, on the one hand, the ecological crisis requires that we comprehend man as a part of nature, and not as a superior being above and beyond natural processes, so also the task of human liberation requires us to see science as a part of human society, determined by particular human aims and values, and not as the depersonalized voice of abstract authority. In order to be able to concretely debate the values and intentions of scientific knowledge, we must first be able to admit that these exist: thus removing the series of screens and defenses erected in an effort to deny the social content of scientific knowledge.

This raises another set of problems with the theme of scientific objectivity, the question of the social position of scientists. We are told that the production of scientific knowledge must be independent of politically motivated interference or direction. Yet we see scientists constantly testifying before congressional committees, we find scientists in law courts, we find scientists involved in disputes at every level of public policy, and it is obvious that the experts take sides. It is also obvious that these "experts" are very often funded by corporate interests and that there are few penalties for those who find their research supports the position of these powerful lobbies.

We may still treasure the mythology of the individual scientist, alone in his laboratory and isolated from merely daily concerns, wrestling with fundamental problems of the physical universe. In reality, the scientist today is a salaried employee, part of an institutional hierarchy—perhaps a small cog in a corporate research team—working on some small aspect of a problem which has probably been formulated by others. His survival depends in a very concrete way on the structure of funding decisions made far from the laboratory; he is dependent on economic and political decisions most often beyond his control or influence. In what way is the average scientific worker independent of the larger political process, and how can we say that science as a whole is autonomous of social organization?

A moment's reflection shows us that the production of scientific knowledge is highly organized, and is closely integrated with the structures of political and economic power. In the twentieth century, the sciences are essential in maintaining the economic, political and military power of all developed industrial economies. The production of scientific knowledge is involved in international competition and power politics; it is naive to present the idea of scientific objectivity as though science itself were above or beyond politics. The assertion of objectivity can, however, be used to mask the actual conditions of scientific work. Because the social position of the scientist, and the particular form of organization of science, are supposed to be irrelevant to the knowledge produced, we may be tempted to ignore the conditions and context of scientific production.

If, however, we look at the history of science, we can begin to see more clearly the ways in which the structures of scientific production depend on the economic and political formation of the society as a whole. Our relationships to nature are socially structured, and may be seen to be a product of human history. The construction of natural knowledge is a social activity; any society will attempt to generate the kinds of natural knoweldge which best fulfill its social, economic,

and political needs.

In the first place, the social formation determines the kinds of questions which can be posed, and the tools for answering them. Greek philosophy, or *scientia*, the production of natural knowledge, was divorced from practical problems of technological production because, in a slave society, the citizen-philosopher had no need to be concerned with manual labor, and the slave had no social possibility for producing formally articulated knowledge. What we know as modern science developed only with the capitalist mode of production, with the development of new kinds of practical activities and economic needs [12].

From the period of city states and mercantile capitalism through the successive stages marked by the emergence of nation states, the industrial revolution, and the development of advanced monopoly capitalism, science has been largely tied to the needs of the dominant class. The early stages of industrial production involved an active intervention in nature and the production of systematic and abstract forms of knowledge which allowed for the control and manipulation of natural processes. Yet the production of natural knowledge itself was initially only minimally organized; not until the late nineteenth century, with the accumulation of capital in large industrial enterprises, were large numbers of scientists hired to produce knowledge in the direct service of industrial production. Scientific knowledge proved so powerful a tool in the further accumulation of capital and in the reproduction of political power, that we now have "big science" —a major social investment, funded by the state and reproduced in private corporations, in universities, and by public agencies.

This modern context for the production of scientific knowledge demonstrates the difficulty of developing a specifically feminist science within our existing economic and political system. First, the problem of the liberation of women would have to become a major social concern, with the necessary social resources devoted to its solution. At the moment, the production of feminist knowledge and theory is a cottage industry; it depends on the energy and ideas of a small number of women, working individually, in response to a collective social movement, but without any significant institutional or financial base. In those areas of knowledge production which are organized (or disorganized) in a similar fashion, such as history, philosophy, anthropology, and literary criticism, it has been possible for small numbers of women to have a major influence in determining new directions for research, in posing new questions, and in developing new knowledge. This is more difficult within sciences that are closely integrated with the reproduction of social and economic power.

If then, we are to examine the production of scientific knowledge, we need both macro and micro studies of social organization and its relation to knowledge production. At one level, the funding and organization of science follows social priorities as established by existing relations of power; at this level, the identity of the scientist is a secondary question, not because he or she is above politics, but rather, because scientists must fit into an existing political reality in which the questions and issues for research are, in large degree, established beyond the laboratory. It will be necessary to explore the role that scientists are given in the reproduction of economic and political power within the context of a class structured society, and to understand how these relationships of power lead to the production of particular kinds of knowledge, and to see why certain kinds of questions are asked, while others are rendered invisible.

At the same time, scientists do have a certain autonomy in terms of the production of knowledge, and have a special responsibility to examine the ways in which particular forms of research may help or hinder the project of human liberation. In terms of the specific issues discussed in this paper, there are several steps to be taken if we are to move in the direction of a more fully human understanding of science. The first is to readmit the human subject into the production of scientific knowledge, to accept science as an historically determined human activity and not as an abstract autonomous force. If we admit that scientific activity is not neutral, but responds to specific social agenda and needs, then we can in turn begin to see how science, and scientists, might relate in a different way to social, including feminist, questions.

On an individual level, we might take the doctor-patient relationship as an example of the required shift of perspective. We are familiar with the situation in which the patient complains, "Doctor, it hurts here," and the physician says, "Nonsense, it can't possibly." The physician has been trained to perceive objective reality according to a specific set of medical theories; if the patient's subjective experience does not fit readily into his trained perception of objective reality, then that experience must be discounted. There is really "nothing wrong," the patient is too emotional, the pain is psychosomatic, a phantom. The patient has no recourse, no way of establishing her own pain as "real," her subjectivity has no claim. Within medicine, an enormous amount of human pain is thus relegated to the shadowy realm of psychosomatic phenomenon; a large proportion of healing is attributed to the placebo effect.

It would require a different kind of analysis, a different kind of investigation, to understand the kinds of pain called psychosomatic and the kinds of healing attributed to the placebo effect. It would require readmitting the patient's subjectivity as a legitimate concern of medical practice, and as a necessary component of healing: an admission which tends, however, to diminish the total authority of the physician. But because scientific knowledge in medicine is necessarily mediated by clinical practice, and by the doctor-patient relationship, many physicians are perfectly well aware of the importance of the patient as a person, and of the patient's active involvement in the process of health and disease, even if they have no theoretically adequate terms in which to express this understanding.

The women's health movement, by refusing to accept the physician

as unquestioned authority, and by insisting on a more active and reciprocal relationship between doctor and patient, has given a new visibility to women's actual experience and thus offers the possibility of opening up new questions which can potentially expand the boundaries of scientific knowledge within medicine. This may require changes in our understanding of what is "real," it may require a shift in the previously rigid boundaries between objective and subjective phenomena, and it may require a more serious examination of the relationship between mind and body. Such shifts and changes do not mean the collapse of medical science or the denial of everything that has been achieved by the previous paradigm, but they do offer the possibility of moving towards an expanded and more complete form of knowledge. If all the forms of pain and illness which were previously discounted as psychosomatic were to be comprehended within a larger theoretical framework, our medical sciences would not be thereby diminished, but would be rendered more complete, more adequate as an understanding of human suffering. The possibility of this kind of shift within medicine suggests the possibility of expanding other forms of scientific knowledge by admitting new questions as valid, and by allowing other problems to become visible. I have argued here that the manifold meanings of the concept of scientific objectivity can be used to defend against such changes. It is also possible, however, for scientists to actively seek ways of negotiating the distance established between knowledge and its uses, between thought and feeling, between expert and non-expert, between objectivity and subjectivity.

On a broader social level, we can ask what kinds of questions might be readmitted into science by allowing the collective definition of both the problems and methodology of research. The recent history of occupational health research in the Italian factories offers an important model for the development of new forms of scientific investigation .

Prior to 1969, occupational health research was done by specialists who would be asked by management to investigate a potential problem in the factory. The expert collected individual, quantifiable information from each worker by means of questionnaires, interviews and medical records, and then statistically combined and manipulated the data to test hypotheses about the causes of the problem. The procedures was rigorously objective; the results were submitted to management. The workers were the individualized and passive objects of this kind of research.

In 1969, however, when workers' committees were established in the factories, they refused to allow this type of investigation. The new structure of direct democracy in the workplace forced a transformation in the methods of occupational health research. Now workers would collectively produce the information needed to define and solve a problem; the generation of hypotheses would be a collective, not an individual, activity. Occupational health specialists had to discuss the ideas and procedures of research with workers' assemblies and see their "objective" expertise measured against the "subjective" experience of the workers. The mutual validation of data took place by testing in terms of the workers' experience of reality, and not simply by statistical methods; the subjectivity of the workers' experience was involved at each level in the definition of the problem, the method of research and the evaluation of solutions. Their collective experience was understood to be much more than the statistical combination of individual data; the workers had become the active subjects of research, involved in the production, evaluation, and uses of the knowledge relating to their own experience.

This example shows us what overcoming the distance between objectivity and subjectivity might mean in practice. Here, the process of transmission of knowledge is not simply from expert to non-expert but is reciprocal; the problems and issues are defined by mutual dialogue [13]. In principle, the same kind of process could be established between scientists and any sector of the population whose experience raises specific problems for investigation.

We still have few models for visualizing what direct democracy might mean for the future of scientific research. Historical investigations of the "woman problem" have considered women as natural objects and as passive in relation to the creation of knowledge: at this stage, we can only imagine what it might mean to be the active subjects in the creation of knowledge about ourselves and the world around us. At this point, while it is necessary to argue the case for the entrance of women into the scientific professions as presently constituted, it is also important to push the epistemological critique of science to the point where we can begin to construct a clear vision of alternate ways of creating knowledge, to use the feminist critique as a tool for seeing what it might mean in practice to liberate science from the inherited habits of thought inscribed by the previous separation of human experience into mutually contradictory realms.

In this effort, it will be important to make use of the analytic methods of Marxist theory. In bourgeois logic, dualisms such as male/female and objective/subjective are seen as absolute and antagonistic; they are ahistoric and eternal. The logic of dialectical materialism offers a way out of this impasse; such dualisms are dialectical aspects of a larger reality; they are historically constructed, and therefore subject to transformation. The developing dialog between feminists and Marxists concerned with the analysis of scientific knowledge is one step toward the creation of a radically transformed science, one that is as yet only faintly visible as a possibility for the future.

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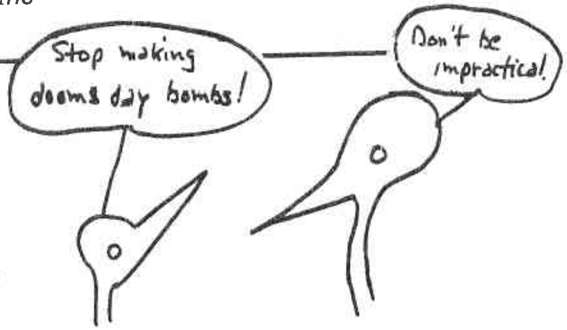
Treating Philosophy as a System

The fact that philosophical doctrines develop and replace one another as systems of knowledge, compelled the author to pay special attention to a dialectico-materialist interpretation of philosophical systems, and in the first place, the system of the philosophy of dialectical materialism. Oizerman gives a detailed definition of the opposing interpretations of philosophy as a system in pre-Marxian thinking and in Marxism. In his view, Marxism rejects, *first*, the opposition of philosophy to the non-philosophical (first of all, practical) acitivity and non-philosophical study, common to all traditional philosophical systems; *second*, immutable elements making up the system; *third*, the very possibility of a complete system that cannot develop further. However, the analysis of philosophy, in particular that of Hegel, shows that philosophy of the past did indeed indicate a number of actual specific features of systems knowledge, including philosophical knowledge, that are now believed indispensable: these are ideas of a *developing* system and of the unity of system-forming principles. However, it is only in dialectical materialism that the systems approach became a more concrete, profound, and substantiated *theory of development*. That is precisely what distinguishes it, in the author's view, from the systems concepts prevailing in present-day bourgeois sociology, most of which reject the principle of development.

[Thus one sees] that Marxist philosophy is a philosophical system of a new type [in contrast to] the older view of philosophical knowledge as a system opposed to non-systematic results of non-philosophical study. [Attention is directed to the problem] of skeptical attitudes to systems in general as well as the problem of relationship between abstract "metaphysical" systems and those based on empirically elaborated principles. — A. Bogomolov examines an essay by T.I. Oizerman, *Social Sciences* (USSR Acad Sci) 12(3): 268-69; 1981.

War Versus Peace in the Scientific Community



DATA POINTS IN THE STRUGGLE AGAINST MILITARIZED SCIENCE

Dr. Strangelove as Babbitt

"I have made the claim that I am a better actor than Jane Fonda is a nuclear engineer." Thus did Edward Teller climax his argument for building nuclear power plants in four years instead of the present twelve years. As for the safety issue, he added: "Nuclear reactors are dangerous for nuclear reactors; they are not dangerous for people. Three Mile Island destroyed itself . . . but not a single person was hurt."

On this intellectual level, the so-called "father of the H-bomb" disposed of the nation's energy problems at a meeting of the New York Academy of the Sciences shortly after the 1980 elections. His repertory included Reagonomics: "The idea that profits are sinful, particularly if they are made by the oil companies rather than the *New York Times*, is a little absurd." The gratuitous sideswipe at the *Times* came out of nowhere.

Repeated insinuations that the Soviet Union will attack in the Persian Gulf (where we already have U.S. forces massed) provided the chorus of Teller's central theme: "the oil spigot is a powerful influence" in the Third World. India was singled out as a spot where U.S. control of the "spigot" can prevent revolution. And the 1964 U.S. invasion of the Dominican Republic was condoned while the Soviet action in Afghanistan was condemned. Such is the imperial logic of the "academic voice" (one of very few) on the Reagan transition team for science and technology, also a big voice in selecting the new science advisor for the White House.

Audience reaction to the performance was mixed. A top Academy officer, leaving the meeting, said that "Teller seems to have made a good case." A more hopeful sign was an audience voice directed to Teller: "I don't know whether you're a better engineer than Jane Fonda but I can see that you're a better actor than your boss [Reagan]."

Suddenly one recalls that both Teller and Reagan got their start in politics back in the era of Joe McCarthy. Reagan as a "friendly" witness at "unAmerican" hearings in Hollywood where he denounced fellow union members as "Reds." Teller by denouncing Oppenheimer for the latter's opposition to development of hydrogen weapons.

(Documents have been released which show that the Oppenheimer committee was basically correct in its unanimous opposition. See Herbert F. York, *Scientific American*, Oct. 1975.)

And it turns out that Teller gave the Academy a canned speech, pretty much the same as he gives to Big Money audiences everywhere (cf. *Mother Jones*, Aug 81, pp. 30-32). Who was responsible for his appearance at the Academy?

Now we have ''Republican science''

The President's new science advisor George Keyworth, though previously unknown except in weapons development, is also director of the Office of Science and Technology Policy as mandated by Congress. In his first policy statement, Keyworth said that "nowhere is it indicated that the OSTP or its director is to represent the interests of the scientific community as a constituency." This should be no cause for alarm, says Reagan confidant Harold Agnew, president of General Atomics and former director of Los Alamos where Keyworth worked, "defense will be the thrust of this Administration, and somebody who has the respect of the people in the defense labs is needed."

Thus we enter the period of "Republican science," so called by one of its advocates, a congressional aide who says it will tend to let the market dictate where science goes. But this market has been rigged for many years now by a continuous shifting of funds toward the military research budget (with no protest from Milton Friedman). Scientists of Carter's White House staff announced in 1978 that science faced an "economic crunch." One signal was the appointment of an engineer to head NSF. "Republican science" is only accelerating a bipartisan program for the impoverishment of academic science and basic research. Though our nation continues to harvest a crop of significant discoveries from the post-Sputnik funding of science education and research programs, the economic basis for futher growth has been eroded steadily since the late 1960s. Economic coercion is driving many young scientists to don the "uniform" of military research as DOD funds come pouring back on the campus. And the ideological climate is driving many young people away from science altogether.

Artificial intelligentsia

V. Chalidze, recent arrival from the Soviet Union, at the American Physical Society symposium in honor of Andrei Sakharov (New York meeting, Jan. 1981) described at length a "Russian intelligentsia" to which both he and Sakharov belong. He characterized this intelligentsia as a "caste" that does not depend on financial or scientific-intellectual accomplishments, rather, its members recognize one another because "they share a morality that is opposed to the morality of the bureaucracy." He lamented that this "caste," because of its long tradition of not participating in politics, missed its opportunity of working Tsars Alexander and Nicholas, though Pushkin tried, and "its punishment now is to be confronted with Mr. Brezhnev." Sakharov, he said, is an exception in that he took a role for awhile in government though remaining a member of this intelligentsia.

Responding to questions, Chalidze said he was unable to define this intelligentsia more precisely. The "caste," he added, is so uniquely Russian that, even if he were able to describe it completely, the result would not be comprehensible to a non-Russian.

Sakharov's daughter, T. Yankelevitch, spoke on the physicist's life in internal exile. His apartment is electronically jammed, she said, so that he takes a radio out on the street to get foreign broadcasts such as the Voice of America which "provide his only source of information about the outside world."

More recently, Sakharov's name was invoked in an effort to discredit the peace movement in the United States. As a "friendly witness" before the Senate Subcommittee on Security and Terrorism, Arnaud DeBorchgrave of Georgetown University said: "A number of leading scientists, ranging from Sakharov to Dr. Fred Hoyle, have suggested that as part of Russia's strategy of control of the western world's oil supplies in the Middle East, the Soviet Union today is playing a covert role in promoting the anti-nuclear lobby." DeBorchgrave's "evidence" was that the Mobilization for Survival, an umbrella organization for the peace and anti-nuclear movements, includes the U.S. Peace Council and the U.S. Communist party among its dozens of affiliates.

One may suppose that Radio Free Europe has not informed either Dr. Sakharov or Dr. Hoyle that the aircraft carriers and the rapid deployment force that now threaten armed (nuclear?) intervention in the Persian Gulf are flying the Stars and Stripes, *not* the Hammer and Sickle!

A new military category for philosophy

A clipping from the Philosophy of Science meeting, Toronto, Oct. 1980, reads in full: "SCIENCE, PHILOSOPHY AND RELIGION. A three-day symposium on a personal philosophy in the changing national defense and energy environment will be held 11-13 October 1980, at the Air Force Weapons Laboratory, Kirtland Air Force Base, New Mexico. Topics to be discussed include the moral and security issues created by (1) the allocation of national resources between defense and energy research in times of heavy inflation; (2) heavy energy usage in the national defense effort; (3) dependence on Middle East Energy; and (4) potential use of advanced weapons (nuclear, chemical, etc.) to secure foreign energy sources. For further information contact: William Moeny, APAR, Inc., P.O. Box 18067, Albuquerque, NM 87185." *One wonders if the philosophers attending were able to*

reach a value judgment on whether the "foreign energy sources" would be worth the cost of 50 or 100 million U.S. lives.

Ten stout-hearted men needed

Top officials of the science establishment have shown little stomach for a struggle to keep from being incorporated in the Reagan war machine. Too many of them share the complacent attitude of Philip H. Abelson, editor of *Science.* His editorial of 6 March 1981 regretted "more than a decade of shortsighted policies" in the funding of science education and research, but concluded passively that "it is unlikely that basic research scientists will play much of a role in the coming Reagan rearmament program. Most of the funds will be used to procure arms and prepare people to fight. How much will be spent to enable the nation to function well under the unpredictable circumstances that will prevail 10 years from now is problematical." Abelson himself, of course, is completely predictable. Over the years his editorials have consistently represented the views and needs of our corporate rulers. Unfortunately, he is not alone in the leadership of the science establishment.

On the other hand, there is a mounting resistance by working scientists against the Administation efforts to make a nuclear first strike "thinkable." Notable in this resistance have been Physicians for Social Responsibility, Union of Concerned Scientists, Federation of American Scientists, and the splendid 3-day AAAS symposium *Directing Science Toward Peace* (Toronto 4-6 Jan. 1981), sponsored by the AAAS Council on the basis of a resolution initiated by a small group of alert scientists. More initiatives of this type are needed urgently.

At the same time, some means must be found to arouse scientists in defense of their own immediate economic and professional interests *as scientists.* A determined and effective drive against the militarist takeover of science and education funding can be a potent contribution to the overall defense of democracy in our land today. By motion of this type scientists can learn of their natural allies in the labor movement and their own stake in the building of a new anti-monopoly political party. Who will raise such a standard, to which decent men may repair?

* * *

NOTE: It seems the standard was being raised as the above lines were written. Science News *(7-18-81) reported formation of a new Science and Technology Political Action Committee, organized by a group of AAAS congressional fellows because activities of existing professional societies are restricted by their tax status. Purposes of SCITEC-PAC are: 1) aggressive representation of the research community interests to the U.S. government; and 2) political campaign funding for candidates sensitive to those interests. So, pitch in everybody, to help the research community find its natural political orientation in alliance with other progressive forces.*

Our Intrepid Instigator Lays Down the Law Again

ON THE TENDENCIES OF MOTION

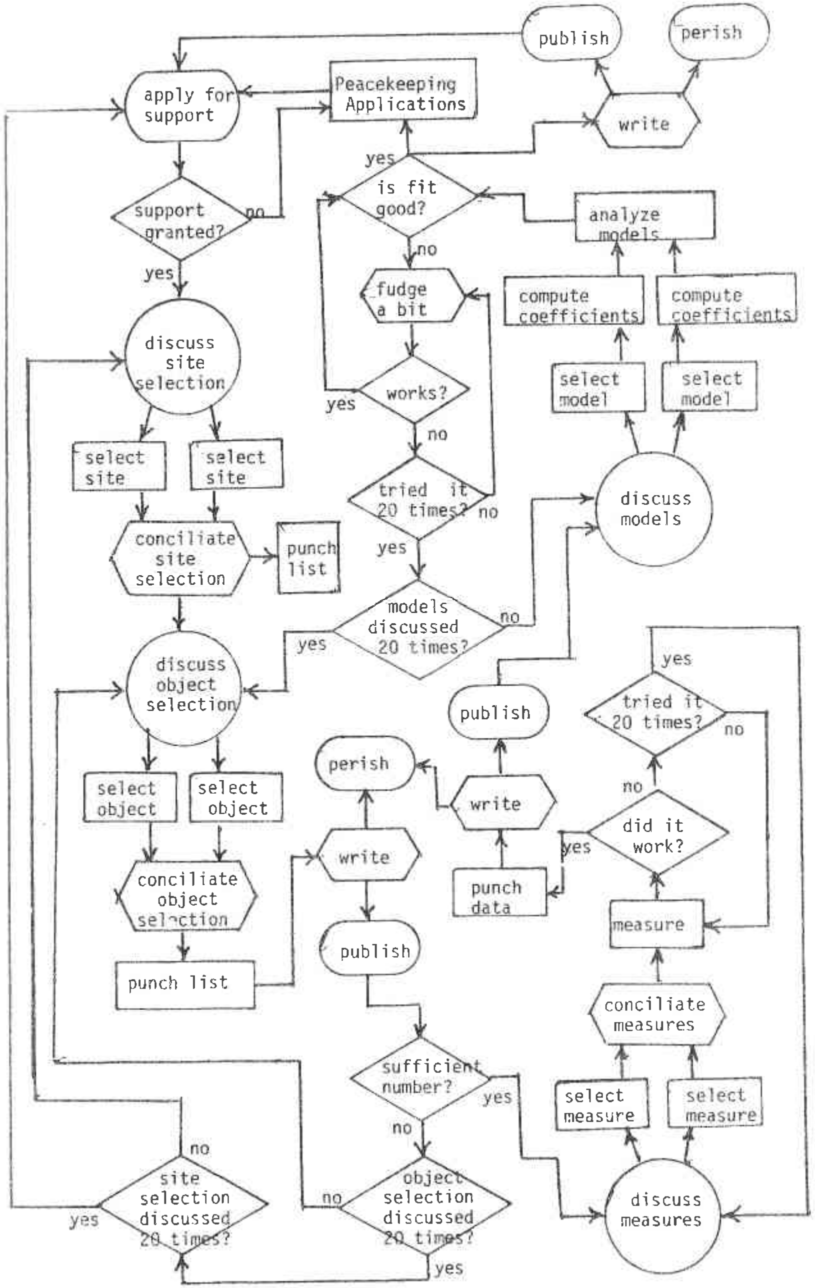
Isadore Nabi
Harvard Colledge [1]

In 1672 the first international conference on the trajectories of bodies was convened in order to organize a concerted systems approach to the problem of motion. This was made necessary on the one hand by the widespread observation that objects move, and on the other by the currency of extravagent claims being made on the basis of an abstracted extrapolation of the motion of a single apple. Practical applications related to our peacekeeping mission were also a consideration.

The organising committee realized that a unified interdisciplinary approach is required in which the collection of data must be looked at over as wide a geographic transect as possible, ancillary information must be taken without prejudice on all the measureable properties of the objects, multiple regression and principal factor analysis applied to the results, and the nature of motion then assigned to its diverse causes as observation and analysis dictate.

It was further agreed that where alternative models fit the same data both are to be included in the Equation by the delta method of conciliatory approximation.

Let M be the motion of a body as a function $F(X_1, X_2 \ldots)$ of the variables X_i (parametric variables of state, such as the location, velocity, mass, color, texture, DNA content, esterase polymorphism, temperature or smell of M) and let $M_1 = F_1(X_1, X_2, X_3 \ldots)$ be a model that fits the observations, and let $M_2 = F_2(X_1, X_2 \ldots)$ be an alternate model that fits the data more or less equally well. Then $(M_1, M_2) = \delta F_1(X_1, X_2, X_3 \ldots) - (1-\delta) F_2(X_1, X_2, X_3 \ldots)$ is the conciliated systems model. The value of delta is arbitrary, and is usually assigned in the same ratio as the academic rank or prestige of its proponents. Similarly, when dichotomous decisions arose such as whether to in-



clude only moving objects or to allow those at rest in the regression, the alternate modes were both followed and then combined by delta conciliation (see block diagram).

Results

A total of 100,023 objects were examined, measured and used in the statistical analysis. From these we calculated 100 main effects, 49,500 pairwise interaction terms, 50,000 three-way and 410 four-way interaction coefficients, leaving 13 degrees of freedom for error variance. The data and coefficients have been deposited in the British Museum and may be published someday. Sample data are shown in Tables 1-1984 [2].

Some of the objects studied were Imperial Military Artifacts (IMA's) such as cannonballs. Since their tendencies of motion were similar to those of non-IMA's and were independent of the nature of the target (the variance due to schools, hospitals, and villages all had insignificant F values), this circumstance need not concern us further. The IMA's were relevant only in that their extensive use in non-cooperative regions (NCR's) provided data points which otherwise would have required Hazardous Information Retrieval (HIR), and that their inclusion in the studies prevented Un-Financed Operations (UFO's).

Conclusions

The motion of objects is extremely complex, subject to large numbers of influences. Therefore, further study and renewal of the grant are necessary. But several results can be reported already with the usual qualifications:

1. More than 90% of the objects examined were at rest during the period of observation. The proportion increased with size and in the larger size classes, decreased with temperature above ambient at a rate that increased with latitude.

2. Of the moving objects, the proportion moving down varied with size, temperature, wind velocity, slope of substrate if the object was on a substrate, time of day, and latitude. These accounted for 58% of the variance. In addition, sub-models were validated for special circumstances and incorporated by the delta method in the universal equation:

(a) Drowning men moved upward 3/7 of the time, and downward 4/7.

(b) Apples did indeed drop. A stochastic model showed that the probability of apple drop increases through the summer and increases with the glucose concentrations.

(c) Plants tend to move upwards very slowly by growth most of the time and downward rapidly occasionally. The net result is mean tendency downward of about $.001\% \pm 4\%$.

(d) London is sinking.

(e) A stochastic model for the motion of objects at Wyndam Wood (mostly birds, at the .01 level) shows that these are in fact in a steady state except in late autumn, with upward motion exactly balancing downward motion in probability except on a

set of measure zero. However, there was extreme local heterogeneity with upward motion predominating more the closer the observer approached, with a significant distance × observer interaction term.

3. Bodies at rest remain at rest with a probability of .96 per hour, and objects in motion tend to continue in motion with a probability of .06.

4. For celestial bodies, the direction of movement is influenced by proximity to other bodies, the strength of the interaction varying as the distance to the $-1.5 \pm .8$ power.

5. A plot of velocity against time for moving objects shows a decidedly non-linear relation with very great variation. A slope of 32 feet/sec/sec is passed through briefly, usually at 1-18 seconds after initiation of movement, but there is a marked deceleration prior to stopping, especially in birds.

6. For $95\% \pm .06$ of all actions, there is a corresponding reaction at an angle of $175 \pm 6°$ from the first, and usually within 3% of the same magnitude.

7. On the whole, there is a slight tendency for objects to move down.

8. A general regression of motion was computed. Space limitations preclude its publication.

9. In order to check the validity of our model a computer simulation program was developed as follows: the vector for velocity of motion V was set equal to the multiple regression expression for all combinations of maximum and minimum estimates of the regression of coefficients. Since we had a total of 100,010 such parameters, there were 2 to the 100,010 combinations tested, or about 10^{30000}. For each of these, the error terms were generated from a normal random variable generator subroutine (NRBGS). Finally, a statistical analysis of the simulated motions is tested for consistency with the model. Computations are being performed by the brothers of the monastic orders, each working an abacus and linked in the appropriate parallel and serial circuits by the abbots. We have already scanned 10^5 combinations, and these are consistent with the model.

Acknowledgment

This work was supported by the East India Company.

Editor's Incidental Intelligence

[1] Who is Nabi? This question arose after a letter from Isadore Nabi appeared in *Nature* 19 Mar 1981 expressing confusion over recent contradictory statements by Richard Dawkins and Edward O. Wilson (who seem to be backwatering in their advocacy of genetic determinism as the result of criticism from the scientific community). The letter listed Nabi's address as Museum of Comparative Zoology, Harvard University.

Then 23 April came a letter from Edward O. Wilson, stating that the name Isadore Nabi is fictitious and making the claim that his (Wilson's) views on sociobiology and ethics had been distorted by Nabi. Wilson's address is also the Museum of Comparative Zoology at Harvard. *Nature's* editor responded: "Isadore Nabi is believed to be the pseudonym of Professor R.C. Lewontin of Harvard University." (Lewontin is on the faculty of the Museum of Comparative Zoology.)

Next (4 June) came a disclaimer from Isidore Nabi, University of Chicago, stating that he was not the "Isadore" who wrote the letter on genetic determinism.

Finally 25 June in *Nature* Richard C. Lewontin, hoping to throw "some light on the situation," wrote to "state categorically that any assertion that Isidore Nabi is none other than R.C Lewontin is incorrect." He added that "Isidore Nabi is the author of several important works which, I am sorry to say, are not at all of my creation," mentioning particularly Nabi's "brilliant *On the Properties of Motion*" [sic] and the "seminal work *An Evolutionary Interpretation of the English Sonnet*."

New questions arise. What, for instance, is the significance of Lewontin's evident inability to spell the name Isadore correctly? And why did he apply a different title to the paper ("brilliant" indeed) published here under the heading: "On the *Tendencies* of Motion"? Should we ever meet Lewontin, we will ask why he failed to reference *Science and Nature* No. 3 where that first "seminal work" appeared. (We don't want to hide our Nabi under a bushel.)

Postscript. The very latest is no less than an editorial in *Nature* (3 Sept 81) about a "non-existent scientist, Dr. Isidore Nabi (whose first name is sometimes spelled Isadore)" created some years ago at the University of Chicago by "a group of scientists including Professor Leigh Van Valen (still at the University of Chicago), Dr. Richard C. Lewontin (now a professor at Harvard), and Dr. Richard Lester (now at the Harvard School of Public Health)."

"Unfortunately," says *Nature*, "the joke has gone too far . . . for example, a letter supposed to be from Nabi was published in *Nature* (*290*, 183; 1981) making an otherwise plausible point . . . Nabi's name has turned up elsewhere, even as the author of articles in the journal called *Science and Nature*." The editors of *Nature* voice a twofold objection to use of a pseudonym in scientific literature: "First, it is a deception. Second, it allows people with known opinions on important controversial matters to give a false impression that their opinions are more weighty than truth would allow."

We think the editors must have been a little groggy when they wrote that last sentence. And they may have gotten mixed up on names since "Richard Lester" seems more likely to be Prof. Richard Levins of Harvard Public Health who frequently coauthors papers with Richard Lewontin. But we are just plain indignant over their conclusion that "somehow Nabi has to be banished from the scientific literature." Let the editors of *Nature* recall the example of Nicolas Bourbaki, pseudonym for a group representing a respected though somewhat controversial viewpoint in mathematics. *Nature* is ready to bury Nabi without discussing the content of what he says (though we sent them a copy of *Science and Nature* No. 3). We, on the other hand, wish Nabi a continued productive career propounding a viewpoint that is useful to science and to society alike. Where the editors of *Nature* say "Isidore Nabi RIP," we say *Long Live Nabi!*

[2] These tables were not supplied by the author and we did not pursue the matter further. □

Philosophical Apposition

Materialism	*Dialectics*
Matter-in-motion	**No matter where we range**
Preceded the word.	**All things always change.**
The converse notion	**Since nothing remains the same,**
Is rather absurd.	**Dialectic's the name of the game.**

—Saul Birnbaum, Bronx (N.Y.) Community College.

ON THE ESSENCE OF CAUSALITY:

Is it statistical? Or is it dynamical?

In the debate on the nature of causality (*Science and Nature*, No. 3) each side appeals to its own preferred set of dialectical materialist principles. Thus, Marquit (Hörz et al.) lean strongly to the principle (from experience) that changes in dialectical philosophical hypotheses are closely associated with (follow from) changes in scientific knowledge (i.e., dialectical materialism is largely based on science). On the other hand, Talkington seems to assume that new science, by itself, is not sufficient reason to change basic dialectical materialist concepts which have long standing because the new knowledge might conceivably be inadequate or incorrect. Of course, there is some truth in both ideas, and this is what makes it so difficult to adopt a clear-cut choice between statistical and dynamic causality, as general philosophic propositions.

Nevertheless, it seems to me that there is a sufficient preponderance of reason, from both science and dialectical philosophy itself, for now accepting a broader concept of causality than dialectical materialists previously held.

If cognition is an adequate reflection of objective reality outside of us (however much mediated by the mind), it is not undialectical to create a mathematical formalism (basically a description of and a methodology for dealing with a realm of nature which is objectively indeterminate, "dual" in nature, i.e., the wave-particle nature of the microworld, and statistical in its interactions with its macroenvironment) which is also statistical in structure. A concept of statistical *causality* follows quite logically from this, especially as it applies to the microworld—even in the absence of a physical mechanism to "explain" why it works. Nor is this the first case, as Hörz et al. explain, in which a dialectical principle or category has had to be revised or broadened in order to come into agreement with new scientific knowledge.

On the other hand, it seems difficult to accept a notion that there can be a *cognitive* mechanism (formalism), which remains to be devised, that is dynamic in nature and yet will adequately reflect or, better yet, eliminate the indeterminate and ontologically statistical nature of the behavior of microparticles. It's as if a fact of nature were to be eliminated by a rearrangement of ideas—obviously idealism!

However, Talkington claims that once we discover, or invent, the *physical* means of following the *precise* motion and the *precise* interaction of all particles (individual), then the statistical nature of their behavior automatically disappears and dynamic causality is restored.

But what is the possibility of achieving such observation? Right now our scientific constructs (quantum mechanics) tell us that this is an impossibility because the uncertainty principle is an objective fact operating in nature, for the only instruments of observation of the microworld are macroinstruments whose use inevitably runs up against this principle. Is the "uncertainty principle" a fact of nature or only an assumption adopted because we are not yet able to follow the tracks of all individual particles *during the whole* period of their interaction with their environment?

Is there a way out of this dilemma? I think not—because thought experiments, scrupulously and scientifically thought out, inevitably lead to the conclusion that *any* physical method of observation of microparticles must interfere with their motion and yield an imprecise result or an infinite regression of observations and calculations for correction. Perhaps this tells us why no one has come up with a more "fundamental" mechanism to "explain" the statistical and indeterminate nature of microparticle interactions, during the last 50 years.

Yet, is it dialectically correct to extrapolate one level of statistical causality (in the microworld) to all cognitive levels and realms of nature? Clearly not. But Hörz et al. have gotten around that dialectical problem by devising a "flexible" causality, using a systems approach to the question and, in a sense, varying the certainty involved in the statistics from zero to 1, depending on the cognitive level involved. This seems dialectically reasonable and satisfactory to me.

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Cohen finds it "difficult to accept" the notion that science will someday come up with a fundamental explanation of microparticle behavior based on dynamic principles, arguing instead that "our scientific constructs (quantum mechanics) tell us that this is an impossibility because the uncertainty principle is an objective fact of nature." On this basis he joins with Hörz *et al.* of the GDR and Erwin Marquit (their English-language editor) to propose that Marxist philosophy be "revised" in order to embrace the concept of statistical causality and give it a role more primary than that of dynamic causality. I shall try to answer here the main thrust of his arguments, referring the reader to *Science & Nature* No. 3 for further clarity on my position.

It seems to me that Cohen's position involves three basic errors in the theory of knowledge. First, his position does not differentiate clearly between a formalism (procedure for calculating) and the natural phenomena reflected (imperfectly) by the formalism. Quantum mechanics provides a somewhat clumsy reflection of the microworld, with well known limitations when applied to real problems. The "fact" that the uncertainty relation plays an essential role in the quantum formalism provides no proof whatsoever that the observed

statistical regularities of quantum physics may not someday be explained in terms of a dynamic mechanism operating at a deeper level of nature. Thus, to portray the uncertainty relation as "an objective fact of nature" is to obscure its actual cognitive status and open the way for idealist interpretation.

Second, the Cohen position does not differentiate between the empirical results obtained by a formalism and the philosophical interpretation concerning what the formalism tells us about nature. The Copenhagen interpretation, at least in name, is acknowledged to be a reflection of the empirical results, though its proponents generally seek to give the interpretation a status of absolute knowledge. The concept of statistical causality differs only in nonessential details from the Copenhagen interpretation: the latter openly denies causality at the microphysical level while the former attributes causality at that level to a stochastic or innately random property of particles. Hence, the concept of statistical causality should not be passed off as anything more than another *interpretation*. For Cohen, however, "thought experiments" (made in the framework of quantum mechanics and thus constituting more interpretation) provide an adequate basis for elevating the uncertainty relation to the status of a "principle" of unknowability, i.e., the "impossibility" of ever knowing the underlying causal mechanisms for the uncertainty phenomena. This is not dialectical logic and it violates every principle of Marxist materialism.

Third, Cohen's position ignores the important lesson from history that when a philosophical interpretation reacts on practice to hinder development of science, it must eventually be overthrown. We know, for example, how the science of mechanics stagnated for centuries because Ptolemy's formalism (epicycles) became linked with the Platonic interpretation of scholastic theology. An analogy with today's particles physics (largely based on quantum mechanics) has been proclaimed by a leading proponent of the Copenhagen interpretation.

> If we wish to compare the results of present-day particle physics with any of the old philosophies, the philosophy of Plato appears to be the most adequate. The particles of modern physics are representations of symmetry groups and to that extent they resemble the symmetrical bodies of Plato's philosophy. [Werner Heisenberg, *Physics Today*, March 1976, p. 38.]

For an instructive contrast, consider the following statement by a physicist whose outlook is more tuned to the contradictions in microphysics and the potential for revolutionary change:

> Let me stress that I do not believe that the standard theory will long survive as a correct and complete picture of physics . . . Physics of the past century has been characterized by frequent great but unanticipated experimental discoveries. If the standard theory is correct, this age has come to an end . . . Surely this is not the way things will be, for nature must still have some surprises in store for us . . . The standard theory may survive as part of the ultimate theory, or it may turn out to be fundamentally wrong. In either case, it will have been an important way station, and the next theory will have to be better. [Sheldon Lee Glashow, Nobel lecture, *Science* 210: 1319 (1980).]

The contrasting positions of these two Nobel laureates emphasizes the utter necessity for discriminating carefully between the various philosophical interpretations offered within the scientific community. The Heisenberg statement reflects a mystical trend in a world dominated by corrupt monopoly capitalism at the stage of acute crisis [cf. Paul Forman, "Weimar Culture, Causality, and Quantum Theory: Adaptation by German Scientists to a Hostile Environment." *Historical Studies in the Physical Sciences* 1: 1-115, 1969.] The Glashow statement, on the other hand, represents the relentless questioning mode of the scientific process, very much in accord with the Marxist historical view of developing knowledge. [cf. J.D. Bernal on the theoretical mess of particle physics today, *Science in History*, MIT Press 1971, pp. 746-51, 849, 861.]

Hence, while we seek (with Cohen) the further development of Marxist philosophy on the basis of new scientific results, we must strive diligently to weed out idealist interpretations that come in "Marxist" disguise. That's what Lenin's polemic against Bogdanov was all about; in *Materialism and Empirio-Criticism*, Lenin repeatedly pointed out that good scientists such as Poincaré could also espouse idealist concepts not to be taken seriously by Marxists.

I will conclude by directing due attention to Cohen's cute though unconvincing argument for a "flexible" causality that changes its essential nature in some unspecified manner along a probability scale of zero to one. Let us ask what such a gimmicky treatment of a fundamental philosophical category can contribute to the resolution of the serious problems raised by quantum mechanics. Here, if ever, *materialist* dialectics are needed as a guide in "the endless process of the discovery of *new* sides, relations, etc. from appearance to essence and from less profound to more profound essence . . . from coexistence to causality and from one form of connection and reciprocal dependence to another, deeper, more general form." [Lenin, *Philosophical Notebooks*, Moscow 1972, p. 222.]

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We Should Not Accuse Nature of Duplicity

In quantum mechanics it is the wave function that describes a system and allows us to calculate the probability, depending both upon the system and our information about it. There are therefore as many wave functions as observers. If I take Bohr's example of twin slits in a screen with a light, I can say that we shall have different wave functions according to the opening of one slit or the other. Naturally, if we know that both slits are open we have another wave function to correspond with the interference fringes. For different information, we have different wave functions. Thus quantum statistical determinism, which makes our predictions depend upon our information, does not restrict the possibilities of science and does not impose upon it any limit other than that of conforming to the nature of things, which is to say, of constructing a more adequate representation of reality.

If we expect more and more exact answers for the questions we ask, but find that the replies of Nature are ambiguous, we should not accuse Nature of duplicity or indeterminism but rather suppose that our questions were badly put or not sufficiently precise.

In my opinion, quantum physics does not represent a failure of determinism but of mechanics, which has become progressively exhausted ever since it was raised upon a pedestal above the other sciences. It seems to me that this principle of uncertainty or indeterminism provides a valuable expression for arriving at certain axioms but is otherwise superficial and inaccurate by its very form: the indeterminism of an exact law yet to be discovered.

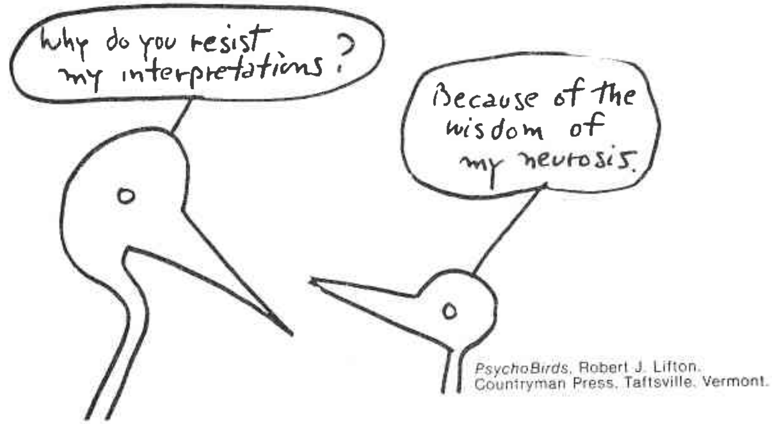
Since the indeterminism for various cases is measured by Planck's constant, it may be supposed that we do not yet grasp the deep significance of Planck's constant in the uncertainty relationship. For an analogous example, consider Avogadro's constant that appears in quantifying various phenomena of fluctation and concentration in all kinds of circumstances. If we did not have the atomic theory at our disposal, we might have supposed that Avogadro's number was connected with the existence of the atomic element. But, given the atom's concrete nature, we can say that it is a reality, and I think that for Planck's constant *h* we should have some analogous understanding.

That living thing which is our understanding is not given *a priori*; it has no rigid framework as was formerly thought could be imposed upon it. As the result of interaction with the world, this understanding approaches closer and closer to that reality which we are obliged to postulate. —Paul Langevin (1872-1946), in *New Theories of Physics*, proceedings of 1938 Warsaw Conference of International Institute of Intellectual Cooperation, pp. 231-235, abridged excerpt.

Invitation to a Revolution?

The content of the principle of causality is recognition of the fact that cause underlies every phenomenon. Prediction of the behavior of an object, on the other hand, is the result of the cause-and-effect connection, of a sufficiently exact fixation of the initial state of the object and the nature of its interaction with the environment. Quantum mechanics, however, does not produce either the first or the second result *at the present stage of its development*. For this reason it expresses causality in the microworld in the form of probability. —A.P. Sheptulin, *Marxist-Leninist Philosophy*. Moscow, Progress 1978, p. 205. *Emphasis added.*

The successes of quantum electrodynamics have demonstrated the correctness of our basic physical concepts within a definite domain of phenomena. However, these successes are relative . . . Apparently, the difficulties of the present theory can be removed only by means of a new change, and, moreover, perhaps a cardinal one, in the basic physical concepts. It is quite probable that even the fundamental space-time concepts of modern physics will undergo a change in this process. —A.I. Akhiezer and V.B. Berestetskii, *Quantum Electrodynamics*. Tr. from 2nd Russian ed. by G.M. Volkoff. Interscience (Wiley) 1965, p. 853.



PsychoBirds, Robert J. Lifton, Countryman Press, Taftsville, Vermont.

Filling a gap in our history of mathematics

PEANO: Life and Works of Giuseppe Peano*

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Peano: Socialism was another of his conclusive counterexamples.

Called a "priority project in the history of mathematics," by Kenneth O. May, the publication of the biography of Peano is a welcome event. It is a companion work to Kennedy's earlier book of translations, *Selected Works of Giuseppe Peano*, U. of Toronto, 1973, and contains 10 years of prodigious research in what appears to be truly a labor of love.

Born 1858 into the exciting events of the Risorgimento (Resurgence of a united Italy) Peano became a Socialist and left the church, despite his closeness to his brother and uncle, both priests. The son of small farmers who sacrificed to send him to school, Giuseppe Peano remained proud of his origins to the day of his death in 1932, when, as he had requested, he was given a simple burial in the common field.

Peano as Mathematics Pioneer

Peano's original work in mathematics began when he was still a calculus teaching assistant in 1883, at the University of Turin where he did all of his work. In the next 17 years his work impacted on many fields in mathematics but in the United States little has been known of his work except the axiomatization of the integers and his space-filling curve [1]. Now, with Kennedy's book, the record stands corrected.

His first works were in analysis: the integrability of continuous functions and of bounded functions with finite number of discontinuities; a theorem on uniform continuity for functions of several variables; the first proof of the existence of a solution for $y' = f(x,y)$ on the sole assumption that f is continuous; the method of successive approximations for solving systems of linear differential equations; a generalization of the mean value theorem for derivatives; and the concept of interior measure of a point set which overcame a difficulty in

*Reidel, 1980. 215 pp. + index. Cloth $34.00, paper $14.95.

Cantorian measure. Interior measure was also defined that same year, independently, by C. Jordan. Commonly called Jordan measure, few realize that it is Jordan-Peano measure. Peano also had an ongoing interest in quadrature and provided new formulas for the Taylor Remainder, the trapezoidal formula and Simpson's formula. He was also a master of the counterexample in analysis; still in common use are the space-filing curve already mentioned and a continuous function whose 2nd order partial derivatives do not commute.

Peano made enthusiastic use of the vectorial methods of Hermann Grassman. The use of vector-analysis became a hallmark of the Peano school but was bitterly opposed by conservative Italian mathematicians. Burali-Forti was denied the libera docenza because of his insistence on vectors, despite Peano's best efforts to convince the rest of the committee to grant the degree. Peano called vector methods in calculus, "that royal road sought in vain since the time of Euclid." It was Peano who first gave the axioms for a vector space and gave an example of an infinite-dimensional vector space.

But Peano's greatest contribution to mathematics, many believe, was his pioneering work in symbolic logic and the axiomatic method of modern mathematics. Bertrand Russell fully acknowledged his debt to Peano whom he had met at the International Congress of Philosophy in Paris in 1900. Kennedy quotes from Russell's *Autobiography*:

"The Congress was a turning point in my intellectual life because there I met Peano . . . always more precise than anyone else . . . owing to his mathematical logic.

". . . his notation afforded an instrument of logical analysis such as I had been seeking for years . . . a new and powerful technique for the work that I had long wanted to do."

The introduction of symbols to logic is a striking example of the dialectical relationship between form and content. To Peano, "the notations of logic are not just a shorthand way of writing mathematical propositions; they are a powerful tool for analyzing propositions and theories". . . And of making new discoveries, his disciplies believed.

Peano hoped that he was fulfilling Leibniz' dream of a universal language in which a basic knowledge of all things could be written and easily assimilated. But Peano was careful to make the distinction between form and content: "The notations are somewhat arbitrary; but the propositions are absolute truths, independent of the notations adopted."

In pursuit of this ideal, Peano gathered around him a school of outstanding mathematicians who attempted to sum up all mathematical knowledge in a collection of formulas and theorems, stated in the new symbols. This encyclopedic undertaking, called the *Formulario*, consumed most of the Peano's time for many years. The *Formulario* contained over 1,400 formulas and theorems, and was an opportunity to clarify ideas and present new findings. But its use at the time was restricted by the highly abstract format and the later use of an artificial, international language, a cause to which Peano devoted the latter half of his life.

The very collective nature of Peano's style of work may have contributed to the neglect of his work by mathematics historians, although the contrary should be true. He often said: "What does it matter that an idea succeed with the name of Peano or another? The important thing is that it succeed."

In the *Aritmetices Principia* of 1889, Peano outlined the axiomatic method of modern mathematics and his famous axiomatization of the integers. The title of the paper indicates his awareness of the historical importance of what we now know as the Peano axioms, which were developed at the same time but independently of Dedikind. Later, Peano proved the independence of the 5 axioms and continued, over the years, to write on the nature of mathematical definitions. But his intensive work in logic did not continue into the 20th century when Russell replaced him as the recognized leader in the field.

In an article (1913) reviewing Whitehead and Russell's *Principia Mathematica*, Peano explained the difference in interest: "In the *Formulario* logic is only a tool for expressing and treating propositions of ordinary mathematics; it is not an end in itself." This restricted interest in logic, Kennedy believes, may explain Peano's failure to develop rules for inference (cf., *Selected Works* , p. 8).

Peano as a Socialist

Even though mathematicians, Kennedy among them, regret that Peano gave most of his time after 1903 to the development of an international language, Kennedy admires Peano's dedication to a cause whose goal was world peace. Peano undertook this study in a scientific manner and in 1914 published the *Vocabulario commune ad latino-italiano-english-deutsch* of 14,000 entries which were common to these languages, plus Greek, Spanish, Portugese, Russian and Sanscrit for many of the entries. He often referred to Leibniz as the source of his inspiration in this work.

Kennedy includes many revealing facts about the political background of the times. For example, he reprints many excerpts from official university statements on the frequent student protests (1881, 1883, 1892, 1894 (two), 1896, 1897, 1900). However, he gives no further background of these events nor does he attempt a *Science at the Crossroads* type of analysis of how the dynamic changes of this period may have affected Peano's work.

A progressive feature of the school of Peano was the active participation of women, especially in later years. Kennedy lists no less than 14 women of the 45 mathematicians associated with Peano, including Peano's last 5 assistants. He also notes that of 242 delegates to the First International Congress of Mathematicians, held in Zurich in 1897, 38 were women. We have *not* come a long way, baby!

Kennedy gives brief biographies of the 7 mathematicians closely associated with Peano at the end of the 19th century; most turn out to

be anti-clerical and left-leaning politically. Vailati had left the church. Burali-Forti asked that he not be given a religious funeral. Vacca was a founder and leader of the Italian Socialist Party (1892) who had been banished from Genoa in 1897 during a period of reaction. Padoa, who was Jewish, had ample reason to fear the rise of fascism in Italy. Although Padoa died before the anti-Jewish laws took effect, another Jewish Peanoite, Professor Gino Fano, was forced to leave the University of Turin in 1938.

Peano's socialist sympathies took a dramatic form during the cotton workers strike of May 1906. The workers were demanding a reduction in the work day with no cut in pay. To show his support, Peano invited the strikers to a picnic at his villa. Four thousand made the march to Peano's garden. Two days later the strikers won their demand of 10-hour day at same pay as the former 11-hour day.

Evidently, Peano's socialist convictions were not Marxist-Leninist because Kennedy reports that Peano supported the Italian entry into World War I. However, he opposed chauvinism, saying that "patriotism is a collective pride that induces us to judge other people inferior." This opinion, said Peano, was "erroneous, dangerous and damaging."

One cause of war, he believed, was the "variety of language." The fascist government looked with disfavor on Peano's internationalist work and denied him a travel permit in 1930.

Peano as Teacher

Peano remained deeply interested in mathematics education, at all levels. His approach to education was democratic and he made great efforts to present the papers of women mathematicians before the Academy of Sciences in Italy. At the University of Turin he opposed separating mathematics majors from mathematics education majors. He favored the abolition of tests which he called "a crime against humanity to torment the poor pupils." At the elementary level he urged the use of interesting problems and wrote two books of mathematical games for this purpose. Unfortunately, his teaching of calculus, once excellent, became highly abstract and overburdened with "the symbols." But he was always able to inspire very capable university students who became lasting followers. He also made use of the history of mathematics in his work, going back to the classics. "The Arabs have taught us much," he asserted.

Although Peano pioneered in reducing mathematics to strictly logical forms, Kennedy concludes that Peano was not a formulist. Peano quotes Horace about chasing nature out with a pitchfork but nature always rushes back. For arithmetic or geometry, he did not think it necessary to prove the postulates consistent: "A proof of consistency of a system of postulates can be useful, if the postulates are hypothetical and do not correspond to real facts." Apparently, Peano regarded mathematics as an abstraction from the real world.

Finally, what is missing from Kennedy's excellent book is any reference to the huge economic changes taking place in Italy at that time, the development of Turin as a major industrial center, the rational organization of large-scale industry and the greater demands this expansion was placing on the sciences, especially that of mechanics. But then, that is another book which I hope someone will write.

[1] *Editor's note.* To demonstrate the ambiguity in Jordan's definition of a curve as single-valued image of the line segment, Peano developed a curve that could fill an area completely so that ultimately the curvilinear image, in the intuitive sense, was lost.

Reviewer Beatrice Lumpkin, who teaches mathematics at Malcolm X College, Chicago (60612), comments further: "It would be reasonable to assume some correlation between Peano's socialist outlook and his approach to mathematics. His insistence on objective criteria for truth, that is, the truth of the content not be dependent on the set of symbols used to represent the proposition, is in refreshing contrast to the dominant view today in bourgeois mathematical circles which holds that mathematics has only internal truth, following the internal dialectics of the mathematical system, with no relationship to anything external (the real world). Furthermore, I believe that Peano did recognize a relationship between form and content, in that he tried to put all of mathematics in the same set of symbols. This type of logical clarity, he believed, would aid in the further development of mathematics." □

Marxism and Humanist Objectivity

In other words, humanism is a mode of thinking and acting, of living in general, that places man and his search for a harmonious life—a life harmonious for all, not for an elite—into the center, without any appeal to supernatural forces. Such humanism has come down to us throughout the ages in different forms: from the Chinese Taoists and the Ionian philosophers to Diderot and Feuerbach. It finds its modern and highest form in Marxism, since its character is social, historical and dialectical.

[We see] how many other forms of humanism, forms different from Marxism, fail in their full understanding of the dialectics of man and the universe around and within him. [It is] clear that in the present, sometimes animated, debates on the relation between science and value judgment, it must be clearly stated that so-called "objective" description always implies a value judgment: "Hence a 'value-free' description or a 'neutral' science is a fiction."

Inquiry is a dialectical process between man and the world, in which the standards for determining value and truth develop in the process itself. This, I may add, is an interesting clue to the understanding of the process of scientific discovery, even in such abstract fields as mathematics . . . Sartre, in spite of his sympathy for Marxism and his humanist dialectics, continues to see man's individuality in praxis as prior to society. This is a crippled understanding of freedom: for Marx man in his very essence is a social being. —Dirk J. Struik reviews *Humanism and Marx's Thought* by Howard L. Parsons (*New World Review* Jan 1976 p 26).

BIBLIOGRAPHIC BRIEFS FOR NATURAL SCIENTISTS

Comment on the literature of science and philosophy from Marxist point of view. Contributions welcomed.

On the Reasonable Effectiveness of Mathematics

Herbert J. Bernstein and Anthony V. Phillips' 1981 Fiber Bundles and Quantum Theory. *Scientific American* July.

Some mathematicians are delighted and others disturbed by the unplanned, indeed unintended, physical applications of some of the most abstract, recently developed mathematical structures. Recent issues of *The American Mathematical Monthly* have featured articles which grope for an explanation of "the unreasonable effectiveness of mathematics." A good, short explanation in the dialetical materialist (scientific) sense concludes the article by Bernstein and Phillips:

"We believe the current usefulness and physical significance of such mathematical concepts is no accident. Neither mathematicians nor physicists are insulated from their cultural and physical milieu, and the ideas and perceptions of workers in each discipline are influenced by the other discipline. Moreover, mathematicians and physicists unavoidably share unspoken assumptions about the everyday world and the logic by means of which the world is projected onto abstract science. Indeed, they share a passionate commitment to such rational work. What seems most marvelous is not what has been called the "unreasonable effectiveness" of mathematical concepts in physics, or the fecundity of physical intuition as a source of new mathematics. Rather one must admire the success of the the common intellectual approach of mathematicians and physicists in creating a rich, coherent and powerful image of the physical universe" [Beatrice Lumpkin].

Dialectical logic survives again

M. Mark Mussachia 1977 On Contradiction in Dialectical Materialism. *Science and Society* 41: 257-280; and discussion 42: 185-198; 1978.

Bourgeois philosophers, unable to explain the emergence of new knowledge that contradicts the old, tend to abandon the concept of truth. They often speak of the creative process as an "irrational act" because it involves free association of information and ideas without benefit of formal logic. Because of political and cultural prejudice, they reject the concept of a dialectical form of logic at play in creative mental processes, a higher form of logic than the classical form taken by the scholastics from Aristotle. Mussachia brings this bourgeois prejudice *in spades* to the problem of contradiction, but he wraps his arguments in Marxist terminology.

His paper develops a mechanistic argument in which human thought processes are restricted to the laws of classical formal logic (including its law of non-contradiction or excluded middle). Mussachia assures us that "Aristotle proposed these as the most general rules implicit in the way we think of things," that "most modern philosophers" agree with Aristotle, and that "formal logical operations are rooted in the activity of the mind-body system." Later, in an aside, he admits: "Real logical contradictions" do occur because of "the complex, dynamic nature of the world." But he has nothing to say about the creative thought processes by which we humans are able to cope with a contradictory world where truth can turn into its opposite, nothing to say about the form of logic which deals with interpenetrating relationships and qualitative transformations.

Instead, Mussachia informs us that Marx, Engels and Lenin "lacked a clear understanding of logic" and bequeathed to us "the burdensome ambiguity in their Hegelian concepts." To help us throw off this burden, he gives us some tricky definitions that misinterpret what the founders of Marxism said on logic. For example, Mussachia develops his main argument on the basis of a distinction between "objective" and "subjective" contradictions. Though he attributes this distinction to Lenin, no basis for his key definition is found in the two references he cites. In the first, Lenin remarks on Hegel's distinction between objective "dialectics of cognition" versus self-conscious "subjective, sophistic dialectics" which fail to "unite the opposites" (*Philosophical Notebooks* 279f.). In the second reference, Lenin discusses the pervading confusion of objective logic with subjective logic in Aristotle's *Metaphysics*, pointing out that Aristotle's logic is objectively "an inquiry, a searching, an approach to the logic of Hegel" (*ibid.* 367-369). In both cases, Lenin clearly refers to the objective role of dialectical logic in thought processes, independent of consciousness. Mussachia's interpretation is exactly the opposite. As Lenin said of the scholastic philosophers, we can say of Mussachia: he takes "what was dead in Aristotle, but not what was *living*" (*ibid.* 367).

In essence, Mussachia uses rigid formal logic as the standard by which to judge the higher level of logic required to deal with the untamed semantic content of the real world, accompanying his revisionism with nasty words about "Papists of the left" who might not agree with him. This is an old technique for attacking materialist dialectics (cf. Sidney Hook, *Reason, Social Myths, and Democracy* 1940). Whether the misrepresentation of Marxist logic is conscious or not seems irrelevant. It comes out just as venemous from one posing as a "friend."

Through the Prism of History

M. King Hubbert 1963 Are We Retrogressing in Science? *Science* 139: 884-890.

A prophetic critique of institutionalized science as we know it in these United States . . . the transition of the university from an educational to a research institution and the careless retreat from the teaching of fundamentals . . . the rise of academic opportunism and the professional entrepeneur . . . the relationship of specialization to the rise of authoritarianism in scientific knowledge. These problems of science under capitalism have grown exponentially in the decades since this paper was written.

Arthur Clegg 1979 Craftsmen and the Origin of Science. *Science and Society* 43: 186-201, with discussion 44: 86, 480-481; 1980.

The materialist thesis is that the modern scientific method and the concept of intellectual freedom both derive from the practices developed in late medieval and early renaissance workshops rather than in the academic halls ("we do not know that even Roger Bacon actually experimented. His mere advocacy of experiment was enough for the order to imprison him!"). In a rejoinder to criticism from Robert G. Colodny, Clegg concedes that European craftsmen did not solve the problem of inertia but "by their success, forced some of the more intelligent academics to adopt both their methods and their philosophy." A timely and stimulating reminder than in general technology precedes and provides the basis for science.

Y. Schienin 1978 *Science Policy: Problems and Trends*. Cloth 330 pages. Progress (Moscow). $5.00.*

Describes the emergence and development of science policy in the USSR. Examines the interaction of science and politics in the context of the 20th-century scientific and technological revolution, the relationships of man and organization in science and technology, the structures and function of the science centers and institute network. An analysis of the Soviet system of science planning and organization compares the 60-year Soviet experience to that of the USA and other countries. [Hyman R. Cohen.]

P.L. Kapitza 1979 Plasma and the controlled thermonuclear reaction (Nobel lecture). *Reviews of Modern Physics* 51: 417-423.

When the tokamak was announced in the 1960s, scientists in the west at first refused to believe the performance figures on this Soviet breakthrough toward practical fusion power. Now, the U.S. and western Europe are cheerfully building ever larger tokamaks and even designing what "hopefully" will be a practical power plant, thought it is well known that the theoretical basis for this hope is quite weak. Now comes Peter Kapitza,

the physicist repeatedly headlined as being under "house arrest" in the Soviet Union, telling why he thinks the pulsed-operation principle of the tokamak makes it unfeasible, outlining his research program for continuously-heated plasma as the basis of a simpler, more efficient reactor, and discussing candidly the unsolved theoretical problems. Will this become another "unbelievable" breakthrough? PS: Kapitza also discusses the unresolved safety problems of fission power.

Reductionism comes in many guises

Richard C. Lewontin 1981 Sleight of Hand. *The Sciences* July, pp. 23-26.

This is a review essay on *Genes, Mind and Culture*, a book by Charles J. Lumsden and Edward O. Wilson that is possibly the most pretentious effort yet to make human society fit the Procrustean bed of Wilson's mechanistic sociobiology. The first author, a physicist, no doubt provided the rationale for the absurd model of human cultural development in which imaginary genetic units, "culturgens," determine culture change by stochastic interaction with one another. Lewontin here renders yeoman service in the struggle against vulgarization of both biology and sociology by providing an historical context for biological reductionism that goes back to Descarte's *Discourses*, to show the historically-conditioned nature of models.

The real value of Lewontin's dialectical and historical analysis emerges when it is compared to a Popperian critique of the same book (*Science* 213:749-751; 1981) that shows the non-falsifiable nature of the theory presented but is unable to go any more deeply into the philosophical problems. Also instructive is comparison with the diffuse review by Richard Dawkins of a comparable book on genes and culture (*Nature* 290: 345-346; 1981) wherein the ethological approach of Konrad Lorenz proves to be much more sympathetic than critical of sociobiology.

H. Soodak and A. Iberall 1978 Homeokinetics: A Physical Science for Complex Systems. *Science* 201: 579-582.

Two physicists baldly propose a "physical basis for reductionism" that provides another horrible example of how far astray scientists can be led by mechanistic thinking. As in the case of sociobiology (see Lewontin, above), construction of a model involves shallow analysis, deceptive use of terms, and a backward-looking ideology. In this case, animal memory and even human societies are supposed to function as "thermodynamic engines" in which "the individual atomisms have many internal degrees of freedom." This approach is justified by positivist statements such as "Physical law provides the only constraint to reality" (thus removing all constraint on the interpretation of mathematical equations which necessarily reflect only partial aspects of reality). While the authors concede that many will find their proposal "philosophically offensive," it is presented so cleverly that it seduced one Marxist reader who was already sold on the concept of so-called statistical causality (debated in this issue of *Science and Nature*).

G. Marmo and B. Vitale 1980 Quality, Form and Globality: An Assessment of Its Catastrophe Theory. *Fundamenta Scientiae* 1:35-54.

A thoughtful study of catastrophe theory, indicating its power for yielding insight on some problems of physics and biology while also pointing out the potential pitfalls that make it dangerous, especially in reductionist application to social problems. The technical assumptions (limitations of the model) tend to be progressively forgotten so that artificial and inappropriate results come to be treated as natural and universal reflections of reality.

Questions of a Useful Outlook

Marx W. Wartofsky 1980 The Critique of Pure Reason II: Sin, Science, and Society. *Science, Technology, & Human Vaules* 6(33): 5-23.

Examining questions of what the "metasciences" (philosophy, history, and sociology of science) "*can* and *should*" do about social issues related to science, Wartofsky resolutely ignores the existent Marxist theory on such questions. In this way he is able to find the need for a "normative theory of what the proper autonomous and undistorted role of the sciences is," since the metasciences now function as apologists "for state policy, dominant ideologies, or class interest . . . whether in advanced capitalistic societies, like the U.S., or in purportedly socialist countries, like the U.S.S.R." His final conclusion is that the proper function of the metasciences is that of critically examining the internal processes of science and its external interactions with the rest of society ("What shows itself as deficient rationality, or irrationality, then becomes the object of criticism."). Asking whether such critical applications of the metasciences make any real difference in the practice of science, he can find nothing more "than a slight ripple in the pond of ongoing research," thus evenhandedly ignoring both the insidious effects that science has suffered from bourgeois thought (pragmatism, positivism, and so forth) and the potential value of the dialectical materialist mode of thought. As one penetrates beneath the scholarly pyrotechnics, it becomes clear that Wartofsky is concerned only with the philosopher as a professional sideline critic, and has no concept of the partnership proposed by Lenin, to help scientists themselves clarify their working philosophy, that which actually guides scientific practice from day to day.

***Working Papers on Marxism and Science*, Winter 1981. A new journal published by the Science Task Force of New York Marxist School (PO Box 419, NYC 10014).**

With so many philosophical problems of science confronting us, a new journal in this field can only be welcomed. In this comment on *Working Papers*, the standard for judging will be the same as that applied to papers appearing in *Science and Nature*, that is, how does it help demonstrate the usefulness of the Marxist outlook for the practitioner of science? On this basis Vicente Navarro (Work, Ideology and Science: The Case of Medicine) seems the outstanding contribution, especially where he shows the linkage between positivist philosophy and the use of statistical methods to obscure the causes of work-related disease ("causality was supposed to be explained by association of immediately observable phenomena").

While one may not agree with all of Navarro's formulations, his paper is highly stimulating. The same may be said of the papers by Eli Messinger (An Introduction to Soviet Psychology), discussing the work of Vygotsky and Luria; the late Arthur Felberbaum (In Defense of Engels), exposing Social-Democratic distortions; and Hilary Rose and Stephen Rose (Metaphor in Orbit), defending scientific materialism and the materiality of science against the attack of idealist authors in *Radical Science Journal*.

Richard Levins (Class Science and Scientific Truth) is more problematic. Under the slogan that "all science is class science," he criticizes a paper by Soviet science historian Bonifati Kedrov (Regarding the Laws of the Development of Science), also printed in the same issue. While recognizing that Kedrov deals with questions of organizing science to serve Soviet needs, Levins is highly critical of the paper because it omits class considerations. Levins does not take into account that Kedrov restricted his discussion to natural science which, to a great extent, belongs to what has been called the gold fund of human knowledge, i.e., knowledge that remains useful no matter what changes occur in the social superstructure. Nor does Levins allow for the fact that the Soviet Union has no internal capitalist class and only some fast-vanishing middle-class elements in the social structure. For meaningful discussion, socialist problems must be discussed in the light of the qualitatively new set of contradictions governing the development of science just as in other social processes. Kedrov should be criticized on the basis of how he deals with that set of contradictions; we should not expect him to help resolve our own situation.

Least useful by far is the contribution of Jim Becker (Economic Formation and the Formation of Economics), a candid and blatant rewriting of basic Marxist political economy. Making "apologies to any remaining biblical Marxists," he interposes "administrative classes" between capitalists and workers, and thereafter concerns himself exclusively with what he defines as "class struggle" between dominant and exploited branches of petty bourgeois administrative elements. Though Becker is properly concerned with the real conflicts of interest that occur within a scientific discipline, he does not acknowledge their historical significance as reflections of the larger struggle between opposing class forces in the Marxist sense. Such middle-class blindness to reality is only too typical of scientists who do not relate their professional problems to the greater problems of society. Without the social consciousness that leads to alignment with labor and working people, scientists are foredoomed to the classical fate of the petit bourgeois – to become irrelevant if not tools of reaction.

One general comment on *Working Papers* concerns a tendency of some authors to ignore the socialist content of the Soviet scene or the historical processes taking place there. An instance is a Felberbaum statement ("Mechancial materialism as exemplified by the Second International and the Third International after Lenin, is the ideology of false disciples of Marx.") which implies an absence of ideological struggle or philosophical development within the Soviet Union. The philosophical literature shows that this is not true. There are problems, of course. Concerning philosophy of science, for example, one U.S. scientist has remarked: "The truth is that I find Soviet scientists too much like American scientists." But this may simply reflect the freedom of thought enjoyed by Soviet scientists. One suspects that the Soviet state gives priority to scientific output over the philosophical interpretations of scientists. And there has certainly been plenty of bourgeois ideology embedded in the international literature to which Soviet scientists have free access. Though mechanist and even idealist tendencies are there, they are far from dominant and the philosophical controversies are quite lively (cf. Loren R. Graham, *Science and Philosophy in the Soviet Union*).

Another book for useful insight on processes of change within the Soviet Union is Giuseppe Boffa's *Inside the Krushchev Era* (Marzani & Munsell 1959). As Moscow correspondent for *L'Unita del Popolo* from 1953 to 1958, Boffa provides a first-hand account of what happened after the exposure of Stalin's distortion, the struggles that took place and the changes that were made to decentralize power and unleash the creative powers of the Soviet people. Come to think of it, this book deserves reprinting now to answer the questions of a new generation. □

A Beginner's Bookshelf on Marxism in Natural Science

- *Reader in Marxist Philosophy*. Howard Selsam and Harry Martel, editors, International, New York 1973. $7.50, $4.50.
- *Dialectical Materialism*. Maurice Cornforth. International, New York 1972. Three volumes, paper $5.85.
- *Marxist-Leninist Philosophy*. Alexander Petrovich Sheptulin. Progress (Moscow) 1978. $5.00.*
- *Materialism and Empirio-Criticism*. Vladimir Ilyich Lenin. International, New York 1970. Paper $2.95, cloth $7.50.
- *Dialectics of Nature*. Frederick Engels. International, New York 1940. Paper $3.50, cloth $7.50.

BOOKS RECEIVED

- V.G. Afanasyev, *Marxist Philosophy, a Popular Outline*. Moscow, 1980. $8.00.*
- Tony Bennett, *Formalism and Marxism*. Methuen, New York 1979. $12.50, $6.95.
- Alan R. Burger, Hyman R. Cohen, and David H. DeGrood, editors, *Marxism, Science, and the Movement of History*. B.R. Grüner, Amsterdam, 1980.
- Donald R. Griffin, *The Question of Animal Awareness*. Rockefeller Univ. Press, 1981, $8.95.
- Dominique Lecourt, *Proletarian Science? The Case of Lysenko*. Schocken, 1977, $14.00.
- Marx, Engels, Lenin. *On Dialectical Materialism*. Moscow, 1977. $2.75.*
- John Mepham and D.H. Ruben, editors, *Issues in Marxist Philosophy:* Vol. 1, *Dialectics and Method*, $25; Vol. 2., *Materialism*, $24; Vol. 3, *Epistemology, Science, Ideology*, $24. Humanities Press, 1979.
- Richard Norman and Sean Sayers, *Hegel, Marx and Dialectic: A Debate*. Humanities Press 1980, $37.50, $12.00.

- M.E. O'Melyanovsky, *Dialectics in Modern Physics*. Moscow, 1981 $8.25.*
- Peter Plath and Hans Jorg Sändkühler, editors, *Theorie und Labor: Dialektik als Program der Naturwissenschaft*. Pahl-Rugenstein Verlag (Köln) 1978.
- Georgi Plekhanov, *Selected Philosophical Works, Vol 5*. Moscow 1980. $19.00.*

*Imported Publications, 320 West Ohio, Chicago, Ill. 60610.

Highlights of Back Issues

Science and Nature No. 1
On Intuition and Dialectical Logic, NIKOLAI N. SEMYENOV.
Karl Marx on Science and Nature, ROBERT S. COHEN.
The Cusp Catastrophe and Dialectics, MARTIN ZWICK.

Science and Nature No. 2
Episodic Change Versus Gradualist Dogma, STEPHEN JAY GOULD.
Christopher Caudwell and Thermodynamics, SHAUN LOVEJOY.
Frederick Engels and Science, J.D. BERNAL.
Sociobiology Examined Philosophically, PYOTR FEDOSEYEV.
Albert Einstein as Peace Advocate, H. JURGEN-TREDER.

Science and Nature No. 3
On Evolution and the English Sonnet, ISADORE NABI.
Mathematics: Its Essential Nature and Objective Laws of Development, A.D. ALEKSANDROV.
Dialectical Materialism in Biology, GARLAND E. ALLEN.
On the Essence of Causality, HORZ ET AL., LESTER TALKINGTON.
Science and Marxism in England, 1943-1945, ROBERT E. FILNER.
Back issues, $3 per copy. Address on inside front cover.

On the Philosphical Fronts

SOCIOBIOLOGY AND RACISM

Steven Rose (*Nature* 289: 335; 1981) pointed out that concepts of genetic determinism from sociobiology are being used by neo-fascists of France and Great Britain to justify racist and chauvinist themes. "Opponents of sociobiology . . . are characerterized as 'Marxist' and 'Jewish'," noted Rose, suggesting that it would be in the public interest that British sociobiologists John Maynard Smith and Richard Dawkins "should clearly dissociate themselves from the use of their names in support of this neo-Nazi balderdash."

Smith (289: 742) responded forthrightly that there is nothing in modern evolutionary biology lending support to "their view that our genetic constitution makes it impossible for us to live in a racially integrated society." Dawkins (289: 528) replied with a narrow denial and a defense of his "selfish gene" concept, from which he launched into a lengthy attack on Rose concerning other issues.

Edward O. Wilson (289: 627) entered the discussion to state that "no justification for racism is to be found in the truly scientific study of the biological basis of social behaviour," but hastened to qualify his position: "If there is a possible hereditary tendency to acquire xenophobia and nationalist feelings, it is a *non sequitur* to interpret such a hypothesis as an argument in favour of racist ideology."

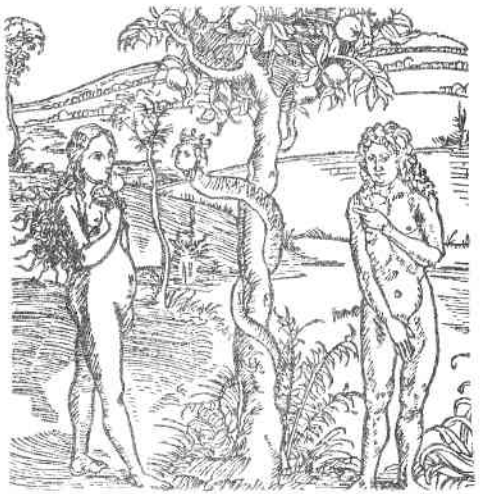
In rebuttal, Rose (290: 356, 432) pointed out that the books of Dawkins and Wilson are "so amenable to neo-Nazi and New Right ideology" because they first "set out the inexorable destiny of genetic predispositions to xenophobia, aggression, patriarchy, or whatever," and only later invoke the "possibility" that humans can overcome these dispositions.

J.R.G. Turner (291: 374) attacked Rose and radical ideology, even dragging in the Afghanistan issue. A rejoinder from Rose (292: 286) may or may not be the end of the matter. [Our own Isadore Nabi joined the fray at one point to comment ironically on the inconsistencies of Dawkins and Wilson, which led to questions about Nabi's identity and culminated in a *Nature* editorial that denounced Nabi rather than racism. See note following Nabi's paper, this issue.]

On the Usefulness of the Marxist Outlook

I and my students in the Worker's University of Paris have discussed biology every week. With them I have learned much; at least as much as I have taught them. If one thing has impressed me greatly it is the ease and accuracy with which a good Marxist can handle a scientific question which is quite new to him, putting forward the right objection, stating the problem with precision, placing it in its proper context.
—Marcel Prenant, Biology and Marxism. New York, International 1943.

Did Adam Eat a Different Kind of Apple?



In analyzing the animal kingdom, a new awareness of the role of females is emerging as researchers challenge stereotyped theories once accepted as fact. One reason more data about female behavior is emerging is because there are now female scientists who are able to analyze data. Take the National Zoo's male bongo, for example.

Dr. Katherine Ralls, 38, a researcher at the National Zoo and 1965 graduate of Harvard, observed the female bongo last year trying to get the male interested in mating. "She would jump around, wiggle her rump, nudge him, trying to get him interested," she said. "She would prance away and if he wouldn't chase her, she would come back, lick him, nudge him and try to get him to chase her." This has recently be described as "proceptive" behavior, which Ralls thinks is nature's fail-safe method of getting an unenthusiastic bongo male to mate, thus increasing the chances of survival of the species.

However, when Ralls had to leave the zoo for a couple of days, several keepers and curators who were watching the bongo pair's activity failed to mention the female's behavior to Ralls when she returned, instead focusing on the male's activity. But a female keeper later told Ralls the female bongo was brazenly soliciting the male to mate. It was clear that human males watched from a different perspective than human females.

"It's true in anthropology," said Ralls, "and that's why they try to use male-female teams. A village may look one way to a male and another way to a female. It's the same when you look at animal behavior." In the past, most of the research was by men and the resulting scientific papers were authored by men. "If you write something from a female bias, it really sticks out," said Ralls, "but if someone writes from a male bias, it's hard to pick it out unless you are sensitized to it. We all try to be objective but we don't quite make it. We're human." —Thomas Crosby, *Washington Star*, 27 Feb. 1978.

ON THE ROLE OF IDEOLOGY IN THE NATURAL SCIENCES*

There is a great deal of confusion about how ideology functions in natural science. Part of the confusion arises because the term ideology is used in many different ways. We will therefore first consider the scientific meaning of the term as Marxists use it:

> By ideology we mean a system of generalized concepts of the world as a whole, of the natural and social processes within it, and of man's attitude to the surrounding reality . . . Ideology expresses and orients human consciousness within the system of social relations and natural interconnections, and provides a set of initial values and tenets which influence the behavior and way of life of social classes, groups and individuals. The concepts and ideas which make up an ideology become a man's convictions and take an active part in shaping his attitude to all the vital phenomena and events in the world . . . One could say that [ideology] is a unity of individual and social consciousness . . . the individual's consciousness, while retaining its characteristics, links up with social consciousness and in a sense expresses it. [Pyotr Fedoseyev, *World Marxist Review*, Dec. 78.]

In this general sense, there can be not only ideologies characteristic of, say, the bourgeoisie or the working class, but there can also be an ideology that is specific to the group we know as the scientific community, or to some particular discipline within that community. Moreover, the term need not signify a false consciousness or distorted awareness of reality though, of course, distortion will always be creeping in and the amount that stays will depend on the corrective processes built into the particular ideology. For example, there is development even in the ideology of capitalism conforming to its needs at various historical stages. We should expect the same to happen in the ideology of science, as A.D. Aleksandrov points out:

> In short, the objective contents of a science are always presented in one ideological form or another; the unity and struggle of this dialectical opposition -- objective content and ideological form — play, in the development of . . . every science, a role which is by no means small. — *Science and Nature* No. 3.

Another source of confusion is the tendency to think that ideology only enters science from without. The fact is that scientists generate their own ideology; it is the manner and content of thinking that characterizes a given scientific community, that reflects the social existence of its members *as scientists.*

Engels gave an example of how such an ideology can operate. Discussing Sadi Carnot's prophetic study of the steam engine, Engels pointed out that Carnot failed to recognize the mechanical equivalence of heat, not for lack of factual data, but only "because he believed in *caloric*" and, moreover, "this false theory [caloric] was not one which had been forced upon physicists by some variety of malicious philosophy, but was a theory contrived by the physicists themselves, by means of their own naturalistic mode of thought." [*Dialectics of Nature*, New York, 1940, pp. 214, 81-82.] Thus, Carnot failed to recognize the physical implications of his own mathematical theory because his thinking on the subject was so strongly influenced by the ideas or ideology that dominated science in Europe at the time. Engels described the historical process by which this ideology arose:

> In the seventeenth century heat was regarded, at any rate in England, as a property of bodies, as "motion of a particular kind" . . . But in the eighteenth century the view came more and more to the fore that heat, as also light, electricity, and magnetism, is a special substance, and that all the peculiar substances differ from ordinary matter in having no weight, in being imponderable . . . [In this way] caloric theory arose in France and became more or less accepted on the Continent. [*Ibid.* pp. 82, 260.]

Consider the implications of what Engels is telling us. In retrospect we know that the interpretive concept of heat as motion of bodies is at least a much better reflection of reality than the concept of heat as a substance itself. Hence, the ideological framework within which Carnot studied represented something of a regression from ideas that existed earlier. Clearly, we have a case of historically-conditioned ideology at work within the scientific process.

But, you may say, this is an unusual instance, not typical of the way in which science develops normally. And I respond that, if you look at science in its historical development, you'll be surprised how many such ideological flipflops are to be found. Another instance of an interpretive structure being toppled in physics is found in the effort to explain the phenomena of light:

> In the 18th century scientists were quite secure in their knowledge that light consisted of Newtonian corpuscles. In the 19th century the work of Young, Fresnel and Maxwell brought them to an even stronger conviction that light is nothing more than the energy of wave disturbances propagated in a pervasive material medium, the hypothetical ether. In the 20th century, the elaboration of Einstein's theory of light quanta and Bohr's complementary principle has convinced scientists that light is inherently dual in nature, consisting of both wave and particle, depending on the experimental means by which it is observed. My question is whether the prevailing 20th century concept is any closer to the absolute truth than the concepts of previous centuries. [Talkington, *Science and Nature*, No. 2, 1979.]

Note that despite repeated radical changes in the concept of light, there was much continuity in the development of methods for manipulating light. An analogy with political economy is suggested: much of the underlying material basis continues to be useful after a revolution in the ideological superstructure.

In biology, where phenomena can often be defined only qualitatively, the ideological nature of the interpretive elements may be quite transparent to the materialist-minded scientist. The theory of evolution, for example, has its operative basis mainly in systematic comparison of qualitative features in organisms and their fossils, while its interpretive superstructure hinges largely on the concepts of natural selection and survival of the fittest. Marx welcomed the empirical

*Adapted from paper given at Dialectics Workshop, Philosophy of Science Association meeting, Toronto, 18 Oct. 1980.

results of evolutionary theory as a splendid example of historical development in nature, but he was amused by Darwin's *interpretation* of these results in bourgeois terms:

> It is remarkable how Darwin recognizes among beasts and plants his English society with its division of labor, competition, opening up of new markets, "inventions," and the Malthusian "struggle for existence." It is Hobbes' *bellum omnium contra omnes*. And one is reminded of Hegels' *Phenomenology*, where civil society is described as a "spiritual animal kingdom," while in Darwin the animal kingdom figures as civil society. . . —*Selected Correspondence*, Moscow, 1975, p. 120.

Not always amusing are the interpretive echoes of Darwin today in ethology (such as the obvious imperialist ideology of the "territorial imperative") and sociobiology (cf., the sexist concept of the male optimizing "his investment" of sperm).

Now it is time to examine the social process by which such ideological phenomena arise and gain dominance over the thinking of scientific communities, sometimes in their entirety. The essence of an ideology intrinsic to science is that it consists of concepts that are used to interpret or ascribe meaning to empirical results based on objective data. If the theory proves useful on practical problems, then the interpretive concepts also tend to be accepted as objective "facts." But such "facts" introduce contradiction into the theoretical structure. On the one hand, a theoretical system embodies *operative* procedures, telling what the theory constitutes in a material sense and prescribing how to use it to operate on problems. On the other hand, the theory embodies *interpretive* concepts—mainly analogies and metaphoric models—that define the semantic content, telling what the experiment means and what the theory implies for human knowledge. As we have seen above, and as we know from Marxist theory of knowledge, the interpretive elements of a theoretical system, ideological in character, can never fully reflect the underlying reality. This is the basis of the contradiction.

The analogy with political economy becomes clear: we can think of a theoretical system as having a material base defined by its operative elements and an ideological superstructure defined by its interpretive elements. These two aspects represent a true unity of opposites; any theory must of necessity contain both of these contradictory tendencies. The concept of an experiment being "theory laden" reflects how deeply these contradictory aspects interpenetrate the fabric of a theoretical system.

But why is it that scientists cannot just "make do" with the measurements, mathematical equations and rigorous logic, as the operationist school has long maintained? I see two basic reasons why the explanatory use of analogy and metaphoric models is both normal and necessary in the process of articulating a theoretical system. The first and more fundamental consideration is that the dialectical mode of thought requires a search for relationships, for interconnections. Outside of its natural connections, any phenomenon is inexplicable and "irrational." There is a need to understand the new in terms of the old, the strange in terms of the familiar, the unknown in terms of the

known. Analogy and metaphoric model are useful tools when searching for the universal in the particular, for law as a form of universality. Secondly, there is the *social* need for the interpretive elements to facilitate the communication process so vital to scientific progress. Natural-language metaphors and visual images are not only aids for the thought processes of the individual but also necessary aids for communicating these thoughts to other scientists. Similarly, they are aids for the retention and reproduction of abstract generalizations from empiric data. Interpretation gives meaning to a research effort and motivation for others to join it. Without the drive for meaning, there is no science.

But it is certainly true that such an interplay of subject and object can lead to error in the theoretical systems of science. Some people, when they come to realize this dread fact, throw up their hands in despair because they can't believe in science anymore. But the situation does not warrant such lamentation. Let us instead look upon such error as the source of motion and development in science.

We know that, in political economy, contradiction between the base and superstructure generates the primary motive power for development and social change. So also in science, contradiction between the operative and interpretive elements within theory is a source of progress, as Engels noted:

> Are the concepts which prevail in the natural sciences only fictions because they by no means always correspond with reality? From the moment we accept the theory of evolution all our concepts of organic life correspond only approximately to reality. Otherwise there would be no change; on the day when concepts and reality coincide in the organic world, development comes to an end. [*Sel. Correspondence*, p. 459.]

Even more pungent was this concise comment on a *faulty* model:

> By the phlogistic theory, chemistry for the first time emancipated itself from alchemy. [*Dialectics of Nature*, p. 5.]

And Soviet Academician Nikolai N. Semyenov has described the process of a research experience in which a certain concept proved of great help in resolving a theoretical contradiction yet later turned out to be quite irrelevant to the new theory that it had helped to bring into being [*Science and Nature*, No. 1, 1977].

Thus, to accept the concept of error within theory as a source of motion and development implies the dialectical view that theoretical "truth" embodies inner contradiction between its ideological and objective elements. In this view, scientific theory can never represent full reality and search will reveal different aspects of reality under different conditions. Such a view helps to explain why so many scientific debates are ideological in character *and* why controversy is the very life of science. Error leads to truth in the scientific process. As Lenin once put it: "Truth is a process. From the subjective idea, man advances toward objective truth through practice (and technique)." [*Philosophical Notebooks*, p. 201.]

If motion toward scientific truth lies along the path of accepted error, it is not surprising that the journey can be arduous and stormy.

While contradiction is necessary and normal to the research process, it is very easy for layer on layer of interpretive metaphor and partially-founded assumption to become enshrined as dogma in a theoretical system. J.D. Bernal gave the following scenario for the process:

> The progress of science depends on the existence of a continuous traditional picture or working model of the universe, partly verifiable, but also partly mythical, where verifications are delusive or altogether missing. It is, on the other hand, equally essential that the tradition, compounded as it is (and must always be) from elements drawn from both science and society, should be continually and often violently broke down from time to time and remade in the face of new experience in the material and social worlds. [*Science in History*, MIT Press 1971, p. 52.]

Bernal also expressed substantial agreement with "the dialectical view" of science history in Thomas Kuhn's account of scientific revolutions, though noting that Kuhn "largely concentrated on the ideological content of science" [*ibid.*, p. 341]. Soviet academician S.R. Mikulinsky has similarly commented on the fruitfulness of Kuhn's concept of scientific community for explaining "the mechanism of the movement of scientific knowledge," concluding that, since it is defined by the adherence of its members to some paradigm or common theoretical system, the scientific community can be regarded as a kind of collective subject of its scientific activity [*Scientia* 69: 83-97, 1975]. The model I have presented here for the dialectical structure of a theoretical system seems to go a step further than Kuhn, who never quite made it clear even to himself about the relationship between the ideological and the substantive aspects of the paradigm.

What Kuhn did contribute, despite the idealism of some formulations, was an historical approach that yielded concepts of evident usefulness to science. So I am going to close with a plea for more attention to the historically-conditioned character of the ideology that is intrinsic to a theoretical system. In fact, I suggest that even in physics and biology the conscious use of the principles of historical materialism may be useful for studying the inner content of a theoretical system, for studying its historical development as a succession of contradictions and qualitative transformations. Semyenov has given a good example of such a study [*loc. cit.*]. This type of analysis may add a powerful new dimension to the epistemology of natural science. It is not enough to study the historical development of the ideas alone, as some philosophers have done. More instructive by far is the study of a theoretical system as a *historical process* of interaction between its ideological superstructure and its operative material base.

Hank Talkington

P.S.: In the opening of this essay, a reference was made to "corrective processes built into the particular ideology" as the means for getting rid of distortion and error. I certainly had in mind the principles of dialectical and historical materialism as the corrective mechanism built into the Marxist ideology. □

Diffraction Experiment 1945. "At Nagasaki Fortress Command at Minami-Yamata-machi, 3.5 kilometers south-south-east of the hypocenter, soldier on picket duty comes down from the observation post as the 'all clear' is sounded. He unfastens his sword, hooks it onto a clapboard, and is unbuttoning his jacket when he sees the flash." —*Hiroshima-Nagasaki: A Pictorial Record of the Atomic Destruction*, Tokyo 1978. Photo by Matsumoto Eiichi.

The Years of the Child

For the first three billion years of dog eat dog
  not a spiral nebula laughed.
Followed an additional three billion years of cosmic alienation.
  Not one microwave in the Crab Nebula cracked a smile.
Finally one bright green morning a mutation called Archimedes
  joked about changing the world with a lever.
And a one-minute routine of scientific vaudeville began
  called *The World as Good Fun*.
But the real gasser came the next minute when some Pentagonian computer
  thought it was the spiral nebula in Andromeda attacking us.
And the last smile of the last child went into orbit
  and cried silently forever after.

—Walter Lowenfals, in *The Portable Walter*, Robert Gover, editor.