

SCIENCE AND NATURE

*The journal of
Marxist philosophy
for natural scientists*

Reader Feedback	<i>Is science revolutionary? Big Bang cosmology</i> Letters to the Editor	1
Academic Reality	JOHN PAPPADEMOS surveys campus research Militarized Science, Capitalist Crisis	6
Review Essay	GARLAND ALLEN points out the dialectics Ernst Mayr and Biological Thought	21
Philosophy in Biology	RÓZSA VARRÓ on equilibrium / disequilibrium Contradiction within Living Systems	30
Discussion Paper	MAX ROBINSON plus Motz, Talkington Is Quantum Mechanics a Theory?	42
Whither Culture?	IGOR NARSKI sees causality as key problem Can Sciences and Arts Be Integrated?	55
Books and Ideas in Natural Sciences	CLAUDE BRAUN reviews Alexander R. Luria Language, Cognition, Brain Function	60
	GORDON WELTY reviews Charles Woolfson Engels and Labor Theory of Culture	63
	LESTER TALKINGTON reviews Abraham Pais The Unfinished Revolution in Physics	69
	IRVING ADLER reviews Michael Friedman Realism versus Positivism in Relativity	74
	STANLEY JEFFERS reviews Martin Harwit To Formalize Astronomical Discovery?	78
	LLOYD MOTZ reviews I.V. Savelyev Theoretical Physics for Undergraduates	80
	<i>From Maurice Wilkins to assassination buff</i> Nuggets from the Literature	82
Miscellany	5, 17, 19, 41, 49, 54, 59, 66-7, 83, 85	

Our masthead emblem symbolizes the dialectical interpenetration of science and nature, suggesting the manifold interconnections between scientific knowledge, ideal in form, and material nature, reflected in this knowledge.

An independent reader-supported journal addressing the philosophical, historical and sociological problems of the natural sciences, i.e., the physical, biological and formal (mathematical and logical) disciplines. *Editorial priorities:* to demonstrate the usefulness of the Marxist world view in the practice of science and to help develop further the principles of dialectical materialism and Marxist theory of knowledge in their interaction with advancing natural science.

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Highlights of Back Issues

- Issue No. 1** *Nikolai N. Semyenov*, On Intuition and Dialectical Logic
Robert S. Cohen, Karl Marx on Science and Nature
Martin Zwick, The Cusp Catastrophe and Dialectics
- Issue No. 2** *Stephen Jay Gould*, Episodic Change Versus Gradualist Dogma
Shaun Lovejoy, Christopher Caudwell and Thermodynamics
J.D. Bernal, Frederick Engels and Science
- Issue No. 3** *A.D. Aleksandrov*, Mathematics: Its Essential Nature and Laws of Development
Garland E. Allen, Dialectical Materialism in Biology
Hörz et al.; *Lester Talkington*, On the Essence of Causality
Robert E. Filner, Science and Marxism in England, 1930-1945
- Issue No. 4** *Dieter Wittich*, Ludwik Fleck: Genesis and Development of a Scientific Fact
Hubert C. Kennedy, Life and Works of Giuseppe Peano
Lester Talkington, On the Role of Ideology in Natural Science
- Issue No. 5** *David B. Adams*, On Sociobiology and Activism
Elizabeth Fee, Woman's Role in Human Evolution
Irving Adler, On Causality in Quantum Mechanics
Joan Bromberg, When Physicists Served Fascism
Bruno Vitale, How the Neutron Bomb Came to Be

Number 6

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AND
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LETTERS TO THE EDITOR

On Science as an Agent in Social Change

I just finished reading your No. 3 (1980), which I happened to stumble across in a bookstore, and I truly regret not having seen whatever other issues you may have published in the meantime. Enclosed is a check for a subscription to your journal.

I was impressed, in general, with the quality of discussion, but I was disturbed by the apparently favorable quotation (p. 69) of a statement by J.D. Bernal to the effect that science "is the chief agent of change in society." As I understand Marxism-Leninism, not science, but class struggle, is the principal agent of societal change. Both steam power and explosives, for example, were invented in antiquity, but were never put to productive use because chattel slavery made them unnecessary. Their widespread application centuries later was an effect, not a cause, of a qualitative transformation in social conditions: the need of capitalists to maximize profits in the new system of wage slavery. Chattel slavery itself gave way to serfdom not as a result of "technical change, paving the way to economic and social changes," but because slave revolts, *a la* Spartacus and others, made it increasingly, if slowly, clear, that people just wouldn't put up with it any more. I do not mean to be glib about all this—there is certainly a lot here to be debated—but Bernal's error is so fundamental, bordering almost on fetishism, that one cannot let it go by unchallenged.

It is natural that scientists, trained and socialized as a relative elite, would be drawn somewhat toward narcissism, admiring our own reflections in an exaggerated view of our own importance. Dialectical and historical materialism, correctly applied, should help to correct this subjective one-sidedness, rather than reinforce it. Science and scientists have much to contribute, as does the scientific outlook applied to all phenomena, but, in the last analysis, it is the on-going struggle of slaves, peasants, and workers, at their respective times in history, that has really moved society forward.

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EDITOR'S RESPONSE: We can all agree with Steven Cushing that social change comes about through class struggle. In fact, I published the Bernal quote precisely because it ends with what I take to be a call for class struggle: "The obstacles to the solution of the problem [of human well-being] are not any longer mainly physical or biological obstacles; they are social obstacles." In the full text [*Social Function of Science, 1939*, p. 383], Bernal is quite explicit about it: coping with such social obstacles requires a new kind of social science, one that is motivated to understand society *in order to change it*.

But I also had in mind the profound Marxist-Leninist implications of Bernal's preceding statement, to which Cushing objects so strongly. The

full sentence reads: "It [science] is the chief agent of change in society; at first, unconsciously as technical change, paving the way to economic and social changes, and latterly, as a more conscious and direct motive for social change itself." This statement is best understood in terms of the historical materialist concept of society as composed of a Basis and a Superstructure, where the Basis consists of the *economic structure* and the social relations stemming from the organization of production, while the Superstructure consists of the ideological and institutional relations (schools, churches, courts, police, etc.) which help maintain the production relations and determine the prevailing social consciousness. While the Superstructure is built upon and necessarily reflects the underlying Basis, the two do not necessarily develop in synchrony and when they get out of step, the resulting social stresses (contradictions) can create some new potential for social change.

The major way in which natural science helps bring about social change *unconsciously* (to use Bernal's terminology) is through its contributions to technological innovation which in turn produces social dislocations and change. Heretofore, and especially under capitalism, these contributions to social change have been mainly the unplanned and unforeseen results of economic drives, e.g., for profit. In fact, today's general crisis of capitalism reflects in good part the dislocations of production caused by the on-going revolution in science and technology. (While socialism seeks to bring about social change through conscious planning, and much progress has been made in learning to plan a modern industrialized society, still scientific discoveries must also provide unplanned stimulus for change even under socialism!)

Then there is the major way in which natural science helps *consciously* to bring about social change. This is through its effects on the ideas of people and their way of looking at the world, with all the tensions and dislocations which science thus introduces into the Superstructure. In a class-divided society, where the Superstructure must necessarily correspond in the main to the needs of the ruling class, the dominant ideology tends to become rigid and inflexible in the effort to maintain the status quo. To a repressive regime, even rational inquiry into the laws of nature can be threatening. And we don't have to go back to Galileo or Bruno for examples. Voltaire was in constant hot water for propagating Newton's ideas in 18th-century monarchist France. Darwin's concept of evolution raised bourgeois hackles in 19th-century Victorian England. And the concept of determinist causality is evidently quite threatening to 20th-century state monopoly capital and transnational imperialism (looking over its shoulder at the rising tide of socialism). Great scientific ideas, and most great scientists, have always been on the side of liberation.

I wouldn't want to argue about Bernal's exact wording on science as "the chief agent of social change." But those who seek to transform society would do well not to overlook the quiet but persistent changes effected by natural science, and the resulting contradictions which provide new openings for conscious efforts toward social change.

Incidentally, it seems to me one-sided to argue that steam power and explosives were not put into productive use in antiquity only because chattel slavery made them unnecessary. It seems more relevant that the production relations deprived both slaves and their masters of incentive to explore more deeply into technological matters. The same was true of feudal relations. This seems the main reason why modern science could not

arise until after the Renaissance was under way. It took a long process of cultural development to go from the simple steam-actuated devices described by Hero of Alexandria (c. 130 BC) to the technology of steam engines (Savery, 1698; Newcomen, 1705; Watt, 1763 onwards) and thence to the Carnot equation (1824) which laid the basis for the science of thermodynamics (1855).

It is a related fact that slave and serf revolts, even when led by intelligent humanitarians such as Spartacus, have only seldom brought lasting gains to the participants and never succeeded in raising human society to a qualitatively new level. There was nothing in the production relations of slave-owning or feudal systems to equip the exploited for supplanting the old system with something better. In the end, revolts of slaves or serfs have chiefly served to weaken the existing system so that others could bring in a superior new system. The barbarians brought feudalism which was an improvement for the slaves if not their masters. The bourgeois brought capitalism which, with all its brutalities, was an advance over feudalism. But, as Marx and Engels saw so keenly, the modern industrial working class is the first exploited class to gain, from the process of exploitation itself, the knowledge and experience which would enable it to supplant the existing system with one offering advantages for all mankind. And let us not forget that the development of our Marxist social science is based to a great extent on the outlook of the natural sciences that developed in the capitalist era.

I have argued strongly for my interpretation of J.D. Bernal. I do not think his Marxist view of science was fetishistic. Despite all the abuses of science which we witness today under capitalism, science *overall* helps to move mankind forward both materially and mentally. □

What About "Big Bang" Creationism?

At a social gathering last evening an interesting discussion developed around the "big bang" theory of the origin of the universe. I had mentioned reading the fine exposition of Marxist theory by John Somerville in his *The Philosophy of Marxism*, where he writes:

Yet at any particular time something must have existed. By the same reasoning, we reach the conclusion that there always will be something. That is, just as we cannot get something from nothing, so also we cannot get nothing from something, though it may sometimes seem that we do . . . the materialist finds especially vulnerable the famous "Argument from Design," which points out that so complex a thing as the entire natural universe could not come into existence accidentally. Logically, so the argument goes, we must assume a supernatural Designer capable of creating all that complexity. But then, by the same logic, we would have to assume a super-supernatural Designer to account for the supernatural Designer, a super-super-supernatural Designer to account for the super-supernatural Designer, and so on. What we here are really saying (again) is that reason tells us there is a sequence of existences, that later existences must have come from earlier ones, and thus existence must always have existed. The materialist concludes that, if there is no beginning, there is no need for a Beginner. (pp. 9, 11)

The substance of Somerville's argument was raised in discussion. There was present an amateur, but fine, astronomer. He said that he faced a quandary. While accepting Somerville's materialist argument, he faced the problem that outstanding professional astronomers, on the basis of current research, accept the "big bang" theory as to the origin of the uni-

verse. Obviously, there is a conflict here between the materialist concept and the "big bang" theory.

Are there prominent astronomers who do not accept the "big bang" theory? If so, what are their arguments, as I should like to present them to my friend. Also, since science historically has validated the Marxist thesis, this question is an important one. I would appreciate any information that you can provide on this issue.

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EDITOR'S RESPONSE: The question of a "big bang" origin for the universe is certainly important because this model smacks of "creationism" and is used for much mystification of the public. Here are some background facts and some thoughts of my own which may help your astronomer friend to look at the matter in a dialectical materialist fashion.

First, the "big bang" model hinges basically on the concept of an expanding universe which, in turn, rests on the assumption that the observed redshift, varying with distance, is due solely to a Doppler shift from receding motion of the galaxies. This theoretical construction may actually be quite flimsy. On page 379 of *The Cambridge Encyclopedia of Astronomy* there is a mild warning to this effect, though coupled with complacent acceptance:

Of course we should proceed with care. It is possible that the simple interpretation of the redshift is not correct, and that the expansion is illusory . . . No other scientifically acceptable hypothesis has yet been proposed. On the other hand, we have no proof that this is the explanation. We take the simplest course and assume that the cosmic expansion is real.

A second major difficulty with the "big bang" model is its lack of physical plausibility. In fact, this model does not originate in *physical* thinking but instead arises as a *mathematical* invention. In the cosmological model based on Einstein's general theory of relativity there occurs a mathematical "singularity" which has been interpreted to represent a "big bang" origin for the universe. In this mathematical interpretation, *all* the matter of the entire universe has to be piled up initially at a single "point" of infinite density. No one has yet suggested how such a *cosmic singularity* could be physically possible. This paradox is discussed somewhat ambiguously by the young astronomers who compiled the *Cambridge Encyclopedia*:

Perhaps one of the central issues of the Big Bang cosmology is the issue of the cosmic singularity. *IF* our ideas about the nature of matter are correct, and *IF* in addition Einstein's theory is the appropriate framework within which to discuss cosmology, the existence of a singularity in our past is inevitable. A breakdown of the presently-accepted laws of physics is required in order to avoid the conclusion that our Universe evolved from a singular state. [p. 385, emphasis added.]

It seems that, in order to save the laws of physics, the cosmologists (as a community) have persuaded themselves to accept the cosmic singularity though this also defies explanation in terms of "the presently accepted laws of physics." Some additional evidence aided in this persuasion, notably the discovery of a cosmic background radiation which is expected in the "big bang" model (pages 378-387 are worthwhile reading), but the highly implausible cosmic singularity still goes unexplained physically.

Only one alternative model is offered that is singularity free. In 1948 astronomer Thomas Gold proposed, and Sir Fred Hoyle continues to popularize, the concept of a "steady state" universe. From the name, this model might be expected to meet the materialist requirements spelled out by John Somerville, but no such luck. Most astronomers turned thumbs down on this model because the "steady state" could be reconciled with the expanding universe only by postulating the "continuous creation" of new matter out of the void (nothing). This form of "creationism" could not, of course, be justified by the laws of physics, so it was back to the cosmic singularity. And that's where matters now stand. Very soft science. Like cotton candy at the State Fair, cosmology today is sweet to the taste, but has little substance.

From the Marxist point of view, it is necessary to remember that all knowledge is relative, that all data are subject to more than one interpretation, and the future may bring us entirely new insights on the physics of the redshift and the proper mathematical model for representing the historical development of the universe. Such considerations are realistic, considering the history of science to date, and they provide good scientific reasons for being skeptical about the "big bang" with its cosmic singularity, in addition to the sound philosophical reasons advanced by John Somerville.

In the meantime, it would seem much healthier for cosmologists to stress how much we *don't know* about these matters. The contrary tendency, to ignore our ignorance while erecting elaborate theoretical structures on the shifting sands of dubious premises, may be explained in great part by the way in which the media will play up any mystification, especially if it wears a "scientific" garb. Scientists, as we know, are only too human, and not necessarily aware when they help to smuggle an alien ideology into natural science. Perhaps it would help your astronomer friend to read my brief essay "On the Role of Ideology in the Natural Sciences" (*S&N* #4, 84-88). □

A Clash of Theologies — — — — —

Why did consensus opinion in 1965 adopt the big-bang? Why, with no proof at all, did big-bang adherents present their theory as well proven? I puzzled deeply on this, until I turned to biology. The word *evolution* has mystic significance for biologists. If you say "x evolves into y," everybody believes it without inquiry, no matter how absurd the proposition. Another fuddled notion is that life began here on earth in a thin brew of organic material. How can grown people be persuaded to such beliefs? The explanation, I now see, lies not in science but in sociology. Consensus opinion on all these issues follows closely the tenets of the Judeo-Christian religion.

Whether the universe originated ten billion years ago or in 4004 BC, the theological implications are the same. On a par with biblical miracles, we have the unexplained condensation of the galaxies, the unexplained origin of life, and the ultimate miracle—"Darwinian" evolution. The real conflict between science and religion has been only a struggle over which will get the support of the State.

Consider now the steady-state theory of a universe with no beginning in time, thus no support for a Judeo-Christian creation by an external God. The steady-state universe contains within itself its own perfection, its own divinity, one might say. Classicists will recognize this cosmology as in essence an expression of Greek theology.

—Sir Fred Hoyle, condensed from *The Sciences*, Nov. 1982, p. 11.

*"Only the working class can convert science from an instrument of class rule into a popular force. . . . Science can play its genuine part in the Republic of Labor."*¹

The vision of Marx and Engels, based on their understanding of the inner contradictions of the capitalist system, led them to contrast the roles of science under capitalism and under socialism in the above prophetic statement which, in the context of contemporary developments in U.S. science, has a most urgent meaning for today. Instead of social progress, we have greater and greater unemployment, racism, poverty, and the threat of nuclear annihilation for humanity as the fruits of the misuse of science in the U.S.

In the present advanced structural crisis of U.S. capitalism, science has reached a qualitatively new stage, becoming directly subordinated to monopoly-corporate interests and the Pentagon's aim of achieving first-strike capability against the USSR.² By conservative estimate, as we shall see, nearly two-thirds of the nation's scientific resources are devoted to war-related research and development. University administrations across the nation have become eager partners in the corporate takeover of campus research; state and local officials pave the way by offering tax benefits to the corporations and subsidies from public funds. But, in a characteristically contradictory fashion, these trends hamper science's further progress in a number of ways, among the most important being the distortion of the structure of R&D by corporate and military interests. Another important brake on science's progress is the crisis of public education.

This paper will examine some numbers that reveal the accelerating process of corporate-military takeover and the crisis of higher education, explore the consequences thereof, and conclude on a more upbeat note by looking at the potentials of response by the working class and its allies.

I. The Accelerating Militarization of U.S. Science

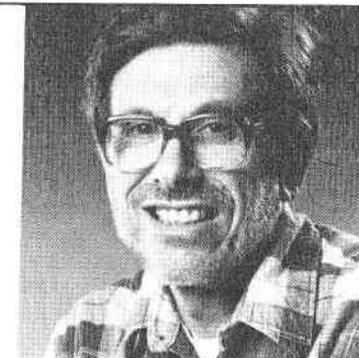
Under Presidents Carter and Reagan, the portion of the federal budget devoted to "defense" has continually increased with the willing acquiescence of both big-business parties. In December 1982 Congress approved a FY1983 Pentagon budget of \$239 billion, 13% higher than the previous year. And in August 1983, Congress voted a "defense" budget authority of nearly \$269 billion. The struggle to cut this enormous military budget and to divert more of the nation's resources to people's needs is growing by the day, involving demonstrations of literally millions. What is perhaps not fully appreciated by many is the extent to which our nation's scientific activity has become subordinated to the interests of the military-industrial complex. The reason for this lack of knowledge, even (or perhaps we should say *especially*) among those who get their information from the *New York Times*, journals like *Science* (the American Association for the Advancement of Science weekly), or *Chemical & Engineering News*, is that these "liberal" media reports are based on the data of the National Science Foundation, which is seriously distorted, as we shall see. Officially, the military portion of our nation's R&D spending declined from 41% of the total R&D in 1963 to a low of 23% in 1980, rising slightly to 27% in 1983.^{3,4} But these data hide the true size of the outlay on military R&D in at least two ways. First, the "defense" R&D figure includes only R&D spending by the Department of Defense (DOD), whereas there are at

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Militarized Science and the Crisis of Capitalism Today

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least two other agencies that carry out significant amounts of war-related R&D. One is the Department of Energy (DOE), which is responsible for the production of all nuclear materials and warheads for the Pentagon, as well as most testing of nuclear weapons. It is estimated that over 40% of the DOE budget goes directly for military purposes;⁵ we will adopt this figure in our calculations below. Another federal agency with a very significant fraction of military spending is the National Aeronautics and Space Administration (NASA). The Space Shuttle, which figures importantly in Pentagon planning, alone accounted for 36% of the NASA budget in 1982.⁶ According to Hans Mark (Deputy Administrator of NASA), more than half of the space effort is military.⁷ We will conservatively use the figure of 40% (same as in the case of the DOE) for the military part of NASA R&D.

A second factor hides the true extent of government financing of war-related R&D. Company funding for industrial R&D (i.e., R&D performed by corporations and not, supposedly, billed to the government) is known to be indirectly paid for by the government by being included as overhead charges on federal contracts. Giant corporations with the large "defense" contracts do most of the R&D in industry, and the government eventually pays for it as overhead. This can be inferred from the distribution of federal contracts for R&D performance by industry: in 1980, for example, just three industries, all of which included big "defense" contractors, took 79% of the federal money for R&D performance. The three were chemicals and allied products, electrical equipment, and aircraft and missiles. The same three industries did 41% of all company-funded industrial R&D that year.⁸ In 1980, the DOD alone funded 62.6% of all federally-funded industrial R&D; by 1982, the DOD share had climbed to 70.4%.⁹ As long ago as 1963, the National Engineers Joint Council noted this phenomenon in calling attention to the nation's civilian research needs:

Present system of allocating resources to U.S. R&D is producing imbalance . . . non-defense agencies in government do not have adequate research programs relating to problems in civilian sector of the economy . . . R&D by private industry is influenced heavily by government allocations.¹⁰

As far back as 1966, Nieburg¹¹ estimated that two thirds of the corporate funded R&D is ultimately passed on to the government as overhead

on federal contracts. We will adopt the figure of two thirds as a conservative estimate in our calculations; i.e., of every dollar of "corporate-funded" industrial R&D, 67¢ is war-related, and ultimately paid for by the government.

Using the above figures for purpose of estimation, we have taken another look at the NSF data on R&D to get a more credible picture of the trend in the federal outlay, both direct and indirect, on military-related R&D. The results are given in Table I. They show that a staggering 59.6% of the nation's scientific research and development was devoted to military purposes in 1980, since rising to 62.0% in 1982. It should be emphasized that this is a conservative estimate. Not taken into account in the estimate is the "leveraging" effect of campus military R&D spending, which, at the state universities, puts a not insignificant portion of state government appropriations at the service of the Pentagon (see Section III). Also not taken into account is the NSF funding that is heavily influenced by military priorities, which is very difficult to estimate. A "guesstimate" would put it at least 10% of the FY1982 \$1.04 billion NSF spending. The changing role of NSF is discussed in Section II. Money alone does not tell the full story of how the creative scientific resources of our society are being utilized. For example, it is conceivable that a lot of very mediocre people might be receiving this enormous subsidy. However, with such resources at their disposal, the military research laboratories of the nation are in a position to offer very attractive financial inducements to attract the top scientific talent—and it is well known that they do so.

A deeper appreciation of the extent of the military takeover of science can be gained from an examination of several trends in scientific research on the campuses of our nation. This we will do in the following section.

TABLE I. Basis for estimating the U.S. funding of military R&D. See text for details of estimating method.

Source of funding (in billion \$)	1980	1981	1982 ^c
R&D FUNDING FOR ALL PURPOSES			
By Federal Government	29.7	33.8	36.6
By industry	30.8	35.9	40.0
By universities and colleges	1.3	1.5	1.5
By other non-profit institutions	0.9	1.0	0.9
TOTAL FOR ALL PURPOSES	62.7	72.1	79.0
R&D FUNDING FOR MILITARY ALONE			
100% of DOD funding	13.5	15.8	18.8
40% of DOE funding	1.9	2.0 ^c	1.9
40% of NASA funding	1.9	2.1	2.3
66% of corporate funding	20.1	23.3	26.0
TOTAL FOR MILITARY (ESTIMATE)	37.4	43.2	49.0
MILITARY, AS PERCENT OF ALL R&D	59.6%	59.9%	62.0%

^cEstimated.

Sources: NSF 82-326, NSF 82-321, Chem. & Engng. News, 25 July 1983.

II. New Trends in R&D at Universities & Colleges

The increasing militarization of science in the nation generally is reflected on the university campuses, where most of the country's basic research is done. The ideal, never lived up to in practice, was that scientific inquiry on the campuses is conducted in an atmosphere of academic freedom. But those who hold the purse strings have, in fact, a great deal to say about what directions of research to pursue. And today, the most active directions of research are, to an increasing extent, in areas that have potential or actual military applications. This military takeover, which we shall demonstrate, is not un-related to an economic crisis of unprecedented dimensions in our nation's universities. Table II shows the amounts contributed by the various sources to university R&D funding over the decade 1970-1980. The increase from \$1.8 billion in 1970 to \$4.0 billion in 1980 for the basic research funding may seem impressive until inflation is allowed for; in real terms, basic research funding from the federal government increased only 1% per year in that decade.¹² Capital expenditures for scientific and engineering activities showed no growth at all, and in fact declined 8% per year in real dollars.¹³ The academic R&D plant support by the federal government in 1979 was only *one tenth* of its 1965 value in real-dollar terms!¹³ Even the president of IBM is alarmed by

TABLE II. Trends in R&D funding at doctorate-granting institutions.

Fiscal years	1970	1975	1980
Total expenditures, millions of dollars	2286	3334	5952
<i>Total expenditures, in 1970 dollars</i>	<i>2286</i>	<i>2422</i>	<i>3044</i>
BY SOURCE OF FUNDS, percent of total			
Federal government	70.7	67.1	67.7
State and local governments	09.4	09.8	08.1
Industry	02.6	03.3	03.9
Institutional funds	10.4	12.3	13.6
All other sources	07.0	07.6	06.7
BY TYPE OF PROJECT, percent of total			
Basic research	77.4	71.0	66.6
Applied research and development	22.6	29.0	33.4
BY FIELD OF WORK, percent of total			
Engineering	13.6	11.3	14.4
Physical sciences	13.0	10.2	11.1
Environmental sciences	05.3	07.4	08.3
Math/computer sciences	03.1	02.5	03.1
Life sciences (incl. biomedical)	51.7	56.4	53.4
Psychology	02.5	02.2	01.8
Social sciences	07.1	07.2	05.4
Other sciences	03.7	02.9	02.4

Sources: NSF 82-319, p. 54; Economic Indicators, June 1978 and Feb. 1983.

the antiquated state of university and college science and engineering laboratories, pointing out that it would take from one to four billion dollars to bring their equipment up to industrial standards.¹⁴

For state university students, the past decade has seen rapidly increasing tuition along with real-dollar declines in state appropriations. In Illinois, for example, operations expenditures per FTE public university student have declined 20% in real dollar terms in the period 1970-1980.¹⁵

The effect of this economic crisis on university research in the U.S. has been a sharp shift in emphasis away from basic research and toward applied research and development, both because of the easier availability of corporate and government funding for applied R&D, and because of the greater possibility of short-term economic gains for the universities from patent licensing, etc. Table II presents NSF figures showing that whereas basic research expenditures accounted for 77% of the 1970 total university R&D, by 1980 the basic research share had dropped to 67%. (The drop in the proportion of basic research on campus is probably greater than indicated by the NSF figures because much DOD-sponsored "basic" research is hardly basic, as will be discussed later.) Basic research support in the U.S. has dropped by 21% as a portion of the GNP from 0.38% in 1968 to 0.3% in 1981.¹⁶ But see Addendum herewith which discusses FY1985 R&D budget.

Another effect of the crisis of the universities (not unrelated to the above) has been the eagerness of university administrators to open wide the doors to military research on the campus. The role of DOD, DOE, and NASA (the three agencies with the most obvious military interests) is escalating rapidly on campus. Whereas in 1979 their total R&D funding amounted to \$922 million approximately,¹⁷ the period mid-1980 to mid-1981 saw \$1,158 million awarded in R&D contracts to 250 campuses around the country from the three agencies, an increase of 26%.¹⁸ Since the campus antiwar movements of the 1970's curtailed their open activity at a number of campuses, the military have been becoming ever bolder in enlisting university science to their needs. For instance, some new faces put in their appearance at the November, 1982 Minnesota meeting of the Society for Neural Research. These were the DOD representatives, present to invite grant applications for those working in areas of military interest such as the effects of nerve-gas ingredients like organo-phosphates.

Is it possible that the DOD, DOE, or NASA will fund basic research on the campus with no military applications in mind? That seems hardly likely, given their military-oriented missions. But, one might still argue, isn't basic research advanced even when funded by military-oriented agencies? The answer to this is again no; by definition, research funded by the DOD (and, to a large extent, DOE and NASA) cannot be really basic. The structure and thrust of any such research program must be seriously distorted; any senior investigator knows that the renewal of funding for his/her program, or the application for funds for a new program, is subject to the scrutiny of officials with a vital interest in potential military applications. Whether the research is classified or not makes little difference. In fact, it improves DOD's image to reduce the amount of classified research on campus; more sophisticated methods tend to be employed to get the job done.

Table III shows the NSF version of the increasing military role in campus "basic" research. Even the NSF acknowledges that in FY1982, the DOD supplied 41.2% of all federal funds for mathematics/computer sci-

ences research, and 38.1% for engineering.¹⁹ In the financial environment on campus today, a very small amount of research support can have an important influence on the structure of university research; in areas like computer science and engineering, the effect is decisive.²⁰ However, the NSF claims that the military (DOD) share of total federal funding for university R&D was only 11.5% (1982). Recalling that the NSF figures completely distort the true extent of the militarization of national R&D generally, we examined data for the University of Illinois, a major research institution. From mid-1980 to mid-1981, all campuses of the University of Illinois (UI) received federal research contracts totalling \$86.6 million; of this amount, \$19.6 million, or 23%, came either from DOD, DOE, or NASA. The total expenditures listed in the comptroller's financial report under "research" in the same period was \$104.9 million, so the federal contracts came to over 82% of the research outlay.²¹ It is not easy to disentangle the DOE and NASA contracts with direct military applications from those that have none, even when the data is available (at the UI Urbana campus it is not), but, as discussed previously, it is safe to assume that a sizable fraction are awarded with military applications in mind, even though they may be nonclassified. At least one federal granting agency (NASA) now requests that every grant proposal be accompanied by a letter certifying that only those faculty, students, and staff who are U.S. citizens will be employed on the project. This project has provoked some resistance from top-level university administrators, who are concerned because it applies to unclassified as well as classified research.²² (The extent of classified research activity at UI seems to be itself classified.) Thus an examination of data for research support at UI reveals a much higher level of military involvement at a major research campus than the NSF data would suggest. That the proportion of war-related research rose still higher in the period mid-1981 to mid-1982, there can be no doubt, although UI has stopped making the Comptroller's report easily

TABLE III. DOD funding of basic research on campus.

Federal obligations to universities and colleges	1980		1982 estimate	
	Federal total	DOD share	Federal total	DOD share
Total, in million \$	\$2291	9.1%	\$2740	11.5%
Life sciences	1219	1.3%	1429	1.5%
Physical sciences	376	10.1%	483	12.5%
Environmental sciences	255	18.0%	296	19.8%
Engineering	208	33.9%	289	38.1%
Mathematics and computer science	80	35.4%	116	41.2%
Psychology	52	17.7%	55	24.9%
Social sciences	64	1.2%	49	2.5%
Other sciences	38	—	22	0.8%

Fiscal years. Source: NSF 82-321, p. 17.

accessible to the public. Figures for the UI Chicago campus alone show that DOD-DOE-NASA awarded \$2.8 million in grants to researchers at the Chicago campus (excluding grants to the Health Sciences Center and two DOE grants not war-related) in the period March, 1982 to March, 1983. This was 25.2% of all external research funding. The FY1983 national budget continued the upward trend in the DOD-DOE-NASA share of government campus research funding, whereas the budgets for NSF and NIH, the other two main supporters of basic research, suffered real-dollar declines of 3.8% and 7.8% respectively.²³

One of the important federal agencies for basic research funding is the National Science Foundation (NSF). Unlike DOE, NASA, and DOD, it might appear that NSF would be devoted to "pure" science, with no connection to military applications. However, it is worthwhile considering the fact that the chairman of the National Science Board (policy-making body for NSF) is and has been a man named Lewis Branscombe, who is a chief scientist for IBM, a company with large war-related contracts. And just recently, the scales at NSF seem to be tilting even more heavily in the direction of the military. The first casualty after Reagan took office was NSF's Director, John Slaughter, a black scientist. Appointed Director in 1980, he left after 18 months, leaving Edward A. Knapp in charge (in November 1982, Ronald Reagan made Knapp full Director). Knapp came to NSF from Los Alamos National Laboratory, a DOE unit charged with responsibility for nuclear weapons development. While at Los Alamos, Dr. Knapp did consulting for at least one corporation (Science Applications, Inc.), furnishing technical services for "national security applications."²⁴ One of Knapp's first acts was to fire two senior administrators, leaving the way open for Reagan to appoint all four top officials at NSF.

President Reagan's science advisor, George Keyworth, also came from Los Alamos. Under Keyworth's influence, the FY1983 basic research budget priorities are, in addition to defense, the physical sciences and engineering, an emphasis which was vigorously protested by the National Academy of Sciences.²⁵ Even before Reagan, the emphasis on government support of the physical sciences, which have potential for direct military applications, had resulted in a declining tendency for research in social sciences for several years: federal funding for social sciences R&D at doctorate-granting institutions decreased from 6% of total R&D in 1970 to 4.3% of total R&D in 1980.²⁶

The indirect effects of the burgeoning militarization of campus research need to be explored in depth. One of them is a profound influence on the faculty reward system; tenure and promotions will tend to go to those who will play the Pentagon's game in order to get contracts. Most often the senior investigators of research contracts hold important departmental positions, surrounded by a circle of younger faculty who must work in related areas in order to get tenure. It is undeniable that another effect must be a stifling of political freedoms within the departments that depend on military-related research. In this connection, it is interesting that universities doing DOD work, even though it is unclassified, sometimes do not permit their own students or faculty access to lists of the names of contract proposals. This is true at UI, for instance, as well as at the University of Michigan.²⁷

Another and to some extent overlapping trend in campus R&D which has emerged within the past several years is its "corporatization," to be discussed in the following section.

III. The Corporatization of University Research

The amount of corporate-sponsored research on U.S. campuses is not particularly large; it amounted to 4.0% of total R&D funding at U.S. universities and colleges in 1982 according to one NSF estimate (see Chart I), or perhaps as much as 6-7% according to an NSF estimate taking unreported corporate research support into account.²⁸ Neither is corporate research funding new; in 1953 it paid for 7.5% of total university R&D. As federal support for campus research rapidly increased during the sixties, the corporate share declined to a low of 2.6% in 1970. The proportion of corporate-sponsored R&D has, however, shown an increasing trend since around 1970 (Chart I), and is marked by a significantly greater degree of corporate control, especially since about 1978. In the words of a dean for research development at UI, "It's a whole new ball game." With the passage of new tax benefits for corporations supporting university-performed research as embodied in the 1981 tax legislation, and the financial crisis of most universities, an intensely competitive atmosphere has arisen among universities seeking to get such corporate giants as Monsanto, Hewlett-Packard, DuPont, Honeywell, IBM, etc., to fund university research projects. Universities are revising their patent control policies to make it possible for corporate research sponsors to realize the profits from inventions and innovations discovered by the university faculty. In some cases the corporate sponsors are getting royalty-free licenses to exploit the results of the research, as is the case with the new MIT-Exxon agreement,²⁹ or even getting outright ownership of resulting patents, as is the case with the Westinghouse agreement with Carnegie-Mellon's Robotics Institute.³⁰

Multinationals have in effect moved right on campus, building new labs or occupying existing facilities. For example, at Stanford, a new facility called the Center for Integrated Systems (CIS) is being set up with \$12 million from industry and \$8 million from DOD. Many of the arrangements being worked out include provisions for corporate employee-scientists to spend extended periods working in such facilities alongside university faculty researchers.³¹ At CIS, a committee of CIS sponsors (which include GE, IBM, United Technologies, and about 14 other giants of the electronics industry) advises on policy and programs at the facility. Half the projects being transferred to CIS are directly sponsored by DOD. The construction of a building for CIS should be completed in late 1983.³¹

Frequently, state and city governments are dipping into their depleted treasuries to help finance the corporate takeovers. At the UI Urbana cam-

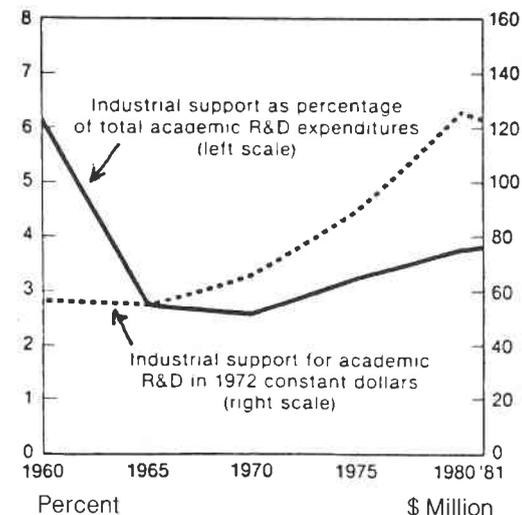


CHART I. Two ways of looking at the university-industry connection

Source: NSF 81-311, and Science Indicators 1980.

pus, for example, an \$8.25 million center for design and testing of microchip technology for Tandem Computers, Gould Industries, and Zenith is being built with over \$5 million of state money. And in Chicago, the city is floating an \$8 million bond issue to help build a \$10 million pilot plant for a biotechnology corporation. This is to be located in a "research park" on Chicago's near West Side, the first building of which is being leased with a million dollars of state money. It will house corporate biotechnology labs in easy communication with scientists at the UI Medical Center. The arrangements with the scientists involved have not been made public, but it is safe to assume that the scientific talent at the university has already been hired through some sort of contractual arrangement. At the present time, the general pattern is for academic scientists to accept equity positions or exclusive consulting arrangements with small biotechnology companies, of which there are now nearly 200 across the country. Most of the nation's leading researchers in molecular genetics and affiliated disciplines are known to have concluded such arrangements.²⁹ It is expected, however, that most of these small companies will be acquired or driven out by the giants such as DuPont, Upjohn, Eli Lilly, etc.

The corporations that are moving into academic research in a big way are not always American. For example, Hoechst AC, a German-based multinational pharmaceutical company, has concluded a \$70 million agreement with Harvard-affiliated Massachusetts General Hospital to establish a new Department of Molecular Biology. The benefits of the research will go largely to Hoechst through an exclusive licensing arrangement.³²

What are some of the features of the new legislation which has provided incentives for corporations and universities to intensify their connections? One has to do with a new patent policy. With the passage of the Uniform Federal Patent Policy Act of 1980, universities, non-profit firms and small businesses can elect to take title to inventions arising from federally funded R&D activities. (The Reagan administration is pushing for this to be extended to all federal contractors.) Another important development was the passage of the Reagan-sponsored Economic Recovery Tax Act of 1981, which gives companies a 25% tax credit for increasing their R&D outlays, whether for direct in-house research or for contracted-out research.³³ Thus the taxpayer is forced to subsidize the research whose benefits enhance capitalist profits.

There is another way in which the burden of subsidizing corporate research is put on the shoulders of the taxpayers, without them having any democratic control over its direction or a share in its benefits. Here we are referring to the "leveraging" effect of the corporate dollars invested in campus research. This applies as well to the federally-funded R&D contracts. For every dollar of research contract money, universities recover on the average only about 33¢ of indirect costs; i.e., an average overhead rate of 33%. (This is to be contrasted with the typical overhead rate of around 200% for industrial or privately-operated research centers.) The remaining 67¢ must be spent strictly on the research project which has been mutually agreed upon. This is a trivial amount in comparison with the cost of maintaining the university buildings, the upkeep of the libraries, paying faculty and staff salaries, etc. Most faculty involved in research spend at least half time on their research projects; some as much as full time. In this way the granting agency or corporation gets the services of a good fraction of the university faculty and staff without having to

pay their salaries. Much of this "leveraged" research outlay is hidden and not listed as "research" in university comptrollers' reports; for example, at UI most salaries (except for summer research appointments) are listed under "instructional costs." Thus an important part of the research expenditure is not even listed as research, which of course has important implications for the reliability of the NSF figures on academic research expenditures.

To the author's knowledge, no study has ever been made of the amount of this "leveraged" research expenditure in a single university department, let alone nationwide. Without access to university financial records, it would be very difficult. A rough estimate for the Physics Department at UI Chicago for fiscal year 1981 showed that the amount thus "leveraged" on DOD-NASA research was around one-fourth the expenditures charged to DOD-NASA grants (roughly one million dollars). Thus, other sources of income had to pay for some \$250,000 of DOD-NASA research costs; mainly state appropriations and charges to students for tuition, fees, housing and meals, sources that together make up 65% of total UI revenues—\$475 million of a total \$730 million in FY1981. (The estimate for the Physics Department took into account the DOD/NASA research-allocated portion of salaries for faculty, technical support staff, and non-academic support: clerical, maintenance, security, etc. Also estimated were the supplies, materials and equipment not charged to grants, library purchases and staffing, telephones, heat, light and electric power, building depreciation and maintenance. Figures for expenditures charged to grants, and for revenues, came from UI Comptroller's Report for FY1981.²¹

With the stagnation in the levels of federal research support, and real-dollar declines in state appropriations, state university administrations will go to almost any lengths to get federal and corporate research contracts, and to pressure their faculty to pursue lines of research that will appeal to the profit motive of corporate boardrooms or the granting agencies in Washington. It is ironic that one hears of cases like the University of California, which recently warned several faculty for getting too heavily involved with profit-making enterprises.³⁴ It is as though they want their faculty to put their creative abilities at the service of capital, but not to reap the financial rewards that may ensue from their work. As noted by Nikolaev,³⁵ the position of the research scientists in industrial labs today, in essence, is no different from any other exploited worker, since the product of his or her work is appropriated by capital. We now see this taking on a new dimension as capital makes its move into the academic scene.

IV. The Growing Racist and Elitist Trend in Higher Education

It is hardly surprising that the militarization and corporate takeover of higher education is accompanied by a massive racist onslaught which threatens to wipe out almost overnight the gains won in the sixties and seventies by Blacks, Latinos, Native Americans and poor whites. The past decade saw a 12% drop in college students from poverty-level families while the actual number of families earning less than \$7,500 per year increased in that period. From 1974 to 1981 there was a 30% drop in the number of black students in the U.S. receiving financial aid (mainly loans) but no decline of whites receiving financial aid.³⁶ As if the rapidly rising tuition weren't enough, admissions to science, engineering and business management programs are being restricted by raising minimum ACT or

SAT scores for entering freshmen.³⁷ At UI Chicago, for example, the Colleges of Engineering and Business both have special admissions requirements based on ACT scores. As a result of an increase in these requirements, the number of entering freshmen in the College of Engineering in Fall 1983 was 20% lower than the previous year; the relative drop in the number of Black engineering students (freshmen through senior) was over twice as great (42%). Throughout the UI Chicago campus as a whole, the number of Black students declined from 17.7% of the total student body in 1979 to only 11.3% in 1983 (fall quarter data in each case).³⁸

The vitally needed academic support programs at UI Chicago (the tutoring service of the Confederation of Latin American Students and the Educational Assistance Program) have been all but eliminated. Because of administrative unconcern with establishing academic support programs at UIC, the “survival rate” for black engineering students at UIC is currently running only about 10%, while the overall rate is not much better; less than 20%.

V. Conclusion

The various trends in science which we have described above—the decline of basic science in the nation, its subordination to the needs of corporate profits and strategic aims of imperialism, the accompanying decline of intellectual freedom of the scientists, increasing exclusion of minorities, etc., are characteristic features of the long-range structural crisis of decaying capitalism. As the scientific and technological revolution has advanced, the objective need for social planning of scientific research for the benefit of all (rather than for the benefit of the multinationals and their global interests) has become an imperative, yet this has proved to be impossible in a capitalist economy. Objectively, the scientific and technological revolution requires an increasing intellectualization of labor, so that science becomes the property of the masses, yet we see the opposite happening, as the level of scientific education has become a national scandal and the universities increasingly take on an elitist character.

This contradiction arises as a result of yet another contradiction: the scientific and technological revolution has and will in the future throw millions of blue-collar and white-collar workers out of their jobs, thereby generating huge drains on unemployment compensation and welfare assistance coupled with losses in tax revenues. Other social programs including those related to public higher education historically have been funded primarily by state governments, which are in financial crisis due to massive unemployment of former tax-revenue generating wage-earners. Thus there is a process set into motion of crippling cuts in higher education which destroy the human potential necessary for progress in science.

Any solution to the crisis that ignores the prevailing struggle between opposing class interests is bound to fail. A widely ballyhooed solution to the present crisis is the “hi-tech hustle,” in which federal, state, and local governments rush to put taxpayer monies into the corporations to prop up their research efforts. But the hi-tech industries create only a handful of jobs for highly-trained workers and cannot overcome the problem of mass unemployment as a permanent feature of our economy.³⁹ Solutions that depend on the militarization of our nation’s science in order to help protect corporate investments abroad cannot but accentuate the decay of our nation’s scientific and industrial potential. American capitalism is caught

Heine 1909
(Germany)
*The Freedom
of the Sciences*



Most senior investigators on war research are surrounded by younger faculty who must work in related areas to get tenure ... another effect must be stifling of political freedoms within departments that depend on militarily-related work.

in a vicious circle from which there is no escape except by redirecting priorities to peace and social needs—and this will only come about by mass struggle. It will come about inevitably as those most hurt by the corporate misrule are objectively forced to struggle; the workers thrown out on the streets by the decline of basic industry, the Black, Hispanic, and Native American peoples hit by racist discrimination that is characteristic of the “hi-tech hustle,” the youth, women, and senior citizens. Furthermore, the class struggle has more than economic dimensions; witness how the corporate misuse of science is provoking a growing outrage on the part of students and scientific workers: examples are the faculty and student actions at Stanford and Berkeley.⁴⁰⁻⁴³

Historically, such struggles have turned out to be the first steps toward truly revolutionary change. Through struggles for such basic democratic needs as jobs, peace and freedom, the people will, and already are, forming coalitions which will put these struggles on a higher plane, one in which the need for a radical restructuring of our economic system—in a word, socialism—will become apparent to millions. Real socialism, which eliminates capitalism, will provide the conditions under which science can *begin* to achieve its promised benefits to the people of our country and the world, to become truly a *popular force* instead of the threat to our wellbe-

ing and even to our existence which it has become in the hands of the capitalist class.

At a time when mankind is threatened with thermonuclear annihilation, scientists cannot afford to concern themselves only with narrow professional problems. In his/her own interest as citizen, and to keep science from being used for oppressive and destructive purposes, each scientist must find a place in the ranks of the vast popular struggles.

ADDENDUM, A Brief Look at the FY1985 R&D Budget

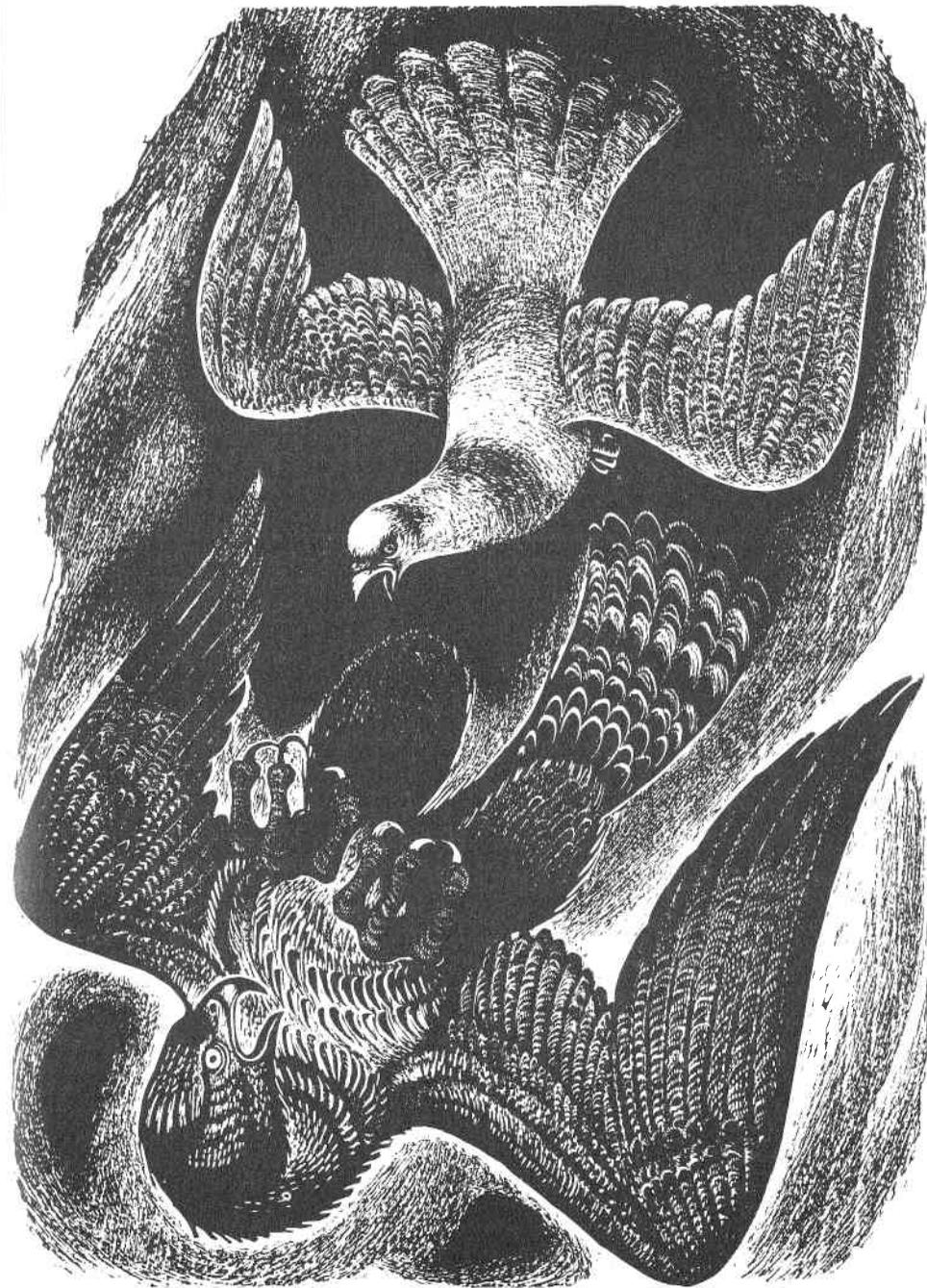
The Administration submitted to Congress on 1 February 1984 an R&D budget calling for \$53.1 billion of R&D funding in FY1985, a 14% increase over current spending levels. This represents a substantially increased commitment to scientific research. But 96% of the spending increase will go for military purposes; in fact, 81.3% of total government R&D support will be spent by DOD and the two agencies with heavy military emphasis, DOE and NASA.⁴⁴ The only other agency getting a large budgeted increase (NSF, with 14% increase) is devoting most of that to the physical sciences and engineering. Overall, Federal R&D support in the physical sciences and engineering is scheduled to rise 14% (in current dollars) while an actual decline (in constant dollars) is projected for life science research support.

Much has been made in the media⁴⁵ recently of a trend toward increasing support for non-military basic research. Supposedly, Federal support for non-military *development* has plummeted since the late 70's, while non-military *basic research* has shown an increase (in constant dollars). This trend certainly warrants closer study by Marxists. In particular, it would be of interest to see which components of basic research are being emphasized, and what fields of development have received the biggest cuts. At the outset, however, it should be clear that: a) because of the increasingly dominant emphasis on military-oriented research, non-military research of *any kind*, basic, applied, or development can hardly be doing all that well; b) those areas of "basic" research that are getting the most emphasis are the physical sciences and engineering, where the spin-offs with military potential are most likely. A good example is "basic" research in excimer lasers (one of the promising areas for development of X-ray lasers). This is a "hot" area because it ties in with Reagan's anti-ballistic missile "Star Wars" research program.

But, even within the physical sciences, support for basic science is more and more lacking as military priorities have gradually eroded the U.S. pre-eminence in science. For example, when a University of Illinois astrophysical group needed the most advanced type of computer available for an extremely involved calculation, they had to get the services of a "super-computer" in Munich, Germany; the handful of supercomputers in the U.S. are at the service of weapons laboratories and other installations with heavy commitments to "defense" work. And, when a search for the W-boson was proposed for the Fermilab accelerator near Chicago, no funds were available for the experiment; and so it was that the discovery of the W-boson was made in Europe last year.⁴⁶

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The Dove and the Hawk, by Fritz Eichenberg
The Peace Museum, Chicago

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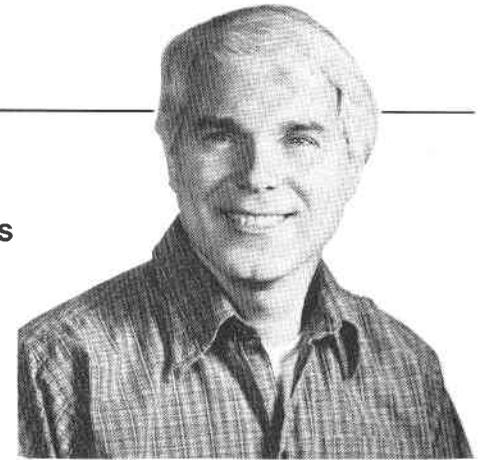
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An Essay Review on Mayr's
Growth of Biological Thought
(Harvard 1982, 953 pp., \$30)

Ernst Mayr and the Philosophical Problems of Biology

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Ernst Mayr intends to stir up controversy, and succeeds admirably, because he knows this is good for science. Writing of a science that he himself helped create, the historical account reflects the developmental process of biology itself. A holistic view illuminates his philosophical discussions—of mechanism, reductionism, emergence, and other persistent issues—which interlace the entire volume. In such ways we glimpse the dialectics of Mayr's own thought processes, learning a little more about that mode of thought which is inherent in the work of a creative scientist whether consciously so or not. I will return to the theme of dialectics after describing the content and some other insights gained from Mayr's impressive work on *The Growth of Biological Thought*, the first in a projected two-volume history of the life sciences.

Before launching into his three major sections on *Diversity of Life*, *Evolution*, and *Variation with Its Inheritance*, Mayr gives an introduction of considerable philosophical interest. Here he gives his ideas on how to write history of biology, reviews the intellectual history of biology from antiquity up to now, and discusses the philosophical problems of biology. The introduction provides an overview for major themes of the volume as a whole: the gradual and persistent emancipation of biology from scholasticism and *a priori* reasoning; the gradual development of populational over essentialist thinking on the diversity and history of life; and the similarity (or, in Mayr's view, more often the dissimilarity) of biology compared to other natural sciences. An epilogue, "Toward a Science of Science," is similarly commended. And Mayr is clearly at his best in the lengthy discussions later on the history of taxonomy as background for development of Darwin's theory, on the conceptual and historical development of the theory of natural selection itself, and, in general, on the late 19th and early 20th century development of Darwinian theory.

Given Mayr's own involvement in evolutionary theory from the 1920s onward, it is surprising (or maybe not) that the chapters on "evolutionary synthesis" (of Mendelian genetics with Darwinian theory) are less illuminating. To be sure he discusses a great deal of primary literature from the 1920s through the 1950s (the period of R.A. Fisher, J.B.S. Haldane, Sewall Wright, Julian Huxley, E.B. Ford, Theodosius Dobzhansky,

George Gaylord Simpson, and Mayr himself), and historians of biology will forever be in his debt for it. Yet one feels that here the text becomes less history than personal memoir. Mayr's long-standing opinions about which work was and is important strut through unabashed, including his persistent habit of ignoring the theoretical mathematical contributions of Sewall Wright who, along with Fisher and Haldane, was one of the major architects of the synthesis between Mendelism, expressed in mathematical terms, and evolutionary theory expressed in terms of Darwinian natural selection. (Wright was a biologist whose mathematical models were often more complex, more subtle than those of mathematician Fisher.) Thus, in this volume we get the evolutionary synthesis as Mayr sees and experienced it: valuable as a special kind of primary document, and also for a not always critical history of science. As Stephen Jay Gould says, "*The Growth of Biological Thought* is Mayr's autobiography writ large" [*Science* 20 Jan 1984, p. 257].

Discussing the pre-Darwinian period, Mayr shows the clearly evolutionary history of evolutionary theory. After a somewhat stereotyped glimpse of the Middle Ages as "a period of depressing intellectual stagnation" (p. 308), Mayr traces the origins of evolutionary thinking from the intellectual revolution, beginning in the 14th century, to the late 16th and early 17th century voyages of exploration, the Protestant Reformation (which attacked the dogmatism and scholasticism of the Catholic Church), the growth of cosmology (the doctrine of plurality of worlds) and geology (with its emphasis on a changing terrestrial surface). The so-called "Scientific Revolution" of the 17th century, on the other hand, Mayr finds of little importance to biology, perhaps not recognizing that it spawned an important trend that influenced much of biological thinking in the ensuing 300 years, namely, the development of materialist philosophy, especially in the works of Descartes. In the Enlightenment Mayr sees the stage for evolutionary thinking set by a variety of philosophical and intellectual developments: the rise of systematics (Linnaeus and the recognition of true affinities in the sexual organs of plants), new discoveries in biogeography which led to a questioning of creationist dogma, the discovery of numerous fossils in unexpected places, the concept of progress (particularly as enunciated by Condorcet), Buffon's elucidation of the criterion of interfertility as a part of his species concept, and the role of German philosophy (particularly the writings of Herder) which emphasized historical development and a temporalized *scala naturae*.

IT WAS GEOLOGY, however, which Mayr claims made the most important contributions toward evolutionary thinking in the late 18th and first several decades of the nineteenth century. In particular, it was the discovery that the earth's surface had undergone immense changes over time, that strata had been uplifted and submerged, that what were once ocean bottoms were now mountain tops, what had once been arid deserts were now lush forests. The catastrophist school, while putting forward oversimplified causes for these vast changes, nevertheless emphasized continual flux in the history of the world—in Mayr's view, a necessary intellectual precondition for the rise of evolutionary thinking.

Mayr emphasizes the influence of Linnaeus and Lamarck on the history of natural history before Darwin. From the new and quite exotic speci-

mens collected on voyages of discovery, Linnaeus reconstructed the system of classification which bears his name and is still fundamentally in use today. This system with its recognition of common affinities in structure (particularly reproductive structures), provided the basis for introducing an hierarchical view of systematics. According to Mayr, the development of an hierarchical system opened up the possibility of seeing relationships among groups as derived from common ancestral forms (groups within a given hierarchical classification can be considered derivative from common ancestral form, though of course Linnaeus did not see species as evolving from one another). Mayr rightly emphasizes the importance of hierarchical thinking not only in taxonomy, but also in evolutionary theory as a whole. Marxists will appreciate the discussion of hierarchies (representing qualitative levels of development) that is usually absent from histories of evolutionary thought.

In surveying Darwin's own cognitive pathway to the concepts of evolution and natural selection, Mayr emphasizes the importance of 5 factors: (1) Evidence from the fossil record; (2) Facts of biogeographic distribution, particularly the intermediate forms on Tristan da Cunha island, halfway between South America and Africa; (3) Island fauna, especially that on archipelagos such as the Galapagos; (4) The similarity of fossil to living forms in South America; and (5) the reading of Lyell's geology, with its emphasis on constant, but minute change (the geological doctrine of uniformitarianism). Mayr incorporates well the many published studies of today's burgeoning "Darwin industry," and utilizes the material in Darwin's reading and research notebooks (especially the "Notebooks on Transmutation of Species") to reconstruct how the theory of natural selection was derived. Mayr first delineates how, while on the Beagle voyage (1831-1836), Darwin was converted from a belief in special creation to one of evolution. Crucial here were his observations in South America, his study of island forms, and his reading of Lyell. The actual "moment of truth," however, came with Darwin's recognition that the idea of common descent provided the necessary organizing principle by which data from geology, biogeography, taxonomy, and the fossil record could be grouped together. As an admirer of William Whewell and John Herschel, both of whom advocated the search for general laws of nature, Darwin was thrilled to see what organizing power the concept of common descent provided to the many facts of natural history.

ON DARWIN'S CONVERSION to evolutionary thinking, Mayr traces (especially Chapter 11) Darwin's arrival at the notion of natural selection. This process proves to be evolutionary itself (with the insight from Malthus' "Essay" appropriately played down). Mayr points out that Darwin's "theory of natural selection" actually consists of some five different theories: (1) The idea of evolution as such ("descent with modification"), (2) Evolution by common descent (the idea of common ancestral forms from which modern descendants are derived), (3) Evolution as gradual, based on the accumulation of small, individual differences among organisms, (4) Populational speciation, the idea of evolution as a phenomenon of populations, not individuals, and (5) The theory of natural selection as a mechanism for the evolutionary process (pp. 505-510). In a very useful analysis, Mayr shows that Darwin's theory is not a unitary paradigm, as evidenced by the fact that many post-Darwinian evolutionists accepted

some of the above 5 components without accepting them all. This discussion helps to clarify not only the logical structure of Darwin's theory itself, but also helps to understand historically the differences between the various schools of anti-Darwinian thought.

MAYR'S MOST SIGNIFICANT POINT, the one that rings most true to me, is the claim that over the past several hundred years (if not all the way back to the ancients) the history of evolutionary theory has been characterized by a gradual retreat from "essentialist" or "typological" toward more populational thinking. Essentialism is the view, largely derived from Platonic philosophy, that behind the imperfect appearances of material reality lie "essences," or perfect and immutable "essences." With regard to species, essentialism meant that actual animals and plants were mere imperfect representations of the "essence" of their kind (a given cat contains the essence of "catness," but is not that essence in and of itself). The essence is stable and immutable, a view which led naturalists for thousands of years, according to Mayr, to overlook individual variations and fail to see their importance. Prior to the 19th century, study of natural history was geared toward discovering the true essence of each species; variations were imperfections to be ignored in the search for the underlying similarity of plan. These similarities led to the foundation of essential categories of forms or "types" of organisms, and thus to another hindrance to evolutionary thinking: typological thinking. As the name suggests, typology is the process of assigning individual forms to a type category. Like essentialist thinking, typological thinking tends to ignore individual differences in favor of emphasizing underlying similarity of plan. It forces individuals to fit into a pre-concerned category, instead of being seen as a spread of variable forms around a mean. Mayr points out that one of Darwin's greatest contributions to evolutionary thinking was his abandoning essentialist and typological thinking, in their place emphasizing the importance of slight individual differences among organisms as the key to their evolution. The emphasis on differences leads logically to statistical treatment of populations of organisms, and hence to population genetics and the new synthetic theory of evolution of the 1930's onward. As Mayr shows, however, essentialist thinking has persisted in various areas of evolutionary thought right down to the present day (for example, "creationism"); it continues to die a very slow death.

The least illuminating and (to me) least valuable portions of the book are those dealing with Mendelian genetics, the theory of the gene and the chemical basis of inheritance (Chapters 17-19). Here, Mayr displays less direct insight from the primary literature, and his vehement prejudices against much of classical genetics (as too mechanistic) and molecular genetics (as too reductionist) become more blatant. Mayr claims that the progress of evolutionary thinking in the 1920's and 1930's was much impeded by the work in classical genetics (the theory of the gene of Morgan and his school), largely because geneticists focused only on "hard" heredity (traits determined strictly by genetic codes, rather than a broader sense of heredity which included developmental processes), and because they pictured the organism as a mosaic of independent genes. This view, Mayr claims, informed much of the early stages of population genetics (it was true to a large degree in the work of R.A. Fisher, work lovingly referred to by Mayr as "beanbag genetics") and led to an over-simplistic,

ultimately false picture of evolution at the population level. While there is truth to Mayr's views, his anti-genetics bias (that is, anti-genetics in the 1920's through the 1950's) leads him to miss some of the important attempts in that period to circumvent an oversimplistic view of the gene and its relations to evolution (for example, the work of C.H. Waddington, which attempted in the 1930's-1950's to integrate genetics, evolution and embryology, which gets no mention in Mayr's book; or that of Richard Goldschmidt, which attempted in the same period to integrate genetics, evolution, embryology and physiology, and which gets scant, and negative, attention).

FROM THE MARXIST POINT OF VIEW Mayr's book raises a number of critical questions, approaching the history of evolutionary thought from a conceptual point of view that is unique and important. Mayr deals clearly and forcefully with philosophical issues that other historians ignore or treat only cursorily. Most important, whether or not he consciously considers himself so, Mayr is clearly a dialectical materialist of forceful persuasion. He introduces topics, especially controversial ones (such as continuity vs discontinuity in the evolutionary process, evolutionary vs appearance-based systems of classification, mathematical vs field studies of populations, typological vs populational thinking) by presenting distinct alternative views and usually taking a very strong stand. As Mayr himself states, he purposefully chose to be provocative: "Whenever possible, I have attempted a synthesis of opposing viewpoints (unless one of them is clearly an error). Where the situation is quite unresolved, I have described the opposing viewpoints in categorical, sometimes almost one-sided, terms in order to provoke a rejoinder, if such is justified" (p. 9). I had occasion to use portions of Mayr's book in a graduate seminar in population biology which I co-taught with an evolutionary biologist last year (1982-83). Immersed in the nitty-gritty of contemporary research in field and mathematical population genetics, almost all the students were highly incensed by many of Mayr's statements and his often strong biases. Nonetheless, they were forced to confront the issues clearly and forcefully because of the sharpness of Mayr's dialectical approach.

More important, however, is the way in which Mayr's dialectical thinking allows him to penetrate into certain philosophical issues within evolutionary theory in a way few other contemporary scientists or historians have been able to do. This point can be illustrated best by considering Mayr's view on the differences between the biological and physical sciences. This topic has often been a bug-a-boo for scientists, historians and philosophers alike, and has seldom been resolved adequately by any of those who have chosen to write on the subject. Most writers today, clearly wanting to avoid the vitalistic view that biology is somehow a special kind of science, defying the laws of physics and chemistry, take, as their only recourse, the reductionist position that living systems are just more complicated examples of physical systems. While Mayr makes it clear that he does not argue that biology defies the laws of physics and chemistry, he clearly shows that it cannot be reduced to them. Here is where his dialectical approach shows most directly. Mayr argues that the natural world must be viewed hierarchically. More complex systems derive out of, but are more than mere extensions of, less complex systems. They have emergent qualities—not mystical qualities—but what the dialectical materialist

would recognize as qualitative differences arising out of quantitative differences. Complex systems are, to Mayr, more than a simple sum of their parts. The parts also interact. Physicists fail to understand that, Mayr thinks, and in their passion to see biology as only a special case of their own more general laws, they miss important aspects of the biological process itself. The great emphasis today on particle physics, Mayr thinks, is supposed by physicists to make everything in biology sensible. He strongly disagrees with this view. Knowledge about ultimate particles will never shed light on how the nervous system works, or how a fertilized egg develops into a complete adult:

Complex systems are usually more than a sum of their parts. A set of genes, for instance, interacts to yield an intricate and integrated product that cannot be discerned from a shopping list of the components. (Roger LEWIN, Interview with Ernst Mayr, *Science* 216, May, 1982: pp. 719-720)

Quoting British philosopher of science Sir Karl Popper, to the effect that we live in a world of emergent novelty, Mayr remarks: "This is very important in studying nature, especially biology. New properties turn up in systems that could not have been predicted from the components, which means you have to study things hierarchically. Reductionism can be vacuous at best, and, in the face of emergence, misleading and futile." (*Ibid.*, p. 720).

Mayr brings the same sort of dialectical thinking to bear on problems within the biological sciences, particularly in critiquing the work of population geneticists. He argues that most population geneticists, starting with R.A. Fisher in the 1920's and 1930's, and continuing to the present, have essentially been reductionists. Fisher consciously wanted to reduce population genetics to the kinetic theory of gases and, to varying degrees, more recent workers in this field have pursued variations of the same theme. The basic error, according to Mayr, is that in their zeal to treat the genetics of populations in quantitative and predictive terms, they make immediate assumptions about real populations and ignore complex interactions that occur in nature. Genes are not units independent either of each other, or of the organisms in which they reside. And yet many population geneticists, Mayr feels, ignore the organism and treat genes only as abstract entities assembled into disembodied collections known as "gene pools." Evolution is not the mere shift in gene frequencies from one generation to another in a large population, Mayr says. Rather, it is a product of natural selection acting on particular combinations of genes in individual organisms.

IN CALLING for a more integrative approach to the study of population genetics, Mayr is continuing his lifelong struggle to synthesize evolutionary theory with genetics, field biology, embryology, and even biochemistry and molecular biology. It is not that Mayr opposes treating evolutionary theory in molecular or mathematical terms. Indeed, he is the first to admit that much important work occurs in these areas and should be incorporated into evolutionary theory. What he opposes is the view which states that once we have brought evolutionary theory to the molecular level—say, in tracing the evolutionary history of hemoglobin or the protein cytochrome C—we can extrapolate back up to a full understanding of the evolution of vertebrates. As Mayr and any thinking dialectical materialist

knows, the reductionist position leads to erroneous views of nature. Although Mayr himself presents few examples of how to integrate different hierarchical levels, his clearly dialectical process points the way toward new approaches which would never be discernible to the traditional reductionist.

In all fairness to modern population genetics, my friends in this area tell me that Mayr's view of what the field is like nowadays is somewhat arcane. While some population geneticists continue to be particularly mechanistic and reductionist, many are trying to incorporate the very approach—especially concern for integrative phenomena—that Mayr calls for so loudly. With the advent of systems analysis and computers, it has become possible to deal with gene interactions in a way that was not possible even a few years ago. More and more, mathematical population geneticists are dealing with realistic parameters derived from studying populations in the field, and thus coming closer to predictive and hierarchically more sophisticated levels of genetic analysis than has occurred in the past. By ignoring these newer developments (which, by the way, often require rather sophisticated mathematics) Mayr perhaps gives the impression that *all* population geneticists are old-fashioned, naive reductionists, which is certainly not true today.

With regard to the differences between physics and biology, Mayr's view is subject to some misinterpretation, even by readers like myself with considerable sympathy for his point of view. Here is where an explicit recognition of his own dialectical approach would be of considerable help. Given the lack of philosophical and historical background of most biologists, it is all too easy to read a kind of eccentric vitalism, or fuzzy notion of emergence into Mayr's views that does not do his thinking justice. For example, in arguing for the distinctness between biology and physics, Mayr makes two points which seem to me to be only half truths unless phrased explicitly in dialectical materialist terms. One is the idea that biology deals with populations of unique units (organisms) while physics deals with populations of identical units (electrons, atoms, etc.). As part of this argument, Mayr stresses that the uniqueness of organisms lies not only in their difference from other organisms in the group to which they naturally belong (species), but also in their historical nature, which is also unique, and which is something inanimate matter lacks. As Mayr seems to see it, biological units evolve—they "remember" their past and replicate aspects of it during every reproductive cycle. Atoms and electrons do not. Here is where some readers are likely to get confused. Mayr would seem to be arguing for a different "essence" between biology and physics. In one sense it is true. The quantitative difference between the degrees of uniqueness found in sub-atomic particles (there must be some differences—does not all matter evolve?) and that found in biological organisms are immense, and lead to significant quantitative differences in their behavior (including our ability to predict that behavior). This is a point any dialectical materialist would understand. But to non-dialecticians Mayr's point is more likely to sound like the old vitalist arguments that claimed biological phenomena involved something mystical, non-physical, or "special" that could never be understood by physics and chemistry. Mayr's point that biology cannot be reduced to physics and chemistry *per se* is correct. It is, however, likely to be misconstrued by philosophically naive readers who do not see how quantitative changes can lead to qualitative changes without introducing mystical or "vital" elements.

ANOTHER EXAMPLE, a distinction which Mayr draws between physics and biology is that the former deals in broad, predictive generalizations called "laws," whereas the latter does not. Stated this way, I find the whole idea misleading. If one means by "laws," highly predictive statements characteristic of the mechanistic and reductionist stage of physics from the seventeenth through the latter part of the 19th centuries, then I would agree. But only the most classical physicists of today would hold such a view. The developments in quantum and relativity theories show that physical laws in the classical sense (inverse square law, law of acceleration, Coulomb's law, etc.) are in principle not really laws in a modern sense either. They may work as approximations, but they neglect fundamental features of the universe (indeterminacy, relativity of mass and motion, curvature of space, etc.) that we now understand to be true. In the same sense that an evolutionary biologist would not hope to predict the future course of evolution for any species, so an astronomer or physicist would not hope (today) to predict the future course of the cosmos. To some degree, I fear, Mayr's argument about the difference between biology and physics is a red-herring—a product of an outdated view of what physicists really think about their own methodologies. (It is true that many biologists, quite often reductionists, share Mayr's misconception about physics; and to that extent he is addressing a real problem.) Still, if Mayr adopted a more openly dialectical approach in which the issue could be broken down into questions of prediction in time frames where history mattered or didn't matter, the whole discussion of different kinds of laws (predictive vs non-predictive) might be ultimately more resolvable. The struggle to develop methods for investigating holistic, interacting processes, may not be the problem of biology alone.

Where Mayr's lack of an openly dialectical approach comes more to the fore is in his failure to treat the history of evolutionary theory in its larger social context. Mayr's treatment is almost wholly as intellectual history. He sketches in only the barest outlines of economic and political history as a backdrop to the growth of evolutionary ideas. While we do learn that Linnaeus' classification scheme gained much from the vast array of new specimens brought back by the voyages of discovery, and that Darwin and Wallace differed much in their socio-economic backgrounds, the deeper implications of these facts for the development of evolutionary ideas are left unexplored. We learn nothing of some newer work which relates Darwin's reading of political economists such as David Ricardo or Adam Smith (not just Malthus) to his metaphors of "competition," "division of labor," "scarcity," or "the war of all against all." We see nothing of how the development of 19th century British industrial capitalism could have influenced Darwin's thinking—indeed molded the very shape and content of the theory of natural selection itself. We learn nothing of the development of equilibrium theory in economics and the social sciences, coming out of the period 1890–1920, and its influence on the study of population dynamics (particularly gene frequency equilibria, etc.) in the 1920's and 1930's. Even the debates in which Mayr himself engaged, between the mechanistic outlook of the early population geneticists (R.A. Fisher, for example) and his own more holistic, interactive view, are presented in abstract form. No background is given for Fisher as an outstanding product of mechanistic thinking in statistics, having worked for a number of years as an insurance statistician; or that his motivation for studying evolution

mathematically came from his elitist views on eugenics. No credit is given to dialectical materialists who had been attacking the mechanistic outlook in biology, as elsewhere, for years prior to the evolutionary synthesis (outstanding among them was Marxist population geneticist J.B.S. Haldane, whom Mayr otherwise much admires). It is now possible, thanks to a number of authors both inside and outside the Darwin industry (Robert Young, Sylvan Schweber, Stephen Jay Gould, Richard Lewontin, Steven Rose, Bernard Norton, Donald Worster, John Greene and Sandra Herbert, among others), to treat Darwin's work far more thoroughly in the context of 19th century economic, social, and philosophical history than ever before. It is a pity that Mayr chose not to explore these important interconnections more thoroughly.

IRONICALLY, Mayr's lack of conscious dialectical thought forces him to miss the clear dialectics in Darwin's own theory of natural selection. Darwin's process of evolution is based on the interaction of two clearly opposing tendencies in the biological world: heredity and variation. Heredity is the faithful replication of traits between parent and offspring, variation its unfaithful replication. Without both of these opposing processes, evolution by natural selection would be impossible. With only heredity, all offspring would be identical, and there could be no change; with only variation, no trait could be preserved, and species (indeed, organisms as we know them) would not exist. It seems to me that one of the most far-reaching aspects of Darwin's thought is his extension of the dialectical process (unconscious, to be sure) to the very origin of organic diversity itself. This dialectical relationship between heredity and variation is in many ways more fundamental in Darwin's paradigm, more of a "law," than the specific process of natural selection itself. There are all kinds of exceptions to the strict Darwinian concept of natural selection; there are no exceptions to evolution through the dialectical interaction of heredity and variation.

There are many other dialectical processes exemplified by the history of biology which Mayr fails to bring to light in his history: between evolution and revolution, heredity and environment, genetics and embryology (that is, genotype and phenotype), to name only a few. Space does not permit me to amplify on these, but I want to point out that had Mayr taken a more openly dialectical approach (he is quite prone to this mode of thought anyway), he might have been able to show more clearly how contradictory elements within biology itself, as well as its interaction with its socio/political environment, contributed to its development as a science; at the same time he could have made apparent how recognition of dialectical aspects of natural selection itself has provided a continual growth in insights among naturalists/biologists from Aristotle's day to the present.

Despite these problems, however, Mayr's book is well worth sampling in detail if not to be read completely from cover to cover. He wrote the book to stimulate discussion, and it serves this purpose well. Even if unconsciously, Mayr is a dialectician, not only in his emphasis on historical development and holistic outlook, but also in his appreciation for the value of controversy and strongly-stated views in the advancement of knowledge. *The Growth of Biological Thought* is a most remarkable and valuable contribution to the history of biology—not least as a primary document recording the views of one of the chief architects of 20th century evolutionary theory. □

Equilibrium and Disequilibrium in Living Systems

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Biological equilibrium presents itself as a philosophical problem in two senses: not only as a matter of the relationship between man and nature, and human control of this relationship, but also as a phenomenon which apparently refutes the objectively contradictory nature of the living world. However, in relation to the biosphere, are the terms *contradiction* and *equilibrium* really mutually exclusive? Not at all; there are at least two ways in which *biological equilibrium* is inherently related to the law of contradiction. Firstly, biological equilibrium is a *specific form in which the unity of contradictions in the living organism is manifested*. As such, it represents an equilibrium between opposing tendencies in processes and biological characteristics: in brief, an equilibrium between opposites. Secondly, equilibrium is a component of one of biology's most important contradictions, representing *one of the opposites in the contradictory unity of equilibrium and disequilibrium*.

The author will argue here against any approach that equates equilibrium with some sort of harmony, against any metaphysical interpretation of biological equilibrium as a mere mechanical leveling out, which would tend to separate the concepts of equilibrium and contradiction. The author also wishes to disassociate herself from a theoretical approach which puts one-sided emphasis on biological disequilibrium and thus tends to deny its contradictory unity with equilibrium.

Before proceeding to discuss *dynamic equilibrium* as a special manifestation of the unity of opposites in the living world, I will try to clarify this concept, especially important since the literature is divided on it.

SOME DEFINITIONS. First of all, one must distinguish between the concepts of equilibrium, dynamic equilibrium, and biological dynamic equilibrium.

EQUILIBRIUM is understood to be a resultant state of balance or equalization between opposing forces, actions or influences. A great number of physical laws (such as the law of action and reaction, the law of attraction and repulsion, and the second law of thermodynamics) refer to tendencies that can work towards equilibrium. For example, the relatively stable structure of a molecule is determined by the positions or distances where the attractive and repellent forces of the atoms have a resultant of zero. Neighboring atoms then fluctuate around the equilibrium distances so determined.

DYNAMIC EQUILIBRIUM, as the name itself suggests, refers to a state of equilibrium in which the system as a whole remains unchanged though, within this state of constancy, opposing processes continue to operate. Dynamic equilibrium is thus the unity of constancy and change, i.e., of opposing conditions or effects. The concept of dynamic equilibrium includes the phenomenon of *drifting equilibrium*, in which a system reacts to external disruption as follows:

... in the case of temporary disruption, it readjusts to the original equilibrium, but in the case of a lasting change of external conditions it reaches a different though similar state of equilibrium. (1)

From this definition, it is clear that a dynamic drifting equilibrium characterizes an *open system*, one which affects the environment and absorbs

effects from the environment, maintaining a continual energy-matter flow relationship with it. Though *living systems*, as open systems, are in a state of dynamic equilibrium within themselves and with their environment, there is no consensus among biologists as to the nomenclature for this characteristic. Claude Bernard spoke of the constancy of the *milieu interieur* or internal environment. Walter B. Cannon describes as "homeostasis of the organism" what we call here dynamic equilibrium. Darwinian thought refers to dynamic equilibrium with the environment as "adjustment" of the organism. On the whole, these different modes of expression denote the same content: dynamic equilibrium.

BIOLOGICAL DYNAMIC EQUILIBRIUM. The characteristics of dynamic equilibrium in living systems differ in significant ways from those of inanimate systems:

a) The flow of matter in the living organism takes place through complex mechanisms which differ from those of inanimate systems, e.g., flow may take place against the concentration gradient. The state of equilibrium in a living system is generally not a balance between equal quantities of a substance but a definite ratio of substances which insures the normal functioning of the organism. (An example is the glycogen formed in animal organisms during fermentation, where 5/6 part of glycogen reaches a state of equilibrium with 1/6 part of glucose phosphate.)

b) In living systems, the exchange of matter-energy with the environment takes place in the form of assimilation and dissimilation, and the organism itself provides the energy required for the metabolic process.

c) The living organism is an open chemical system that produces by itself the catalysts required for an enduring dynamic equilibrium that will insure survival. The organism obtains catalysts by three means: through nourishment, general metabolism, and genetically. The renewal of the enzyme catalysts in general metabolism is effected by already existing enzymes. The catabolism, transformation and incorporation of substances by the organism could not take place without enzyme activity, which is therefore necessary for reaching a state of equilibrium in metabolism or in life processes based on metabolism.

d) In the living system, dynamic equilibrium is achieved by and is inseparable from the internal self-regulating systems of the organism. For

example, homeostasis under physiological conditions is none other than a state of dynamic equilibrium reached via the regulative effect of several organs (or via the nervous system, a sophisticated form of self-regulation in living organisms of a higher order). As French biologist Ernest Kahane writes:

Life may be characterized as the coordination of several mechanisms within a structure that insures their functioning . . . I am inclined to say that one living organism is more highly developed than another when it possesses more effective correctional mechanisms to prevent the disruption of its natural equilibrium by changes of the external environment. (2)

On the same issue, Belgian physiologist León Frédéric wrote in 1885:

The living organism is made up in such a way that any disrupting effect will automatically trigger off a compensating mechanism, the task of which is to neutralize and repair the damage. The more sophisticated the organism, the more numerous, more perfect and more complex these regulating apparatuses become. (3)

WHAT KIND OF RELATIONSHIP exists, then, between self-regulation and dynamic equilibrium? Does self-regulation lead to dynamic equilibrium or, conversely, does self-regulation arise from dynamic equilibrium? Both interconnections are true. The organic relationship between dynamic equilibrium and self-regulation is most clearly manifested in *the principle of internal self-regulation known as feedback*: the regulatory mechanisms of organisms do not influence dynamic equilibrium "from the outside" but, on the contrary, these are triggered by the disruption or restoration of the equilibrium itself. One characteristic of dynamic equilibrium is fluctuation near the state of balance. Since information about this fluctuation is fed to the center controlling the process, dynamic equilibrium thus becomes the internal content of feedback and consequently of self-regulation. (Examples of self-regulating mechanisms in plants are tropisms, such as responses to gravity or to the direction of light, in which motion away from the desired position of equilibrium leads to bending in the opposite direction and thereby toward the state of equilibrium.)

Dynamic equilibrium thus becomes a link in the chain of self-regulation. Material processes reaching a state of equilibrium are in this way not passively subordinated to self-regulating mechanisms (humoral or neuro-endocrine) but themselves constitute an integral part of this mechanism through dialectical feedback interactions.

THE AUTHOR WISHES NOW to demonstrate that biological dynamic equilibrium is an integral part of the internal contradictions of the living organism, a specific and extremely general case of the unity of contradictions. The contradictory aspects of dynamic equilibrium to be discussed are relations within the organism and its relationship with the environment.

In every living system, it must now be pointed out, *the contradictions themselves are at the same time in a state of dynamic equilibrium with each other*. The ion balance of an organism, for instance, is a state of equilibrium reached through the contradictory relationships between the positive ions (Na, K, Ca) and negative ions (OH, C, HCO₃, HPO₄). Fertilizers for aqua-culture must be "equalized" solutions in which the antagonistic ions balance each other. Similarly, the physico-chemical properties of plasma are shaped to a considerable extent by the balance of antagonistic K⁺ and Ca⁺⁺ ions. Such a balance, as the manifestation of a unity of

contradictions, must be relative, i.e., neither stable nor permanent, and shifts with the state and even the age of the organism. In plants, the optimum balance of the two cations is upset at the expense of potassium reutilized in the young organism, whereas calcium is dominant in the aging.

The nitrogen and protein balance of the organism is another highly important physiological phenomenon. There is a nitrogen balance in the normal adult organism, meaning that the quantity of nitrogen absorbed equals the quantity released. Why can this nitrogen balance be described as a balance of contradictions? Because this state of equilibrium is the balance of two opposing processes, protein metabolism and protein catabolism. That such an equilibrium is definitely not a secondary characteristic of the unity of contradiction is shown by the fact that hunger destroys the nitrogen balance and a nitrogen deficit occurs which, beyond a certain point, leads to irreversible loss of the balance and then to death of the organism. A closely related dynamic state characterizes protein balance.

Equally important to the animal organism are the carbohydrate balance (manifested in blood glucose concentration), the balance of energy stored in phosphate compounds (reversible creatine + ATP + ADP reaction), the water balance and its inseparable salt balance, and so forth. Each such balance can be discussed in terms of the dynamic equilibrium required for a healthy organism and the limiting conditions under which balance is lost and death occurs.

No rigid distinction should be made between the components and the contradictions in the functioning of the organism. The arguments given above are based on the assumption that a state of equilibrium among components arises from a state of equilibrium in the life processes and, conversely, any change in the balance of components is accompanied by a change in the life processes.

Special mention must be made of a contradiction (to be discussed more fully later), namely, the unity of conflict between assimilation and dissimulation, as the fundamental, nearly all-embracing characteristic of life, and practically synonymous with dynamic equilibrium. Here, equilibrium does not mean that the opposing processes pass into one another with the same quantitative balance at all phases of the organism's existence. In the developmental phase of all living organisms, the processes of assimilation are dominant. Then there is a middle phase in which the two tendencies are roughly of the same magnitude. Finally, in the phase of aging the rate of dissimulation exceeds that of assimilation. But, in a certain sense, dynamic equilibrium is constantly present in the organism since the rate of dissimulation increases for more energy-intensive processes, as in mobilization of resources (carbohydrates, fats and proteins, in that order). Beyond a certain point, of course, the organism must compensate by increasing the rate of assimilation processes or else the imbalance becomes irreversible and leads to death. Hence, adaptation to external environmental conditions is only possible through dynamic changes in metabolism which enable the organism to "stave off" the effects of the environment (adjustment of the organism for warm climate, manual work, desert existence, and so forth—obviously such adaptation must be confined within certain limits).

THE UNITY OF THE CONFLICT between the organism and its environment constitutes the dynamic conflict between them. Conversely, dynamic equilibrium is realized in the adjustment to the environmental conditions.

Hans Selye regarded stress as the embodiment of all adaptational reactions of the organism, and the stress effect itself as the totality of those effects which tilt the organism out of its original equilibrium, bringing into play mechanisms which react by readjusting the equilibrium. Hence, the postulate of equilibrium in Selye's stress theory does not mean the elimination of opposing tendencies. For example, in discussing illness, Selye quotes Hippocrates:

Illness is not only suffering but, at the same time, is exertion (ponos), more precisely, the struggle of the body to restore its health. (4)

A new dimension of contradiction is introduced by considering conditions of nutrition where reaching a state of equilibrium is, at the same time, also a question of the balance between one living organism and another. Survival in the living world hinges on the dynamic equilibrium between the species making up the unified chain of nourishment as well as between each "link" in this chain. It is convenient to categorize the individual links of the chain as *producing*, *consuming* or *reducing* organisms, where the producers are the plants creating organic matter autotrophically, the consumers are either directly plant eaters or indirectly, feeding on plant-eating animals or on animals generally, while the reducers are organisms that decompose organic matter (fungi and certain bacteria) so that, while nourishment is obtained, inanimate nutritional components are made available to the food chain.

In a sense, the species of producing, consuming and reducing organisms balance each other; the existence of the one depends on that of the other; the proliferation or drop in the number of one affects the numbers of the other. Plant species can proliferate only if inanimate environmental conditions (sunshine, temperature, soil conditions, etc.) are suitable. Consuming organisms must reach a balance with the producers as well as with each other, while the existence of reducing organisms depends on the other two. However, consuming and reducing organisms do not depend solely on producing organisms while plant life depends on the other two for its requirements of CO₂ and reduced organic matter. So dynamic equilibrium exists not only amongst these three major categories of living organisms but, as stated previously, between every link in the food chain. The so-called *biocoenotic equilibrium*,

$$\frac{\text{rate of reproduction}}{\text{rate of mortality}} = \frac{V}{S} = 1,$$

supposedly holds true for individual species: when the value of V is higher, the species proliferates; when S is higher, the species faces extinction. In nature, however, extinction seldom happens this way because a decrease or increase in the number of a given species, for any reason, can affect the numbers of other species up and down the food chain. For instance, when the number of fungi increases, usually fungi-consumers will also proliferate because their environment has become more favorable (more nourishment available). When the fungi-eaters proliferate, the number of fungi decrease and, with this reduction in available nourishment, the number of fungi-eaters decreases until equilibrium is restored.

The preceding discussion helps demonstrate dialectical principles as follows:

a) Contradictions exist between the constituent elements of living organ-

isms or between processes engendering them. These conflicting aspects are in a state of dynamic equilibrium with each other.

- b) Equilibrium with the environment has never signified some kind of one-sided balance followed by a state of stability. Adaptation to temperature conditions, for instance, does not signify adjusting the organism temperature to that of the environment. Rather, through mobilization of compensating mechanisms, the organism's activity dynamically creates internal equilibrium.
- c) This dynamic equilibrium is constantly disrupted and recreated, so the system fluctuates around the state of equilibrium rather than maintaining a static equilibrium. For instance, the aforementioned biocoenotic equilibrium by no means signifies that the reproduction and mortality rate of the various species is a constant number. The state of equilibrium shifts in one direction or another.
- d) Finally, the dynamic equilibrium of the organism with the environment not only represents the unity of the conflict relationship but, at the same time, is a factor in it and, again emphasizing its dynamic character, is also a consequence of the struggle of opposites. Thus adaptation (another term for achieving dynamic equilibrium with the environment) is not usually direct adjustment but rather the result of the struggle for life and the process of natural selection, as a result of which those individuals survive which are most capable of maintaining equilibrium.

In his *Philosophical Notebooks*, Lenin wrote on the equilibrium of opposites in relation to the unity and identity of conflicts. We are dealing here with concrete and characteristic manifestations of this.

As indicated earlier, the state of equilibrium is also one pole of a contradiction that exists with the state of disequilibrium. To accommodate this contradiction, our philosophical concept of dynamic equilibrium may be further defined as the relationship, within a system, between processes increasing and decreasing, compensating and excluding, extending and limiting each other, i.e., a relationship between opposing tendencies where the extent of a shift in either direction, primarily through feedback, remains within the system's threshold of tolerance, not endangering the system as a whole but rather enhancing the dialectical unity of the system's relative permanence and changeability during its natural life expectancy. This is a type of contradiction in the living system which, in this form, has not been discussed in either the biological or philosophical literature, where we may read about either equilibrium (though not as equilibrium between contradictions) or about disequilibrium (a lesser known and seldom recognized concept).

* * * * *

THE STATE OF DISEQUILIBRIUM was discussed by Ervin Bauer in his *Theoretical Biology* (5), where he attempted to formulate a general law of the living organism which he called the principle of *constant disequilibrium*. (Perhaps, for the benefit of posterity, we should call it the *Bauer principle*.) Bauer arrived at this principle through a far from simple induction from fundamental life phenomena. Seeking to uncover the characteristic traits and laws of living systems, he resorted to the even more general laws of the material world, such as the laws of thermodynamics formulated in physics, in order to grasp the most general law of the living world.

Which, asked Bauer, are the most characteristic general traits or "requirements," as he calls them, of living matter? The most important, he answered, is that the living system is capable of changing internal states, even with constant external conditions.

It is, above all, characteristic of all living systems that spontaneous changes take place in their state, changes of state which are not brought about by external causes. (p. 32)

A precondition, however, for such changes of state is that differences of potential exist within the system which permit adjustment even without external help.

This first characteristic is not in itself sufficient to characterize the living organism: *the living system also manifests a definite behavior under changed environmental conditions*. Unlike the behavior of inanimate systems, however, this behavioral motion of the living system does not exclusively correspond to the forces of inertia and friction. The living organism itself participates in the response to environmental change:

The animal reacts to pushing or pulling; it either runs away or resists, that is, the resistance is either larger or smaller than the force of inertia or friction. (p. 32)

Bauer then connected this second characteristic with the first, demonstrating that, *for the active participation of the system, it is necessary that the system be also capable of spontaneous changes of states*. Then, from the connection of these two characteristics, Bauer inferred the existence of a third, according to which *a system can only meet the two aforementioned requirements when it is capable of doing work*, that is, when it can use the free energy, which exists during unchanged conditions, for doing work whereby its own working ability is improved. Taking a closer look, Bauer's assertion that "the ability to work must be utilized in the interest of the working ability itself" (p. 44) need not be regarded as a circular argument. Clearly, this abstract precept expresses the everyday fact of the living world that the energy released in the breakdown of nutritive matter is used by the organism largely to take up and incorporate energy-producing materials required for continuity of the life processes. But what is the basis of the organism's working ability? Bauer answered that it is the system's state of disequilibrium. For, he argued, in a state of equilibrium, no change can occur without some kind of external effect and the work of the organism must be directed *against* reaching such a state of equilibrium:

The work of the living system, regardless of the environmental conditions, is aimed against reaching the state of equilibrium which, in the given environment and on the basis of the initial state of the given system, is bound to be reached. (p. 51)

Obviously, the characteristic of the living system that makes it capable of changing the impact of external effects, which could be expected on the basis of initial conditions, is none other than active adjustment to the environment. The same holds true for regulative functioning (work carried out to prevent the reaching of equilibrium), i.e., restoration of the system's difference of potential (preserving its ability to perform work).

On the basis of his discussion of the characteristic traits of the living system in general, Bauer formulated his universal law of biology:

The living, and only the living, systems are never in equilibrium, and continually carry out work at the expense of their free energy content to prevent the coming

about of equilibrium which, under the given external conditions and under the laws of physics and chemistry, ought to come about. (p. 51)

This work to maintain a state of disequilibrium is carried out at the expense of the organism's structural energy. In actual fact, the energy source of the organism is never directly the external environment, but the ATP containing macroerg bonds stored as part of the organism's microstructure. It is in the course of the structural breakdown of ATP to ADP that the energy required for functioning of the organism is released.

We thus arrive at the general conclusion that the structures of living matter possess free energy content which, at the given moment and under the prevailing conditions, can decrease at any time; therefore the structures are in a state of disequilibrium and any internal work in living systems, or any work opposing their changes of state, is done exclusively at the expense of this structural energy, i.e., at the expense of the so-called systemic forces. (p. 64)

Bauer thus pointed out the difference between the machine and the living system, for no machine obtains the energy for its functioning through breaking down its own structure.

Unusual as Bauer's theory may seem, it must be regarded as professionally prescient and philosophically significant. Offered prior to the development of our present-day molecular biology (which provides its ultimate verification), Bauer's theory may be regarded as the precursor of the contemporary explanation of disequilibrium in terms of the state of disequilibrium obtaining in molecular structure. By interpreting external work as work carried out at the expense of structural energy, he defined the existence and significance of structural change in the living system as the internal source of the state of disequilibrium.

Also indicative of Bauer's prescience is the stress in his theory on the role of internal factors in the living organism which, in accord with contemporary thought, is alien to any mechanistic tracing of characteristic traits of the living system back to purely external effects. Indeed, in this respect, Bauer goes so far as to regard the free energy, which insures disequilibrium on the basis of primarily endogenic factors, as being dependent on the free energy of the ovum:

The total quantity of calories which the organism is capable of transforming in the course of its whole life depends solely on and is proportional to the free energy of the ovum. (p. 129)

Though this assertion is highly debatable, it does indicate how, against the one-sided "ectogenic" attitude, Bauer ascribed great significance to internal forces.

Bauer's prescience enabled him to arrive at conclusions on a whole series of biological problems which either tally with present-day findings or at least lay a foundation. But further discussion of such work is beyond the scope of this study.

Approaching the living organism from considerations of energetics enabled Bauer to conceive of the living organism as an entropy-producing system. And, though he did not use the concept explicitly, his firm theoretical stand (on the basis of the living system in a state of disequilibrium) is that entropy increases only in the dying organism, reaching its maximum upon death.

The author believes that Bauer's conception of the disequilibrium of the living system is corroborated by the findings of modern biology. It must be stressed that Bauer regarded the molecular structure's state of disequi-

librium as the source of the system's free energy, basing this conclusion on the fact that a lattice structure is best suited to the state of equilibrium and the further fact that there are no lattice (crystalline) structures in the living system (these occur only upon death). Another of his approaches to the same problem is that

living matter . . . is in an electrically polarized state even without the application of an electric field of force and, upon death, i.e., when the structure reaches a state of equilibrium, this polarization decreases. Accordingly, upon death, the dielectric constant must also decrease. (p. 809)

These assertions are, in essence, substantiated by present-day scientific findings, though from different angles. Here is one set of his relevant findings: Radiating bodies must be in a state of (induced) disequilibrium, and success has crowned the efforts to repeat the experiment by Kuvic (Ervin Bauer mentions him) which demonstrated that, prior to division, normal cells emit UV radiation while pathological, malignantly-changed cells do not emit such radiation. (In a certain sense, neoplastic cells can be regarded as dying cells and this may explain their not being in a state of disequilibrium.) Dean and Hinshelwood have also demonstrated that, prior to division, cells are chemically in a state of disequilibrium. The agreement with numerous such findings and precepts of modern biology indicate that Bauer has grasped the real and general characteristic trait of the living world.

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THE ABOVE OUTLINE of Bauer's concepts and the analysis of their significance has concentrated on their positive features. This, however, does not mean that the whole of what he says is acceptable when confronted with the findings of modern biology and the principles of Marxist philosophy.

One criticism which may be leveled at Bauer's basic concept is that disequilibrium represents only one side of reality. While his view of reality has been presented so as to demonstrate what is correct and enduring in Bauer's concepts, the author has also concluded, on the basis of biological facts and philosophical considerations, that his basic concept is not free of one-sidedness. The one-sidedness of Bauer's theory lies in its regard of disequilibrium as the almost exclusively characteristic trait, thus excluding and making superfluous the acknowledgment of biological equilibrium.

To Ervin Bauer, dynamic equilibrium usually meant the state of a system which, unless affected by external influences, remains unchanged. Such a system is unsuitable for doing work. Bauer characterized the process of reaching this state of equilibrium as one that neutralizes the impact of any change within the system by a simultaneous though opposite change of equal magnitude. All of this holds true for the state of equilibrium of inanimate material systems. Biological science, however, has never, and still does not, interpret the dynamic equilibrium of living processes in this manner. Thus, Bauer unjustifiably equated the inanimate and the animate, static and dynamic equilibrium, and since this equation was irreconcilable—indeed, it is as irreconcilable in reality as in his theory—he concluded that dynamic equilibrium is not characteristic of the living system:

The idea that the living system is in a state of dynamic equilibrium is, from a physical viewpoint, basically incorrect and therefore leads to erroneous biological conclusions which do not correspond to the facts. (p. 57)

This unequivocal negation of the existence of dynamic equilibrium within the living system may, at least for a first try, be traced back to three factors:

a) Bauer did not analyze all the implications of dynamic equilibrium in the life process because, as indicated above, he generally identified the dynamic equilibrium of the living system with what is defined as dynamic equilibrium in the inanimate systems of physics, and he thereby rejected it from the start.

b) His approach to the life process was one-sidedly based on the laws of thermodynamics to such an extent that, within this realm, he commits the mistake of reductionism (though, incidentally, he consciously tried to avoid this), insofar as he one-sidedly reduces the life process to general relations of energetics.

c) Bauer came excessively under the spell of his own law, the theory of disequilibrium, so that his otherwise brilliant dialectical reasoning was transformed into a metaphysical one-sidedness which makes *absolute* the theory of disequilibrium and rejects its dialectical opposite, equilibrium.

The justifiability of these criticisms will be demonstrated by a few quotations in which Bauer contradicted himself: while trying to prove that no kind of equilibrium may characterize living systems, he acknowledged the existence of equilibrium in these systems.

In one instance, describing mitogenetic radiation, Bauer referred to dissimulation as a state of equilibrium:

According to our theory, radiation occurs when the molecules of living matter pass from their deformed state of disequilibrium into a state of equilibrium or approach it. This process, however, continually takes place as it is the initial, the very first phase, as it were, of dissimulation. (p. 100)

Later, repeating this negation of his fundamental concept, Bauer almost went so far as to use it as a definition: "We call the processes of equalization dissimulation" (p. 124).

Bauer also attributed significance to the state of equilibrium in the process of reaction to a stimulus. His point of departure here was the theoretical precept that the lattice structure corresponds to a state of equilibrium, while a stimulus which disrupts a structure in the state of disequilibrium must of necessity transform it into a state of equilibrium:

Let us briefly mention . . . that when a stimulus, in the terms of our definition, disrupts the maintenance of a structure in a state of disequilibrium, then deformation must obviously be reduced at the stimulated point and the structure must get closer to the state of equilibrium. (p. 86)

Bauer expounded this idea in concrete form when he wrote that, upon muscle contraction, the "damage" (caused by the stimulus) brings the molecules closer to the state of equilibrium, which is also manifested in the negative charge of the tissue.

As a final instance, in the relationship between the organism and its environment, where making *absolute* the state of disequilibrium would seem most justified, Bauer nevertheless left room for some recognition of equilibrium, at least in terms of content. From the viewpoint of energetics, Bauer defined the organism as the totality of energy-increasing and energy-decreasing effects, acknowledging however that effects of the former type may only grow in conjunction with the growth of energy-reduction processes. What is this if not a certain form of equilibrium? And Bauer described this relationship as one of adjustment:

We refer to the special relationship described above between the changes of state of the environment and living systems as adjustment. (p. 217)

These excerpts from Bauer demonstrate clearly that, even though he carried the idea of disequilibrium to the point of absurdity, this great biologist, when faced with the facts of science and in relation to certain (highly important) details, could not but acknowledge the existence of equilibrium. Admittedly, he usually refers to nearing the state of equilibrium, which is quite justifiable. But, in those situations where he should have been explicit that some kind of equilibrium was involved, he would almost negate the facts by stressing the opposite, as in this instance:

Upon reaching the limit of assimilation the living system reaches a stationary state. Dissimilation and assimilation must balance each other. (p. 126)

Bauer adds that this has nothing to do with dynamic equilibrium, justifying his stance by arguing that the system is not brought to this state by some kind of external influence but by the system's own continuous work.

THIS BRINGS US TO THE POINT where the author must put forth her own position, even though it is pretty much revealed in the preceding critical remarks.

The existence of dynamic equilibrium in the living world is generally acknowledged in the present-day biological literature. As its name implies, it is a moving or, as often called, drifting equilibrium representing the unity of constancy and change (as opposites). Its dynamism is rooted in the fact that opposing motion from forces or influences within the system lead to continuous equalization and to continuous upsetting of this equalization.

But what does disruption of equalization mean if not transformation into its opposite, i.e., into a state of disequilibrium which, in turn, will be "disrupted," again entailing adjustment to a state of equilibrium. It follows from this that the author, as a positive result of her polemic against Bauer's views, now interprets dynamic equilibrium not simply as fluctuation around the state of equilibrium, i.e., as the cessation and reformation of equilibrium, but as the continuous transformation of equilibrium into disequilibrium and vice versa. Only the unity of these opposing tendencies may be termed dynamic equilibrium, thus further developing and thereby complementing what has already been said about dynamic equilibrium.

This is a quite different outcome in the effort to gain better knowledge of reality than that where one's approach is to disregard one side of reality and stress either equilibrium or disequilibrium. *In actual fact, neither equilibrium nor disequilibrium exist exclusively in the living world, only the two together exist and this is how we interpret dynamic equilibrium in the modern sense.*

A characteristic contradiction of the living system is the unity of equilibrium and disequilibrium, which presuppose each other under the principle of mutual interaction and complementarity. This is a part of the relative self-movement within the living organism. But, as the conflicts of every contradiction are in one way or another unequal, the same holds true for the relationship between equilibrium and disequilibrium. Equilibrium plays the dominant role in the living system as a biological whole. But disequilibrium plays the chief role in the thermodynamics of the organism as a producing-consuming system. It follows from the dialectical approach to the problem that, if we investigate a process from the aspect of equilibrium, then, from the interpretation of dynamic equilibrium given

above, it follows that the stress will be not on equilibrium but on *dynamism*.

This is why the author ascribes great significance to Bauer's theory of disequilibrium, though it neglected the dialectical opposite. Despite its excessive emphasis, Bauer's theory of disequilibrium contains the brilliant discovery that the state of disequilibrium is indeed the chief, the dominant characterizing trait of the living system, to which all relationships of equilibrium are subordinate.

In criticizing Bauer's concept, the author did not wish to stress its unacceptability but the fact that it needs to be complemented. Bauer's mistake is not to be regarded as solely negative but has its positive side as well, for elimination of the error may lead to a new conception of *biological dynamic equilibrium* and a new opportunity to delve into one of the major contradictions of the life process, one which has great philosophical significance.

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[EDITOR'S NOTE: There is also, presumably in Russian, *Teoreticheskaya Biologiya* by E.S. Bauer (reprint of 1935 edition with new preface plus biographical and critical essay), Akadémiai Kiadó, Budapest, 1982.]

Nature's Dialectics Can Be Yours — — — — —

We find the poles of an antithesis, like positive and negative, as inseparable from each other as they are opposed; despite all opposition, they mutually penetrate each other. So it is with cause and effect, concepts which have validity for a particular case but may not when that case is considered in its general connection with the world as a whole, where they merge and dissolve into the concept of universal action and interaction—with cause and effect continually changing place so that what is here and now an effect becomes there and then a cause, and *vice versa*.

Such processes do not fit into the frame of metaphysical thinking. The polar antagonisms, put forward as irreconcilable and insoluble, with forcibly fixed demarcation lines and distinctions, are what give theoretical natural science its restrictive metaphysical character. To recognize that such antagonisms and distinctions are found in nature *only with relative validity*, and that their imagined rigidity and absoluteness is introduced into nature *only by our minds*, this recognition is the kernel of the dialectical conception of nature.

The accumulating facts of natural science compel us toward the view that Nature's processes are dialectical, not metaphysical. But we will achieve this view more easily if we approach Nature equipped with consciousness of the laws of dialectics. For this transition, the natural science community must keep in mind that its results take the form of concepts. The art of working with concepts is not in-born nor given with ordinary, everyday consciousness. It is an art that requires real thought, a form of thought that has a long empirical history, not more nor less empirical than natural science itself. Only by assimilating 2500 years of philosophical development can natural science get free of a philosophy that stands apart from it, outside it, above it, and from an equally limiting mode of thought inherited from English empiricism.

Dialectics grasps things and their images (ideas) in their interconnections, their sequence, their movement, their birth and death. But scientists who have learned to think dialectically are still few and far between. Hence the continuing conflict over the meaning of new discoveries: the traditional mode of thought is the source of the boundless confusion which now reigns in theoretical natural science.

—Engels in essence; adapted from *Anti-Duhring* (New York 1939, pp. 19, 29).

EDITOR'S PREFACE. *This paper argues consistently and forcefully for a materialist approach to the problems of interpreting quantum mechanics. The author is not consistent, however, in his treatment of the Marxist dialectical method. When it was pointed out, for example, that his idiosyncratic use of terminology results in confusing reductionism with dialectics, his response was: "Print it my way and then criticize me." The author has been taken up on this challenge, as you will see in the appended comments. May our readers not only benefit from this interchange of ideas but also send their own contributions to the discussion.*

In response to the question asked in the title, this paper answers with an unqualified *no*. Like Einstein, Schroedinger, de Broglie, and an increasing number of other physicists and chemists (1), I claim that quantum mechanics is not a scientific theory but rather a computational tool that lends itself to useful and sometimes extremely accurate calculations. For want of anything better, every scientist working in the field of the microstructure of matter is obliged to use quantum mechanics (= Orthodox Quantum Mechanics = OQM); but if we continue to confuse this valuable set of cookbook recipes with scientific theory, we shall never achieve any genuine understanding of the fundamental laws of matter (2).

But what is a scientific theory? My answer is that a scientific theory is one in accordance with the implicit philosophy of every competent scientist (excepting, of course, those scientists who invoke obscurantist doctrines in futile attempts to uphold untenable theories such as spiritualism, creationism, or OQM). Putting aside certain differences in detail, language, and style (3), I find similar descriptions of the philosophy of modern science in Engels (4, 5, 6), Lenin (7), Einstein (8, 9), Planck (10, 11), Popper (12), Bohm (13), de Broglie (14), Russell (15), and Bunge (16, 17). Following Engels and Lenin, we may call this philosophy *dialectical materialism* (18); or following Bunge, we may refer to it as *scientific materialism* and thereby try to avoid the stigma attached to the former label in respectable society.

Whatever the name we choose, the *working philosophy* of modern science has the following features (19):

I. *A scientific theory is materialist* (20); that is, it concerns itself solely with matter in motion (21) and does not accept descriptions of nature based on spirits, mysticism, or divine intervention. OQM is not materialist inasmuch as it postulates a semi-mystical "Observer" who "reduces wave packets" by means of some supernatural mental power (22). Furthermore, OQM flirts with mysticism via mathematics (23) which, since 1925, has become akin to a holy language such as Latin or Hebrew. According to Heisenberg and his followers, the mathematical formulation of quantum mechanics cannot be translated into ordinary language or into physically intuitive concepts. Also, OQM supposedly refers not to matter but to

Is Quantum Mechanics a Scientific Theory?

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"observables." Like the positivists, the orthodox quantum theoreticians consider the idea of a material world existing independent of human consciousness to be a "metaphysical" prejudice, outside the concern of science.

II. *A scientific theory is reductionist* (24) in the sense that "qualitative changes can only occur by the quantitative addition or subtraction of matter or motion (so-called energy)" (6). In the light of modern chemistry and biology, we should add to this the concept of geometrical structure as a quantitative change resulting in qualitative change. Examples of reductionism are thermodynamics in terms of statistical mechanics, chemistry in terms of molecular physics, optics in terms of electrodynamics, biology in terms of biochemistry, and so forth.

Perusing the philosophical works of Engels and Lenin reveals that reductionism (i.e., the unity of matter) is one of the essential features distinguishing dialectical from mechanical materialism. The alternative to reductionism is either a return to mechanism or a retreat into mysticism (vital forces, spirits, free will, etc.). OQM is *anti-reductionist* inasmuch as it includes the "Observer" whose influence upon matter cannot be explained in terms of any natural law. In the more timid formulations, it is the "Apparatus" which cannot be analyzable at the microscopic level.

It should be emphasized that every experimentalist completely disregards the so-called "quantum theory of measurement" and develops the theory of the experimental apparatus and the measuring process in terms of microphysics which today means, in the ultimate instance, quantum mechanics. For example, the experimentalists seek to understand photography in terms of the physics and chemistry of the solid state, and from there to understand the solid state in terms of quantum mechanics, at least to the extent that this is possible today. Only the orthodox quantum theoreticians (Bohr, Heisenberg, and their disciples) pretend to reverse this process.

III. *A scientific theory is logical*; that is, starting from a set of consistent fundamental laws (postulates or axioms) formulated in terms of basic (primitive, irreducible, or indefinable) concepts, other laws (theorems,

rules, or formulae) are deduced according to the rules of formal logic. Examples of primitive concepts are particle, mass, charge, position, time, electromagnetic field. Examples of fundamental laws in a scientific theory are Schroedinger's equation of a given system, Maxwell's laws, Newton's law of gravity, etc. (25).

OQM is not a logical theory because, first of all, its postulates contain terms such as "observer" and "measurement" which are not primitive terms but rather highly complex concepts that belong to the very pinnacle of human knowledge, not to its foundations. Worse still, OQM presupposes processes such as the "reduction of the wave packet" and "transitions" (the so-called quantum jumps) which contradict not only its own fundamental laws of motion (the equation of Schroedinger in the nonrelativistic approximation, or the equations of Dirac or of Klein-Gordon in the more exact relativistic theory) but also Maxwell's equations of electromagnetism. This "minor detail" is glossed over, if mentioned at all, in the textbooks (26).

OQM is once more illogical in that the symbol ψ (the so-called wave function) has two distinct and contradictory meanings. At times ψ determines the statistics of an ensemble of particles, giving the spread in position and momentum. Here, electrons, protons, neutrons, nuclei, etc. are treated as classical particles with definite size, shape, internal structure, position, momentum, charge and mass (27). At other times, ψ represents a wave or cloud in both real and momentum space; that is, the individual particle is not only spread out in real space but is simultaneously moving in different directions with different speeds (28). Both of these interpretations are physically intuitive, although the second is incredible. To cover up this internal contradiction, the orthodox deny making any intuitive interpretations, invoking magic words such as "observation," "measurement," "complementarity," etc.

We wish to emphasize that, in OQM, electrons and other quantum particles are not visualized as objects having properties intermediate between particle and wave; instead the theoretician switches back and forth between the interpretations of particle and wave (in real and momentum space) and only thus is able to obtain agreement between the calculations and the experimental results.

IV. *A scientific theory must be testable*; that is, it must have some consequences that can be checked against experiment. Here, however, we must be very careful, remembering that no experiment is theory free, just as no theory is philosophy free. The so-called experimental "facts" that appear in the literature are in reality often long, involved and at times even doubtful theoretical calculations, based on experimental data which in turn is built upon a mixture of theory and observation.

Also, it should be remembered that before a theory can be testable it must be logically consistent, since it can be readily shown that a self-contradictory theory can be used to prove anything and will thus always agree with experiment. By this criterion, OQM is not a testable theory since it is illogical. On this point, it must be made perfectly clear that Schroedinger's (or Dirac's) equation does NOT predict the observed spectrum lines of atoms and molecules; in fact, it does not predict any spectrum lines whatsoever, and the observed spectra represent experimental violations of the quantum equation of motion. It is only after we throw in the ad hoc postulate of the reduction of the wave packet (or transition or

quantum jump) that we are able to calculate the observed spectra. However, as stated previously, the quantum jumps violate the known laws of physics, including the Schroedinger and Dirac equations.

V. *Finally, a scientific theory must be deterministic* (29); that is, despite the occasional and unconvincing objections of Bunge and Popper, the theory must rest on the proposition that identical conditions produce identical results. If, under supposedly identical conditions, we observe different results, then, in accordance with the methodological rule laid down a century ago by Engels (5) and half a century later by Popper (30), we are obliged to look for the unobserved difference in the conditions; that is, to search for the hidden variables.

According to popular mythology, OQM has abolished determinism, but this is denied even in the standard textbooks. Thus Dirac (31) and Messiah (32) assure us that the wave function evolves deterministically between "measurements." Only the "measurements" introduce indeterminism! And so we have it that the entire world and all its history was, and will be, completely deterministic *à la Laplace* except for those rare occasions when some scientist decides to "measure." Actually these OQM "measurements" have nothing whatsoever to do with real laboratory procedures; they are merely shabby tricks permitting the theoretician to come up with a calculation in agreement with experiment.

Furthermore, even in OQM it is accepted that energy and momentum are always and exactly conserved. But this would be nothing short of miraculous if OQM "measurements" truly introduced indeterministic elements into nature; it would be equivalent to the dice coming up seven on each and every throw. According to the same mythology, OQM is a probabilistic or statistical theory. Again, the claim breaks down under careful examination. Any attempt to assign a consistent probabilistic interpretation to ψ very quickly ends up in disagreement with experiment and in internal inconsistencies (33, 34, 35). For lack of space we shall consider here only one example. It is well established experimentally that every hydrogen atom has the same ground state energy, -13.6 electron volts, in full agreement with the solution of Schroedinger's time-independent equation. However, according to the same calculations, the potential energy of about one-fourth of these atoms is greater than -13.6 eV. Since the kinetic energy is always positive, we arrive at an obvious internal inconsistency and a disagreement between theory and experiment. For good measure, I wish to add that in all applications of probability theory it is invariably assumed that the fundamental laws are deterministic (5, 13, 35). Only in OQM is it assumed that there is a contradiction between probability and strict Laplacian causality.

With this I end the discussion of OQM according to the above five criteria, but wish to debunk one more myth: the claim that Bohr and Heisenberg showed that, at the microscopic level, the measuring procedure interferes drastically with the measured system, in contrast with the macroscopic level where this interference is negligible. First of all, even if this myth were true, it would not be of the slightest importance since science has never been limited to dealing with observables (7, 8, 10, 12, 14, 15, 16). Secondly, it is completely false, even absurd. No sane scientist

could possibly claim that a spectrometer interferes with the light emitted by a distant star or that a particle detector influences radioactive decay (36). Moreover, the electric field inside the atoms is of the order of megavolts per centimeter, far greater than any field we may apply in the laboratory during an experiment. On the other hand, there are countless examples in the microworld where the measuring process interferes with the object under observation, even destroying it. Consider destructive testing, for instance; think of what the chemists and biologists do to cells, germs, and viruses, and so forth. Yet, because of this, does anyone invoke indeterminacy or complementarity or any of the other magic charms of OQM?

Before concluding, I wish to stress that it is not enough to analyze OQM; "the point, however, is to change it," to quote Marx's well-known thesis (5). One plausible attempt in this direction is stochastic electrodynamics, which postulates that quantum effects are due to random electromagnetic fluctuations in the background (34). Despite certain initial successes, it now appears that this approach leads in some cases to results in complete disagreement with experiment (37, 38). In my opinion, the most likely line of attack is to start with the Pilot Wave Interpretation (PWI), originally proposed by de Broglie (39), then rediscovered and extended by Bohm (40). They showed that Schroedinger's equation is equivalent to classical mechanisms plus a quantum field proportional to the inverse of the mass of the particle. This field accounts for the diffraction effects that occur, for example, when an electron passes through an extremely small aperture. In PWI, the probability relations of OQM are no longer valid in general (41, 42). While normally ψ determines the probability density of position (but not of momentum, except for the case of free particles), this does not necessarily always hold (43, 44). As for Heisenberg's famous uncertainty relation, it reduces to a scatter relation for free particles (12, 41, 45), making it possible in practice as well as in principle to determine the simultaneous values of position and momentum with an accuracy far beyond the limits set by Heisenberg (46, 47).

From the above we see that, despite Bohm's claim (40), PWI does not agree with OQM: and the two interpretations can be differentiated experimentally. However, neither is it true that PWI accounts for the observed spectra of atoms and molecules; in fact, Andrade e Silva *et al.* showed that this could only be achieved by a nonlinear equation (48).

Recently, my colleagues and I at the Universidad de Oriente showed that including the effect of radiation damping adds a nonlinear term in Schroedinger's equation (49), with the result that to a first-order approximation the so-called stationary states are stable and the system resonates at frequencies given by Bohr's conditions. It seems likely that we shall be able to explain the quantum jumps as rapid transitions from one quasi-stable state to another. So far, these results are encouraging enough so that we have some grounds for hoping that we are moving in the right direction. We are convinced that none of the present interpretations of quantum mechanics are correct even to a first approximation (50). Some of them, however, are at least physically plausible, in the tradition of modern science, and therefore may provide a starting point for some future scientific theory. In the meantime, we must continue to use OQM, remembering that it is no more than a computational tool and thus avoiding its obscurantist trappings even when camouflaged in Hegelian jargon.

References and Notes

1. In my opinion, Orthodox Quantum Mechanics, as it emerged almost in its present form in 1927, was based essentially on the work of Planck, Einstein, Schroedinger, and de Broglie. Of these, Planck came to terms with OQM very reluctantly, while the other three criticized it in the strongest terms. All four had more or less regarded their efforts as tentative steps towards an understanding of microphysics, and were appalled at the manner in which the other physicists juggled formulae and the meanings of the symbols ("Dancing on eggs," as Einstein put it) in order to "predict" theoretically the experimental results known beforehand.
We must mention, of course, other brilliant physicists such as Born, Dirac and Pauli who also took part in the development of OQM. In my opinion, however, the contributions of Bohr and Heisenberg have been grossly exaggerated. I am convinced that it is mainly due to their obscurantist philosophical teachings that Bohr and Heisenberg have received so much attention.
In the 1930s only Einstein and Schroedinger, among physicists, dared openly to oppose OQM. Popper's *Logic of Scientific Discovery* (1935) is worthy of mention and praise for the manner in which he criticized positivist tendencies in physics, especially the obscurantist aspects of OQM.
After Bohm's historical articles in *Physical Review*, de Broglie, influenced by Vigier, returned to his original ideas of the Pilot Wave Interpretation (PWI) of quantum mechanics. Since then, a steadily growing number of physicists have become disenchanted with OQM. The list is too long to enumerate, but Dirac deserves attention because, since the mid-1970s, he has been admitting that Einstein seems to have been right after all.
2. As a simple example of a cookbook recipe used in 20th-century physics, we have Bohr's model of the hydrogen atom, for which he postulated that an electron could move in certain allowed circular orbits without radiating and also could jump without cause from one orbit to another, emitting at the same time a quantum of electromagnetic energy. On the basis of these hypotheses he was able to calculate the spectrum of hydrogen. The fact that these assumptions violated Maxwell's equations did not bother Bohr nor any other physicist in the least, Einstein and Schroedinger being the only exceptions of which I know. It should be emphasized that everybody still believes in the validity of Maxwell's equations, but most of us simply forget this when it is convenient.
For such a scientific abomination, Bohr received the Nobel prize in 1922. About that same time, physicists were ready to drop the Bohr model, not because it was logically inconsistent but because the recipe simply didn't work except in a few isolated cases. In modern quantum physics, the logical inconsistencies are hidden under more sophisticated mathematics and positivist double talk.
Of course, there is nothing wrong about cookbook recipes *per se*; they can, in fact, be extremely useful and indeed much of our present knowledge is in this form. The harm begins when science is confused with a set of isolated, unrelated, and even contradictory formulae, each having some validity under special conditions.
3. Though such differences can at times be very important, most are not relevant to the present discussion; a few exceptions will be noted.
4. F. Engels, *Anti-Duhring* (International, New York 1966).
5. F. Engels, *Ludwig Feuerbach* (International, New York 1941).
6. F. Engels, *Dialectics of Nature* (International, New York 1940).
7. V.I. Lenin, *Materialism and Empirio-Criticism* (International, N.Y. 1972).
8. A. Einstein, *Ideas and Opinions* (Dell, New York 1973). See, in particular, the essays "Principles of research," "On the theory of relativity," "Geometry and experience," "The mechanics of Newton," "Maxwell's influence on the evolution of the idea of physical reality," "On the methods of theoretical physics," "Physics and reality," "The fundamentals of theoretical physics."
9. A. Einstein in *Albert Einstein: Philosopher-Scientist*, P.A. Schilpp, ed. (Open Court, La Salle 1969).
10. M. Planck, *Where Is Science Going?* (AMS, New York 1977).
11. M. Planck, *Scientific Autobiography and Other Papers* (Philosophical Library, Greenwood, Westport 1949).

12. K.S. Popper, *The Logic of Scientific Discovery* (Basic Books, New York 1959).
13. D. Bohm, *Causality and Chance in Modern Physics* (Routledge & Kegan Paul, London 1957).
14. L. de Broglie, *The Current Interpretation of Quantum Mechanics* (Elsevier, Amsterdam 1964).
15. B. Russell, *Human Knowledge, Its Scope and Limits* (George Allen and Unwin, London 1948). In this, his last major philosophical work, Russell severely criticized positivism and adopted a fully materialistic attitude without, of course, fully admitting it.
16. M. Bunge, *Foundations of Physics* (Springer Verlag, New York 1967). This brilliant book is marred by Bunge's futile attempts to present OQM as a scientific theory.
17. M. Bunge, *Scientific Materialism* (Reidel, Dordrecht 1981). Essentially dialectical materialism under another name. Despite my disagreements with Bunge on some important points, I consider him to be the most important living philosopher of science.
18. I refer to dialectical materialism rescued from its Hegelian jargon and elaborated in terms of 20th century, not 19th century, science. As regards Hegel, it is worthwhile to quote Marx:

"The mystifying side of the Hegelian dialectic I criticized nearly 30 years ago. But . . . it was the good pleasure of the . . . epigones to treat Hegel as a 'dead dog.' I therefore openly avowed myself the pupil of that mighty thinker and . . . flirted with the modes of expression peculiar to him. The mystification which the dialectic suffers in Hegel's hands, by no means prevents him from being the first to present its general forms of movement in a comprehensive and conscious manner. With him, it stands on its head. It must be turned right side up, if one is to discover the rational kernel within the mystical shell." (Postscript to German edition of *Capital*.)

Both Engels and Lenin expressed similar opinions. While Engels occasionally lapsed into Hegelian jargon in *Anti-Duhring* (1878), he had completely abandoned it in *Ludwig Feuerbach* (1888). To continue today the use of expressions such as "negation of the negation," "interpenetration and unity of opposites," etc., is to cover over "the rational kernel" with "the mystical shell."
19. In this short essay I am not attempting anything more than to clarify those features of dialectical materialism (= the working philosophy of modern science) which directly concern my analysis of OQM.
20. Respectable materialists prefer to call themselves realists in order to be more acceptable in polite society.
21. At present we regard ordinary matter as consisting of particles (electrons, protons, etc.) and of fields (electromagnetic, gravitational, etc.) but it is reasonable to suppose that there are other forms of matter that would, for example, blur the distinction between field and particle, as predicted by Einstein, Schroedinger and de Broglie.
22. Originally, Heisenberg (*Zeit. f. Physik* 43: 172; 1927) and Bohr (*Nature* 121: 580; 1928) presented the "reduction of the wave packet" (though not using this expression) as due to the interaction of the measuring apparatus with the micro-object (e.g., electron). By 1930, however, Heisenberg, in *The Physical Principles of Quantum Theory* (U. Chicago Pr.), found himself forced to back up the "Apparatus" with a conscious "Observer." Von Neumann, in *Mathematical Foundations of Quantum Mechanics* (Princeton U. Pr., 1955) was clearer on the need for a conscious "Observer" who, in some mysterious fashion, causes the reduction of the wave packet.

For the benefit of the nonspecialist, "reduction of the wave packet" implies that, when we "observe" the position of a particle, the packet instantly bunches up into a single point, and when we "observe" the momentum of a particle, the packet immediately spreads out into a pure sinusoidal wave. We cannot emphasize too strongly nor repeat too often that the reduction of the wave packet represents a violation of every known law of physics, and that without it OQM is in complete disagreement with experiment.

23. Mathematics is, of course, indispensable for arriving at a full understanding of physics. While mathematics enables us to express our ideas in a precise and compact form, it is nevertheless an extremely limited language that can be used only to describe idealized models of reality. Every mathematical formula can be expressed in words, though it is often impractical to do so. While ordinary language lacks the same precision, it is far more powerful. Think, for example, of the difficulties in describing a tree mathematically.
24. According to Professor Bunge (personal communication), I am not using "reductionism" in the precise philosophical sense of the word. I trust that the context will clarify my use of the term. "Reductionism" implies that the new laws and entities which emerge at a higher level must be deducible, at least in principle, from the laws and entities at a more fundamental level. Reductionism, however, should be understood within the following framework:
 - (a) We have no reason to believe that we shall ever arrive at the most fundamental laws of nature.
 - (b) It is almost certainly impractical to deduce step by step the highest laws of nature (say, of human society) from the most fundamental laws, even if we knew them.
 - (c) According to Gödel's theorem, mathematics contains an infinite number of theorems that we shall be unable to prove or even know with certainty to be true, at least within the same level of mathematics. This theorem would seem to apply to mathematized science as well.
 - (d) Reductionism implies that in principle we can reduce the laws of society to those of the psychology and physiology of the individual, and from there to molecular biology, to chemistry and finally to atomic physics. But then physics is a science formulated by physicists whose beliefs are determined not only by formal logic and experimental evidence but also by their psychological make up and by the values and opinions that they absorb from the society of which they form a part (see, e.g., P. Piaget, *Psychology and Epistemology*, Viking, New York 1970). Thus reductionism in a formal sense moves in one direction; in a social sense, in the opposite direction.
25. There seems to exist the mistaken view that dialectical materialism, which views the world as teeming with contradiction, invalidates and supersedes formal logic. The confusion lies in the different meanings we ascribe to the term *contradiction*. As often used, contradiction implies instability; thus when we speak of the internal contradictions of the capitalist system we are claiming that it is unstable and must at some time or other change drastically to a more viable form of society. Another sense is that of opposition or conflict such as when we speak of the contradiction

Lenin Looks at Hegel's Dialectics — — — — —

OF COURSE, this study, this interpretation, this propaganda of Hegelian dialectics is extremely difficult, and the first experiments in this direction will undoubtedly be accompanied by errors. But only he who never does anything never makes mistakes. Taking as our basis Marx's method of applying materialistically conceived Hegelian dialectics, we can and should elaborate this dialectics from all aspects, interpret them materialistically and comment on them with the help of examples of dialectics in the sphere of economic and political relations, which recent history, especially modern imperialist war and revolution, provides in unusual abundance. In my opinion, the editors and contributors of *Pod Znamenem Marksizma* [Under the Banner of Marxism] should be a kind of "Society of Materialist Friends of Hegelian Dialectics." Modern natural scientists (if they know how to seek, and if we learn to help them) will find in the Hegelian dialectics, materialistically interpreted, a series of answers to the philosophical problems which are being raised by the revolution in natural science and which make the intellectual admirers of bourgeois fashion "stumble" into reaction. [Coll. Wks. xxxiii, 233f.]

between capital and labor.

A third use of the word directly concerns us here. It refers to the contradiction between our theories based on partial understanding (idealized models of the world), and the real world itself. As Engels says: “. . . one is always conscious of the necessary limitation of all acquired knowledge, of the fact that it is conditioned by the circumstances in which it was acquired. On the other hand, one no longer permits oneself to be imposed upon by the antithesis, insuperable for the still common old metaphysics, between true and false, good and bad, identical and different, necessary and accidental. One knows that these antitheses have only a relative validity; that that which is recognized now as true has also its latent false side which will later manifest itself, just as that which is now regarded as false has also its true side by virtue of which it could previously have been regarded as true.”

All this implies that we must continually revise (in the progressive, not the retrograde sense) our theories and philosophies, including dialectical materialism, correcting them, clarifying them, and making them ever more exact and profound. It does not imply in the least that our science and philosophy should be illogical (= self-contradictory = inconsistent).

26. Nevertheless, something like “quantum jumps” does occur in certain cases, which means that Maxwell’s and/or Schroedinger’s equations break down under certain conditions. This point will be discussed briefly in closing this paper.
27. For example, the dimensions of the nuclei are in the 10^{-13} – 10^{-12} range. Furthermore, it is assumed that nuclei have internal structure and definite shape which, in general, is not spherical. Also, chemists, when they assign a shape and structure to molecules, implicitly assume that the wave function gives a statistical, rather than a complete picture of microparticles. Recently some physicists and chemists, notably Woolley in England, have remarked that the chemist’s picture of the molecule is in contradiction with OQM. For a discussion of this problem, see P. Claverie, S. Diner, *Isr. J. Chem.* 19:54; 1980, an article containing extensive bibliography. Also, see P. W. Anderson, *Science* 177: 393; 1972.
28. In the calculation of energy levels of atoms and molecules, physicists and chemists speak of “electron clouds.” See, for example, J.C. Slater, *Quantum Theory of Atomic Structure*, v. 1 (McGraw-Hill, New York 1960). In reality, it is presupposed that the electrons are “clouds” in both real and momentum space but, as far as I know, only Dirac, in his well known textbook, admits this clearly and openly.
29. I do not differentiate between determinism and causality, and think that to do so is merely to play with words. It is, of course, the fashion these days to sneer at determinism, affixing the adjective *mechanistic* or *Laplacian*. As far as I am concerned, Laplace’s clarifying example is essentially correct: if it were possible to know the state of all matter at a given instant of time, to know all the laws of nature, and to possess an infinitely rapid and powerful computer, then it would be possible to predict the future as well as know the past with perfect certainty. Now, neither Laplace nor any other scientist has ever imagined that such knowledge or capability is remotely attainable, but to claim that determinism is thereby refuted is complete nonsense.
30. As to the claim that determinism is anti-dialectical, we turn to Engels (4): “But where on the surface accident holds sway, there actually it is always governed by inner, hidden laws and it is only a matter of discovering these laws.” This view is held in all engineering and science—in fact, in all human endeavor of civilized society, excepting of course the most fanatical religious circles and the partisans of OQM.
31. Unfortunately, Popper retreated from his deterministic position and with time became completely anti-deterministic.
32. P.A.M. Dirac, *Quantum Mechanics* (Clarendon, Oxford 1958).
33. A. Messiah, *Quantum Mechanics* (North Holland, Amsterdam 1961).
34. L. Cohen, *Phil. Sci.* 33: 317; 1966.
35. P. Claverie, S. Diner, in *Localization and Delocalization in Quantum Chemistry*, O. Chalvet *et al.*, eds., v. 2, 395 (Reidel, Dordrecht 1976). This article contains an excellent analysis of the difficulties in quantum mechanics from the chemist’s point of view. It also includes a clear summary of the stochastic interpretation and an extensive bibliography.

35. M.C. Robinson, *Abstracts, 7th Intl. Congress of Logic, Methodology, and Philosophy of Science*, v. 4, 194 (1983).
36. We repeat that no sane scientist could make the claim; nevertheless, the scientific journals continue to publish accounts of detectors causing the reduction of the wave packet even inside the nucleus. Such statements are accepted without question by the orthodox quantum theoreticians.
37. P. Claverie, L. Pesquera, F. Soto, *Phys. Lett.* A80: 113; 1980.
38. L. Pesquera, P. Claverie, *J. Math. Phys.* 23: 1315; 1982.
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40. D. Bohm, *Phys. Rev.* 85: 166, 188; 1952.
41. J. Andrade e Silva, *Comptes Rendus* 264: 909; 1967.
42. M.C. Robinson, *J. Phys* A15: 113; 1982.
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49. M.C. Robinson, C.E. Avelado, L.A. Lameda, D. Bonyuet Lee, *J. Phys.* A16: 2987; 1983.
50. For this reason, I have not discussed the Einstein-Podolsky-Rosen paradox, Bell’s inequalities, and the various experimental tests of these inequalities. It seems to me that as long as we are unable to explain scientifically the spectrum of the hydrogen atom, there is no hope at all of explaining in a satisfactory manner the far more complicated phenomena connected with the tests of Bell’s inequalities. All that may have been proven up to now is that one class of hidden variable theories called “local” has been ruled out, but even this has been contested in a recent article by Marshall, Santos and Sellers in *Physics Letters A*. At any rate, none of the experiments has contradicted PWI; in fact, Bohm and Aharonov, and also Vigier and his collaborators, correctly predicted beforehand the results of these experiments, including Aspect’s.

LLOYD MOTZ COMMENTS:

1) Robinson’s definition of a “scientific theory” is not adequate in that it makes theory dependent on the state of mind of the scientific community at any particular time. A scientific theory must have an intrinsic validity independent of what any group of scientists may think. A scientific theory is essentially a collection of laws that enable anyone competent in the field to correlate any set of events.

2) Most of Robinson’s objections to OQM (Orthodox Quantum Mechanics) have to do with the interpretations it offers. Development of the mathematical formalism of quantum mechanics has been accompanied by the growth of a body of interpretations that are not warranted by quantum mechanics as a *physical* theory. To fault the theory because such misinterpretations have arisen is unjustified. One can accept quantum mechanics as a scientific theory without accepting any particular interpretation of it, such as that of the Copenhagen School (OQM).

3) Robinson’s concern with the interpretation of the role of the “observer” and of “measurement” in quantum mechanics is understandable, but all scientific theories, classical or otherwise, have had to deal with the effect of the observation process on the measurements obtained. Quantum mechanics as a physical theory simply states that gain of knowledge of one kind (via measurement) leads to unavoidable loss of knowledge of

another kind. Nevertheless, quantum mechanics is fully deterministic in the sense that it says a system in state A will evolve to state B according to precisely formulated (mathematical) laws. However, in trying to determine the path along which the system evolves, we disturb it unavoidably so that its final state is B' and not B.

4) The essential features of quantum mechanics which make it a physical theory and not just a mathematical recipe for calculating results are a) the existence of the constant h (the quantum of action) and b) the law of addition of probabilities (Feynman's "sum of paths"). From the existence of the quantum of action h one can deduce (as I have done in a series of papers referenced below) the Schroedinger equation from the classical Hamilton-Jacobi equation as an algebraic exercise. Formally, then, there is *no difference* between classical and quantum mechanics.

In fact, as my papers show, if S is the classical action of a particle, the particle behaves as though it were described by the wave function $\exp(iS/h)$, which always gives the probability *one* for finding the particle in its classical path. But if the particle can take N different paths which do not differ by more than h in their action, then $\exp(iS/h)$ must be replaced by the sum, $\exp(iS_1/h) + \exp(iS_2/h) + \dots + \exp(iS_N/h)$, where S_N is the action of the N th path. This gives us the Schroedinger equation and Feynman's sum-of-path approach to quantum mechanics. This in turn leads to the quantum mechanical treatment of probabilities which can be illustrated by the simple example of a particle that can go from state A to state B along two different paths which are defined by the classical actions S_1 and S_2 . Classically, since the particle *must* be in either path 1 with probability P_1 or in path 2 with probability P_2 , the probability of the particle to be in state B is $P = P_1 + P_2 = 1$. The quantum mechanical probability, however, is obtained by using the (complex) probability amplitudes a_1 and a_2 , and adding these for each path. Thus, the total probability amplitude becomes $|a|^2 = |a_1 + a_2|^2 = a_1^2 + a_2^2 + 2a_1a_2$, with the last term leading to interference so that the equation describes a diffraction pattern.

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LESTER TALKINGTON COMMENTS:

I wish first to dwell upon two positive aspects of Max Robinson's materialist polemic which make a useful contribution to our discussion.

First, Robinson is very effective in revealing some of the contradictions of OQM. A highlight for me was the contradiction between the idealism of the theorists (OQM) and the empirical practice of the experimentalists who not only disregard the so-called quantum theory of measurement but also proceed to develop a "theory of the experimental apparatus and the measuring process," neither of which is supposed to be possible with

OQM. Furthermore, this new theorizing by the experimentalists makes use of quantum mechanics itself.

Robinson also brings out some contradictions between quantum theory and the classical theory which it disowns but must nevertheless use. For example, there has never been a problem solved in quantum mechanics without making use of Maxwell's equations in its formulation, yet the conceptual interpretation of quantum mechanics violates basic *physical* tenets of Maxwellian electrodynamics.

Second, Robinson does not stop with criticizing OQM but then proceeds to make a thought-provoking effort to follow Marx's precept that "the point, however, is to change it." I hope that some of our physicist readers will study Robinson's scientific papers and comment on them for us. (From the philosophical standpoint, I can only wonder, with Einstein, whether it is possible to change quantum mechanics in any fundamental way from *within* the formalism.)

Now it is necessary to delve into the philosophical weaknesses which plague the paper and detract from its usefulness. One example is that his definition of theory fails to distinguish clearly between scientific laws and their interpretation. But the main source of confusion concerns the meaning of reductionism, a topic to which I will restrict my remaining comments.

Robinson defines *reductionism* as more or less equivalent to *dialectics*. He equates reductionism, for example, with the "unity of matter." I think there may be two principal sources for this confused definition. One has to do with how physicists and other scientists tend to misuse the concept of reductionism, the other with how proponents of OQM have misused the concepts of dialectics.

To philosophers, *reductionism* refers to the Machist view that theories can always be reduced to (or represent) merely the organization of sense perceptions. In this view, theory would not constitute some new, qualitatively higher level of knowledge, nor would it be a means for discovering new realities in the world. Since it leads only from sensation back to sensation, theory, to the Machian reductionist, represents merely some fictional or heuristic mechanism, entirely imaginary.

Among scientists, the term *reductionism* has acquired a specialized meaning as the hypothesis that any system can be explained in terms of its fundamental elements, i.e., the whole can be explained through knowledge of its parts. This idea was firmly refuted in a paper, referenced by Robinson in another context (Note 27), where Nobelist Philip W. Anderson says:

The main fallacy in this kind of thinking is that the reductionist hypothesis does not by any means imply a "constructionist" one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less those of society. [*Sci.* 177: 393; 1972.]

Needless to say, Marxism rejects both forms of reductionism as one-sided and undialectical. While it is true that we can always gain new insight on a system through more understanding of its component parts, still, to understand the system itself we must study the higher-level laws governing the system as a whole, laws which are qualitatively different from the lower-level laws governing isolated parts. Max Robinson does tend to recognize something of this dialectical quantity-quality relation-

ship, though he insists on referring to it as a "reductionist" relationship and thereby introduces his own idiosyncratic form of philosophical confusion. (See esp. his Note 24.)

Robinson also willfully distorts Marxism by his lumping Frederick Engels with Bertrand Russell and Mario Bunge, going so far as to portray Bunge's philosophy as "dialectical materialism under another name" (Note 17), and this despite the fact that ex-Marxist Bunge devoted a chapter of his recent *Scientific Materialism* to disputing the usefulness of Marxist dialectics in science. Briefly, I would characterize Bunge's main thesis as that of reducing all scientific theory to formal axioms, not recognizing "that axiomatic structures can never lead into the new and hitherto unknown, that they are, precisely because of their logical coherence, quite unfruitful" [R. Harré, *Ency. Philos.* vi, 291]. Obviously, Robinson shares Bunge's *reductionist* misconception that the human mind (or a scientific study) can proceed fruitfully and efficiently only by means of rigorous formal logic. Even for Marxists, it takes serious effort to overcome the prevailing prejudice (and ignorance) concerning the *informal logic* of Marxist dialectics, and thus be able to recognize that creativity involves a dialectical process of discovering new relationships and new levels among the infinite forms in which the motion of matter is manifested, a process in which deductive logic is necessary but far from sufficient.

It may also be that some of Robinson's prejudice against Marxist dialectics stems from the misuse of dialectical concepts by proponents of OQM itself, a misuse which the late Alfred Landé termed "dialectical idealism." In fact, Robinson seems to be telling us so in the last sentence of his text which refers to the need for avoiding the obscurantist trappings of OQM "even when camouflaged in Hegelian jargon." We can all agree with him on this. In my opinion, the current efforts to justify the concept of "statistical causality" by resort to Marxist terminology also provide an example of just such misuse (see S&N: #3, 22-40; #4, 67-70; #5, 66-80).

Let us hope that Robinson will learn to discriminate between the concept itself and the possible misuse of it, so that the baby will not be thrown out with the bath. No amount of quoting Marx and Engels on jargon can change the fact that these two pioneers were Hegelian dialecticians before they became materialists, and their mode of thought remained always that which they learned from Hegel. As Karl Korsch pointed out, "The sense of their materialism is distorted in a disastrous and irreparable manner if one forgets that Marxist materialism was dialectical from the very beginning. It always remained a historical and dialectical materialism, in contrast to Feuerbach's abstract-scientific materialism and all other abstract materialisms, whether earlier or later, bourgeois or vulgar-marxist." [*Marxism and Philosophy*, New York and London, 1970. pp. 76f.]

Tappan, New York 10983

The Scientist As Marxist Philosopher — — — — —

Dialectical materialism is not a substitute for the rigours of the scientific method. It enters into science to point the way towards what is to be discovered . . . more concerned with the strategy than the tactics of scientific advance. . . . The good Marxist should be able to see more clearly, avoiding the preconceptions and conventional views that prevent people seeing what is under their noses.

—J.D. Bernal, S&N #3, p. 43.

*A Key Step Is to Understand
Causality at the Microlevel*



On Problems of Integrating the Natural and Social Sciences and the Arts

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EDITOR'S NOTE. *By helping define the problems of integrating all human knowledge, this thoughtful paper poses some challenging questions for natural scientists as well as other workers in the sciences and humanities. It is adapted from a 20-minute contribution to a roundtable discussion organized by Erwin Marquit at the World Congress of Philosophy (Montreal, 25 Aug. 1983). Though Narski's themes are far too ambitious to be dealt with adequately in such a brief and skeletal exposition, they should serve here to initiate discussion in the course of which the concepts can be developed more fully. [Explanatory notes by the editor are inserted in brackets.]*

To begin, we have the ontological unity of the world in which we live, a unity of existence which is plainly revealed in the interactions and interrelationships we observe in this world. Then, reflecting this unity of existence, we have the tendency for the natural and social sciences to take a common approach in the development of knowledge (actually, a tendency for their unification). This tendency has grown stronger in our era of scientific and technological revolution. It seems reasonable to conclude that this tendency will flourish in the future.

It seems equally reasonable that the historical approach to knowledge will flourish in all fields of science, along with the growing struggle to master all realms of human activity and cultural life by the application of scientific principles to inquiry and practice. What such a process of development implies was shown long ago in the pithy if symbolic statement by Marx that in the future "the only" science will be that of history. (Such an assertion, of course, is not to be taken out of the context in which Marx made it.)

As it increases in its scope, science in its *historicist* form also penetrates into the arts. For the realm of the emotions, Hegel developed a thesis that, from the universal point of view, at the peak development of self-awareness of the absolute, it is not art itself that one must consider but rather the philosophy of art, meaning that only the *science* of art will have lasting value for mankind (we know indeed that for Hegel only philosophy, in the

full sense of that word, is to be considered a science). According to Hegel, *belles lettres* could be written programmatically, on a scientific basis, though he considered that such an approach had been completely ignored. Actually, attempts along this line were present, but only in the bud and hence not easy to observe. For example, Hegel was not aware of the effort by the realist Emile Zola to write in this fashion. And Balzac's book *La Peau de Chagrin* was highly valued by Goethe but not by Hegel. Whatever the assessment of Hegel's idealist exaggerations, he was quite reasonable and correct in his formulations on art: without connections to science and dissolution into science, he said, the art of the future cannot flourish but must perish.

IN THE REAL WORLD, of course, one never finds such unification tendencies isolated or unopposed. As the result of *contrary* tendencies that lead to new and different events and manifestations in our unified but *inexhaustible* world, one finds everywhere the relative independence of things, processes and phenomena, occurring at different levels of reality which cannot be reduced to each other.

Similarly, in the world of knowledge, there appear, at one and the same time, two opposed processes. One is the process of integration of the sciences. The other is the process of differentiation and penetration into new and different regions of knowledge. New sciences arise, and some of them tie together existing branches of knowledge (complex theories and such) which previously existed only apart from each other. As examples of simultaneous integration and differentiation of science, I can mention bionics [the development of nonliving systems that function like living systems], general systems theory [concerned with the goal of unifying science], and mathematical logic, a qualitatively new region of specific lawfulness and incredible wealth, discovered in the boundary region between two previously existing sciences.

Can one then say that differentiation of man's culture enriches while its integration impoverishes? No, this thesis would be one-sided, for the unification of the sciences reveals their deepest coherent relationships. For example, in discovering historical materialism, Marx revealed not only the gross differences between society and nature but also their deep interrelationships.

In addition to the natural and social sciences, we have the technical, agricultural, biomedical, and other applied sciences which also play a big role in culture today. The reciprocal interactions of all these sciences are highly complicated. On the one hand, the applied sciences connect the natural and social sciences together but, on the other hand, all the specifics of man's practical activities manifest themselves most strongly in the applied sciences and this creates a separation of the applied sciences from other fields of knowledge.

Now, in considering how the social sciences fit into this whole intricate process, I have developed the thesis that there occurs a special cooperative interaction between the sociological sciences and what I term the humanitarian sciences proper. By the latter, I refer to sciences dealing with the spiritual world of man—art theories, cognitive psychology, theoretical ethics (in contrast to metaethics which developed essentially as a sociological discipline), and so forth. According to my preliminary assessment, at present the process of differentiation will predominate in the sociological sciences, and only later will the overall tendency to unification with the

humanitarian sciences gain the upper hand. But I think this lack of unity between the sociological and the humanitarian sciences is not due to any kind of *incommensurability* between them, in the sense of Thomas Kuhn or Paul Feyerabend. Why then do the indicated tendencies occur?

AS I SEE IT, we have here two different problems which are intimately tied together. *The first problem*: As science seeks to investigate more deeply the emotional world of man, might it not prove impossible to completely reduce the emotions to detailed objective analysis? And might this not destroy the human essence? The Soviet psychologist P.W. Simonov has demonstrated in many ways that the emotions are necessary for the self-confidence of a person as well as to make up for any lack of needed knowledge, i.e., the emotions are necessary to activate the functioning of a person (manifested in unrest, anxiety, and so forth). And, if the knowledge is more or less sufficient, the emotions (excitement, alertness, passionate desire, confidence of success, etc.) permit the person to apply the available knowledge in an efficient way. While it is necessary to investigate these various affects and forms of excitement in a scientific manner, this does not mean that the emotions can be replaced by scientific concepts. And the essentially spiritual sciences (the arts, literature, morality, and so forth) would be vulgarized by reducing them completely to the sociological sciences; see, for instance, Lenin's comments on Shulyatikov who tried in a gross and vulgar manner to reduce the history of philosophy to that of class structure and changes in society (*Philosophical Notebooks*, pp. 486-502).

The treatment of the emotions in terms of information theory, developed by P.W. Simonov in numerous publications and in his book *The Emotional Brain* (Moscow 1981, in Russian) cannot, by and in itself, definitively handle the question of the prevailing divergence between the social sciences and the spiritual sciences.

The second problem concerns the existence of *freedom* in the philosophical sense. Now and then one hears that Marxism has fully reduced the essence of the phenomenon of freedom to the formula of Spinoza, Schelling and Hegel, namely, that "freedom is the recognition of necessity." This formula is not correct because it does not lead us out of the domain of fatalism.

Without man's freedom (within the limits imposed by the laws of inorganic nature), theoretical and normative ethics are impossible—because of the impossibility of duty, guilt, etc. And here I cannot agree with Professor Simonov when he says that it is sufficient for this purpose that man or mankind have the *illusion* of freedom of will and decision. He writes:

Rejecting the recognition of freedom of choice would mean a fiasco for every moral or ethical system. This is the reason why, in evolution, that part of man's motivation which produces behavioral activity remains hidden in the subconscious and manifests itself through the illusion of freedom of choice. The feeling of this freedom, and the personal accountability stimulated by the apparent freedom thus manifested, forces man to analyze many times on many sides the consequences of any kind of activity and this gives a motivational basis for the proper choice. [P. Simonov and P. Jeršov, "Desire and Consciousness," *Nauka i Zisn* 8: 72; 1983.]

The authors turn here to Kant's treatment of regulative ideas ("Behave yourself *as though* [als ob]" . . . and so forth), even though, concerning the question of free will, Kant recognized unconditionally the will as a

component of the noumenal [non-phenomenal] world for ethical purposes. Simonov's thesis does not solve the problem of how we are to act *when we know* that our freedom of will is an illusion!

THE PROBLEM of the relationship between necessity, chance, and freedom is one of the most complicated but also one of the most pressing problems of philosophy, and it is small wonder that this problem stands at the very center of Marxist discussion.

The Marxist solution to this problem, standing opposed to fatalism as well as to voluntarism [the concept of will as the dominant factor in the world], brings within the scope of the problem the infinitude of causal-acting properties of matter in the universe (mega-, macro-, and micro-worlds). If all causal chains of events in the world passed through a single starting point, and all properties of the microworld had definite lower limits, we would have arguments for fatalism since, by discovering this initial point of world development and by its measurement, one could foresee the future with absolute precision. But such assumptions are false.

The young Marx, in his doctoral dissertation (1841) directed his attention to the ontological infinitude of existence in considering Epicurus' concept of "declination" in atoms [i.e., departure of their motion from a straight line, a swerving effect attributed to *intrinsic* properties of atoms]. Marx wrote that this behavior of atoms is connected in some way with man's situation in the struggle for liberty, and is incompatible with fatalism. Modern physics, in considering the problem of *chance* in the microworld, rejected the assumption of causal relationships producing direct determinate effects; we have a whole series of statistical regularities in the subatomic realm which, from a superficial study, may also seem to manifest "freedom of will" in their properties. [The uncertainty relation discovered in quantum mechanics has been used as the basis for much mystifying interpretation, some scientists and philosophers even seeing it as the basis for resolving the conflict between the (bourgeois) doctrines of free will and determinacy.]

Naturally, there is no *freedom* (in the human sense) to be found in the subatomic realm. One can realistically contend that the essential connection between "freedom" and chance in the microworld cannot be reduced to a statistical phenomenon but must have an underlying dynamic basis (or perhaps *bases*, in *endlessly receding levels*, recalling here that Lenin in *Materialism and Empirio-Criticism* put forth the thesis of the inexhaustibility of the electron as well as the atom). I think that the function of further physical investigation is not merely to find the relationship of necessity and chance at the statistical level but rather to search for the dynamical laws operating at lower levels of the microworld. [For a relevant debate on the nature of causality in quantum physics, see *Science and Nature* #3, 4-21; #4, 67-70; #5, 66-80.]

We can hardly deny that such dynamic causal relations are manifested very clearly today in the macro- and megaworlds, *yet these relationships are not fatalistic* because of the infinity of interconnections and interrelationships in the universe of mega-, macro- and microworlds (revealed in geology, astrophysics, etc.). Since the world is an indivisible unity and all its aspects are causally bound together (even though they cannot be reduced to each other), these dynamic causal connections are not mainly of a direct fatalistic nature (see I.S. Narski, "The Delineation of the Category 'Chance.'" *Filosofskie Nauki* 1: 50-53; 1970). The social world in all

of its specifics provides no exception to this materialist point of view (and don't forget the unity of micro- and macroprocesses in the human brain!).

V.I. Lenin in 1904 recognized a certain degree of freedom of choice within a determinate range when he wrote:

We cannot get outside of the bourgeois-democratic boundaries of the Russian revolution, but we can vastly extend these boundaries. (*Collected Works*, Moscow 1969, ix, 52.)

And decisions on such extensions (which represent, so to speak, the *second* step of the decision process, where the *first* step involves decisions on capability or lack of it) provide a basis for relative approximations in social and cultural prognosis and prediction, so that when we want to estimate the future we must take into account "all possible, and even all *conceivable* combinations" (*ibid.*, Moscow 1961, vi, 460). The phenomena of nonlinear or indirect causal connections have social and individual aspects which are not completely reducible to each other.

If science in the future is truly able to solve the problem of the causal description of these phenomena *at the microlevel of the psychophysical processes* and, in general, to establish the connections between objective knowledge and the social aspects of these phenomena, then we will have the beginnings of a final process for a common approach and ultimate unification of the social sciences with the humanities. But the key step toward this goal is for natural science to solve the problem of the inner coherence of determinism at the microlevel and its relationship to the relative freedom of human actions. Only then will the integrating tendencies predominate in the development of all knowledge. □



The Mystics Were Quick to Find "Freedom" —

It is being asserted widely that science has at last abandoned its early materialist taint, swinging strongly toward religion and mysticism. The furor started small in a corner of physical science. It was discovered a few years ago that things as small as electrons do not individually obey the law of cause and effect. This phenomenon is known as the principle of indeterminacy, or the principle of uncertainty, and its discovery came about when the German physicist Werner Heisenberg showed that it is impossible to ascertain both the position and speed of an electron; we can ascertain the one or the other singly, but not both. Evidently, then, the behavior of an electron is indeterminable. Right here a number of thinkers made false deductions and, as A.S. Eddington put it, "science went off the gold standard." What these thinkers failed to grasp was that mere indeterminability does not in itself establish indeterminacy. A thing may be indeterminable but not indeterminate. Nature knows what she is doing, and does it, even when we cannot find out. It did not take the mystics long to discover the principle of uncertainty. If we could no longer predict, at least in theory, the entire future of the universe, given the position and velocity of every particle in it, then perhaps there was freedom in it after all. The return of science to some sort of modern mysticism would be essentially a slip in man's hard-won progress away from one of his most ancient bad habits—that of ascribing to the supernatural whatever he did not yet understand.

—Fifty years ago in *Scientific American* (October 1933).

The Subjective and Objective in Higher Brain Function

Alexander R. Luria, *Language and Cognition*. Wiley, 1982. 264 p. \$25.95.

What could be a more fitting contribution to the advancement of our knowledge of dialectics and of materialist psychology than the study of higher order brain processes? Luria's last book before his death, his *opus vitae*, is an elegant example of such a contribution by the world's foremost neuropsychologist.

The first chapters are a brief summary of Luria's view of the most important Marxist general concepts which are relevant to his field of inquiry. This comes as a surprise to readers familiar with Luria's previous English language publications, where he had always refrained from explicitly developing Marxist concepts. In his recent autobiography (*The Making of Mind: A Personal Account of Soviet Psychology*, Harvard Univ. Press, 1979) we learned that Luria felt he did not possess Marxist theory well enough to presume to claim direct guidance from it. Indeed, we find that Luria never refers to an intellectual source or influence except in terms which are very much integrated into his own outlook or synthesis of the scientific "facts" available to him. In the book under review Luria chose to emphasize and integrate four Marxist concepts which form the kernel of his own very personal, life-long orientation:

1. *The study of cognition, and hence of science, is not so much a study of things in and of themselves, as it is the interrelationship among them.*
2. *Consciousness, a central object of theory and of scientific psychological research, is in the last instance a product of social relations.*
3. *Consciousness concerns how humans reflect the real world in which they live (i.e., how they form a subjective image of the objective world).*
4. *Psychologists must not be content with description of the psyche, but must seek to explain its emergence on the basis of observation, experimentation, and reason.*

Luria starts out by theorizing that the first forms of communicative utterances must have been limited to situation-bound (sympractical) social relations created by the initial divisions of labor (as Engels had surmised). Sympractical words acquire their meaning from concrete, practical situations or activities. He then explains the appearance of codes, or "abstract" (synsemantic) language, as a liberation of the information-bearing aspect of the utterance from practical immediate situations. Luria states that the synsemantic word viewed ontogenetically, instead of designating virtually everything and anything—as do the child's first sympractical utterances—designates determinate (restricted) objects, actions, properties, and eventually, relationships.

The middle portion of the book is an overview of Luria's many theoretical contributions to the understanding of language development and language function—always masterfully intertwined with numerous references to foreign and Soviet empirical research results, much of which he col-

lected himself. In the last two chapters, Luria applies these findings to the study of focal cortical and subcortical lesions in humans, bringing to bear his unique integrations of these multiple research avenues.

An example is found in Luria's very explicit opposition to a traditional (and still very much alive) trend in Russian neurophysiology and psychology, namely, reflexology. In his discussion of the emergence of voluntary acts he argues that the prehensile or grasping reflex cannot be construed to be the prototype for future voluntary movements. Interestingly, it is by basing himself on Pavlov that Luria argues that voluntary action requires that the grasping reflex and other subcortical reflexes be inhibited for cortical control of voluntary acts to occur. The important stake in this confrontation is not a question of neurophysiological detail, it is between biological reductionism (reflexology, positivism, etc.) and the cultural-historical or Vygotskian approach. As Vygotsky had enunciated, and as Luria later helped to demonstrate in detail, the child develops self-regulation of behavior through relating to the significant adult(s) in his/her life. The emergence of this psychic ability is mediated and determined essentially by language, conceived of—in opposition to the somewhat naive conception of the youthful Piaget—as adult-child communication. Piaget believed the child begins with egocentric speech and that his language becomes socialized only subsequently. In reality, the child learns to control his/her own behavior by imitating instructions formulated by the adult. Luria did a great deal of research to explicate the details of this process. His most important contribution was not simply to confirm and expand Vygotsky's ideas. Using clinical methods as well as his vast and detailed knowledge of the modern neurosciences and linguistics, Luria proceeded to develop an empirically based theory of the brain mechanisms of language acquisition and language disruption in their connections with many other fundamental psychic functions such as motivation, memory and self-regulation. He found, for example, that the ability to self-regulate behavior by means of speech is selectively impaired in certain cases of frontal lobe lesions.

MANY EXAMPLES of ideas summarily enunciated by Vygotsky and researched developmentally, experimentally, and neurologically by Luria could be given. An important instance is Vygotsky's discovery that language acquisition proceeds from 1) affective communicative single-word utterances, to 2) external primitive verbal thought, both communicative and uncommunicative, to 3) abbreviated internalized speech, to 4) expanded external communicative speech. The ontogenetic developmental scenario can be summarized as follows. When children begin to utter spontaneous words, these represent pure affective needs addressed to the adult. With the first phrases children can be observed speaking out loud to themselves even in the absence of interlocutors, especially when confronted with problematic situations. Such "egocentric" speech is typically used by the child to describe the situation or to plan his/her actions. The child exploits the regulative potential of speech to adapt to new situations or achieve implicit goals. In a third phase, children tend to condense such "egocentric" utterances grammatically and to gradually whisper them, or mimic them with lip movements. (Many adults never cease to manifest pharyngeal muscular activity during internal speech.) Finally, the condensed internal speech, in turn, serves as a starting point for both expanded communicative external speech and inner private speech (thought). The early structure of a speech utterance plan includes a

“theme” (that which the speech will be about) and a “rheme” (the new information in the utterance). So-called “spontaneous” or voluntary speech requires a restructuring of the internal utterance plan (or “initial semantic graph”) to a social form which is more expanded and less idiosyncratic than its original internal form. Luria’s unique contribution to this field was to discover that focal cortical lesions may selectively impair the ability to formulate internal speech programs (left post-frontal or left frontotemporal lesions) or the ability to formulate the phonological, lexical, semantic programs necessary to execute such initial speech utterance plans (left parietal lesions). Furthermore, he analyzed with great sophistication the intricate relations between the various cortical and subcortical brain structures, the levels of speech production, and the functionally related non-linguistic psychic functions such as motivation, arousal, attention, memory, etc.

ANOTHER OF LURIA’S important contributions to our understanding of the neuropsychology of language consists of his discovery of the remarkable applicability of Jakobson’s concepts of “syntagmatic” and “paradigmatic” organization of speech to neurodynamics. “The paradigmatic organization of language is concerned with the way that a given element of language is included in a system of oppositions or hierarchical systems of codes.” This general principle applies in phonetics (every sound is opposed to another) and in lexicology (each word can be opposed to another, and each word can be placed in a class which is opposed to another class, and a more inclusive class which is again opposable to another class). “According to the syntagmatic principle, what organizes an utterance is not a hierarchy of oppositions, but the transition from one word to another.” The basic form of syntagmatic organization is the subject-predicate pair. Luria was the first to observe that patients with inferior pre-frontal lesions may manifest intact paradigmatic speech organization and yet be profoundly deficient in the syntagmatic aspect of speech production (telegraphic elocution, loss of kinetic melodies). On the other hand, patients with left temporal lobe lesions may be capable of producing smoothly flowing speech but be severely impaired not only phonologically but also in the nominative aspects of speech (i.e., they confuse like-sounding words and within-class designations, i.e., they produce paraphasias). Left occipito-parietal lesions also result in paradigmatic deficits characterized by semantic and word-finding deficits. Luria’s neuropsychological findings suggested to him the important psycholinguistic rapprochement between internal and syntagmatic speech and higher motor function on the one hand, and elaborated (expanded) paradigmatic speech and gnostic function on the other.

Another idea originally enunciated by Vygotsky and then researched developmentally and neuropsychologically by Luria is that of “levels” in text comprehension. A first superficial (formal) comprehension level of a text consists of grasping the (literal) *meaning* of the text. The second level consists of grasping the *sense* or *subtext*—the underlying universal or wide sense of a metaphor or proverb for example (it is to be noted that Luria believed that any text has a subtext). The third level consists of grasping the deep motive (a somber metaphor may express, for example, a deep motive of sadness, bereavement, guilt, loneliness, etc.). Luria discovered that certain focal cortical lesions (frontal) selectively affect the ability to comprehend the subtexts and motives underlying stories that are read to,

or by, a person. Luria noticed that the comprehension of subtexts and motives is not primarily an intellectual function. It is more a function of a person’s emotional sensitivity.

INDEED the whole psychopathology of the frontal lobes—a field which was pioneered nearly exclusively by Luria and his immediate colleagues—illustrates the subtlety which is required in neuropsychological investigation. In 1939, Canadian neuropsychologist D. Hebb discovered that numerous intellectual functions are left intact after frontal lobe damage. Luria’s theoretical explanation of frontal lobe function explains why we should expect such patients to be able to generate brief appropriate responses to the questions and tasks which comprise an IQ test, and yet also why such patients are, socially, among the most handicapped of all the categories of victims of focal cortical lesions.

Luria mentions several times in his book that neurolinguistics is a science which is in its very early youth. The sophisticated linguistic analyses (for a psychologist) which Luria contributed to the study of focal brain lesions in humans illustrate the important fact that language disturbances are not just a compendium of bizarre speech behavior. Luria’s genius was to look behind the symptoms at the functional systems underlying complex behavior from the vantage point not of a narrow-minded taxonomic clinician but of a cognitive psychologist, a linguist, a neurologist, a developmental psychologist, an experimental psychologist, and a Marxist.

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Was Humanity Born in Labor?

Charles Woolfson, *The Labour Theory of Culture: A Re-examination of Engels’s Theory of Human Origins*. Routledge & Kegan Paul, 1982. viii + 124 pages. Paper, \$12.95.

The 19th century encompassed some of the greatest advances in the history of biological science. The predominant scientific view of the realm of life in the 1830’s tended to emphasize the fixity of species, the rhythmic changes overlying a basically stable system of the biosphere. The anticipations of a more dynamic conception of nature such as those of Lamarck or J.W. Goethe remained just that, anticipations. Marx and Engels received their education during this pre-modern period, a period of biological thinking summarized by the greatest German idealist philosopher, G.W.F. Hegel’s *Philosophy of Nature* (1827/1830): “Man has not developed himself out of the animal, nor the animal out of the plant” (§339 *Zu. 2*). Despite suggestive remarks by Marx and Engels against Feuerbach regarding the historicity of nature in its symbiotic relationship with mankind (*The German Ideology*), their understanding in the 1840’s of biological science reflected the predominant view.

But the advances of natural science, spurred on by developing capitalism itself, soon swept away the static conception of nature and the biosphere. One of the most shattering developments was the dissemination of the theory of biological evolution in the late 1850’s by Charles Darwin and, independently, by Alfred Russel Wallace. This historical theory (and

the complementary theory of genetics developed by the Austrian Gregor Mendel, revealed to the world only in 1900) disclosed the materialist kernel of modern biological science.

The significance of the theory of natural selection was not lost on Marx and Engels. *The Origin of Species* was published toward the end of November, 1859; Engels wrote to Marx as early as December 12 recommending Darwin's book on the "historical development of nature." Both Marx and Engels recognized the contribution to their thought from scientific advances such as the theory of natural selection, and came to discount somewhat their early writing in the *German Ideology*. In the 1888 "Foreword" to his *Ludwig Feuerbach*, Engels acknowledged that their earlier understanding of historical materialism was "incomplete." Marx in the 1859 "Preface" to the *Critique of Political Economy*, pointed out the great pedagogical value which those earlier texts retain to the present day: "self-clarification." In part, of course, the earlier writings in the *German Ideology* were discounted because of Marx's and Engels' own researches during the 1850's and 1860's.

THE 19TH CENTURY encompassed some of the greatest advances in the history of the social sciences as well. Just as capitalist development generated contradictions which promoted scientific advances (such as Louis Pasteur's work on wine fermentation, silkworm parasites, etc.), so it also generated contradictions which promoted scientific advances in the social realm. The corrosive effects of capitalism upon domestic life and the family indicated that *all* institutions of antagonistic society were transitory. The gathering evidence from the global expansion of capitalism suggested the viability under specific social conditions of a variety of kinship structures and domestic institutions other than the patriarchal forms. A major scientific advance in this area was Lewis Henry Morgan's *Ancient Society* (1877), itself an historical materialist account of the evolution of primitive social forms (1). This book was deeply appreciated by both Marx and Engels. Marx carefully analyzed Morgan's work in his 1881-2 *Ethnological Notebooks* and Engels utilized these notebooks as well as *Ancient Society* in his own writing.

Thus Marx and Engels' early writings were supplanted by Engels' mature writings, including the *Anti-Dühring* (1877), the *Origin of the Family, Private Property and the State* (1884), and the posthumous publications, *Dialectics of Nature* and "The Part Played by Labor in the Transition from Ape to Man." On the one hand, these works incorporated into historical materialism the 19th century advances in natural and social science, and thus were more "complete" than the early writings. (Of course, these publications did not *complete* historical materialism, nor did Marx and Engels ever suppose they did.) On the other hand, these works jointly provide a comprehensive statement of historical materialism, setting the terms for our own philosophic and scientific researches.

PARTICULARLY IMPORTANT, and often overlooked, is the contribution made by the Engels essay, "The Part Played by Labor in the Transition from Ape to Man," which provides a historical materialist theory of the emergence of *Homo sapiens* as a distinct species with a unique potential for further socio-cultural development according to its own laws. It is this new mode of evolution which is addressed by Charles Woolfson in *The Labour Theory of Culture*.

Woolfson's brief but excellent book provides an insightful and comprehensive review of Engels' theory of human origins in the light of modern anthropological, palaeontological, and linguistic research. He provides a timely and useful supplement to the important work of those scientists, such as Nancy Tanner and Adrienne Zihlman, who are reclaiming the hitherto overlooked role of women in early human society (2). Also, in contrast to some bourgeois feminists writing on this topic, who follow Claude Levi-Strauss into the cul-de-sac of eclecticism and, ultimately, incoherence, Woolfson demonstrates both the feasibility and the significance of a scientifically rigorous "labor theory of culture."

By limiting the scope of his study, Woolfson is able to carefully treat the various aspects of the transition from ape to human at a theoretically appropriate level of discourse. First he presents Engels' analysis recorded in "The Part Played by Labor in the Transition from Ape to Man" and shows how this articulates with Marx's and Engels' major writings. Darwin's researches were clearly signals for Engels' own study of human origins, yet Darwin stressed the continuities between ape and human, while Engels, with a much clearer conception of the significance of human labor, dialectically relates these *continuities* to the qualitative *discontinuities* in the transition.

Then Woolfson reviews the fossil record from *Ramapithecus* through *Australopithecus* to *Homo* and finds that tool-making culture had most likely emerged prior to cranial expansion, the pairing family, etc (3). Thus the "family" as a social institution (i.e. the nuclear family) is correctly viewed as neither the *initiator* of the transition from ape to human (social labor came first) nor as the "building-block" of society then or now, as Marx and Engels had already noted in the *German Ideology*. Contrary to the pro-familistic doctrines of writers such as C. Owen Lovejoy, the family is (and was) a *subordinate* institution, subordinate to economic and other social institutions and processes. Woolfson next devotes a chapter to foraging societies (somewhat inappropriately but popularly called "hunting and gathering" societies) and finds, in contrast to the "killer ape" doctrines promoted by Robert Ardrey *et al*, that this foraging "way of life" was conducive to cooperative labor and social harmony among hominids on their way to becoming humans (4).

In the fourth chapter, Woolfson shows how anthropoid apes can neither make nor use tools in the way that is characteristic of humans. A crucial human trait is the cultural transmission of tool-fabrication and use; this implicates the use of language to facilitate the "displacement" of the subject from the time and place of the object of discussion. Displacement permits the accumulation of experience of labor-practice, not simply within a life-span but across generations.

In the next chapter, Woolfson reviews the studies of anthropoid communication (e.g., the language mastery possessed by the chimps Washoe, Sarah, and Nim), and demonstrates that there is a difference in kind between anthropoid communication and that of the human adult. In this context, he shows how sociobiological reductionism fails to comprehend the role of human language.

THE TOPIC OF LANGUAGE ORIGINS is addressed in Chapter 6, with analyses of the theories of G.A. De Laguna, M.F.A. Montague, Ludwig Noiré, and Geza Révész which provide corroboration for Engels' thesis that lan-

guage was born of the necessities of social labor (5). In contrast to various idealist explanations of language origins, explanations which Woolfson indicates ultimately beg the question, these non-Marxist writers have recognized the necessity of an extra-linguistic and extra-symbolic moment in order to explain the emergence of human linguistic behavior.

In the final chapter on "Labor and Culture," linguistic theory is assessed in light of 20th-century Soviet psychological research. Woolfson discusses the Pavlovian conception of environmental stimuli as the content of a "first system of signals" for a "passive" organism, and speech as the content of a "second signalling system" whereby human *actors* may symbolize and thus consciously plan and modify their own collective and individual behavior (6). Woolfson discusses how these Pavlovian insights were further developed by the work of Soviet scientists A.R. Luria and L.S. Vygotsky into a systematic understanding of human consciousness through the interrelationship of tools and symbols, labor and language (7).

Thus Woolfson's reconsideration of Engels' essay in the light of contemporary research carries us through the hominid stage to the dawn of human society in its pre-antagonistic mode of existence, a transition which Charles Hockett and Robert Ascher referred to as the "human revolution" (8). It is, of course, only the first act in the drama of human social development, since private property is waiting in the wings. Indeed, the Engels essay was to have been the introduction to a book he was writing on *The Three Basic Forms of Slavery*, unfortunately never completed

Human Culture: Two Evolutionary Views — — — — —



Frederick Engels

TOOL-MAKING AND SPEECH provide the twin foundations for all subsequent development of human culture. Appreciation of the environment, the seasons and the habits of prey, an understanding of materials and processes for manufacture of tools to accomplish various purposes, all these served for continued expansion of specifically *human* action as mankind increasingly brought both the external world of nature and its own activity under conscious control.

"In short," says Engels in his essay, "the animal merely *uses* external nature and brings about changes in it by its presence; man by his changes makes it serve his ends, *masters* it. This is the final, essential distinction between man and other animals, and once again it is labour that brings about the distinction."

This general process of mastery is what we call *culture*. It consists of material achievements accumulated through social practice and the social and spiritual achievements conditioned by them. At any stage of historical development, culture provides the measure of man's humanization, the degree to which he has separated himself from his animal origins. In effect, Engels proposed a "labour theory of culture" that clarifies what is unique in explaining the possibilities for human development. For Marx and Engels, realization of these possibilities would come only with con-

(9). (The identity of these forms can be inferred from his *Origin of the Family* where he discussed the "three great epochs of civilization" characterized by the "three great forms of servitude," viz slavery, then serfdom, and finally wage labor (10).

WOOLFSON'S TREATMENT of the transition period is in the best Marxist tradition of scholarship. His introduction emphasizes that empirical research achieves importance only through its interpretation, pointing out that the achievement of Engels was to reinterpret existing theories "within the perspective of dialectical materialism." In his conclusion, Woolfson acknowledges that his own arguments, like Engels' original essay, are incomplete in several respects and that the framework of dialectical materialism, within which his interpretations have been made, "provides no ready-made formula for the solution of any given problem, particularly a problem of such complexity as the origins of humanity." But Marxists and non-Marxists alike will find that his philosophical outlook provides the basis for a thoughtful and perceptive study of significant questions concerning how we evolved into human beings.

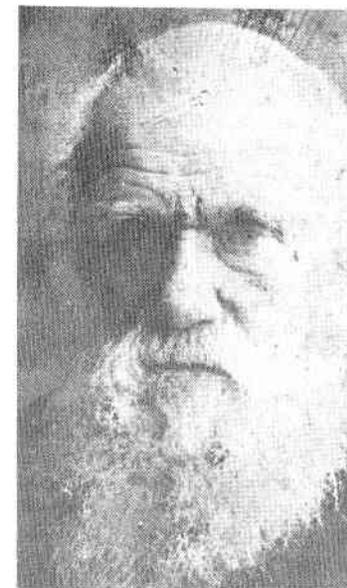
In fact, more such books are urgently needed in the English language. Historical materialist studies of human origins and social evolution must be undertaken to counter the rising tides of "creationist" confusion (11). On the one hand, we have religious "scientific creationists" (D.T. Gish, H.M. Morris, and their likes). On the other hand, we have an analogue in

sciously planned organization of social production in a future society freed from the distorting effects of exploitative modes of production.

Darwin had a much more limited understanding of human mastery over the environment. Lacking a clear concept of social labor, he could not identify precisely what qualitatively new characteristics could account for the phenomena of human evolutionary development. Seeing only quantitative and gradualist aspects of this process, he denied there was any fundamental difference in mental faculties between man and higher mammals, i.e., the difference consisted "solely of man's almost infinitely larger power of associating together the most diversified sounds and ideas. . . . the difference in mind between man and the higher animals, great as it is, certainly is one of a degree and not of a kind." [*Descent of Man*, 1930, pp. 95, 104.]

This essentially idealist proposition is still propagated widely today, though in more sophisticated form. Continuity and quantitative change are stressed at the expense of discontinuity and our qualitative differences from other species. In this way, the role of labor in defining human uniqueness remains hidden from view.

—Charles Woolfson, *Labour Theory of Culture* (pp. 8-9, condensed excerpt).



Charles Darwin

patriarchalist “social science creationists”—who might well pontificate that “from the beginning, as Aristotle, A. Comte, and T. Parsons affirm, there was the patriarchal nuclear family . . .”. This patriarchalist theory, which reigned in Western social science for the first half of our century, is being confronted by an increasing number of excellent studies which can be understood as contributing to the further scientific development of Engels’ thought on human prehistory. It is important that these studies be reviewed and synthesized along the lines of Woolfson’s book.

No less urgent is the wide public dissemination and popularization of historical materialist studies of our human origins and development. On August 23, 1982, a major Mexican television network, in a prime-time broadcast, featured a book review of Engels’ *Origin of the Family*. That such a broadcast is quite unimaginable in the English-language world is an indication of how far we have to go.

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Notes and References

1. For an account of this field before Morgan made his contribution, see Elizabeth Fee “The Sexual Politics of Victorian Social Anthropology,” in Mary Hartman and Lois Banner (eds.), *Clio’s Consciousness Raised*. New York: Harper and Row (1974), pp. 86–102. Morgan intended to include a very important discussion of household architecture in *Ancient Society*; it was ultimately published as *House and House-Life of the American Aborigines* (1881). For a path-breaking account of the significance of this theme during the late 19th century, see Dolores Hayden, *The Grand Domestic Revolution*, Cambridge: MIT Press (1981).
2. See, for instance, Nancy Tanner and Adrienne Zihlman “Women in Evolution,” *Signs* Vol. 1 (1976), pp. 585–608; *idem*, “The Evolution of Human Communication: What can Primates Tell Us?” *Ann. New York Acad. Sci.* Vol. 280 (1976), pp. 467–480; *idem*, “Gathering and the Hominid Adaptation” in L. Tiger and H. Fowker (eds.) *Female Hierarchies* Chicago: Beresford Books (1978), pp. 163–194; also A.L. Zihlman “Women in Evolution, Part II” *Signs* Vol. 4 (1978) pp. 4–20; N.M. Tanner *On Becoming Human* Cambridge: Cambridge Univ. Press (1981) and the review essay by Elizabeth Fee “Woman’s Role in the Evolution of Humankind” *Science and Nature* No. 5 (1982), pp. 20–29.
3. C. Owen Lovejoy “The Origin of Man,” *Science* Vol. 217 (1981), pp. 341–350.
4. Robert Ardrey, *The Territorial Imperative*. New York: Atheneum (1966); *idem*, *The Hunting Hypothesis*. New York: Atheneum (1976).
5. See G.A. DeLaguna *Speech: Its Function and Development*, Bloomington, Indiana University Press (1929); M.F.A. Montagu “Tool-making, Hunting, and the Origin of Language,” *Ann. New York Acad. Sci.* Vol. 280 (1976), pp. 266–274; Ludwig Noiré *The Origins and Philosophy of Language* Chicago: Open Court Publishing Co. (1917); Geza Révész *The Origins and Prehistory of Language* London: Longmans Green and Co. (1956).
6. Ivan Pavlov, *Selected Works*. Moscow: FLPH (1957), pp. 651–652.
7. See A.R. Luria, *The Working Brain*. Harmondsworth: Penguin (1973); L.S. Vygotsky, *Thought and Language*. Cambridge: MIT Press (1962); *idem*, *Mind in Society* Cambridge: Harvard Univ. Press (1978).
8. Charles Hockett and Robert Ascher, “The Human Revolution,” in Y.A. Cohen (ed), *Man in Adaptation*. Chicago: Aldine Press (1968).
9. See Wilhelm Liebknecht’s June 10, 1876 letter to Engels.
10. Marx and Engels, *Selected Works*. Moscow: FLPH (1962), Vol. II, p. 324.
11. On “creationism,” see the historical study of Ronald L. Numbers “Creationism in 20th Century America” *Science* Vol. 218 (1982), pp. 538–544; and the landmark judicial decision of Wm. R. Overton “McLean *et al* versus The Arkansas Board of Education *et al*” U.S. District Court, Eastern District of Arkansas (Jan. 5, 1982), available from the Arkansas ACLU, P.O. Box 2832, Little Rock, AR 72203.

The Unfinished Revolution in Modern Physics

Abraham Pais, *‘Subtle Is the Lord. . .’: The Science and the Life of Albert Einstein*. Oxford University Press 1982. xvi + 552 pp.

For the Marxist who seeks to understand a theoretical structure in its historical development, Pais provides much food for thought concerning relativity theory and quantum mechanics. However, to grasp the subtle dialectics of Einstein’s work, the Marxist reader must bring to bear his/her own interpretive powers because Pais looks down his nose at philosophy: “special relativity . . . caused the inevitable confusion in philosophical circles” [page 27]. As is usual with such an attitude, Pais seems unaware of his own philosophical bent, characterized by a Humean or empiricist scepticism concerning objective reality, as in a prefatory remark that Einstein “lived by a deep faith—a faith not capable of rational formulation—that there are laws of Nature to be discovered” [v]. Though this outlook inevitably colors the interpretations offered, Pais is scholarly and thoughtful, contributing insights from his years of personal collaboration with Einstein at Princeton, candidly revealing his own position where he differs with Einstein, and generally placing matters in a clear historical context. Most important, Pais lets Einstein speak for himself a great deal. This review will do the same, concentrating on some philosophical highlights from the riches of the biography.

The most salient thesis of the book to this reviewer (though only an aside by Pais) is that “Einstein’s one truly revolutionary contribution is his light-quantum hypothesis of 1905 . . . he never believed that the physical meaning of the light-quantum hypothesis had been fully understood” [38n]. Considering that the paper initiated a whole series of revolutions in microphysics, that the concept of the light-quantum or photon was not taken seriously in the physics community until after 1923, and that his mysterious entity is even today not fully understood or fully recognized as a particle, there was prescience indeed in Einstein’s characterization of the 1905 paper, even before it was published, as “very revolutionary” [30].

By contrast, relativity theory did not challenge the basic ideas of physics in any manner comparable to that of quantizing the radiation field. Einstein in 1921 deprecated the idea that the new principle of relativity was revolutionary, maintaining that it was rather the natural completion of the work of Faraday, Maxwell and Lorentz, with nothing intentionally philosophical about it [30]. And that’s, in fact, how it was generally received, fitting smoothly into the mainstream of physical thought with little of the controversy that accompanied the light quantum hypothesis through the succeeding decades.

In the Pais volume the history of relativity theory also flows smoothly, though the reviewer has marked several passages where “contradiction” was noted within relativity theory itself. One such passage concerns the “clock paradox.” According to Pais,

[Einstein] called this result a theorem and cannot be held responsible for the misnomer *clock paradox*, which is of later vintage. However, as Einstein himself explained some time later [1916, 1918], the logic of special relativity does not suffice for the explanation of this phenomenon (which has since so often been observed in the laboratory), since frames other than inertial ones come into play [145].

Though this explanation certainly contradicts the textbooks, it seems rea-

sonable to the reviewer since the clock (or twin) of the thought experiment must obviously undergo acceleration and deceleration in the round trip, something forbidden to an inertial frame. Such a resolution of the “paradox,” however, is not acceptable to Einstein scholar John Stachel who protests:

More exasperating than puzzling is Pais’s repetition of the old chestnut that the explanation of the “twin paradox” requires the general theory of relativity—especially since he misattributes this claim to Einstein [*Science* 218: 989f; 1982].

Let us note that there may well be justification for the Pais attribution: A.I. Miller also asserts that in 1918 Einstein turned to “the principle of equivalence from general relativity for proposing a dynamical explanation for the clock paradox” in order to discuss “how the symmetry was broken between the two inertial systems” [*Albert Einstein’s Special Theory of Relativity*, Addison-Wesley 1981, pp. 264, 272]. Note also that Pais seems no more ready than Stachel to consider that there may be real contradictions in the body of relativity theory. (Stachel, despite this and other criticisms, rates the Pais book as “indispensable.”)

Another significant sign of contradiction was found in the Pais account of the long struggle between the classical concept of electromagnetic mass and Einstein’s new relativistic mass. Concerning the final experimental verdict (1914–1916), Pais concludes:

Special relativity killed the classical dream of using the energy-momentum-velocity relations of a particle as a means of probing the dynamic origins of its mass. The relations are purely kinematic. The classical picture of a particle as a finite little sphere is also gone for good . . . *But we still do not know what causes the electron to weigh.* [159, emphasis added.]

Doesn’t this conceptual standstill in itself indicate the existence of contradiction within special relativity that must be uncovered and resolved in order for physics to progress?

Concerning the origins of general relativity, Pais gives us a refreshing account of a very human Einstein in the travail of giving birth to a new principle [250–257]. It is a dialectical discovery process in which Einstein repeatedly enthuses over some new formulation, then has his hopes dashed when the flaw is found. Truth is obtained through error, and knowledge emerges from ignorance—all in a manner that no deductive logic can accommodate or tolerate.

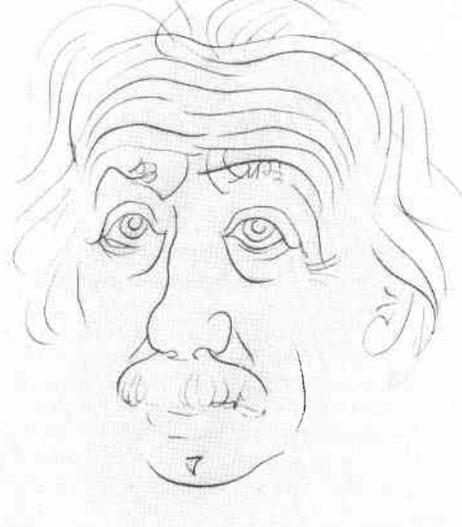
One revelation to this reviewer, something he felt to be true but had never before seen in print, is Einstein’s 1915 statement:

Any physical theory that obeys special relativity can be incorporated into the general theory of relativity; the general theory does not provide any criterion for the admissibility of that physical theory . . . Finally, the general theory of relativity is closed as a logical structure [256].

Equally striking is Einstein’s 1931 opinion that general relativity theory “is a purely formal point of view and not a definite hypothesis about nature” [273]. Such an abstract principle seems to constitute little more than a hunting license for the physical investigator. Small wonder then that Pais sums it up thus: “no one today would claim to have a full grasp of the rich dynamic content of the nonlinear dynamics called general relativity” [267].

Perhaps the purely formal nature of general relativity was only recognized by Einstein some years after he had made it part of physics. The

Drawing by
Josef Scharl,
courtesy of
Institute for
Advanced Study,
Princeton NJ.



younger Einstein evidenced a low regard for the formal approach to theoretical research, as in this bit of dialectical advice for Felix Klein in 1917:

It does seem to me that you highly overrate the value of formal points of view. These may be valuable when an *already found* truth needs to be formulated in a final form, but fail almost always as heuristic aids [325, emphasis in original].

Another profoundly dialectical statement by Einstein appears in his correspondence with Klein the same year:

However we select from nature a complex [of phenomena] using the criterion of simplicity, in no case will its theoretical treatment turn out to be forever appropriate (sufficient). Newton’s theory, for example, represents the gravitational field in a seemingly complete way by means of the potential . . . This description proves to be wanting; [a general relativity function] takes its place. But I do not doubt that the day will come when that description, too, will have to yield to another one, for reasons which at present we do not yet surmise. I believe that the process of deepening the theory has no limits [325].

Concerning Einstein’s rejection of quantum mechanics, Pais makes it clear that, from 1931 on, Einstein no longer believed that quantum mechanics was wrong, but simply that the physics community was wrong in attributing to the postulates of quantum mechanics a degree of finality that he held to be naive and unjustified [449]. In the heady period 1925 to 1931, while quantum mechanics was finding its feet, the great debate between Einstein and Bohr helped to make the philosophical issues clear. One issue concerned Bohr’s linguistic definitions, such as limiting the use of the word *phenomenon* to refer exclusively to observations obtained under specified circumstances (a usage now general in the physics community). Einstein’s position on this issue is described by Pais thus:

In contrast to the view that the concept of phenomenon *irrevocably* includes the specifics of the experimental conditions of observation, Einstein held that one should seek for a deeper-lying theoretical framework which permits the description of phenomena independently of these conditions. This is what [Einstein] meant by the term *objective reality*. After 1933 it was his almost solitary position that quantum mechanics is logically consistent but that it is an incomplete manifestation of an underlying theory in which an objectively real description is possible [455, emphasis in original].

Einstein's conception of objective reality is made concrete in his trenchant comments of 1928 and 1933 concerning the probabilistic aspects of quantum mechanics:

I still believe in the possibility of giving a model of reality which shall represent events themselves and not merely the probability of their occurrence [460].

I believe less than ever in the essentially statistical nature of events and have decided to use the little energy still given to me in ways that are independent of the current bustle [320].

Einstein's staunch defense of materialist causality and objective reality gave rise to a certain tension between the biographer and his subject, since Pais frankly gives his support to the present interpretation of quantum mechanics and has correspondingly little use for classical causality or reality. But Pais is always fair in his expressions of dissent or disdain. An instance is this comment on the well-known 1935 paper introducing the so-called EPR paradox:

this paper contains neither a paradox nor any flaw of logic. It simply concludes that objective reality is incompatible with the assumption that quantum mechanics is complete. *This conclusion has not affected subsequent developments in physics, and it is doubtful if it ever will* [456, emphasis added].

The philosophical divergences of Pais and Einstein are also expressed clearly and fairly in this estimate:

I do not believe that Einstein presented valid arguments for the incompleteness of quantum theory, but neither do I think that the times are ripe to answer the question of whether the quantum-mechanical description is indeed complete, since to this day the physics of particles and fields is a subject beset with many unresolved fundamental problems [363].

What seems to be missing from the personal outlook of Pais is any hint of a personal urge or drive to participate in changing things from the way they are. His philosophical approach, in fact, seems to tend in the opposite direction—to keep things the way they are. Hence, he may not be able to recognize opportunity for change when it knocks at the door. An instance concerns the conceptual origins of wave-particle duality in the initial Einstein suggestion for a “fusion” of two theories, made in his much-neglected 1909 papers and excerpted by Pais as follows:

It is my opinion that the next phase in the development of theoretical physics will bring us a theory of light that can be interpreted as a kind of fusion of the wave and emission theory . . . [The] wave structure and [the] quantum structure . . . are not to be considered as mutually incompatible . . . [It seems] that we will have to modify our current theories, not to abandon them completely [404].

After noting the extraordinary significance for modern physics of this suggestion, Pais comments that in 1909, at age thirty, Einstein was prepared for a fusion theory when Planck was not and Bohr had yet to arrive on the scene, yet “when the fusion theory arrived in 1925, in the form of quantum mechanics, Einstein could not accept the duality of particles and waves inherent in that theory as being fundamental and irrevocable” [404].

If Pais had been more open to change, he might have perceived that Einstein had a different notion of fusion in mind. In one of the 1909 papers from which Pais quotes there also appears the following pregnant conjecture by Einstein:

Still, for the time being it seems to me most natural to assume that the occurrence of the electromagnetic field of light is just as much linked to singular points as is, according to the electron theory, the occurrence of electrostatic fields. It is conceivable that in such a theory the total energy of the electromagnetic field could be considered localized in these singularities, just as in the old theory of action at a distance. I imagine each such singular point to be surrounded by a field of force, which essentially has the character of a plane wave and whose amplitude decreases with the distance from the singularity. If many of these singularities exist in intervals small compared to the dimensions of the field of force of a singularity, the fields will be superimposed upon each other and in their totality will produce an undulatory field of force which may hardly differ from an undulating field of the present-day electromagnetic theory of light. That such a model need not be taken seriously, as long as it does not lead to an exact theory, hardly requires any special mention. [*Some Strangeness in the Proportion*, Harry Woolf, ed. Addison Wesley 1980, p. 257. Tr. from *Phys. Z.* 10: 824f; 1909.]

This passage has been noted by several physicists concerned with development and change in microphysics. In fact, the quotation above is taken from a comment by Res Jost on Pais' work. Though Jost also deprecates the model as not to “be considered scientifically valid,” he nevertheless admits that “it exerts a strange fascination” and goes on to suggest how it may have influenced Louis de Broglie in developing the concept of matter waves.

The great significance of this tentative 1909 model is that Einstein proceeds from *physical* reasoning about discrete particles, rather than from a *mathematical* search for partial differential equations to describe a unified field. Note also that *there is no duality here*: the particles are primary, with the fields arising physically from the particles. Contrast this with the quantum electrodynamic model, highly successful but highly inelegant because the field is primary and the particles are injected by mathematical simulation (as, no doubt, they would have to be in Einstein's elusive unified field theory).

One could wish that the implications of this 1909 model had been explored further, especially since Einstein later began to have grave doubts about the field approach he had taken. To Infeld in 1941 he wrote: “I tend more and more to the opinion that one cannot come further with a continuum theory;” and to his friend Besso in 1954 he wrote: “I consider it quite possible that physics cannot be based on the field concept, i.e., on continuous structures” [467]. (For more on the question of whether wave or particle is primary, see Talkington, *S&N* #2, 19–23.)

Incidentally, Einstein may never have been at ease with the concept of duality, witness his 1924 comment:

There are therefore now two theories of light, both indispensable, and—as one must admit today despite twenty years of tremendous effort on the part of theoretical physicists—*without any logical connection* [414, emphasis added].

One last point from my reading of Pais. Einstein considered quantum mechanics to be a limiting case for a new theory yet to be discovered. And he felt that one should seek the new theory not by trying to refine or reinterpret quantum mechanics but rather by starting over from scratch [461]. There are many more such treasures awaiting those who turn to the excellent biography of Albert Einstein by Abraham Pais.

Lester (Hank) Talkington

Realism versus Positivism in Relativity Theory

Michael Friedman, *Foundations of Space-Time Theories. Relativistic Physics and Philosophy of Science*. Princeton University Press 1983. xvi, 385 pages, \$35.

Historically, the emergence of the theory of relativity and the development of logical positivism, twentieth century descendant of Machian empiricism, have been entangled with each other. On the one hand, Albert Einstein, though not a positivist, was influenced by positivist ideas in the development of his theories. On the other hand, the positivists claimed to find overwhelming support for their views in the essential content of relativity theory. Friedman, in this book, undertakes to disentangle relativity theory from logical positivism in order to demonstrate that "Relativity theory neither supports nor embodies a general positivistic point of view," and to present his own "realist" interpretation of relativity. Friedman carries out his program in four parts.

1. In Chapters III, IV and V he outlines a systematic construction of models of Newtonian physics, Special Relativity, and General Relativity. Since Minkowski, both Special and General Relativity have been constructed as geometries of a four-dimensional space-time manifold. Following Anderson, Friedman constructs Newtonian physics, too, on a four-dimensional manifold. He does it in two forms, one in which gravitational force is not built into the geometric structure, and one in which it is. These constructions are preceded by a brief outline in Chapter II of what a differentiable manifold is (supplemented by an appendix on differential geometry), covering the significance in a manifold of the tangent space at each point, the covariant derivative, the affine connection, the geodesics determined by the latter, the curvature of the space, the concept of reference frame, and the notions of covariance of equations and invariance of geometric objects under certain groups of transformations.
2. He introduces a distinction, derived from J.L. Anderson (*The Principles of Relativity Physics*, 1967), between the *absolute* objects of the theories and the *dynamical* objects of the theories. On the basis of this distinction he rejects the usual formulation of what constitutes a relativity principle, and replaces it with one of his own.
3. In Chapter VI he examines and tries to refute the "relationalist" interpretation of space in which space expresses only relations between concrete physical objects.
4. In Chapter VII he examines and refutes the "conventionalist" views that different and incompatible theories which fit the same empirical data are equivalent, and that the choice of one over another is merely a matter of convention.

Two parts of Friedman's book are quite successful. The first is his construction of Newtonian physics, Special Relativity and General Relativity on four-dimensional manifolds. His introduction to the concepts of differential geometry is very clear, and, were it supplemented by exercises, might even serve as an introductory text-book on differential geometry. His use of these concepts in the construction of the physical theories is systematic and clear, and provides a good foundation for the philosophic discussions that follow.

THE SECOND SUCCESSFUL PART of Friedman's book is his refutation of the conventionalist position. The conventionalists argue that certain *prima facie* incompatible systems of description are in reality equivalent descriptions if they are consistent with the same set of empirical data. They then conclude that the choice of one description in preference to another is only an arbitrary convention. To refute this view, Friedman shows that certain measures that conventionalists consider to be mere conventions are not conventions at all, but have a physical significance and are completely determinate (up to choice of unit). For example, the length of an object in relativity theory depends on the frame of reference of the observer. But once this frame of reference is specified, the measures of length made from it are fully determined. Although the length of an object is not a property of the object alone, it is a property of the relation of the object to the frame of reference. Friedman also emphasizes correctly that the choice of one description rather than another, like all inductive inference, "depends on methodological principles that go beyond mere conformity to evidence." These principles include "simplicity, parsimony, non-adhocness, explanatory power, and so on." He stresses in particular the principle of "unifying power." He says, "A theoretical structure that plays an explanatory role in many diverse areas picks up confirmation from all those areas."

However, other philosophical sections of the book do not deliver as much as they promise. It is this reviewer's opinion that: 1) the distinction between absolute and dynamical objects supplements rather than refutes the usual formulation of what constitutes a relativity principle; 2) the attempted refutation of "relationalism" is both misguided and unsuccessful; and 3) although Friedman describes his position as "realist" it is essentially a variety of logical positivism. We deal with these points one at a time below.

(1) Friedman cites as the usual formulation of a relativity principle the statement by Bergmann that the covariance of equations is the mathematical property which corresponds to the existence of a relativity principle for the physical laws expressed by those equations. He then asserts that this statement "is simply not true," offering as a correct formulation of a relativity principle one that is based on the distinction between absolute objects and dynamical objects as defined by Anderson: "The absolute objects of a theory are thought to be those objects not affected by the interactions described by the theory. They are independent of the dynamical objects, part of the fixed 'background framework' within which interactions take place . . . The symmetry group of a theory in Anderson's sense is a group of manifold transformations preserving the absolute objects of the theory" [page 150].

To replace Bergmann's relativity principle, Friedman proposes the following two propositions: (R1) *All inertial frames are physically equivalent or indistinguishable*; (R2) *If two frames are indistinguishable according to a theory T, they should be theoretically identical according to T*. Then he shows that if *T* satisfies the relativity principle (R1) & (R2) then the group of transformations which permutes inertial frames equals the symmetry group of *T*. But this, in turn, he says, is the same as the covariance group of the standard formulation of *T*. Thus Friedman himself arrives at the covariance of equations that Bergmann referred to. Instead of refuting Bergmann's statement, he has elaborated on it by linking it to Anderson's concept of the symmetry group of a theory.

FRIEDMAN ARGUES that all known space-time theories can be written in generally covariant form (covariant with respect to all sufficiently differentiable transformations), and hence covariance can not distinguish general relativity from other theories. However, he provides his own refutation of this statement when he says on page 212 that while "all space-time theories can be given a generally covariant form, . . . What is new about general relativity is . . . the *necessity* for a generally covariant formulation." That is, the *necessity* of *general* covariance in its formulation does indeed distinguish general relativity from other theories.

(2) Friedman's attempt to refute "relationalism" is misguided since he is trying to refute something that he himself proves to be well established both by theory and observational evidence. He defines relationalism as the view that "we should regard the use that physical theory makes of space-time and its geometrical structure merely as a convenient way of saying something about the spatio-temporal properties and relations of concrete physical objects." He points out that the arguments for and against the relationalist point of view depend "on the distinction between matter on the one hand and space-time on the other, between the set P of concrete physical events and the manifold M of all actual and possible events, between points in M that are 'occupied' and points in M that are 'unoccupied'." If all points must be construed to be occupied then the relationalist point of view is upheld. Friedman then cites the evidence for the view that all points of space-time are occupied: a) The real universe appears to be completely filled with background radiation. (This background radiation, predicted by the theory of the expanding universe, has been detected.) b) In the theory of general relativity, the curvature of space depends on the metric tensor g , which depends on the distribution of mass in space. Moreover, in general relativity, the space-time metric g carries energy.

Hence, the geometry of space-time is determined by the matter in it. After clearly proving in this way that "all space-time points turn out to be occupied," Friedman then makes an about-face and says "all known theories of motion . . . imply the existence of a class of probably unoccupied inertial trajectories . . ." He attempts to justify this statement by redefining the term "occupied." He says the criterion for a point being occupied should be, not the non-vanishing of the metric tensor g , but the non-vanishing of the stress-energy tensor T . However, this juggling with definitions merely begs the question. No matter what arbitrary definition he may choose for his use of the term "occupied," the fact still remains that in relativity theory a point has *physical* properties and not only position. In fact, this is even true in classical Newtonian mechanics where even points with zero mass-density may have non-zero gravitational potential.

Friedman also tries to bolster his argument by making a distinction between a structure being actually part of a more inclusive structure and its being "merely" isomorphically embeddable in it. He says that the relationalist position, instead of asserting that the set P of concrete physical events is *actually embedded* in an inclusive space-time M as one particular subset of space-time points, asserts only that the set P is *isomorphically embeddable* into a space-time M . In his lengthy discussion of this distinction between being *actually embedded* and being *isomorphically embeddable*, he makes it clear that his notion of *actually embedded* is compounded of two ideas: A) that the set P of concrete physical events *actually is* rather than is merely represented by a set of points; B) that it is

a sub-structure of the space-time manifold M . We shall discuss the significance of idea (A) in section (3) below. In this paragraph we consider only Friedman's distinction between idea (B) and being isomorphically embeddable. Friedman relies on this distinction to argue against the relationalist position. However, since mathematical structures that are isomorphic are essentially the same, this is a distinction without a difference. Hence his whole argument breaks down. In an attempt to show that there is a difference, he uses a one-to-one mapping into \mathbb{R}^4 as an example of *embeddable* in contrast to being embedded. But this attempt fails because a mapping that is merely one-to-one is not necessarily isomorphic.

(3) One of Friedman's goals is to demonstrate that "Relativity theory neither supports nor embodies a general positivistic point of view." He does this effectively to a certain extent when he demonstrates that the relativism of such measures as length and duration in the theory of relativity does not imply that these measures are mere conventions. However, what Friedman gains when he refutes conventionalism he surrenders when he presents his own supposedly realist point of view. Realism (epistemological realism), as I understand it, and as the term is used in modern philosophy, refers to "the view that material objects exist externally to us and independently of our sense experience" [*Ency. of Philosophy*]. With this definition, realism is synonymous with materialism as defined by Lenin: ". . . the sole 'property' of matter—with the recognition of which materialism is vitally connected—is the property of being *objective reality*, of existing outside our cognition."

However, this is not what Friedman means by realism. Realism to him means that "physical theories postulate a structure . . . that is intended to be taken literally, this is supposed to have physical reality." Notice that he says the theoretical structure is supposed to *have* physical reality. He does not say that it is supposed to *represent* physical reality. With this assertion he in effect equates physical reality with the conception of it embodied in a theory. In other words, he equates the object with a concept of the object. But this equating of object and concept is precisely the essence of logical positivism. Carnap, for example, in *The Logical Structure of the World*, says that "the object and its concept are one and the same." Thus, while Friedman rejects the positivism of the conventionalists, his "realism" is the same as the positivism of Carnap, which substitutes a mentally-constructed concept for an object that exists outside of and independent of the mind.

This is not the place to undertake a thorough analysis of the fallacies in the positivist outlook. But, to indicate how far away it is from a genuine realist position, I call attention to one important difference in their implications. If an object exists independent of cognition, the concept of the object is always only an approximation to the truth about the object. The object itself, through further study, can be a source of new information on the basis of which revision of the concept may become necessary. On the other hand, if the concept and the object are one and the same, the object cannot be a source of new information for correcting the defects of the concept. Equating concept and object closes the door to further scientific progress.

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Can the Discovery Process Be Formalized?

Martin Harwit, *Cosmic Discovery: The Search, Scope and Heritage of Astronomy*. Basic Books, 1981. 334 pp., \$25.

Martin Harwit has written an original and stimulating book which discusses the nature of astronomical research from a number of perspectives, with the intent of contributing to the rational planning of research in astronomy. Through understanding the history of astronomical research, he maintains, it should prove possible to anticipate future developments and plan for them accordingly. No Marxist would quarrel with such a historicist approach to the development of science. However, in developing his arguments Harwit makes a number of remarkable and contentious claims. The most contentious is the claim that the number of distinct cosmic phenomena is finite (and calculable!) and that future astronomical research will involve seeking and recognizing the remaining number of distinct phenomena (about 80). Such a line of reasoning will seem strange to those scientists who think of the universe as diverse in its unity, unified in its diversity, and infinitely complex in terms of both its physical content and the relationships between its component parts.

In evaluating Harwit's arguments it is important to keep in mind the restricted sense in which he uses the terms *cosmic phenomenon* and *cosmic discovery*. He offers a working definition of cosmic phenomenon as that entity which in some respect (temperature, density, magnetic field strength, luminosity, etc.) differs by a factor of 1,000 from some other phenomenon. Such a definition allows the known contents of the universe to be differentiated into 43 categories: stars, novae, clusters (galactic and globular), quasars, etc. These data are displayed in an interesting graph of accumulated number of discoveries against historical epoch. A simplistic interpretation of this graph is that the accumulated number of cosmic phenomena is increasing at an exponential rate. Harwit, however, considers this interpretation to be naive, arguing that we are rapidly approaching a peak in the rate of astronomical discoveries and that by the year 2200 we will have in fact discovered 90% of cosmic phenomena! Completely neglected in such a definition, of course, are discoveries that bear on the interrelationships between cosmic phenomena. Thus, the Hertzsprung-Russell diagram, the period-luminosity relationship for Cepheid variables, or, indeed, even the expansion of the universe would not constitute cosmic discoveries! Harwit's definition of cosmic phenomena is strictly empirical and one-sided.

THE BOOK IS DIVIDED INTO FIVE SECTIONS AS FOLLOWS:

(i) *The Search*. Active techniques of search for new astronomical phenomena, such as sending probes or astronauts outside of the solar system, are dismissed as impractical in the present epoch, so that only passive techniques of receiving and analyzing radiation and particles are considered. It should be stressed that Harwit is only concerned with discovery at the primitive level of cognition of a phenomenon as such, and not with subsequent detailed physical analysis.

(ii) *Discoveries*. On the nature of past discoveries, Harwit is particularly interesting. Several important characteristics of the astronomical discovery process are identified: the importance of technological innovation, the role of serendipity, the high frequency with which discoveries are made by

physicists and engineers who have no astronomical training, and the startling fact that, since World War II, some 70% of the discoveries of new phenomena involved the use of infra-red photometry, image intensifiers, or other technology developed originally for military purposes (often using surplus military equipment). This discussion clearly illustrates the interplay between the development of science and its societal context.

(iii) *Observation*. Marxist philosophy of science stresses the dialectical interplay of theory and practice in the development of science, according equal status to each. However, as Harwit makes clear, in astronomy the role of theory is not at present that of a guide to practice; astronomy has traditionally been and to a large extent remains a purely observational science. Harwit notes only the neutron star as an example of a phenomenon predicted by theory, but then dwells on the present theoretical difficulty of explaining the existence of pulsars (which have been identified as neutron stars). If, however, we go outside of Harwit's definition of phenomenon, we can find other examples where theory has led observation. The most famous exception would be the 1917 eclipse expedition to check the theoretical predictions of Einstein's general theory of relativity. More recent is the attempt to detect gravitational radiation. Harwit's own background as an observer may in part explain his disparagement of theory in astronomy.

(iv) *Detection, Recognition and Classification of Cosmic Phenomena*. The limitations of Harwit's approach are evident where he "suggests that recognition, classification and distinction of major cosmic phenomena might center on the strikingly disparate *appearance* of different phenomena," an approach which he concedes is "superficial" but defends on the basis that "in many areas of science, classification based on superficial features has had significant success" (p. 198).

Thus he tends to ignore the existing role of theory in astronomy, which has been largely to explain phenomena already investigated observationally (at least in some restricted sense) and then try to build models of complex systems which reproduce a limited number of observed parameters whose values are imprecisely known. With this as a basis, it is a rare theoretician who would stick out his neck and make firm predictions concerning unknown phenomena. The history of astronomy includes instances of prediction by eminent theoreticians (including Einstein) which have come unstuck. The wide license that this situation gives to the theoretician is shown in an ironic remark reportedly made by a well-known theoretician to an observational colleague: "Don't show me those observations of yours; they only serve to restrict my theoretical speculations."

Such a situation reflects several particular aspects of astronomy in its current level of development as a branch of science: a) the great diversity and remoteness of its subject matter; b) the paucity of accurate and extensive data on the phenomena in the universe; c) the relatively small number of people working in the field (some 3,000 professional astronomers, as compared with some 20,000 professional astrologers). These characteristics typify a branch of science at a relatively immature stage of its development. One can be confident that, as astronomical data become more extensive and accurate, the coupling between theory and practice will become much stronger. It is certainly not inconceivable that astronomy will develop to the stage where lack of a theoretical breakthrough *per se* could hold up future development.

(v) *The Fringes of Legitimacy: The Need for Enlightened Planning.* Harwit's final section includes recommendations for rational planning of astronomical research which make eminent sense to me. Here in Canada, where relatively little is spent on scientific research, such planning has, until quite recently, amounted to little more than asking "Whose turn is it now?"; i.e., the optical astronomers get a large telescope, followed by a large facility for the radio astronomers, while the theoreticians get relatively little support.

Harwit's first recommendation is that students in astronomy be trained in physics as thoroughly as physicists themselves. Such an obvious recommendation, which may surprise readers, does reflect a traditional bias of classically-trained astronomers to imagine that their discipline is in some sense apart from physics. This elitist attitude is breaking down; modern astronomy is no longer the tedious compilation of data and production of catalogues. Astronomers are drawing on physics, mathematics and, increasingly, chemistry, in their efforts to understand the physical nature and evolution of the universe and its contents. Thus, astronomy is becoming interdisciplinary in its makeup. Other recommendations include the need to introduce technological innovations as soon as possible and to train older astronomers in their applications, the need to loosen the peer review system in order to give researchers more freedom to pursue novel ideas without excessive justification, and the need for the future of astronomy to be planned by generalists as opposed to established vested-interest groups. Harwit makes a total of 13 such recommendations, all of which deserve to be taken seriously by the astronomical community.

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A Discovery in Undergraduate Textbooks

I.V. Savelyev, *Fundamentals of Theoretical Physics*. Progress, Moscow, 1982 (Imported Books, Chicago). 2 vols, 792 pages. \$16.95.

For the undergraduate student in most American colleges, "Theoretical Physics" consists of some catch-all lectures that touch lightly on a diversity of topics, introducing mathematical techniques needed for pursuing physics or related sciences (astronomy, chemistry, etc.). The textbook literature for such a general course is rather meager because it has not been considered important in the same way as the specialized courses (mechanics, thermodynamics, etc.). For the graduate student in physics, we do have the famed two-volume Morse and Feshbach, but hardly anything of comparable quality for the undergraduate. To fill this gap, Savelyev's new two-volume work is an important and welcome addition in physics textbooks.

He has produced a work that continues the tradition of excellence established by such high level European texts as those of Courant & Hilbert, Frank and V. Mises, Sommerfeld, and Landau and Lifschitz. The first volume deals with classical mechanics, electrodynamics, and the special theory of relativity, all beautifully developed and elegantly presented in 296 pages; the remaining 122 pages are mathematical appendices, which

progress from the calculus of variations through differential equations, matrices, determinants, quadratic forms, tensors, vectors and Fourier analysis.

The high level and superb quality of the presentation are indicated on the first page of the text where Savelyev discusses the continuum and the discrete particle approach to mechanics and gives his reasons for choosing the latter, which lead him to the application of the variational principle in preference to Newton's equations.

The methods developed in the mechanics section prepare the student for the special theory of relativity and the electrodynamics that follow. The contents of some of the chapters in this section show the wide range of the material: Lagrange's equations in generalized coordinates, the principle of least action, conservation principles, the two-body problem, elastic collisions, Hamilton's equations, Poisson brackets, Euler angles, the symmetric top, the Hamilton-Jacobi equations.

SAVELYEV PRESENTS the special theory of relativity in the 32 pages of chapter 5, starting from the concept of a world point and line, and deduces the Einstein-Lorentz transformations from the invariance of the space-time interval. Here he immediately introduces the important distinction between space-like and time-like intervals which play such crucial roles in the treatment of determinism and causality. Relativistic dynamics are developed from the 4-vector point of view, which leads to the invariance of the square of the momentum-energy vector and thus to Einstein's famous energy-momentum-mass equation. Everything here is presented with great simplicity and with unencumbered derivations that are easy to follow.

The seven chapters (140 pages) of part II take the student through classical electrodynamics. In a chapter devoted to its relativistic formulation, the vector and scalar potentials are combined into a 4-vector with the electromagnetic field components forming an anti-symmetric tensor. In another chapter Maxwell's field equations are deduced from the principle of least action. This is very instructive since it introduces the student to the application of the variational principle to fields. Here, too, the treatment is impeccable and the reading is easy. Savelyev's treatment of magneto-statics is particularly useful since it is either neglected or skimpily treated in most books.

VOLUME 2 DEALS ENTIRELY WITH QUANTUM MECHANICS, covering the subject more completely than is usual at this level. The foundations are developed carefully in the first chapter and thereafter the various mathematical techniques for handling quantum mechanical problems are presented to give a student the tools needed to solve problems. For example, the student who masters the 45 pages on perturbation theory will have the analytical tools necessary for solving most practical problems. A chapter effectively portrays the relation of quantum mechanics to classical theory, so the student will understand that quantum mechanics does not stand alone. Another chapter takes the mystery out of spin, giving the student a feeling for the physics as well as the mathematical methods for handling it. Included is a discussion of molecules rarely found in undergraduate texts.

The fact that these two volumes were originally conceived as a text for engineering students may account for some of the simplicity and effective communication, but it does not in the least detract from their suitability for other courses. The use of familiar units and notation, the uncluttered

design, the attractive print and the translation by G. Leib are all excellent. Considering the fantastically low price of \$16.95, every student of physics should have these two volumes within easy reach at all times.

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EDITOR'S NOTE. I queried Motz about his unqualified approval, but he simply couldn't find anything to criticize. From my own acquaintance with Soviet scientific texts, I'd say the unusually effective exposition stems basically from the Marxist materialism in the outlook of the authors.

BIBLIOGRAPHIC BRIEFS

Maurice Wilkins, "Message to the Berlin Round Table." *Scientific World* No. 2/3, 1983, p. 5.

How can we draw more scientists into the campaigns against the qualitatively new types of weapons such as the Cruise and Pershing? The analysis of Nobelist Wilkins can be summarized as follows:

1) Unfortunately a great many scientists are working *enthusiastically* to devise new weapons; most scientists shelter behind the progressive achievements of science, ignoring its destructive effects; only a minority is concerned about the terrible disgrace that our civilization causes scientists to work for destructive ends.

2) As scientists, we all share some responsibility for all aspects of the work of the scientific community because science is essentially cooperative and depends on sharing of values. But moral exhortations no longer help much. Scientists are too confused or frightened to face the full horror of what is happening. We must draw on the enthusiasm of scientists for their work, directing it so this enthusiasm is brought to bear on the problem of war and science.

3) At the root of scientists' enthusiasm, and of the scientific endeavor, is a faith (like Einstein's) that there is *order* in Nature, that the wide-ranging order discovered by science may correspond to a universal order, universal harmony, extending throughout nature and human life. This enthusiasm resembles religious enthusiasm; it gives dignity to science. But harmony and destruction do not fit together. We must work toward the day when science is no longer contaminated by war research.

4) Many scientists draw a sharp line between science and its applications, or between science and politics. They use a mechanistic philosophy which does not approach the problem as a whole, only as independent parts, claiming that this philosophy has served science well through the centuries. But today it makes no sense to argue that knowledge and its application are separate, when this leads scientists to invent new weapons which can eventually destroy science itself.

5) It is not enough to say that we must remove the political causes of war. If we see clearly the essentially peaceful nature of science, this can help remove the causes of war. Today scientists are inhibited, feel a loss of



Drawing by Anton Refregier

PEACE

Each dawn makes its own demand:
not a shout . . .

Just a universal nudge
with the calm of a billion
star years behind it
"Please . . . I will be heard."

Each generation
lives its own morality
Ours is in the flame of being
all of us together for peace
to bring peace from its
million year embryo
into the arms of being human.

Each age advances
with its own song
Peace has become
the poem of our lives
This is the revelation
of our century
the atomic weight of each life
measured out
for everyone to see.

—WALTER LOWENFELS

dignity, and seldom speak of science in the way Einstein did, because they are ashamed of the atomic bomb.

Fellow scientists, [he concludes,] do not let us hide our shame, let us feel it openly. Let us accept our responsibility for what science has done. If we see this clearly it should help us to persuade other scientists to accept responsibility. Let us at the same time appreciate the true spirit of science, for that vision will inspire us in our work for peace.

All that I would add to Wilkins' message is "Amen, Brother, Amen."

Margaret Fay, "The Influence of Adam Smith on Marx's Theory of Alienation." *Science and Society* 47(2): 129-151; 1983

This paper provides a splendid case history of how Marx applied dialectical materialism in his own researches. It is worth study by natural scientists, since the principles are the same. The late author (1944-1979) confessed grave doubts about her hypothesis (in title of paper) but did not seem to realize what a substantial contribution she has made through detective work which traced out the actual research process and produced a concrete example of how the "refutation of Hegel's idealism allowed Marx to 'demystify' the dialectical method and to transform it into a tool for demonstrating" the way historical processes develop in reality (p. 136).

Her illuminating description of Marx's use of the dialectical method,

which applies equally well to the natural sciences (considered as social processes), may be summarized as follows:

1) The dialectical method has both negative (critical) and positive (re-constructive) aspects. The first stage of the dialectical method, its negative aspect, is the process of *immanent critique*, meaning the total immersion of the critic in the content of the subject under study in order to extract the ambiguities, inconsistencies, and contradictions internal to (immanent in) the available knowledge of that subject (e.g., textual analysis of a given body of work, selectively arranging quotations and notes to reveal the contradictions within the material studied).

2) In order to interpret the underlying reality, this analysis must proceed by successive *levels of understanding*, seeking to separate and codify aspects of the concrete totality in the form of fixed and semi-independent concepts concerning the individual aspects. In this analytic process the emergence of inconsistencies and ambiguities (at some level) is inevitable rather than accidental.

3) The transition from the negative to the positive aspect of the dialectical method is made when the critic examines these internal contradictions for a common theme. The content of the account developed through levels of understanding, once they have been liberated from previous divisive and inflexible conceptual boundaries, become the raw material for the dialectical critic's reconstruction. The reconstruction describes and analyzes the same phenomenon, but as *a process of self-development*. The positive aspect of the dialectical method seeks to identify and explain the hidden dynamic that propels and shapes the particular process of self-development.

The process of self-development provides the integrating link between successive stages of development and reveals, at any given stage, the inner interconnections among the phenomena. The dialectical critic's unifying concept of self-development thus stands in sharp contrast to the many categories and sub-categories employed by the 'Levels of Understanding' to analyze and organize the empirical data of the external world and of historical events (p. 147).

For example, studying the emergence of capitalism, Marx found that

the inner dynamic of this movement, the principle of self-development, is not private property itself, but labor: labor understood not as acquisitive activity but as "*the direct relationship between the worker (work) and production*" [Marx, his emphasis], a relationship which is always a relationship of alienated labor in a society in which the producers do not own and control their own conditions of production. Private property therefore is "the product, the necessary result, of *alienated labor*" [Marx, his emphasis]. (p. 148)

Unfortunately, most such descriptions of Marx's dialectical method deal with the social sciences and there is very little in the way of concrete examples of how this method applies in the natural sciences. From what has been said, however, it should be clear that what Marx and Engels called the dialectical method is, as Lenin said, "nothing more or less than the scientific method" (*Sel. Works* xi, 445). For those who wish to read more about the Marxist dialectical method as it applies to the scientific process, good introductions are Engels *Anti-Duhring*, pp. 15-19, 26-32, and *Fundamentals* (1982), Ch. VIII and esp. 173-177 [see Basic Bookshelf list]. Also, useful is Robert S. Cohen, "Karl Marx" in *Dict. Sci. Biog.*, xv, suppl. 1, pp. 409-412. Greater depth of discussion will be found in E. V. Ilyenkov, *The Dialectics of the Abstract and the Concrete in Marx's Capital*, Ch. 3 [see Books Received].

Richard D. Schwartz, "Mirror for Lawyers." *Science* 223: 481f; 1984 (review of *Chicago Lawyers* by J.P. Heinz and E.O. Laumann. Basic Books 1983).

Will the conflicting interests of our class-divided society tend more and more to divide scientists into opposing camps? The question is brought to mind by reading this review of a penetrating sociological study which finds a division within the legal profession that reflects our divided society. Corporate lawyers have a dignified, privileged, conservative practice. Lawyers working for individual clients live in a quite different world. The two types of practitioners have little in common. The trend toward a similar divergence in our scientific professions can be seen in the numerous instances where the same data receive opposite interpretations that clearly reflect the divergent interests of those to whom the scientists give their allegiance. Government science and corporate science tend more and more to line up on the same side in our era of state-monopoly capitalism, while desperate battles rage in the academic research community to prevent complete takeover by encroaching economic penetration. In this situation, scientists clearly need allies among the progressive forces of what Jesse Jackson calls the *Rainbow Coalition*.



"The public and press is demanding the truth. I want you to come up with three versions of it." [Nuke Watch.]

Julie Ann Miller, "Mendel's Peas: A Matter of Genius or Guile?." *Science News* 125: 108-109; 18 Feb 1984.

The notorious reductionist Sir Ronald Fisher concluded in 1936 that Mendel's data on pea genetics were too good to be true and that, even though his genetic law proved correct, the data had been "falsified." Now science historian Robert S. Root-Bernstein has found from a test, with students counting the peas, that Mendel got his results from the normal process of exploratory study in which subjective judgment on degree of wrinkling in peas was necessary for "inventing" the categories which led to discovering the laws of genetics.

"The matter of Mendel's peas," writes Miller, "is only part of a larger problem of distinguishing inspired scientific insight from fraud. Recent analyses, such as *Betrayers of the Truth* by William Broad and Nicholas Wade (Simon and Schuster, 1983), put the questionable data of such scientists of the past on a continuum with the deceit of some more recent scientists who faked experiments, invented data or plagiarized papers to further their own careers." Miller concludes that historians have to be judged by the same standards that apply to scientists. She asks: "Are the historians of science who set out to find fraud biased in how they examine the records and fuzzy in their interpretations?"

A bit of background: when historian Richard Westfall grabbed headlines last winter ("New Attack on Galileo Asserts Major Discovery Was Stolen," Wm. J. Broad, *NYT*, 13 Dec '83), the same Root-Bernstein came

once more to the defense of genius, again providing—in terms of the normal scientific process—“a simple explanation for the Galileo story . . . that fits Westfall’s data yet exonerates Galileo of wrong-doing” (*Science News* 17 Dec 1983 p. 387). In the *Times* story Broad played up the sensationalism of Westfall’s accusation that Galileo had appropriated the discovery of a former student without giving credit, but then covered his rear by getting an opinion from Stillman Drake:

The tug of war over Galileo may ultimately tell more about the process of scholarship than about the man, according to one observer. “Everything Galileo ever did has been challenged,” said Stillman Drake, a historian at the University of Toronto who is a biographer of Galileo. “But ultimately it stands up.”

There may be a philosophical question here which goes deeper than matters of venality and opportunism. How often do scientists, historians and philosophers (e.g., Feyerabend on Galileo) jump to an unwarranted conclusion of this type because they try to reconstruct a discovery process according to the rules of formal deductive logic? Mendel was manipulating his data, concerned only with the *content* of his pea categories, Galileo was groping for *meaning* in his observations. Formal logic could be of no use to them because it is oblivious to the content of its subject matter. Dialectical logic, on the other hand, is not only concerned with the content but also with the relationships or connections between the observations or categories. That’s the informal kind of logic a scientist has to use in the discovery process, whether or not s/he has a name for it.

F.V. Constantinov et al., editors, *The Fundamentals of Marxist-Leninist Philosophy*. Moscow: Progress 1982, tr. from Russian 1979 edition. 480 pp., cloth. (\$9.95, see Basic Bookshelf).

From a quick sampling, this new edition of *Fundamentals* seems pretty much the same as 1974 edition. Many small and subtle changes improve the clarity and a few very happy new formulations were noted. Characterizing Marxist-Leninist philosophy as “a constantly developing *theory*” qualifies it properly as “an autonomous branch of *knowledge*” (p. 13, emphasis added). On the basic question of *consciousness* and *being*, a new emphasis is on causal dependency: “Which is the cause . . . ?” and “what may be considered primary?” (p. 17). On quantum mechanics, some cautious modifications were seemingly made to avoid giving any credence to the Copenhagen interpretation, e.g., a new statement that “Microprocesses obey certain laws, they follow a sequence” (p. 133).

The format is new, with larger page size and estimated cut of 9% in total words. Most deletions involved no significant loss of content but there was a regrettable tendency to eliminate lively explanations and helpful examples: four such cuts were found in the discussion of cause and effect (pp. 128–134) and a quote from Soviet physicist Vavilov on the heuristic significance of experimentation is sadly missing from the excellent chapter on dialectics of the cognitive process (p. 175). Deleting the 13-page chapter on the scientific method may be justified since it did not sufficiently equate this with dialectical method.

A change noted in the discussion of ideological struggles today has significance for all who wish to survive the Reagan era: previously the emphasis was on how the bourgeoisie depended on revisionism of both the left and right varieties; now the stress is on the importance for peace of the

struggle against the “ruthless anti-communism” of imperialists (p. 363).

Few would agree with all the formulations in *Foundations* but, for the natural scientist, it continues to be the most complete and systematic, most accessible and useful source concerning dialectical materialism and Marxist theory of knowledge.

David R. Lifton, *Best Evidence: Disguise and Deception in the Assassination of John F. Kennedy*. New York: Macmillan 1980.

A physicist provides us with an object lesson in the scientific method, recounting 14 years of intensive research on the events of 22 Nov. 1963. When his results were written up as an abstract analysis of the evidence, he found no publisher. Recast as a personal narrative, however, it grabs you (as it did a publisher)—like the best detective fiction. The boundaries between truth and falsity keep shifting as Lifton discovers new information and peels away more layers of unfounded assumption. Similarly, the distinction between good guys and bad guys repeatedly dissolves and reforms as experience leads to deeper understanding of the particular humans involved. These dialectics of the discovery process seem to emerge from his candid historical treatment rather than from philosophical consciousness. (Lifton’s only mention of contradiction is in relation to evidence, and leads him to recall an Ayn Rand theme “that, in logic at least, contradictions do not exist.”)

Perhaps the biggest transformation occurs in Lifton himself. At the start he believes an assassination conspiracy to be out of the question because too many people would be involved. But he ends up with the most definitive evidence yet for an actual conspiracy, showing realistically how it could have evolved through normal processes of government (he leaves the reader responsible for what action to take on the revelations). The book is recommended not only as “an exercise in epistemology” (p. 698) but also as a human document that penetrates beneath the appearances of our society. Not least of Lifton’s lessons concerns the profound differences between legal and scientific truth.

Lester (Hank) Talkington

Basic Bookshelf on Marxism in Natural Science — — — — —

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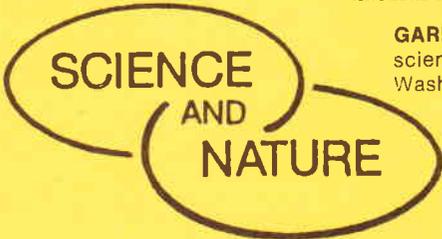
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