Scott H.

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# SCIENCE AND NATURE

*The journal of Marxist philosophy for natural scientists*

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

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

**Editorial correspondence**

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# LETTERS TO THE EDITOR

## On Philosopher-Scientist Teamwork

Maybe my ironies masked the point, or maybe Talkington missed it in his review of my "Sin, Science and Society" paper (*S&N* #4, pp 79-80). I wouldn't want the contemporary philosophy of science community to have much to say about science, science policy or research, because I think they would say mostly wrong things; and this on the grounds that they have very little relation to either the practice of science or to the social uses of science. The refinements of methodological critique and of historical reconstruction are very important, but not to the point I was talking about.

I also don't think Lenin solved the problem; and Soviet practice until quite recently has been a disaster in this area. It was Mitin and dogmatists of his sort who creamed the Soviet scientific establishment, and were almost universally despised by the working scientists, dialectical materialists among them included. So which philosophers? Soviet philosophy of science of the last period has become quite sophisticated, less dominated by the Apparatchiki, but is little known in this country. What its critical or guiding role for scientific practice is I don't know; but that is because, I suspect, it doesn't pretend to assume one.

I wouldn't argue that philosophers should *not* have a role in the clarification and critique of scientific practice, in general, because I don't think this is a general question. What is general is that philosophy should have a role in science—a position I am quite clearly associated with in much of my published work: but not any old philosophy, and certainly not the pseudo-philosophy which characterized the Stalin period and the Lysenko disaster. A "guiding" role would be, to my mind, much too strong, and also dangerous. Platonic philosopher-kings are to be dethroned wherever they arise. They are autocrats. Science has to become philosophical—and historically has been, in its deepest moments. But no philosophical vanguard of the scientific proletariat, please. Critical collaborators, yes; but modest ones, willing to learn from the practice of scientists and the social practices of a society what science is and what science needs. Where Lenin spoke of a partnership—with which I agree—that's fine. Where a senior partner decides policy, I disagree.

Talkington makes, I think, an elementary error in misinterpreting what I *described* as a "slight ripple in the pond"—which is in fact, the case for the philosophy of science I was talking about *concretely*—for a normative argument that philosophers in general *should* be "sideline critics" (his words, not mine.) At present, philosophically-minded scientists and scientifically-minded philosophers are beginning to make a bigger ripple—e.g. in current debates in biology (about genetics, evolutionary theory, sociobiology) and in some of the social sciences. That's all to the good. But it is a beginning only. In any case, I prefer a small ripple, to a big splash, if the splash comes from dumping philosophical garbage into the pond of science.

I must say that among working scientists in the physical sciences (including Marxists) the "potential value of the dialectical materialist mode of thought" remains a vague promise, because the heuristics of the mode of thought count for very little unless they can be interpreted specifically and in detail—and they haven't been in a very long time. I am all for following through on this, but it will require an internal critique of older and inadequate versions of dialectics in science which badly need to be discarded or "aufgehoben". Appeals to the classics don't bake any scientific or philosophical bread.

*Marx W. Wartofsky*
Dept of Philosophy
Boston University

My criticism of the Wartofsky paper (*S&N* #4), was intended to be *concrete* and based on the internal evidence of the paper inself, which seemed to be written from the viewpoint of a sideline critic rather than that of a philosopher working in partnership with scientists. Since Wartofsky agrees with Lenin on the need for such a partnership, his paper would have been more constructive if, along with his perceptive criticism of bourgeois philosophy of science, he had presented also the alternative Marxist approach.

I think that Wartofsky will find *Science and Nature* in basic agreement with the substance of his arguments. For example, we do not propose in any way that philosophy should have a dominant role. In criticizing his paper, I proposed only that Marxist philosophers can and should "help scientists themselves clarify their working philosophy, that which actually guides scientific practice from day to day." This urgent goal can be achieved only by *voluntary* cooperation in which the philosopher seeks to grasp the essence of the concrete scientific problem while the scientist seeks to understand how Marxist philosophical principles can help illuminate the same problem. Creative collaboration along this line will help advance both science and philosophy. But we can expect that the process will be neither easy nor peaceful all the time. Better to argue out the issues in the pages of *Science and Nature.*

Secondly, we all agree that the Stalin period was a disaster, but this must be seen as the result of an arbitrary intervention by the state into the internal affairs of not only science but also philosophy of science (and both may still suffer somewhat in the USSR from the effects of the Stalin distortion). On the other hand, our immediate responsibility is more directly concerned with the distorting effects of the system under which we live and practice. Whatever degree of professional freedom we may enjoy under state monopoly capitalism and multi-national imperialism must not be permitted to delude us concerning the insidious effects of bourgeois philosophy on the scientific enterprise. Problems of both these sorts, concerning the three-way relations of science, philosophy and society, are open for discussion and debate in *Science and Nature.*

Lastly, though Lenin has useful things to say, we can agree that he did not solve all our problems. The physical sciences, as Wartofsky suggests, need particularly the articulation of Marxist philosophical principles as they apply to concrete problems. But to see this urgent need only in terms of "an internal critique" of dialectics seems quite one-sided. Equally necessary is the need for analyzing the prevailing positivist/empiricist agnosticism which permeates physics. Personally, I think that J.D. Bernal has pointed the way toward such an analysis:

> Positivism is not at root a philosophy derived from physics . . . but it has bitten very deep into physics, especially in Britain and America, where a traditional distrust of all philosophy makes scientists unconsciously an easy prey to the first mystical nonsense that is sold to them. The relativism of Einstein, the indeterminacy of Heisenberg, the complementarity of Bohr, take a positivist form, not for any intrinsically physical reason but because they were conceived by men brought up to have a positivist outlook . . . As it stands, the whole of modern theoretical physics has no coherence: it is full of logical inconsistencies and circular arguments. [*Science in History*, MIT Press 1971, p. 861.]

That's my opinion. And, of course, it's subject to rebuttal in the pages of *Science and Nature.*

Our primary editorial purpose is to demonstrate that the principles of dialectical and historical materialism provide the most useful philosophical framework for the cognitive problems of the practicing scientist. And this is *not* a "vague promise": see the excellent statement by Nobelist Nikolai N. Semyenov, "On Intuition Versus Dialectical Logic" (*S&N* #1). We believe that our pedagogical purpose is often best served by publishing side by side the opposing views and critical comments of Marxists who disagree on how Marxism applies to scientific problems. An instructive example is the continuing discussion of causality in quantum mechanics (*S&N* #3, #4 and this issue). There are many more such issues in biology, physics and mathematics which need the same kind of ventilation.

*Lester Talkington*
Tappan, New York

## The Dialectics of Dialectical Logic

Thanks for the material you sent. Your journal looks quite interesting, and I wish to subscribe, starting with the 3 back issues.

I appreciate the opportunity to comment on your Bibliographic Brief [*S&N* No. 4] of my article on contradiction in dialectical materialism [*Sci & Soc* 41: 257; 1977]. My intent was to show that real dialectics and their representation in thought are not incompatible with classical logic, and that their presentation in strict Hegelian terms is unnecessarily obscure and even misleading. For the record, I do maintain that nature and that part of nature we call the mind, and also society, are dialectical. All the essential characteristics are there: the unity and struggle of opposites, qualitative transformations, etc. The contention is over the compatibility of classical logic and dialectics, and the worthiness of a Hegelian logic (or of a separate dialetical logic at all).

To restate the crux of my position: a formal logic *per se* is an abstract system, rather like a game, and need not have any relation to either natural thought or nature. Formal logics are non-contextual, so let's forget about them for the time being. We are concerned here with natural logic, the logic implicit in natural thought, and its ability to represent the fundamental, dynamic patterns of the natural world. Natural logics are contextual, and I would suggest that any dialectical process can be described in terms that are compatible with classical logic taken contextually, i.e., the rules of classical logic applied to the particular dialectical context (for example, discussions of dialectics are, for the most part, consistent with classical logic). Some will object that doing so will distort dialectics into a nondialectical form, but this is not inevitable —if I am correct in contending that the essential characteristics of dialectical processes are not incompatible with a classically consistent representation (description).

That strict Hegelian dialectical logic and classical logic (as man-made representational systems) are incompatible, is not the point here. The question is what framework most lucidly describes (or captures) the characteristics of actual dialectical processes. Every concrete example I have ever seen purporting the incompatibility of classical logic and dialectics has misrepresented and misapplied the former. (Several such examples originating with Hegel, Marx, *et al.* are discussed in my *Science and Society* paper.) We can argue all day in the abstract (because we're speaking different languages), but can any defender of Hegelian dialectics come forward with a concrete example, perhaps from the natural sciences, of a dialectical process that cannot be described within a classically consistent framework?

Concerning the accusation that I am a bourgeois philosopher, I believe as a Marxist that "individual" consciousness is social in nature, and that, being a philosopher in a bourgeois society, my philosophical consciousness is bound to have socially and historically delimited constraints. Until the day we are born and raised in a mature socialist world, we are all "bourgeois philosophers". In the meantime, we might restrict our use of the term to those ideologues who push a clear pro-capitalist, anti-Marxist line.

*Michael Mark Mussachia*
180 Calle Cuervo
San Clemente, CA 92672

We welcome Mussachia as a subscriber and look forward to more dialog with him on the philosophical problems of science. We find some definite areas of agreement in his letter. For example, when Mussachia affirms that the "part of nature we call the mind" is dialectical, and when he defines "natural logic" in terms of "natural thought, and its ability to represent the fundamental, dynamic patterns of the natural world", it seems that only differences of terminology separate him from the Marxist concept of dialectical logic. We can further agree with Mussachia's central argument that the *description* of a dialectical process must be "compatible" or "consistent" with traditional logic; scientific discourse demands logical construction of descriptive statements.

But Mussachia's discussion stops short; it fails to deal with some essential aspects of natural thought. We must ask whether natural thought consists exclusively of descriptive statements? Is not Mussachia's account incomplete since he fails to discuss those creative thought processes which, it is widely agreed, cannot be explained in terms of classical, formal logic? How does Mussachia propose to account for the origin



and development of new scientific concepts and hypotheses? Karl Marx, that incorrigible dialectician, has shown how a different kind of logic is required for thought processes at the inquiry stage (before descriptive presentation is even possible). Discussing his own use of the dialectical method, Marx wrote that

> the method of presentation must differ from that of inquiry. The latter has to appropriate the material in detail, to analyse its different forms of development, to trace out their inner connexion. Only after this work is done, can the actual movement be adequately described. If this is done successfully, if the life of the subject-matter is ideally reflected as in a mirror, then it may appear as if we had before us a mere *a priori* construction. [*Capital,* N.Y. 1967. *i*, 19 (preface to 2nd German edition).]

Thus Marx explains why the usual description of scientific results makes it *appear* that scientific thought proceeds according to the laws of classical logic, though the actual thought processes develop dialectically (whether or not the scientist has ever heard of dialectics as the natural mode of investigative thought). Engels and Lenin dealt at much greater length with the special role that dialetical logic plays in the conceptualization processes of scientific research; the interested reader may turn to their works to learn more about what is missing from Mussachia's account of natural thought (see Basic Bookshelf list this issue).

We must also address the central question posed by Mussachia: "what framework most lucidly describes (or captures) the characteristics of actual dialectical processes?" Agreeing already that any description must consist of logically consistent statements, the answer is simply that the framework of the so-called "laws of thought" based on classical or formal logic are necessary but not at all sufficient for the purpose. Here, Marx provides an excellent "concrete example" in *Capital* itself, where the dialectical mode of inquiry is forever shining through his logically constructed statements describing the results obtained. "To Marx," says Robert S. Cohen, "exposition and articulation, when carefully accomplished, showed the movement of thought, a conceptual dynamic." [*Dict. Sci. Biog.* xv, 411.]

Finally, there is the matter of name-calling. I objected to Mussachia characterizing as "Papists of the Left" those like myself who find dialectical materialism a useful philosophy. He objects to my characterizing as "bourgeois prejudice" his attacks on dialetical materialism. I agree that we should drop all such labels and work together toward rooting out bourgeois elements within Marxist philosophy, learning to speak the same language, and moving the world toward mature socialism.

*Lester Talkington*

## In Defense of History

I enjoyed the item on Popper in which you saw fit to invoke my authority [*S&N* No. 4 p2] and agree with it wholeheartedly. It seems to me that his resistance to scientific analysis of historical subjects has done great harm to both historical science and philosophy of history.

*Arthur L. Caplan*
Associate for the Humanities
The Hastings Center
Hastings-on-Hudson, N.Y.

## Sociobiology Deja Vu!

Here is a nice quote from Lenin on sociobiology. Note that even back in 1906 the same terminology was used:

> The author [Bogdanov] begins . . . by refuting the "eclectic socio-biological attempts of Lange, Ferri, Woltmann and many others" . . . Can anyone imagine anything more sterile, lifeless and scholastic than this [Bogdanov's] string of biological and energeticist terms that contribute nothing and can contribute nothing in the sphere of the social sciences? . . . meaningless terms which seem to lend "profundity" to the questions but which *in no way* differ from the eclectic biologico-sociological attempts of Lange & Co.! . . . all he is doing is to reclothe the results already obtained . . . in a biological and energeticist terminology. The whole attempt is worthless from beginning to end, for the concepts "selection", "assimilation and dissimilation" of energy, the energetic balance, and so forth are, when applied to the sciences, but *empty phrases*. In fact, an inquiry into social phenomena and an elucidation of the *method* of the social sciences cannot be undertaken with the aid of these concepts. [*Materialism and Empirio-Criticism*, N.Y. 1970, pp. 339-340.]

*Val Dusek*
Dept of Philosophy
Univ. of New Hampshire

PS: With respect to the conflict over "real" contradictions in nature, see the pamphlet by G. von Wright, *Time, Change, and Contradiction* (Cambridge Univ. Press 1969). One of the world's most eminent logicians argues here for the choice: *either* real contradictions, *or* totally discrete, atomistic time. V.D.

## On Feminist Critiques of Science

Elizabeth Fee has presented a very constructive analysis, grounded in the realities of our time. She clearly makes a good case for the "feminist critique as a tool for seeing what it might mean in practice to liberate science from the inherited habits of thought inscribed by the previous separation of human experience into mutually contradictory realms" . . . ["Is There a Feminist Science?" *S&N* No. 4].

A good point was made by Fee when she notes that scientists today are salaried workers in "big science." As a matter of fact, engineers and scientists have a long history as mercenaries serving feudal lords and military empires, and this pattern has now extended to the present era, dominated by the large industrial corporation. In the early days of modern science, objectivity and disinterestedness were a part of the self-protective ideology of small-scale science. Scientists, whether they know it or not, have now evolved beyond this idealistic "objectivity" and must seek personal integrity in ethical and political commitment.

*Norma Undershaft*
445 S. Kensington Ave.
La Grange, Ill. 60525

Science must indeed be considered relative to its historical and social contexts but *in principle* it is one, unified body of knowledge. It makes sense to pursue the study of scientific socialism but transposing



the terms into "socialist science", or now into "feminist science," is not particularly meaningful. Elizabeth Fee ["Is There a Feminist Science?", *S&N* No. 4] supports the notion of Jean B. Miller that the male psyche, as socially created in the western capitalist world, is peculiarly unable to integrate self-creative activity with a primary concern for others. What are we to make of such a thesis as a criterion for evaluating the comparative contributions of Rosa Luxemburg and V.I. Lenin to the scientific analysis of imperialism? With all due respect to the need to explore fully the problems of thought and feelings, the "radical feminist view of science" pursued by Ms. Fee is diversionary as it stands, needing much more solid work to make it *intellectually convincing*.

*Robert A. Griffin*
Southern Connecticut
State College

*EDITOR'S NOTE:* What Dr. Fee really advocates can be summarized in the following excerpts from her paper [*S&N* No. 4, pp. 48-49]:

> The radical feminist view of science is only one of the forms in which the growing popular distrust of scientific institutions and authority is expressed . . . Because science has been presented as an objective force above and beyond society, and because it has been seen as a monolithic power, it may appear that the claim of science to be the arbitrator of truth must be accepted or rejected wholesale . . .
>
> We need not, however, go so far as to reject the whole human effort to comprehend the world in rational terms, nor the idea that forms of knowledge can be subjected to critical evaluation and empirical testing . . .
>
> The radical feminist critique of science and of objectivity, therefore, needs to be developed in ways which will allow us to identify those aspects of scientific activity and ideology which need to be questioned and rejected, without at the same time abandoning the ideal that we can come to an ever more complete understanding of the natural world through a collective and disciplined process of investigation and discovery.

## Marxist Internationalism

I appreciate very much your efforts in publishing an interesting journal. I will try to urge my colleagues in other Japanese universities to subscribe to your journal. Please send me five copies of each issue (Nos. 1 to 4) as well as the bill and subscription forms.

I will urge my colleagues to send you English versions of their papers, but I am afraid that very few Marxist philosophers and social scientists in Japan write their works in European languages.

I am president of the Tokyo Ass'n for Japanese-Vietnamese Friendship and, in this context, I would like to urge you to send your journal to Institute of Philosophy, Academy of Social Sciences, 27 Tran Xuan Soan, Hanoi, Socialist Republic of Vietnam. If you cannot mail the journal to Hanoi directly, I am ready to forward it there. As Professor at Hiroshima University I have been also strongly engaged in the struggle against nuclear weaponry and for human survival.

*Shingo Shibata*
Hiroshima University

## Asks Info on Militarization of Science

I am looking for material and information to help me and a group of colleagues in Italy in a research programme on *The role of military funding (USA, NATO, local military institutions) in shaping research policy and priorities of NATO countries, particularly Italy.*


We don't care how many degrees you have, Doctor . . . How is your killer instinct?

Please contact me if it is possible for you to help us obtain statistical data or bibliographic information on either US funding of research in Europe through military channels (air force, navy, etc.) or NATO funding of research in Europe. Also helpful would be information on any kind of similar analysis published in the US or elsewhere on the relation between military funding and definitions of research policies/priorities. Many thanks for your cooperation.

*Bruno Vitale*
c/o C. Othenin-Girard
14, Nant du Crève-Coeur
1290 Versoix (Genève)
Switzerland

### *Grant Swinger Surveys the Reign of Reagan*

**Q.** How's it going for the Institute?

**A.** We almost got approval for an oil-solidification project.

**Q.** Oil solidification?

**A.** You've got to keep moving in this business. Carter staked us for coal-liquefaction, so we tried to move on to the next step when Reagan came in — oil solidification. Nothing like it in the world.

**Q.** What went wrong?

**A.** No Russian angle. It flopped on that. Each administration has its own style for research. This one is bananas on the Russians. If they're doing something, or could do it, or might do it, you're home free. . . .

**Q.** Yes, what else? . . .

**A.** Take MX missile basing . . . We had a good five-year run on trucks versus trains, shelters in the desert, midget subs, airplanes. And then we had combinations of some of them, and when all that was worked over, one of our bright fellows came up with a blockbuster idea that brought another contract from Defense.

**Q.** What was that?

**A.** He raised the possibility of doing nothing — no MX missile, no nothing to hide. That was there all along, but it takes a special kind of mind to dig it out from the background noise. So, we went back to the beginning and studied doing nothing. We'll soon have the findings. . .

— Daniel S. Greenberg, *The Grant Swinger Papers*, 1981. ($4.95 postpaid, from Science & Government Report, PO Box 6226, Northwest Station, Washington, DC 20015.)



*Scientist in the Struggle*

# On Sociobiology and Activism in Academia

**DAVID B. ADAMS**
Psychological Laboratory
Wesleyan University

Interviewed by
Lester Talkington

**Q.** *Do you see today's sociobiology 'boom' as a unique phenomenon in science?*

*A.* The importance of sociobiology is more political than scientific. From the standpoint of science, sociobiology has produced a great deal of speculation but very little in the way of empirical results with any lasting significance. In my own field of comparative psychology and physiology, for example, people use the same methodology as before the meteoric rise of sociobiology 'theory'. Considered politically, however, the matter is more complex. Edward O. Wilson and his followers have received so much publicity, and so much of it very reactionary, that sociobiology cannot be ignored. The impression has been given that human behavior is mainly determined by genetic factors, so that political efforts for social change are hopeless or, at best, severely hampered by a supposed inflexibility of 'human nature'. Anyone with even a slight knowledge of history knows, of course, that this pessimistic view is simply not true. The task for progressive scientists is to show that what we know about the biology of human nature is consistent with the ability of people to change their social system, with the ability of people to make their own history. To get this message across is very important, epecially in the struggle for peace and equal rights. I'm looking for others to join in a massive fightback from the scientific community.

*Q. Let's go into your fightback plan later. First, how do you account for the meteoric rise of sociobiology in the scant six years since Wilson published his treatise on the subject?*

*A.* This is primarily a media event reflecting, of course, the class outlook of those who own and control the media. But there is a background factor, a weakness in our whole system of biological education, of which few people are aware. It goes back to 1957 — to the time of Sputnik — when Congress began to appropriate huge sums for scientific education and research in order to "catch up with the Russians". Though part of the rationale for this spending was to boost our medical research, the basic rationale was always military. As far as I know, the National Academy of Sciences continues even now its long-time policy of testifying for larger scientific appropriations on the basis of strengthening the military. Because of this rationale, the government funding of science was one-sided. The physical and biological sciences were heavily funded while research on basic social problems was neglected. There has never been adequate federal funding or university support for cross-cultural research or truly analytic studies in sociology and economics, as opposed to superficial studies which take the present system for granted.

I know about this because I'm literally a child of Sputnik myself. I graduated from high school the year Sputnik went up and, for the next ten years, was totally supported by the government in my scientific education. Hence, I became a victim of the one-sided funding. Though I belong to the generation that has produced the greatest scientific output in the history of the world, I have had to look back and become aware of how it developed a false consciousness in me. Instead of learning about the interaction of culture and biology, I studied and did research only in biology. Most of my colleagues suffered from the same lop-sided education process.

*Q. Would you say that McCarthyism also helped foster this one-sided development and thus provided a social environment for the acceptance of sociobiology?*

*A.* Of course, McCarthyism laid the ground in the 1950s by effecting a virtual ban on comparative cultural studies, especially on the Marxist approach. This led to our lost generation of students who would learn a lot about biology and very little about society. When you combine this historical fact with the bias of federal funding towards hard science and away from social science research, then you get a cultural void in which the mass media operates. Add to this the bias of advertisers' influence on the mass media that stresses the status quo and opposes social change. All things considered, it's small wonder that newspapers and TV stress biological explanations rather than social explanations for human behavior.

*Q. And the void starts right at your own level, among the teachers and the researchers?*

*A.* That's pretty much the picture. I don't know many biologists who have done what I had to do recently, take myself out of the lab and read about crosscultural studies for the first time. This way I have learned some useful techniques of behavioral study for truly investigating the relation between the biological and the social. I'm afraid the more usual outcome is that a scientist sees the chance for cheap publicity by jumping on the sociobiology bandwagon.



*Q. Does sociobiology offer career opportunities for academics?*

*A.* Yes and no. In applying for an NIH grant, it probably doesn't help that much to say you're a sociobiologist . . . but it may help you get money from certain rich foundations. It can help get a job in some academic departments (especially in the social sciences), and it sure helps in getting a book published. But I think it's a poor foundation for building a career, too much like putting up a house on shifting sands.

*Q. Do you see any possibility of weaning the academic world away from the one-sided sociobiology craze?*

*A.* It will take a long time to turn the tide. But I think it can be done if progressive scientists learn new ways of working together and also learn to be more sensitive to the political implications in their own research and publishing.

*Q. Are you suggesting some kind of self-censorship?*

*A.* Quite the contrary, I'm suggesting that scientists have to be more wary of some subtle forms of censorship they face. I learned about this the hard way. In 1979 I was principal author of a study, based on a questionnaire about women's sexual activity day to day, which demonstrated in quantitative detail for the first time that the human female has a period of estrus like other mammals. In other words, a woman's sexual activity tends to increase when estrogen levels are elevated during ovulation. While our study was designed to reveal this effect, the data also showed that estrus was weaker than other non-biological influences such as the 'weekend effect', a tendency toward greater sexual activity on weekends. We submitted our report to the *New England Journal of Medicine* in such a way as to emphasize only the part dealing with hormones, ignoring the social-cultural aspects of our study. The result was predictable. The hormonal factors got a great deal of publicity in media around the world while the cultural aspects, which appeared in less prestigious journals, were never cited or publicized.

Progressive colleagues have criticized me, and rightly so, for allowing the biological and cultural components of the report to be published separately, and thereby increasing the likelihood that the results would be misquoted and distorted in the media. We learned from this experience the importance of keeping intact the interactions of the biological and the social when we publish.

Even more basic, I have come to see, is the obligation to design the experiments in such a way that interaction of biological and developmental/cultural factors can be revealed clearly. Otherwise, we always run the risk that experimental results can be taken out of context and used to support an ideological bias. I had never realized the importance of such matters until I came to see how much distortion can be introduced because of the 'sociobiology' bias toward the biological.

*Q. Isn't a big source of such bias the emphasis in sociobiology on sex and especially the relative roles of male and female?*

*A.* Obviously. Sex differences go over big with the media in the present political climate, what with the defeat of E.R.A. and the high rate of unemployment which creates competition for jobs between men and women. In particular, we see claims that women lack mathematical aptitude or spatial skills because of innate sex differences but these supposed scientific claims are based on data taken out of context or from isolated and unreplicated studies. Some people, of course, are looking for such differences for reactionary reasons. Other scientists lay themselves open to such misinterpretation of their work when they simply throw in sex as an experimental variable without regard to what they are studying, whether in animals or humans. And, of course, sex differences are found. To say that the sexes differ is hardly a discovery. But what is the purpose of the research? After all, the mere finding of a significant difference in a population is not the essential task of science. We could find differences between rich and poor people on just about every known physiological variable, but it would not be scientifically useful to demonstrate them all. Instead the purpose of science is to understand the mechanisms, the major causal relations and variables, of significant natural phenomena. The more I work in science, the more I am convinced that to understand the purpose of science we must study its function in society rather than in some disembodied abstract philosophy of science. I know that *Science and Nature* is devoted to this task and I hope to continue learning from it.

Back to the question of sex differences, it seems to me that some are trivial and are being exploited for regressive political purposes, while others are not trivial and need to be researched. For example, I have been investigating the question of why warfare is mostly carried out by men and not by women.

*Q. Warfare would seem to be an activity where biological sex differences would clearly dominate.*

*A.* Of course there are biological factors, but they are so obvious that they are practically trivial, and they still interact strongly with cultural factors. Warfare and hunting go hand in hand; they require the same weapons and skills, and the same mobility for long excursions. Women are not in as good a position to carry out these activities because a big part of their lives must be spent bearing, breast-feeding and otherwise caring for children. But note that this is a statistical tendency, a strong trend, if you will, not an all-or-nothing phenomenon. Some women do not bear children and they, presumably, are as physically capable of using weapons as are men. In fact, we find that in some primitive societies there are often some women



who go along to fight. The interesting question, then, is why this happens in some societies and not in others.

*Q. Can it be just a matter of social conditioning, how they are brought up?*

*A.* The problem is much more complicated than that. The answer seems to lie in a contradictory relationship between the social institutions of marriage and warfare. What we find is that in a majority of cultures that have frequent warfare, the marriage residence pattern is that of patrilocal exogamy, that is, marriage partners come from different communities, the wife going to live with the husband's family. The contradiction arises because, under such circumstances, there is a certain likelihood that warfare will find the husband and his community on one side, the wife's father and brothers on the other side — in which case, the wife would have split loyalties. Should she support her husband or her brothers? Historically, it seems, the simplest way to resolve this contradiction has been to exclude women from warfare altogether. Women are not allowed to attend the war-planning meetings, not allowed to own, make or even touch the weapons of war (nor, since they are often the same, the weapons of hunting), or even to sleep with their husbands in time of war. In one culture, the fingers used to pull a bow string are cut off from little girls, making it certain they cannot take up arms.

The power of this analysis emerges when we go on to consider those cases in which the marriage residence pattern is not one of patrilocal exogamy. If the marriage system is endogamous (marrying within the community) or if the warfare is exclusively external to the area from which wives are drawn in exogamy, then there is no occasion for split loyalties and women do sometimes take part in warfare. I have found this to occur in 25% of the cultures surveyed. By contrast, I found no cases of women taking part in warfare in those societies where the war might be fought against their own kin.

In sum, the important thing here is the interaction of the biological *and* the cultural factors. Taking this many-sided approach makes it clear that men do not have some kind of "war instinct" that is lacking in women. To the contrary, we can see that war is a cultural institution that interacts with other cultural institutions, and thus is amenable to change.

*Q. Edward O. Wilson has a new book in which he argues that genes also determine culture [see book review, this issue]. How do you counter such a claim?*

*A.* Again there is no reason to take Wilson's claims seriously from the standpoint of science. There is no direct correspondence of genes and behavior. His claims are, instead, ideological statements addressed to the mass media. When Wilson was cornered by a New York Times reporter, he admitted that biology could account for no more than 10% of the variance in social behavior while cultural factors would account for the other 90%. This, of course, is not the ratio of emphasis given in the mass media.

*Q. Recalling the essay by Engels on the role of labor in the transition from ape to man, I wonder if it's possible to see the actual biological roots for cultural behavior at the pre-human level?*

*A.* Indeed, yes. Japanese workers have amassed a nice body of literature on the transmission of cultural behavior in the macaque monkey. They discovered an interesting law of cultural transmission in these primates which seems to apply also to humans: older males tend to be the most fearful of change—the most receptive to new behaviors and new objects are the young animals, with older females second. The usual order for developing a new behavior in a macaque troop is that it is begun by a very young animal and slowly picked up by others until, perhaps after several years, it is adopted by an older male, then the new cultural trait soon becomes locked in for the whole group. Once in a great while, however, an innovation will begin with an older male and then adoption by the whole group is very rapid. When we look at our own culture, we see that innovations of typical cultural traits such as slang, new clothing styles, changes in food habits, and so forth, all tend to be initiated by the young and then by women.

*Q. And sometimes by minorities? By Blacks, for instance?*

*A.* Yes, but there also it is usually the young who initiate things which are then passed along. Apparently this law of cultural transmission applies to all primates including us humans.

*Q. What about universities as initiators of social change? Does the equation still hold here with regard to young and old, male and female?*

*A.* I'd like to think academia is an exception but, generally speaking, it seems we follow the same primate pattern. Academics, especially the older males, have an amazing ability to speak but not listen, to teach but not learn. Marx knew about this. Somewhere he wrote that the professors would be the last to see how society is changing.

*Q. Was he referring to the revolution?*

*A.* It was just a general statement about professors. And it seems to be generally true on our campuses today. The activist who concentrates all his effort for social change in campus work is likely to get very discouraged. Most professors lack class consciousness or, better said, they have a false consciousness of their class interests. They tend to be elitist. They don't want to be considered workers. Instead, they consider themselves to hold a privileged status — which some of them actually have. It is no accident that the professors think this way. They are products of the academic tenure



track. It starts with the untenured who are afraid to speak out politically. Those who do speak out, especially the young Marxists, are usually denied tenure. So the general picture is one of political paralysis which tends to persist even after tenure is achieved. This selection process produces a backward faculty, not only isolated from the working class but even from the workers on their own campus. One result is that you find little correlation between Marxist "ideas" and activism. Those who talk Marxism in class or over cocktails are not necessarily the same as those who work for a faculty union.

*Q. I'm reminded of Krupskaya's* Reminiscences of Lenin *where she recalls how Lenin reorganized the Russian Social Democratic Party in a fundamental way by forcing the discussion groups of intellectuals first to admit workers into their midst and then to let the workers take over leadership. That, of course, is how Lenin laid the basis for the revolution.*

*A.* I'm afraid that most of my colleagues would not admit workers to their discussion groups. But we have to find ways to raise the class consciousness of academia.

*Q. Does contact with colleagues in socialist countries help raise the consciousness of American academics?*

*A.* Not as much as one might hope. When American academics meet their socialist counterparts, they're likely to feel they are meeting poor cousins, and there is some basis for this feeling. First of all, it's economics. The American academics are relatively rich. Socialist countries, like most other countries in the world, cannot fund science to the same extent. Secondly, the elite of the American academics are part of the ruling elite of our society. For example, the Vietnam War was really stage-managed by people like the Rostows and Bundys, from Harvard, Yale and Princeton. As far as I can tell, this privileged position is not matched in the socialist countries where the governments tend more to be run by workers rather than by a moneyed and intellectual elite.

*Q. During your years of work in the Soviet Union, what have you observed about the quality of scientific work there?*

*A.* It's all right. To my mind, in science you get what you pay for. If you put up a billion dollars, you get a billion dollars worth of science. If you put up a million dolars, that's how much you get. And it takes a long time. You've got to pay out for a generation. A scientific generation means the period of time you contract for bright students from high school through college. You train them with good professors (in this country we had good professors coming from Europe before and during World War II). You give graduate students their stipends and good laboratories and equipment. Then you give them positions with tenure. That's a full generation of almost 30 years — a lot of time and a lot of money. That's what the U.S. had from 1957 to 1980. The Soviet Union had it, of course, but not to the same extent.

*Q. You're speaking about your own area of brain research?*

*A.* No, about research generally. The Soviet Union has made breakthroughs in particular areas of concentration, but the general size of their scientific establishment is just not as great as here. The U.S. is the world leader in science for that reason. Though ideology may influence science in certain ways (such as the fact that we have a biological rather than social emphasis on behavior), still and all, given a particular problem, ideology doesn't make that much difference. Building science is like building a house. Whether here or in the Soviet Union, it still costs so much in materials, in man hours, and so forth. After you work in science for a long time, the mystification is gone. It's good valuable labor, but it's just labor. The analogy with building a house goes further, in that a great many people have to take part. One individual doesn't do it; there are carpenters, electricians, and so forth. Also, the building materials have to be available: if you run short of gypsum board or copper plumbing materials, you can't build the house. The same is true in science; there has to be a broad, general advance in science so that all the components are there for scientific discovery. Part of the *malaise* now in the U.S. is that this is being dismantled.

*Q. As the Soviet Union progresses, do you think they will invest more as we invest less?*

*A.* They are very steady, actually. I don't think their budgets are changing much. What I have seen there in recent years is an emphasis on increasing the quality of education for science — I have a feeling that a lot of it has been poor quality — and a push to get economic payoffs from science in a shorter period — trying to make science more applicable.

*Q. Don't you see science as a revolutionary force?*

*A.* Not by itself. Even socialist science *per se* is not a revolutionary force. It is simply science in the service of a socialist society, and it is the society that is the progressive force.

What the Soviet Union can do for the visiting American like myself is to make concrete the fact that socialism exists; it's not going away; it's an irreversible event of human history. And it delivers the essentials. It's a society in which people have food, clothing, shelter, health care and a fairly decent level of education, certainly better than the U.S. And the people there understand better what's going on in the world than do Americans.

*Q. Don't you think that science is revolutionary in its contribution to technology and the forces of production?*

*A.* Yes, but it's an idealist notion that scientists become consciously political or revolutionary just by the fact that they serve a



socialist society. Laboratory scientists as such are not necessarily in daily contact with the masses of people, whether in the Soviet Union or in the United States. That makes an important difference in their consciousness.

*Q. Then you don't expect that your research will contribute to political revolution?*

*A.* Not directly, but I'm beginning to feel that I should use my knowledge to influence the media. Scientists in general should develop a positive relationship with the press in order to communicate with the public. The press is not an impervious monolith. The same struggle goes on there as everywhere else. There are good people in the press and there are turkeys. Of course there is a hierarchial organization of the press with a conservative administration at the top, which is ultimately dependent on the advertisers. But even there it is not impervious. It's possible to get to know media people who will publicise a progressive point of view. Sometimes you can even help neutralize the reactionary publicity by setting yourself up as a consultant so that media people will call you, say, before they write about Edward O. Wilson. This needs to be done especially through formal committees of scientific organizations — media committees, ethics committees, or whatever.

*Q. Can this be done at the university level?*

*A.* I think the natural way is along professional lines. But we've had university groups in the past. During the Vietnam war, we had a Science Action Group at Yale which was effective because it was a part of the larger mobilization against war. That kind of thing will happen again, given the economic crisis, El Salvador, and so forth. I think we will see mass struggles to an extent that we haven't seen in a long time, and this will eventually involve the university people, including scientists. You must have some of that same feeling about your journal.

*Q. Well, we're getting* Science and Nature *on more and more campuses. And we believe it will spark some Marxist thinking. Even though the journal sticks pretty close to the professional interests of natural scientists, I think that it also helps raise consciousness in a way that will eventually lead to the involvement of more scientists in the political struggles of this tortured land.*

*A.* The key word there, I think, is "involvement." To make our professional work relevant, we scientists need to understand the role of this work in history. This means that we have to get involved in movements for social change that extend beyond the borders of the university. We have to learn from personal experience how popular pressures can influence events. This way we come to know how activism is essential to keep theorizing honest.

*Q. That's been true for me. I wouldn't really understand the dialectics of science without my years of rank-and-file work in politics where I could see after awhile how dialectical changes actually occur. But tell me how your political activism affects your professional life.*

*A.* First of all, I have become more and more involved in broad social movements outside of academia. This has given me a much better perspective on the significance of my work and on the problems of combatting the propaganda of sociobiology. Working in the peace movement and in local electoral politics, I see that people are really searching for answers to the big questions. They want to know: "Is war inevitable?" "Are we capable of developing economic and political alternatives to the present mess?" They realize that biology is important, but is it so fully determinate that it warrants pessimism about social change? Because of my professional work. I can give definite answers to their questions. I show them how biology and culture interact. I emphasize that humans are unique in their ability to create and re-create their own "human nature" many times over in their historical development from one culture to another in the repeated process of social revolution.

*Q. That's how you use professional knowledge in your external activism. But how does your external activism affect your professional activities?*

*A.* That's my second point. I engage fellow scientists and academic colleagues in the same pursuit, trying to involve them through professional organizations in work that relates to mass movements outside the ivory tower. For example, I recently published a paper calling on others in aggression research to get more directly involved in influencing the mass media and government agencies, and suggesting that we should all be working directly with community activist groups. Related to this, I have made changes in my own research work as I described before, choosing the topics of my research more carefully and trying to design research studies so that they will be more relevant to the kinds of questions that people ask who are involved in movements for social change.

You see, I am concerned about working with people who are already active — the people in the nuclear freeze movement, in the fight against Reagonomics, in the civil rights movement, and in the trade union movements. I think we should help them understand clearly both the biology and the sociology of "human nature."

*Q. Do you think we need a polemic on this subject directed against Edward O. Wilson in the style of the polemics by Engels against Dühring and Lenin against Bogdanov?*

*A.* My preference at this point is to talk about the issues rather than the individual proponent. The people I want to reach don't care about Wilson or the sophistry of his arguments. But they do need to be able to analyze the media stories and explain these matters to the general public, combatting the pessimism on human nature that is so corrosive, undercutting the faith of working people in their own abilities.

*Q. And how do you propose we approach the media?*

*A.* As you know, the sociobiologists have their own direct contact with the media. Wilson, for example, has the Harvard PR system at his beck and call. Well, we should be able to fight fire with fire. I propose that we set up committees within our professional organizations that are dedicated to the defense of human nature from its detractors. Those of us who contribute the majority of active people within an organization such as the AAAS should have the ability to call our own press conferences and confront these issues directly.

*Q. What kind of response are you getting in academia?*

*A.* I find that there are a fairly large number of academics already working in popular social movements. And there are more ready to do so when approached. It does take time for us to find each other. But once we have a critical mass of academics with links to the mass movements, you're going to see some changes in the relationship of science to the public. □

***The Whole Organism Is Greater Than Its Genes***

**I find a fatal flaw in Dawkins' [Selfish Gene]. No matter how much power Dawkins wishes to assign to genes, there is one thing he cannot give them — direct visibility to natural selection. Selection simply cannot see genes and pick among them directly. It must use bodies as an intermediary. A gene is a bit of DNA hidden within a cell. Selection views bodies. It favors some bodies because they are stronger, better insulated, earlier in their sexual maturation, fiercer in combat, or more beautiful to behold.**

**If, in favoring a stronger body, selection acted directly upon a gene for strength, then Dawkins might be vindicated. If bodies were unambiguous maps of their genes, then battling bits of DNA would display their colors externally and selection might act upon them directly. But bodies are no such thing . . . Hundreds of genes contribute to the building of most body parts and their action is channeled through a kaleidoscopic series of environmental influences: embryonic and postnatal, internal and external. Parts are not translated genes, and selection doesn't even work directly on parts. It accepts or rejects entire organisms because suites of parts, interacting in complex ways, confer advantages . . . Dawkins will need another metaphor: genes caucusing, forming alliances . . . But when you amalgamate so many genes and tie them together in hierarchial chains of action mediated by environments, we call the resultant a body. — Stephen Jay Gould, *The Panda's Thumb*, Norton 1980 pp. 90-91.**

*Origins of human society seen in a new light*

## Woman's role in the evolution of humankind

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A review essay:
Nancy Makepeace Tanner's
*On Becoming Human**

Reconstructing the very earliest history of humankind—the transition from the ancestral apes to *Homo sapiens*—involves two rather distinct activities. One facet of the process of evolutionary reconstruction requires locating, examining and questioning the technical evidence that can provide fragmentary glimpses of the past. Data drawn from the study of fossil teeth and bones, primate anatomy and behavior, and biochemical methods of dating divergent evolutionary strains may be augmented by comparative studies of contemporary hunting-gathering societies. The other part involves the imaginative creation of modern "origin myths": models that can not only incorporate and explain the existence of specific kinds of data but also, and perhaps more importantly, provide a comprehensible and plausible story of our development. We want to know how and why humans evolved, and in the process we expect to learn something about the essence of humankind, about human "nature," about our relationship to "nature" and about the "meaning" of human history. The development of our anthropological understanding thus involves two series of debates: one about the technical data and its interpretation, the other about the adequacy, accuracy and plausibility of the overall models devised to explain human history.

In any science, one can find gaps between the general explanatory structure or theoretical framework and the empirical data. Indeed, the lack of a precise fit is a precondition for progress in the development of a science. In evolutionary anthropology, however, the gaps between empirical "facts" and theoretical statements are especially marked because of the relative paucity of the technical data on the one hand and the enormous importance of the theoretical reconstruction for our political and philosophical sense of ourselves.

* Cambridge and London, Cambridge University Press, 1981.



The construction of the story of human evolution from the available technical information and data—the fossil finds, etc.—has been likened to creating the plot of *War and Peace* from thirteen random pages. The fossil fragments indeed provide fragmentary clues to a huge history. The use of alternative kinds of evidence, as for example, the study of contemporary primate societies, is highly controversial. The selective use of such indirect evidence may be influenced or in fact dictated by ideology; there is great freedom to create interpretations of the past based on the author's conscious or unconscious political commitments.

A central problem with almost all evolutionary reconstructions is that they ignore or trivialize the role of women. The females will typically be placed off-stage (perhaps in the trees) while the males invent the first tools, practice walking upright, discover fire, or explore the relative advantages of cooperation and competition. Exploration, discovery, the use of tools—almost any active relationship to the natural environment—are assumed to be male activities.

An amusing example of this rather common perception of females' limited, indeed, singular role is the novel *You Shall Know Them*, published in America in 1953.[1] Here, we are told that the "missing link" between man and ape has been discovered in New Guinea by a group of visiting anthropologists. The "tropis," midway between man and ape, were lively little beasts with bare faces and soft downy hair. They had discovered fire, showed a certain aptitude for mechanical construction and demonstrated a distinct preference for smoked ham. The female tropis are usually invisible in their caves, although one of them does get a starring role in the novel: she is artificially impregnated with human sperm and gives birth to a human-tropi baby. She is then promptly packed off to the zoo while the humans argue about the philosophical, religious, moral and legal dilemmas created by the existence of the tropis: were they man or animal? Could they be exploited as a source of cheap labor? Could they be eaten? Should they be baptised?

Much of the sexism built into evolutionary reconstructions is unconscious and unintentional. In part, it derives from the fact that science and technology in advanced societies are dominated by, and identified with, men; this is assumed to be an ahistoric "natural" reality, which can then be read back into the earliest uses of tools, and the first explorations of the natural world.

When many of the earliest tools discovered appeared to be used for hunting or fishing, this underlined the initial assumption: males were identified with hunting; and males would be the first to use tools. The hunt seemed to be the quintessential "primitive" activity, and also the quintessential masculine activity; it seemed but a small step to seeing hunting as a crucial activity in making the transition from ape to human society. Learning to hunt is then "becoming human." In Robert Ardrey's blunt formulation, "we are uniquely human even in the noblest sense, because for untold millions of years we alone killed for a living."[2]

The very language we use to describe the transition to *Homo sapiens*—from ape to man—also renders women invisible. The subject is "the evolution of man"; one thinks of "woman" only in connection with sexuality, birth, reproduction. It would sound absurd to speak of "the evolution of woman" or "the transition from ape to woman"; it even sounds awkward to speak of "the evolution of humankind" or the relationship of "humanity" to nature.

Language provides us with a set of common ideological presuppositions and orders our perceptions of the world; our language indicates that "man" is the essential sex and "woman" the (possibly interesting) variation. Thus "man" must be at the center of the evolutionary process. Most evolutionary reconstructions have argued this case implicitly rather than explicitly, when they have failed to address the assumptions built into our language. However, as the feminist movement raises such issues to the level of consciousness, writers are forced to deal more directly with the practical political and ideological questions of male-female relations, with the problems involved in reconstructing a valid account of the early history of "humankind," and with the relationship between contemporary assumptions and commitments and a knowledge of the past.

Recent popular accounts of the early history of "man" have indeed tended to be more self-conscious about the reconstruction of gender relations. There has, however, been little direct discussion of women's activities during the course of human evolution; instead, we have been assured that sexual inequality and discrimination have been naturally and necessarily programmed into humanity by our animal ancestors. Social hierarchies, competition, aggression and warfare have been seen as inherited from the dominance hierarchies of the apes; warring nation states and imperialist adventures have been understood in terms of the "territorial imperative." The behavior of our hominid ancestors has therefore been used to justify many types of human behavior from social discrimination to modern war, and has also been the basis for justifying the "natural order of things" vis-a-vis women's roles and rights. Our most recent versions of Social Darwinism no longer emphasize the survival of the fittest—the direct competitive struggle of early capitalism; instead, they speak of an organized and often rigid power structure, a dominance hierarchy of powerful males—the subdued struggle of the corporate board room. And while there are a few women in the corporate boardrooms of America, there are still no female baboons in the ideal dominance hierarchies of the sociobiologists.

The writings of Ardrey, Fox, Tiger and Wilson have been especially provocative in their assertions that human behavior is naturally programmed and in their explicit challenge to the women's movement.[3] Their work created a new "Me Tarzan—you Jane" school



of social evolution, and was given an enormous amount of media attention. The debate about the origins of human society came out of the academic closets onto the front pages of popular magazines, and became, at the same time, an issue in the women's movement.

Where could women begin to find an alternative analysis of the beginnings of human society? Across the country, feminist study groups turned to Engels' *The Origin of the Family, Private Property and the State* as one of few works that addressed the early history of marriage and the family—and one of very, very few that posed women's subordination to men as a problem with an historical cause and, therefore, a future solution.[4] Engels provided an interpretation of human history that linked the subordination of women to the rise of private property. (The transition to settled agricultural production for the first time allowed the accumulation of wealth in the hands of individual males; it permitted the development of exploitative relations. Individual property undermined communal property and kin relationships; the separation of the family from the clan reflected these new forms of private property while monogamous marriage provided the necessary basis for the inheritance of property between males.)

In the *Origin of the Family*, however, Engels said little about the earliest period of human history. If little is definitely known about this subject today, less was known in 1884. Engels' discussion of "savagery," the period roughly corresponding to what today would be known as hunting-gathering societies, is therefore extremely brief and sketchy. He suggests three periods of savagery: in the first, man lives in tropical or subtropical forests, eats fruits, nuts and roots, and develops articulate speech. In the middle period, fire is discovered and fish become available as a new source of nourishment; man learns to cook roots and tubers in hot ashes. Although game might sometimes be eaten, Engels stated that "the tribes which figure in books as living entirely, that is, exclusively, by hunting never existed in reality; the yield of the hunt was much too precarious" (p. 88). In the upper state of savagery, we find the invention of the bow and arrow, which makes regular eating of game possible, and also the invention of many other means of subsistence: wooden vessels and utensils, fingerweaving, plaited baskets, sharpened stone tools, dugout canoes, beams and planks for building. In the period of savagery, Engels suggests that a form of primitive communism with group marriage prevails; pair relationships are not based on an assumption of sexual exclusiveness; food is shared, and there is no accumulation of wealth.

Engels had also addressed the question of the earliest stage of human evolution in a brief, unfinished essay written in 1876, "The Part Played by Labor in the Transition of Ape to Man."[5] In this essay, Engels ignored issues of women and the family and concentrated on the role of labor in the creation of man. In an elegant although brief argument, Engels asserted that man's "nature" was created through labor, that the physical form of the human hand and brain were themselves the products of human labor. The differences between man and ape could therefore be understood as the products of labor, as man transforming himself in the process of transforming "external" nature.

According to this formulation, the critical moment in the transition from ape to man lay in the use of tools—and Engels believed that the earliest tools were hunting and fishing implements. At this point, Engels also believed that the transition from ape to man involved a transition from an exclusively vegetarian diet to a meat diet: "With all due respect to the vegetarians, man did not come into existence without a meat diet . . ." (p. 257)

The issue of the value of meat in the diet is critical to much of our subsequent discussion because meat is so closely associated with hunting and with male activity. If meat eating is the mark of *Homo sapiens*, then the hunter is the true representative of early man. Although anthropologists have long recognized that plant food must have formed a considerable part of the earliest human diet, this fact has been obscured by the importance given to meat and to hunting. Until recently, most anthropologists implicitly assumed that meat eating was an important human advance and that meat provided the nutritionally most significant part of the diet of hunter-gatherers. Indeed, the general nutritional wisdom stated that meat (as high quality protein) was the single most important constituent of any good diet. Only recently have nutritionists started to reach a new consensus that a diet high in animal fat and low in vegetable fibre is a threat to health, and have therefore urged a beef-loving population to return to grains, fruits, vegetables and nuts as dietary staples.

Many studies of contemporary hunting-gathering societies have now shown that in almost every society, the plant foods gathered by the women form the largest and most consistent part of the diet. Popular images of "man the hunter" are glamorous but mythical. Animal meat produced by hunting is a status food, but plant food gathered by women is literally critical to maintaining the social group.

Recognition of this fact suggested that the material existed for a radical reintepretation of human history based on an acknowledgement of the contribution of female labor. If the perceptual shift was made from "hunter-gatherer" to "gatherer-hunter," much of the biological and paleontological evidence could be seen from a fresh perspective. Nancy Tanner and Adrienne Zihlman began to construct this alternative view, building on the contributions of other anthropologists who were increasingly dissatisfied with the standard interpretations.[6] *On Becoming Human* represents a fuller elaboration of this revised view concerning humankind's emergence.

In her book, Tanner brings together information and evidence



from fossil studies, primate behavior research and social anthropology, integrating biological and social sciences into a new view of "becoming human" which maintains that plant gathering by females (to share with dependent children) played a critical role in the transition from ape to human. The mix of sources of information is rich, provocative and only occasionally confusing. Given the enormous amount of reference material—the bibliography stretches for a daunting 80 pages—the resulting argument is remarkably clear and persuasive.

Tanner begins with a review of the biological and physiological evidence linking humans to the African apes. In addition to the kinds of anatomical comparisons known to Darwin and Huxley, we now have molecular methods of measuring the relative genetic distance between organisms. For example, similar proteins in different species will differ in their precise sequence of constituent amino acids, the difference depending on the evolutionary distance between the organisms. Genetic affinities can also be measured by the differences in nucleotide sequencing making up the genetic code in DNA (although such a method is expensive and time-consuming), by immunology (comparing antigen-antibody reactions) and by DNA hybridization (which measures the extent to which separated DNA strands will bond together.) From a review of the available studies of molecular relationships, Tanner concludes that humans are most closely related to the chimpanzees, and that these evolutionary strains probably diverged within the last 4 or 5 million years. Tanner therefore argues that chimpanzees provide the best reference point for imagining and reconstructing the ancestral population from which both humans and modern apes evolved. The choice of such a reference point or model is important because it implies specific physical, behavioral and social characteristics: chimpanzees demonstrate some bipedal behavior and carrying, some tool use in the food quest, food sharing, reliance on plant foods plus limited insect collecting and predatory behavior, long-term mother-offspring interaction, effective communication, general intelligence, a flexible social organization, and adaptability to a range of habitats. In fact, Tanner devotes a considerable part of the book to arguing her case for the chimpanzees as our closest ape relations.

Chimpanzees are intelligent and appealing; they are used here to build images of early hominid society quite different from those of Ardrey's "killer apes" (or even from the dear old tropis). Chimpanzees use tools and display considerable ingenuity in using and modifying objects to obtain food and water. One example cited is the patience displayed by the females in "fishing" for termites with grass stalks and stripped sticks:

> If the ancestral and transitional populations and the early hominids likewise collected insects with tools, and found them a significant protein source, the "hunting" image might need considerable revision! What if

early hunting largely consisted of australopithecine females with tools sitting for long periods collecting insects, rather than of those long-imagined ferocious groups of half-human males racing after big animals? (p. 73)

Tanner emphasizes the many different kinds of tool use displayed by chimpanzees, their omnivorous eating habits (based mainly on fruit and termites), their social flexibility and small, mobile living groups. Highly sociable animals, chimpanzees have a range of social communication including facial expressions, vocalization and gestural communication such as embracing, kissing, handshaking, body patting, grinning, pouting, etc. Infants are highly dependent on their mothers; young chimpanzees stay with their mothers for nine to twelve years and sibling ties seem to persists even into old age. There is a small degree of sexual dimorphism and some behavioral differences between the sexes—adult females give and rceive greetings more than males, while adult males display aggression more often than do females. In general, relationships between chimpanzees are relaxed and peaceful; occasional displays of violent aggression are forms of communication that rarely result in discernible injury.

Tanner builds her conception of the transitional ape-hominid population (living 8 to 4 million years ago at the forest fringes of Africa) in part by extrapolation from this discussion of the social character of chimpanzees. She argues that as the generalized ancestral ape population differentiated, gorillas evolved and adapted to rain forests, chimpanzees to less dense forests, and hominids to the savanna. Gathering—a new way of exploiting plant food with tools—emerged as the basis for the hominid divergence. According to Tanner's hypothesis, the comparatively open areas and less dense food supplies of the savanna required that food be gathered and carried to more protected areas. Effective gathering was required and made possible by the development of upright, two-legged locomotion. Tools would have been developed for digging, carrying and preparing food: pointed sticks for digging, sharp-edged stones for cutting and scraping roots and tubers, rocks for cracking open nuts and seeds, sharp implements for cutting and dividing food, elementary containers for carrying it. Tanner argues that the survival of offspring would have depended on the mother's effectiveness in gathering; the mother-offspring group was the elemental social unit and offspring depended on the mother's ability to provide a consistent source of food. Males would occasionally share food with females—as do chimpanzees—while mothers shared their supplies with their young.

Many accounts of the transitional hominids assume some kind of monogamous "family" unit. This is often linked to the supposition that, with the loss of the estrus cycle, females became continually sexually receptive. The continuous sexual receptivity of the human female is then said to have produced male-female "pair bonding."



This particular theory has led to some amusement on the part of most women anthropologists and some sceptical male colleagues: "No human female is 'constantly sexually receptive.' (Any male who entertains this illusion must be a very old man with a short memory or a very young man due for a bitter disappointment.)"[7]

Tanner suggests that the loss of the estrus cycle (with the visible sexual swelling indicating a female's readiness for intercourse) probably required females to initiate sexual activity more directly—by overtly soliciting intercourse. (Since female chimpanzees have been observed making the appropriate nonverbal (proceptive) signals, there is nothing particularly bizarre about this notion, except in contrast with the neo-Victorian anthropological models that assume sexually active males and passive females.) Tanner then resurrects a part of Darwinian theory that has never enjoyed much popularity—the theory of sexual selection. (Darwin believed that female birds, peahens, etc. must have selected mates for their beauty; the glorious colors and shapes of the males of many species could not be explained by natural selection.) Tanner gives the theory of sexual selection a new twist, however, for she suggests that females selected the more friendly and sociable males:

> Females probably had sex more frequently with those males who were around often, playing with offspring, helping in protection, occasionally sharing meat and foraged plants, and who were generally friendly. (p. 164)

By this stage in the book, our macho killer ape has become a pussycat, and the reader is likely to be either intrigued and delighted, or bursting with outrage. Having laid out her theory or "model," Tanner then devotes the second half of her book to considerably more technical material, showing how the theory can be used to explain and interpret existing fossil evidence.

While this section will doubtless be of most interest to specialists, and will feed the flames of current disputes about the proper interpretation of various fossil fragments, the general reader will notice only that Tanner appears to build a serious case for the gathering hypothesis. She argues that the data on size, tools and teeth of australopithecus do not fit the hunting model but make a great deal of sense when viewed within a gathering context. Fossil teeth indicate powerful chewing and grinding capacitites, needed for a diet containing a high proportion of tough, uncooked plant food. Further, humans and apes share complex Pb and PPb salivary proteins, thought to make tooth enamel more resistant to decay caused by large amounts of plant carbohydrate—thus indicating that plant food must have been important throughout human evolution.

Tanner reviews the earliest stone tools uncovered and argues that these were more probably used for gathering than for hunting. Crude stone tools would hardly have been adequate for hunting down large animals (although they could have been used for butchering immobilized or dead animals). Perhaps most importantly, Tanner argues that the gathering hypothesis can best explain the confusing data on the variability of early hominids. The differences between the "robust" and the "advanced gracile" australopithecines* in face, skull and teeth can be related to their different use of tools in preparing plant food, if we assume that the "advanced gracile" population made more extensive use of tools for food preparation.

Tanner thus builds a persuasive case for the gathering hypothesis. Evidence has been building for some time that gathering played a more important part in human evolution than has been generally recognized; in part, Tanner's role is to bring together this evidence and underline its significance. But she is doing more than this: she is changing, or at least challenging, our "origin myths." By putting gathering at the center of the ape-human transition, Tanner is presenting a different view of "human nature"—the human gatherer as a sociable and usually gentle creature in comparison to the energetic and aggressive hunter. Then, too, our images of the gatherer are female—most gathering in contemporary societies is done by women while most hunters are men. Gathering activities are more compatible than hunting with the care of dependent offspring. Equally, if we suppose that gathering marked the earliest human societies, there is no need to imagine monogamous male-female bonding with the male bringing home the bacon—females could simply forage for themselves and their offspring.

It seems hardly plausible to suppose that human evolution could have depended on the chances of the hunt when plant gathering could have provided a more dependable and consistent source of food. It is, however, possible to accept the gathering hypothesis without admitting the "independence" of hominid females. C. Owen Lovejoy has accomplished this feat in a recent issue of *Science*.[8] Lovejoy agrees that hunting could not have been critical to early hominid survival, but he argues that the key issue in the origin of bipedalism must have been the male's need to carry home food to his mate and offspring. Lovejoy thus places the origin of the nuclear family "long before the dawn of the Pleistocene" and restricts his hominid females to their tree houses. One problem with Lovejoy's view is that the hominid males could hardly be certain which offspring were "theirs": an assumption of sexual monogamy seems a good deal less plausible than the alternative view of maternal-focused gathering with some food sharing between the members of a small social group. The fact that an anthropologist so committed to the ideology of the nuclear family would nevertheless reject the hunting hypothesis does suggest that Tanner's gathering thesis is both timely and credible.

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* Tanner considers the "advanced gracile" *habilis* to be an australopithecine, sometimes referring to it as (*A.* or *H. habilis*). This is not a confusion between *Homo habilis* and the australopithecines, but reflects a technical argument over the system of classification.



Tanner's thesis is clearly feminist, and it is also compatible with a materialist view of the origins of human history. She emphasizes the use of tools in making the transition from ape to human, though her tools are not the instruments of the hunt but those of the preparation of plant foods. Her interpretation is consistent with the later development of matrilocal society, characteristic of primitive communism (as discussed in Engel's *Origin of the Family*), though it differs with Engels and with later theorists in that it gives a central role to female labor in the labor of "becoming human." (Males also gather, but female labor is the most important for the survival of the offspring and therefore of the species.) It is still necessary to argue that motherhood is not a simple biological fact but the central biosocial task of any species; Tanner's book should help to reintroduce the female of the species—particularly mothers—as active agents in the process of becoming human. She reminds us that the "family" is not a natural necessity but a changing historical form. She provides a coherent and well documented thesis, and a welcome alternative view to some of the reactionary and sexist ideologies that have been paraded as sociobiological theory.

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*How Eager-Beaver Scientists Anticipated the War Machine*

# The Science and Politics of Neutron Bombs*

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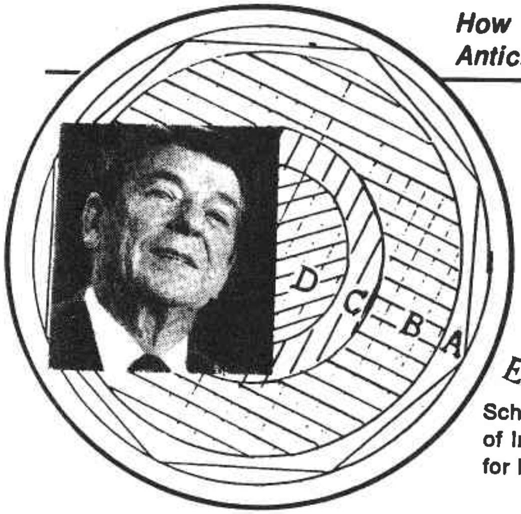


Schematic View of Instrument for Mass Murder

It all began, they say, because a group of American nuclear physicists found themselves — at the end of the war — full of clever ideas, but unable to test them in practice because of the peace. General L.R. Groves, who was responsible for the Manhattan Project during the war, and therefore for the construction of the first atomic bombs, says:

> I think you can refer back to history as to the attitude of the average academic man in 1945 when the war was over. They were exactly like the average private in the Army who said to himself, the war is over, how soon can I get back home to mom and get out of this uniform. That was the way the average academic scientist felt. He wanted out . . . What happened is what I expected, that after they had this extreme freedom for about six months, they all started to get itchy feet, and as you know almost every one of them has come back into Government research, because it was just too exciting, and I think still is exciting.[1]

Among the exciting ideas then boiling in the heads of some of the best American scientists (not only Americans, to tell the truth, also others like E. Teller from Hungary, E. Fermi from Italy) were some that have determined the tactical and strategical choices of the United States since the war. While the only nuclear weapons operational at that moment were the *fission bombs* (either uranium bombs — as at Hiroshima, or plutonium bombs — as at Nagasaki), scientists were already planning for the future, as Oppenheimer tells us:

> Even during the week when Hiroshima and Nagasaki were being bombed, we met at Los Alamos to sketch out a prospectus of what the technical future in atomic energy might look like: atomic warheads for guided missiles, improvements in bomb designs, the thermonuclear program, power propulsion, and the new tools available from atomic technology for research in science, medicine and technology. This work absorbed much of my time . . .[2]

* Originally published in the Italian journal *Sapere*, July 1981.



The *thermonuclear program* covers the construction of a *fusion bomb* (in reality, a fusion bomb triggered by a fission bomb), called also a *Hydrogen bomb* or *Superbomb* (*Super* they had nicknamed it in the labs). And the reference to atomic warheads indicates the first step toward the nuclear missiles and the tactical applications of nuclear weapons that will lead, later on, to the *neutron bomb*.

From the very beginning, the natural scenario for the use of tactical nuclear weapons was Europe. Oppenheimer describes the objectives posed to scientists working for the military in summer 1951:

> Dr. DuBridge was in charge of it, under the name of Project Vista, and its function was generally speaking to talk about ground combat and the support of ground combat. What they finally come down to was the study of the defense of Europe and what it came down to was the study of what you do to defend Europe at any time, as soon as possible, if necessary. The men involved in this project worked very hard on it, and they kept asking me to come out and talk about the use of atomic weapons in this picture . . . What we attempted to do was to be sure it was clear to them how varied and useful atomic weapons could be in ways that are probably not quite obvious to you and ways which were not completely obvious then . . . The anti-air use of atomic weapons, their use to put out enemy air fields . . . is an obvious example. This was the complement to the panel report I spoke of earlier on getting the atom to work on the battlefield as well as in the heartland.[3]

This is the germ of the theory that nuclear weapons could be of use tactically: *on the battlefield*; a message very sweet to the military and at least equally sweet to the scientists, who felt an urge *to get the atom to work* everywhere.

The first hydrogen bomb was successfully tested in 1952; heavy military investments were allocated to explore all tactical and strategical potentialities of fission and fusion bombs. In particular, the appetite was growing for a pure fusion weapon (or, at least, one triggered by a very small fission bomb), that could best exploit the effects of neutron emission.

This is the kind of bomb that is generally called a *neutron bomb* (there is no neutron bomb as such, but a whole family of variations on the theme). Once again, the best physicists of the best military labs in the USA became the most active salesmen in favour of the new weapon: once again, the interests of the ruling classes and the military — power and global control of the world — coincided with the internal logic of discovery, experimenting and planning by the scientists. But, in this case, scientists found a somewhat perplexed military elite and they had to use their strongest endeavors to overcome objections. A (heavily censored) transcript of a 1973 hearing of the US Congressional Joint Atomic Energy Commission is illuminating. H. Agnew, director of the military labs in Los Alamos, stated:

> It may be that people like to see tanks rolled over rather than just killing the occupants. It is quite clear there is rethinking going on . . . I know we at Los Alamos have a small but very elite group that meets with out-

side people in the defense community and in the various think-tanks. They are working very aggressively, trying to influence the Department of Defense to consider using these (deleted) weapons.[4]

That was the neutron bomb. There are those who do not like the term and prefer a more scientific name: *ERW: Enhanced-radiation weapon.*[5] Call it what you wish, it is the bomb that can make more and more fuzzy the watershed between conventional and nuclear weapons, and can therefore contribute to make more and more probable the use of nuclear weapons in local and limited wars.

It is therefore worthwhile to discuss a few technical and political details about this bomb, a bomb toward whose production and deployment huge pressure groups are converging:

> Schlesinger apparently felt compelled to make some concessions to the advocates of tactical nuclear weapons, an assortment of converging interests that included the Atomic Energy Commission, the Congressional Joint Committee on Atomic Energy, the weapons laboratories, certain military departments and the Atomic Energy Division of the Office of the Secretary of Defense . . . To gain support from these disparate interests for his plans to further build up conventional forces for NATO, he gave them money for the modernisation of tactical nuclear weapons.[5]

To these economic and power interests one should add the general political interests of the United States (and other countries, like France) to possess another weapon, handy for intimidation against any tendency for revolutionary transformation of the world power structure.

It seems therefore essential, if extremely difficult, to find ways to block this further expansion of American control over the world. As a first step, it could be important to clarify the tactical, strategical and political role of the neutron bomb, starting from a minimum of technical information and making clear the role played in all this by interested scientists and by the public media.

### *The story of the neutron bomb*

We can reasonably start from 1957, when scientists from the Lawrence Livermore Laboratory, led by Teller, (yes, the so-called *father of the hydrogen bomb* who clearly liked the idea of generating new children), met with then President of the United States, Eisenhower. The scientists told the President that they could perfect a new nuclear weapon, producing mainly radiation and therefore with lethal effects on human beings but non-destructive of material. This meeting was later referred to by then Secretary of State, J. Foster Dulles:

> The resourcefulness of those who serve our nation in the field of science and weapon engineering now shows that it is possible to alter the character of nuclear weapons. It seems now that their use need not involve vast destruction and widespread harm to humanity. Recent facts point to the possibility of possessing nuclear weapons, the destructiveness and radiation effects of which can be confined substantially to predetermined targets.[6]

For a few years the scientists worked in the dark, silently finding their way toward the military complex. The military did not seem



very impressed by the perspectives of the new weapon: a study commissioned in 1960 by the then Defense Secretary, McNamara, ends with a critical assessment on the possibility of using tactical nuclear weapons in case of a limited war in Europe.[5] In 1961, the *Bulletin of the Atomic Scientists* published a short note on the neutron bomb by F.J. Dyson (a Princeton physicist long involved with the military) who states that he cannot enter into technical details:[7]

> I am unfortunately not permitted by security regulations to state my views upon these questions with any precision. I therefore confine myself to general statements, involving mainly political rather than technical judgements.

However, he confronts the main problems concerning the possibility and the utility of constructing neutron bombs:

> If heavy hydrogen could react with itself according to the formula $D + D \rightarrow He3 + n$, the energy of each neutron would be about 3 million electron-volts. The reaction of one gram of hydrogen would yield $7 \times 10^{17}$ ergs of energy (i.e. of the order of 10 million kilo-calorie) in the form of fast neutrons. Converting this from physical to biological units, it means that one gram of hydrogen could in principle give five times the lethal dose of radiation to anybody within one kilometer radius, if neutrons were not absorbed in the atmosphere. Atmospheric absorption and scattering will change the numbers but not the order of magnitude.

Are these new bombs useful? Dyson concludes that they are not:

> I do not believe that neutron bombs are militarily advantageous to the US, nor that they will alleviate any of our military problems. On the contrary, neutron bombs, like hydrogen bombs, will in the long run only complicate our lives, increase our insecurity, and possibly facilitate our extermination.

All the same, in spite of lukewarm interest on the part of the military and warnings like the one above from scientists related to military research, activity toward the production of a neutron bomb has been going on in the military laboratories in the USA. It seems that an experimental prototype was fired in 1963 and that a neutron bomb for a Sprint missile was tested underground during the Winter 1977/78.[6] Kaplan summarized the situation in 1978:

> Today enhanced-radiation nuclear warheads are being developed for the Lance missile and for the 8-inch artillery shell. An ERW for the 155-millimeter artillery shell is also in prospect, although it still appears to be in the early stage of development . . . Currently deployed Lance warheads have explosive yields ranging from 1 kiloton to 100 kilotons (1kt: the equivalent of 1,000 tons of TNT); the charges of the 8-inch nuclear shells range from 5 to 10 kilotons. (Remember that the Hiroshima bomb was about 13kt.) The new enhanced radiation version of the Lance warhead will have two yields, which can preset simply by pushing a few buttons: one yield is considerably smaller than a kt and the other is slightly larger than a kt.[5]

On 7 April, 1978, Carter stated that the United States was renouncing the large scale production of neutron bombs, but would go on with a project to build up their "principal constituents". One would have thought that the danger of a large-scale proliferation of neutron bombs was past, until the French President, V. Giscard d'Estaing, declared in a press conference, on 27 June, 1980:

> France has proceeded with experimenting on a neutron bomb.[8]

And now,

> Only 15 days into the new administration (Reagan's), US Defense Secretary Weinberger announced that he favoured the production of the neutron bomb and its deployment in Western Europe.[9]

The ghost of these small, lethal weapons is therefore still around and we will increasingly hear about them. It will be useful, at this point, to look at some of the technical aspects of the neutron bombs.

### *A few technical notes on the neutron bomb*

The technical details on the construction of the neutron bombs are unknown (both for the American and the French versions). It is possible, however, to say a few simple things about the general principles on which the bomb works; this technical information will be necessary to discuss some of the tactical and political implications of the bomb.[6]

At the beginning, the research toward an ERW was for a purely *fusion bomb:*

> If a method of initiating thermonuclear explosions by means other than the fission bomb — a method which, by itself, would produce neither a strong blast nor radioactive fallout — could be developed, then thermonuclear bombs could be made as small as desired (at least, within a certain low limit analogous to the minimum amount of chemical explosive which can be brought to detonation) . . . The explosion would produce a powerful flux of neutrons with energies of 14 million electron volts on the target — a flux great enough to destroy living organisms, even behind substantial shields. This device is the now widely discussed neutron bomb.[10]

In reality, in a neutron bomb (as realised at present) fission and fusion still coexist: the fission in order to trigger the fusion mechanism, the fusion to produce most of the destructive yield and most of the radiation. In this sense, all neutron bombs are in reality hydrogen bombs of some sort. There is however an essential difference between the standard hydrogen bombs and the neutron bombs: the first being enclosed in a jacket of *Uranium-238*, which blocks the radiation and so enhances the destructive effects of the bomb; the second being enclosed in jackets of a different material, so that most of the fast neutrons produced during the fusion process can escape and contribute to the overall radiation effect.[11]

Recent technological progress has led to ERW with more and more fusion power with respect to fission power. Kaplan gives:

50% fission vs. 50% fusion for 200 mm. artillery warheads, 1kt
40% fission vs. 60% fusion for Lance warheads
25% fission vs. 75% fusion for 200 mm. artillery warheads, 2kt

(in a theoretical, pure fusion neutron bomb, 20% of the total energy would go into blast and thermal energy, 8υ% into radiation, mostly neutrons)



For a brief look at the technology, let us imagine a sphere (the size of a soccer ball, it seems) as in the schematic diagram above. The explosion will follow the sequence:

1. Firing the external layer (A) of high-yield conventional explosive projects the layer (B) of Berillium, as a neutron mirror, against the layer (C) of fissionable material.
2. Under the strong compression, the fissionable mass (Uranium-235 or Plutonium-239) reaches critical density, triggering fission and releasing energy to strongly compress and heat the internal sphere (D) of Deuterium and Tritium.
3. Heat and pressure trigger fusion in (D) producing fast neutrons:
   D(euterium) + T(ritium) → He(lium) + n(eutron) + energy
   Most of the energy is released as high-energy neutrons.
4. Radiation leaves the bomb; the shock wave, or destructive phase, follows. There will be an external jacket (E) such as not to absorb neutrons — perhaps of Renium or Wolfranium.

We have here all the ingredients for an ERW: most of the energy goes out in radiation; there is a little radioactive fall-out (due to the fission part of the bomb, mainly); there is little blast and thermal energy (which could be produced, as it is in a standard hydrogen bomb, by containing the radiation and absorbing it via a Uranium-238 jacket). There is a shortcoming, however: the need to use the fusion mixture of Deuterium and Tritium, instead of the Lithium of the standard hydrogen bombs. Tritium has a short lifetime and it has to be continuously refuelled into the operational bombs: every 10 years or so more than half of the Tritium content of a bomb has disappeared. The development of neutron bombs had triggered an intense funding — on the part of the military — for old and new techniques to produce cheap Tritium: an old power plant has been recommissioned in the United States in order to exploit its high neutron flux to produce Tritium via Lithium-6; in France, research on nuclear fusion (a potential new way to produce Tritium) has been put under control of the military. Most of the recent emphasis on fusion research (generally presented as a search for cheap and clean energy for the future) is suspiciously related to Tritium production and, indirectly, to ERW research and development.

We can now say something about the tactical, strategic and political rationale of the neutron bombs. They are presented by the military and the scientists at their service as *clean* bombs — in the sense that they should produce little radioactive fall-out, as the amount of fission energy used to trigger the bomb is reduced. They are presented also as *non-destructive* bombs, as the fission energy should be small and the absorbed radiation energy captured by the external shield (and therefore transformed into blast and thermal energy) should be small. No contamination [illegible]er, little contamination, as compared to the standard nuclear weapons), no widespread destruction, buildings and other permanent structures should be able to survive. But then, what is the use of the bomb? *Just to kill. Just to kill every living organism in a radius of 400-900 meters.*

Let us compare with a standard (atomic) fission bomb of 1kt (equivalent therefore to 1,000 tons of TNT): following an explosion of a 1kt fission bomb (about ground level), every person in a radius of 375 meters from the centre of the explosion will be exposed to at least 8,000 rads of radiation: a dose that incapacitates and makes death inevitable in a few days; buildings, trees . . . will be destroyed all around. Following an explosion of a 1kt neutron bomb, the death radius becomes about 900 meters (with the explosion taking place at about 500 meters above ground); even soldiers in armoured tanks would be killed; but the tanks would be mostly intact, as well as buildings, railways, canals in the region. It has been well said that the neutron bomb is the best of all capitalistic weapons: *it saves the property, it destroys people!*

The tactical scenario invented by the military for Europe is then the following: a sort of *Maginot line* (the defense line, totally inefficient, that France had prepared against a German attack before World War II) created by neutron bombs exploding, at about 500 meters above the ground, each of about 1kt, at a distance of some 900 meters. This should go through most of Central Europe, so as to be able to stop an army of Russian tanks. Of course, the civilian populations would be killed as well (and even faster than) the soldiers in the tanks . . .

### *The selling of the neutron bomb by the mass media*

The financial costs of the reconversion of the present tactics and strategy to one centred on the large scale deployment of neutron bombs are staggering. Kaplan estimates that a single 8-inch enhanced-radiation artillery weapon will cost about 900,000 dollars:

> In other words, if the US decides to invest in enhanced-radiation devices, NATO will be acquiring an extraordinarily costly weapon that would probably never be used, at the expense of comparatively cheap weapons that would markedly improve NATO's defense posture.[5]

It would be hard to give precise estimates, but the order of magnitude of the proposed investments are horrific. The combined interests of the military-industrial-scientific complex in the United States, in France and probably elsewhere (we know very little about possible Soviet experimentation on neutron bombs) are trying to impose this gigantic effort on their citizens. As always, they have to try to sell their goods: and what better than that the media, authors and journalists, books and journals create a new wave of hysteria, a new *popular need* for the new adventure?

Scientists are in the forefront. Already many years ago, E. Teller was very skilled in selling his ideas:

> Is the neutron bomb technically possible in the forseeable future? . . . A new breakthrough is needed to solve this problem. For four years Edward Teller and his colleagues at Lawrence Radiation Laboratories, Livermore, Calif. have been hinting that they have promising ideas for a possible solution.[10]



Now we have, emerging from the dark regions of military secrets, a *father of the neutron bomb*, nuclear physicist S.T. Cohen, working at the Lawrence Radiation Laboratories. Together with a French colonel, M. Geneste, he has published a book: *Echec à la guerre* (Check-mate to war).[12] They tell about the story of the neutron bomb, they propose new strategical scenarios for Europe, they emphasize the humanitarian aspects of a tactical war in Europe based on the neutron bomb.

A book is not enough, a well orchestrated campaign is waged in magazines and newspapers in France (seen, possibly, as a weak point; and the country that would suffer more, together with Germany, from a neutron bomb *limited nuclear war* in Europe): the same arguments as in the Cohen-Geneste book are repeated and simplified and instilled slowly in the public consciousness.[13]

### *Conclusions*

Why this race to impose the neutron bomb in NATO? Not a word on the costs; on the risks to civil population; on the danger of nuclear reprisal . . . The role played by the United States (possibly through S.T. Cohen) is rather apparent; but why this pressure now on public opinion to accept the *umbrella* of the neutron bomb (as well as the new missiles, Pershing, Cruise, MX . . .)?

I think that the reason (and the danger) — or, at least, one of the reasons — is rather apparent: the neutron bomb is *the* nuclear weapon that could make local nuclear war possible: it would allow nuclear weapons to become — after the international outrage about Hiroshima and Nagasaki — respectable weapons. Neutron bombs would help imperialist powers and their military groups to blur, in the public consciousness, that strong watershed that still divides conventional weapons (even the most horrible ones) from nuclear ones. Once the watershed is overcome—by a very limited use of a few very small-yield neutron bombs in a far away theatre: Cambodia? El Salvador? — nuclear weapons will have entered a new age. It would be shown that they do not necessarily imply doom: that they can be controlled and specifically directed on restricted targets. And then the long range scars of standard nuclear weapons will not be there for tens of years to tell the tale: only dead soldiers and civilians and partisans and opponents buried somewhere . . .

With some irony one can read today some of the sentences in Dyson's 1961 paper:

> A committee of scientists could report with perfect correcteness: 'We do not need neutron bombs. Anything these bombs can do can be done just as well with oldfashioned bombs'. And again the public would rightly dis-

regard the committee's advice. The importance of neutron bombs would lie, like that of hydrogen bombs, in their being technically a symptom and politically a symbol. Technically, neutron bombs would be a symptom of a further general advance of nuclear technology which would be pushing ahead in many different directions. Politically, these bombs would be the symbol of military power in the eyes of the world, the latest, most modern, most refined, most chillingly murderous of mankind's instruments.[14]

It would seem that Dyson believed that public opinion is free to make its choice, is never manipulated (by those whose interests, in several forms, are power) so that it should ask for "symptoms" and "symbols". The recent wave of mass media pushing the neutron bomb is a case in point: they want *us* to ask — at our cost — for our daily "symptoms" and "symbols."

Perhaps the Left will be able to provide some well concocted antidote?


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14. F.J. Dyson, *op.cit.* □


***A Byte of Logic***

**In 1953, while thousands of sheep were dying from the fallout of Nevada bomb tests, Commissioner Eugene Zuckert of the AEC said: "In the present frame of mind of the public, it would take only a single *illogical* and unforeseeable accident to preclude holding any further tests in the United States." [*Science* 5 Nov 1982 p 545, *emphasis added.*]**





# Marx, Peano, and Differentials


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One of the important philosophical steps in the history of the calculus was the replacement of the differential by the derivative as the fundamental concept of infinitesimal analysis. This process was carried out by Lagrange and Cauchy, but had its beginnings with Euler. Behind it was the foundational problem posed by differentials, for which there were self-contradictory claims. Before this replacement was made, the foundational problem could hardly have been solved; after it was made, the problem of the interpretation of the differential still did not have a satisfactory solution until near the end of the 19th century, when Karl Marx, working independently in London — and without knowledge of the foundational work that had been done by Cauchy and later mathematicians on the Continent — arrived in 1881 at the concept of the differential as an operational symbol for taking derivatives.

This concept could not have been achieved in the time of Leibniz. As Henk Bos [p.4] has pointed out: "There are three processes in the history of analysis in the 17th and 18th centuries which are of crucial importance for the history of the concept of the differential. The first is the introduction, in the 1680's and 1690's of the Leibnizian infinitesimal analysis within the body of the Cartesian analysis, which at that time may be characterized as the study of curves by means of algebraic techniques". The second, according to Bos, was the separation of analysis from geometry, which took place in the first half of the 18th century. The third, just mentioned, was the replacement of the differential by the derivative as the fundamental concept of infinitesimal analysis. He then shows that, in the Leibnizian calculus, the derivative would have had to be interpreted as a ratio that was correlated to a variable having the dimension of length. This implies that the operation of derivation cannot be repeated in a natural way because it is not clear what sort of quantity it would correlate with a ratio. "Thus the derivative could not occur in the geometrical phase of the infinitesimal calculus" [Bos,p.8].

Although Marx was not aware of contemporary developments in the foundations of calculus — indeed he began his study with an 18th century text (of Abbé Sauri) — the basic concept for him was the derivative and he said of the differential: "$dy = f'(x)dx$ appears to us as another form of $dy/dx = f'(x)$ and is always replaceable by the latter" [Marx.p.62]. What, then, do the symbols dy and dx represent? Marx answered this question by means of a dialectical analysis of what happens in mathematics in the crossing over from algebra to a differential calculus. (On Marx's approach to this question, cf. Kennedy [1977].)This aspect of Marx's study was already brought out by V. I. Glivenko in 1934, one year after the first publication of a part of Marx's mathematical manuscripts [cf. Kennedy 1978]. Glivenko [p.85] concludes: "As a result of his investigations also appears the concept of the differential calculus as its own kind of algebra, constructed over the usual algebra and containing, besides numbers, differential symbols" and (referring to the opening pages of Hadamard's *Cours d'Analyse* of 1927) finds confirmation that "mathematicians, too, are beginning to arrive at such a concept of the general character of the differential calculus".

The philosophical question, however, remains: What is it that is reflected by the symbols dy and dx? One answer is that the differential is the principal linear part of an increment. Thus, if $y = f(x)$ and $\triangle y$ is the increment brought about in y by an increment $\triangle x$ of x, then the principal linear part of $\triangle y$ is $dy = f'(x)\triangle x$. (In this context, the increment of x is necessarily linear, so that $dx = \triangle x$.) This idea goes back at least to Euler and, according to S. A. Yanovskaya, editor of Marx's *Mathematical Manuscripts*, Marx was aware of it, and of course it is known to all later mathematicians. But this interpretation is valid only for first order differentials and only for functions of a single independent variable. The difficulty shows up in the case of functions of two variables, each of which is a function of another variable — a case studied by Marx — and it shows up even more strikingly in the attempt to define second order differentials, as Glivenko pointed out.

Thus, according to this interpretation, if $y = f(x)$, then $dy = f'(x)\triangle x$ and $d^2y = d[f'(x)\triangle x]$. Following the usual rule for differentiating products, since $\triangle x$ is independent of x, the derivative of $f'(x)\triangle x$ is $f'(x)\cdot 0 + f''(x)\cdot\triangle x$, so that, since $dx = \triangle x$,

(1) $d^2y = f''(x)dx^2$.

But if x is a function of t, then by the chain rule for differentiating composite functions, we have $dy/dt = f'(x)(dx/dt)$, and a second differentiation leads to

(2) $d^2y/dt^2 = f'(x)(d^2x/dt^2) + f''(x)(dx/dt)^2$.

Multiplying through by $dt^2$ yields

(3) $d^2y = f'(x)d^2x + f''(x)dx^2$,

which does not agree with (1).



The dangers arising from such difficulties are beautifully illustrated in a story told by Henri Poincaré in 1899. He says he was present at an examination at which the candidate explained the theory of the speed of sound as follows: "We have to integrate the equation $d^2z/dt^2 = a^2(d^2z/dx^2)$. I divide by $d^2z$ and multiply by $dx^2$. I then have $(dx/dt)^2 = a^2$ from which $dx/dt = \pm a$, which proves that sound may be propagated in both directions with speed a." According to Poincaré, the examiner, an excellent physicist whom he does not name, replied: "That's remarkable; your proof is much simpler than all those I know," and he gave him a mark of 19 of a possible 20 [Poincaré 1899; quoted in Peano 1957, p. 384].

The concept of the differential as an operational symbol in the sense of Marx-Hadamard can be extended to second order differentials and Hadamard proposed to write (3) as designating that and only that which holds in (2), whatever the functional dependence of the variables x and y on the parameter t. Thus, as Glivenko [p.84] remarks: "The concept of the differential as the principal linear part of an increment turns out to be an *interpretation*, useful only in certain special cases. . . . The result at which we have arrived may explain just this, that precisely the operational concept of the differential calculus correctly and completely reflects reality," and he adds in a footnote: "Even if only because in reality there are no absolutely independent variables".

The anecdote by Poincaré was quoted by Giuseppe Peano [1912] in an article proposing a radical solution of the problem of the concept of the differential. Peano simply identified the differential with the derivative: "Modern texts of infinitesimal analysis usually define the derivative of a function as the limit of an incremental ratio. They then define the differential of a function as the product of its derivative and the differential of the independent variable. This latter is defined as an arbitrary quantity, constant or variable, or as an increment of a variable, finite or infinitesimal: and the infinitesimal is variously treated. Some authors, such as Todhunter, Veblen, consider dy/dx as a symbol to indicate the derivative, indecomposable into the elements dy and dx. The affair becomes much simpler if *differential* is defined as synonymous with *derivative*. The identity between differential and derivative will be explained here with logical and historical arguments. The very simple logical argument is that wherever *differential* is written, one may read *derivative*, and the truth of the proposition remains" [Peano 1957, p. 369].

Thus far, Peano would seem to be in agreement with the operational view just described, at least to the extent of saying that differential formulas have just the same content as the corresponding derivative formulas. But I think he goes too far in suggesting that Leibniz, for example, thought in derivatives and not differentials, thus attributing to Leibniz the sophisticated thought processes of Poincaré [1897]: "As for myself, I ordinarily use the differential notation. first because it is the language most of my contemporaries speak, and then for the small practical reasons just mentioned. But if I write in differentials, most often I think in derivatives" [quoted in Peano 1957, p. 383].

Though this sophistication was possible for Poincaré, the historical reasoning given by Bos makes it seem hardly possible for Leibniz to have thought in the same terms. But I suggest that Peano's error was due less to any lack of concern for historical accuracy than to a lack of consideration of the philosophical question concerning the meaning of dy and dx. Peano made no pretence of being a philosopher and, indeed, denied competence in this field. Fearing, perhaps, the excesses of the 'schools' then current in the philosophy of mathematics (formalism, logicism, intuitionism), he drew back from a philosophical discussion even of the concept of number — even though he is best known for his Postulates for the Natural Numbers.

I am suggesting that it was Peano's failure to consider philosophical questions that allowed him to fall into the historical error regarding Leibniz. This is the other, and necessary, side of the touchstone: "Objects are best understood in terms of their historical development" [Adler, p. 59]. An understanding of the historical context helps us appreciate the philosophical questions; a concern for philosophical problems alerts us to historical possibilities. Peano's viewpoint further ignores the fact that philosophical questions of mathematics are also relevant to an understanding of more general philosophical problems. As the author of the article on "Mathematics" in Rozental's *Philosophical Dictionary* [p. 230] wrote: "The philosophical questions of mathematics . . . have always appeared in the arena of the struggle between materialism and idealism."

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### *A Byte of Logic*

**Sir Fred Hoyle, if we read him right, says that cosmology today is pretty much a matter of theology, and he only wants to replace the prevailing model with his own brand. [*The Sciences*, Nov. 1982, pp. 9-13.]**





## When Physicists Served Fascism

Alan D. Beyerchen, *Scientists under Hitler: Politics and the Physics Community in the Third Reich*. New Haven and London, Yale Univ Press, 1977.

Sometime in the 1930s, Alan Beyerchen tells us, a visitor asked Albert Einstein if he could take back any messages to Germany. "Greet Laue for me." But did Einstein not want to include any of the other German physicists in his greeting? " Einstein's anwer was simply to repeat, 'Greet Laue for me.' " (p.65) By then, Max von Laue had publicly likened National Socialism to the Italian Inquisition, aided Jewish colleagues, and sent his son to be educated in America. Radiochemist Otto Hahn had also resisted the Nazis' dismissal of Jewish scientists. They were virtually alone. In his meticulously researched book, Beyerchen demonstrates that most of the physicists who remained in Germany cooperated in one way or another with the Nazis.

Why did the physicists collaborate? It is worthwhile evaluating Beyerchen's answers. To be sure, his book treats more than this single question. *Scientists under Hitler* was rightfully hailed as a good book filling an essential need, when it appeared in 1977; the picture of the interaction of physics with Nazi politics that Beyerchen painted is indispensable for a full grasp of the history of the concepts and philosophy of physics in the interwar period. Nevertheless, for radical scientists, it is interesting to see how Beyerchen's own answers to the central question of the physicists' guilt are only partially satisfactory.

Beyerchen, first of all, explains the events by pointing out that most German academics had viewed the Weimar government with "icy reserve", and had supported at least the nationalist aims of the Nazi movement. Further, in the 1930s, he writes, Nazi atrocities, such as the extermination of the Jews and the instigation of World War II, were still in the future. One could reasonably hope that the Nazis' roughness would moderate with time. Secondly, Beyerchen cites the German academic "mandarin" tradition. According to this tradition, the state was different from, and higher than, any political party which might be transiently in government. High patriotic duty was owed to this abstract state, and could best be rendered by maintaining the standards of German science and German scientific institutions. Thirdly, Beyerchen reminds us that for physicists to acquiesce to a government had a different meaning in the 1930s than today. Then physics seemed an esoteric specialty like philosophy or science fiction; today everyone can recognize it as the bone and sinew of military and economic strength.

Beyerchen concludes that their mandarin academic heritage made it natural for the physicists to "oppose" the Nazis only to the extent of protecting the professional standards of their discipline. And while he does not excuse them, his condemnation is mild: "The truth was not that the scientists were political cowards but that they did not know how to be political heroes. Their actions were in complete accord with a set of standards we have come to recognize as too narrow." (p. 207)

But can we excuse the physicists' shortsightedness in the thirties? Einstein saw the danger. Von Laue saw the Nazis' immorality. The labor leaders, Communists and socialists whom the Nazis were sending to concentration camps knew how to evaluate National Socialism even in the thirties. Indeed, the German Left's fight against Nazism is curiously absent from the historical backdrop that Beyerchen unrolls behind his history. If not in the book, however, yet in real life the German physicists must have known of this struggle, and had access to another view of Hitler.

Beyerchen's second argument, that physicists acted in line with their mandarin traditions, is more persuasive, though one suspects that other, less altruistic motives also played a part. The story he sketches of their fight for the professional independence and integrity of phyics is instructive. Their opponents, Johannes Stark, Phillip Lenard and others of that small group of scientists trying to create an ideological "Aryan" physics, ridiculed the separation that the majority made between science and its social context. For the ideologues, "Objectivity in science was merely a slogan invented by professors to protect their interests . . . Hitler maintained 'The simple question that precedes every scientific enterprise is: who is it who wants to know something?' " (p. 134)

Simplistic, distorted and racist as the Nazi argument was, there was a truth in it that the "professional" physicists missed. Society is larger than science, and commitment to a science must only be undertaken within the frame of a critical, morally sound commitment to humanity as a whole. Lacking this understanding, and acting from mere simpleminded devotion to the discipline, the professional physicists descended a spiral of disgrace. Though they fought back by exposing pre-Nazi "pro-Jewish" publications of the "Aryan" physicists (p. 183) and even published rebuttals against them in SS magazines (p. 142) . . . ultimately, they carried out war research for the regime.

By then, the 1940s, the "excesses" of the Nazis must surely have been visible, even to them. But war-work was another tactic to preserve the autonomy of professional physics by showing it to be indispensable. The struggle for the independence of physics degenerated



into just another episode in the petty fights among party, government bureaucracy, and institutions that Beyerchen shows us to have marked the Nazi epoch. When you lie down with dogs, you rise up with shit.

Where does Beyerchen himself stand on the relation of science to society? It is hard to be sure where Beyerchen speaks for himself, and where for the Germans, but the impression is that he shares the limited view of his protagonists: science is somehow "objective", "value-free", even "above society." If we are correct, it would be here that his tendency to waffle on the German physicists' amorality would be rooted. For he would be snared in a dilemna of his own: the professional stance of the physicists was correct and yet it led them step by step to collaboration. One could then understand his view that it is only in retrospect that the standards of the physicists appeared narrow.

There may be an additional cause. Historians of modern physics have pusillanimity as an occupational hazard. We depend upon the good graces of the scientists we treat for our raw material — the interviews, access to archival and personal collections, even, occasionally, our funding. The temptation to treat scientists with kid gloves is enormous. This problem is forcefully brought home by comparing the writings of professional historians with the more forthright observations (though less accurate history) in novelist C.P. Snow's history, *The Physicists*. Perhaps by perceiving this particular connection of our own profession with society, we can find a way to gather courage.

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## Marxism in West German Science and Math

Peter Plath and Hans Jörg Sandkühler, editors. *Theorie und Labor: Dialektik als Program der Naturwissenschaft* (Theory and Laboratory: Dialectics as Program of the Natural Sciences). Kleine Bibliothek 106. Pahl-Rugenstein Verlag, Köln, 1978, 341 pages, DM 19,80.

Science and technology play such an important role in present-day national and big business affairs that vital questions concerning their social influence are raised. Scientists and engineers are on the firing line, many of them deeply affected by questions of social responsibility. Among those who feel this strongly and try to do something about it are the men and women influenced by Marxist thought.

There is also the problem of what is happening inside the turbulent domain of present science with its opposing trends: the split between physical and manual labor (*Hand und Kopfarbeit*) and their possible association in a democratic society, between academic theory and industrial practice, the uniqueness of each science and at the same time the unity that binds them, the alienation of man versus the desire for brotherhood. The authors of this book on *theory and laboratory*, each in his own domain, try to investigate the new trends in the sciences and to understand them in what they call, after Frederick Engels, the *dialectics of nature*.

The editors Peter Plath, a physical chemist, and Hans Jörg Sandkühler, a philosopher, both at the University of Bremen, have collected eleven essays by relatively young authors (the oldest was born in 1938)—one Italian, one Danish, the others West German. They address subjects pertaining to mathematics and the natural sciences—physics, chemistry, biology—and the philosophy of science. With their Marxist outlook, they see the interrelations of science and philosophy in historical perspective. They relate science to the productive forces in society, stress the necessity of the concrete to reach the abstract. They think in terms of what they call *democratic science*—an important concept indeed.

Two of my favorites are the essays by Wulf Krause and by Horst-Eckart Gross. Krause, in a study of "Galileo and the problem of the separation of physical and intellectual labor," seeks a path between the conflicting opinions on Galileo's recourse to experiment in his discovery of the law of falling bodies, and concludes that Galileo did not come to his abstractions before he studied the physical realities concretely. He makes the point that abstract "mathematical physics" really did not start before Newton and Lagrange (I would prefer to say: not before Euler in his *Mechanica* of 1736). Gross sketches, in the light of the present participation of mathematicians in the production process, the way mathematics was related to this process in previous forms of society, notably antiquity, the mercantilist and the industrial periods. He also devotes space to technology and education in this valuable contribution to the sociology of mathematics.

The essays by Thomas Mies/Michael Otte and by Plath deal with the interdisciplinary character of so much present work in the sciences, Plath dealing with chemistry in particular. Science, as a product of social praxis, reflects its complexity in the many relationships that bind the departments of science together. A paper by Christa Thoma-Herterich/Peter M. Kaiser, on the relationship between scientific and dialectical materialism, moves in the same direction while discussing biological problems of evolution, mutation, metabolism, genetics, and information.

Papers by two philosophers, Uffe Juul Jensen and Kurt Bayertz, try to bridge the separation between philosophy and science so typical of bourgeois philosophy, which leads them to study the relationship between empirical and ontological aspects of a materialist dialectics of nature. Here is some criticism of Althusser and others for failing to see the meaning of science as a concrete historical process, and of Sartre for scepticism on the possibility of a dialectics of nature in Engels' sense.

Sandkühler tackles the problem of the role of determinism in historical materialism in the light not only of modern philosophers such as Adorno, but also those of the 18th century such as Holbach. Here, E. Bernstein's revisionism with its retreat to Kant comes under criticism. With a paper by Harald Boehme we come to a critique of positivism and of the "language fetishism" of Wittgenstein. Related trends apepared in the student movement of the 1970s: a doctrine that saw mathematics as an "ideology" of the ruling class, that alienated students from the real world, and that separated their movement from that of labor.

Other papers are by Giulio Giorello, on mathematical abstraction and the dialectics of knowledge in natural science research, and by Wolf Jürgen Richter, on chirality as an organizing principle in living matter. [Chirality refers to mirror-image forms, e.g., left and right shoes or optical isomerism.]

What bothers me about some of these papers, typical of much appearing today on ontological and epistemological problems of Marxism, is the question: Why are so many academic philosophers, especially among the Germans and French, unable to express their thoughts in a language that an ordinary person can understand without the risk of a headache? This question concerns Marxists especially because of their avowed desire to bridge the gap between academics and the honestly toiling *Jimmy Higgins* (Upton Sinclair's epitome of the rank-and-filer). Engels could solve this problem of communication. Why not take him also as a model for style?

And, oh, the golden days when Descartes and Locke wrote their prose! As Walter Scott once quoted an old Scotsman: "What signifies me hear, if me no understand?"

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## Contradiction in Relativity Theory

Arthur I. Miller, *Albert Einstein's Special Theory of Relativity: Emergence (1905) and Early Interpretations (1905-1911)*. Addison-Wesley 1981.

There will come a day, no doubt, when we get some dramatically new insight concerning relativity theory and its origins early in our century. In the meanwhile, Arthur I. Miller has given us a highly useful account of the interplay between empirical data and theoretical formulation by which things got started along the path to where we are now. Whatever it may lack in readability, Miller's text is rich with detail on the scientific environment out of which emerged Einstein's formulation of relativity theory and on the process by which that theory began to transform its environment. This review touches on some philosophical highlights of Miller's account.

Careful reading reveals a central theme concerning the struggle between two opposing methodological approaches. On the one hand are *constructive theories*, based on assumptions concerning the nature of matter and explaining why phenomena occur (e.g., statistical mechanics). On the other hand are *theories of principle*, based on assertions or postulates concerning the form that physical laws must assume in order to forbid certain phenomena (e.g., classical thermodynamics, which forbids perpetual motion). The definitions are from Einstein himself. He placed an equally high premium on each approach, finding merit in theories of principle for "their logical perfection, and the security of their foundation (within their domain of applicability)" [p. 133]. It turns out that special relativity emerged as a theory of principle because Einstein in 1900 had despaired of "discovering the true laws by constructive efforts" [p.137]. In the 1905 paper, Einstein showed confidence in the "logical perfection" of his new theory of principle by stating, for example, that it constituted "a complete expression for the laws according to which . . . the electron must move." Miller comments: "To most of the readers of 1905 who were steeped in the primacy of experiment over theory, this statement may well have sounded like a haughty command" [p. 333]. Such a methodological approach went against the grain of leaders in the physics community — Lorentz, Abraham and Poincaré — who were seeking to solve the same problems constructively, in terms of an electromagnetic world picture in which material electrons interacted with an all-pervading ether.

Lorentz, whose model for the electron was mathematically equivalent to Einstein's, acknowledged in 1912 that the Einstein formalism contained no inconsistencies [p.259] but, to the end of his life, preferred his own interpretation preserving absolute time and simultaneity [cf.p.256]. Poincaré, who stood with Lorentz in never recognizing any theory of relativity, wrote in 1912 that "some physicists want to adopt a new convention" for simultaneity but "those of us who are not of this opinion can legitimately retain the old one" [p.255]. Abraham's electron model predicted a slight difference in "transverse mass" from that of the Einstein-Lorentz model. Though experiments of 1908 and 1914 were generally taken as decisive confirmation for the Einstein-Lorentz formulation, new experiments in 1938 revealed that the earlier measurements had not been precise enough to support such a conclusion [pp.350f.]. Abraham himself had never accepted the empirical data as conclusive and, even while acknowledging the accomplishments of relativity theory, continued to hope for its empirical disproof [p.385]. Miller adds: "It turned out that Abraham was correct, but by 1938 the problem . . . was moot." In the meantime, of course, Einstein's theory had become the cornerstone of fundamental theory in modern physics, exercising profound influence on philosophical thought of our century [cf.p.4].

These gleanings from Miller's text, surely symptomatic of a vanquished paradigm, raise a question as to why Miller found the Kuhnian notions of Gestalt switch, paradigm, and scientific revolution were "inapplicable" to his subject matter [p.9]. To this reviewer it seems that Miller's history might have been made accessible to more readers by dramatizing it in terms of methodological paradigms competing at a crucial moment in the development of physics. (Revolution is a long-established concept in science, and it is possible to employ Thomas Kuhn's useful, historically-based concept of "paradigm" without accepting his idealist formulations such as treating "truth" as an absolute.) An even better framework for Miller would have been a consciously dialetical treatment of early relativity history in terms of an imperfect new theory, emerging from the contradictions of older theory, and bringing into being a new set of contradictions.



Miller does give some attention to the contradictions in physics that gave rise to Einstein's theory. He discusses, for example, how "Einstein resolved the tension, or incompatibility, between the laws of mechanics and electromagnetism by proposing a single principle of relativity applicable to both" [pp.137,164]. And, similarly, "Einstein realized the contradictory conceptions of light propagation held by electrodynamics and astronomy. The two postulates of relativity theory enabled Einstein to eliminate the asymmetrical treatment of light propagation by two branches of physics which he took axiomatically to be on equal footing" [p.196].

Miller also recognizes that contradictions existed within the newborn theory. We learn, for example, that Einstein himself acknowledged in 1923 that it would have been "logically more correct" to have deduced the properties of measuring rods and clocks from a "stipulation of meaning" applied directly to laws governing the properties of matter [p.193]. Miller notes, in relation to the clock paradox, that Einstein "was not concerned over whether there were any inconsistencies in the special relativity theory" but did resort to dynamical arguments from general relativity theory in order to resolve contradictions in the lower-level kinematic theory of special relativity [pp.272f.]. Einstein also recognized "logical weakness" in his concept of inertial system [p.193] and Miller exhibits uneasiness over Einstein's 1905 use of the term "resting system" as a generalization of Lorentz's concept of a reference system fixed in a resting ether [p.202] since, for Einstein, the resting system could be any inertial system [p.287].

But some paradoxes pass unremarked in Miller's account. An instance is von Laue's theoretical justification of Einstein's relativity theory, in which "the mechanics of particles could be deduced from the mechanics of continuous media, but the converse was not true" [p.373]. And, speculating on why Einstein kept the notion of light quanta separate from his "theory of principle" [p.363], Miller similarly ignores the contradiction between the concept of discrete energy bundles (later, photons) and special relativity as a formalism based entirely on a Maxwellian continuum, a contradiction which haunts present day quantum electrodynamics in the form of infinite energies and other mathematical anomalies [cf. L. Talkington, "Contradiction in Wave-Particle Duality," *S&N* No. 2, pp. 19-23].

No philosophical framework is evident in Miller's account. For example, the discussion of Mach's influence on Einstein [pp.127-

131] is quite uncritical. Mentioning Einstein's characteristic tendency to frame Machian arguments in a "quasi-esthetic form" to reveal "asymmetries that should not be contained in the laws of nature" [p.130], Miller seems unaware that the heavy emphasis on symmetry considerations in modern physics, largely due to Einstein's example, is now seen by some as a major handicap in theorizing. (The old prejudice in favor of symmetry is dying out, reports Arthur L. Robinson, because of asymmetries discovered in fundamental phenomena, e.g., parity violation; one theorist is quoted: "You used to have to explain why when [symmetry] fails. Now you have to explain why [when] it is respected." [*Science* 210: 619; 1980.])

One gets the impression that the contradictions in relativity theory appear in Miller's book just as he found them in the historical record, rather than having been consciously selected. Even so, the evidence of contradiction in relativity theory seems to have irritated one reviewer, who complained that material is included "for historical completeness . . . that interrupts the essential conceptual development" [A. Douglas Stone, *Physics Today*, March 1982]. Another reviewer simply ignored the presence of contradiction, asserting instead that Einstein's insights "led, as we all know, to a straight-forward, exact and complete explanation of all the phenomena that had so exercised the experts" [A.P. French, *Nature* 293: 766; 1981]. This assertion typifies the prevailing positivist pretense that relativity is perfect and complete, with no internal contradictions.

Neither Miller nor his reviewers are able to take a long-range view in which Einstein's theory appears as just one more transient phase in the historical development of physics. For this I turn to Marxist critics. In 1908, when the electrodynamics of Lorentz and Poincaré still dominated the scene, Lenin could insist on its temporary character:

> The "essence" of things, or "substance", is *also* relative; it expresses only the degree of profundity of man's knowledge of subjects; and while yesterday the profundity of this knowledge did not go beyond the atom, and today does not go beyond the electron and ether, dialectical materialism insists on the temporary, relative, approximate character of all these *milestones* in the knowledge of nature gained by the progressing science of man. [*Materialism and Empirio-Criticism*.]

Similarly, physicist J.D. Bernal could insist more than a half-century later on the temporary character of Einstein's theoretical milestone:

> We are just entering a new phase of criticsm of physical theory where the evident *malaise* of mathematical physicists at the inadequacy and inelegance of the quantum and relativistic theories is giving rise to efforts at radical reconstitution. [*Science in History*, MIT 1971.]

Thanks to his conscientious historiography, Miller's book can contribute a great deal towards such a reconstitution of relativity theory. Even while vigorously praising Einstein's 1905 paper for its unparalleled "intellectual virtuosity" [p.xiii], he has provided much evidence of the internal contradictions that make possible further



theoretical development. In this sense, the usefulness of the book may extend far beyond the author's conscious intent. Just as Miller rejected the concept of scientific revolution to characterize Einstein's overthrow of the constructive approach in physics, he fails to perceive that his own historical analysis of failed efforts in the past may provide useful clues for the development, sometime in the future, of a successfully constructive relativity theory. This would be the negation of Einstein's negation, marking the completion of another upward spiral in the motion of physics.

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## Scientific Concept As Historical Process

*Dictionary of the History of Science*. Ed. by W.F. Bynum, E. Janet Browne, and Roy Porter. Princeton Univ. Press 1981. $40

Scientific concepts are usually presented in their logical form, expressing the necessary connections between phenomena so far as they are known. Such logical expression, however, tends to obscure the connections that are missing or only imperfectly grasped. Thus, the inevitable gaps in knowledge are more likely to be revealed by studying the history of a concept in its actual concrete development. We can therefore truly welcome the appearance of this reference volume dealing in a serious way with the history of scientific ideas, especially since it gives due emphasis to the philosophical aspects of conceptual developments.

The new *Dictionary of the History of Science* should be very useful for a quick survey of such conceptual (and terminological) developments. It contains 700 entries averaging about 500 words each, so that it resembles a small encyclopedia. It is well organized for reference purposes, with a biographical index and an analytic table of contents listing relevant articles under ten overlapping subject headings.

The 167 entries related to biology include, for example, race, recapitulation, reflex, and regeneration. Medicine is similarly covered by 139 entries; the human sciences (including psychology) by 103 entries. The philosophical problems of the life sciences are treated historically in entries such as reductionism, mind-body relation, classification, vitalism, and spontaneous generation.

Physics-related entries (103) include field and fluxions, vacuum and vis viva. Entries for astronomy are 64; for chemistry, 60; for earth sciences, 60; for mathematics, 38. To a considerable extent, the historical development of a concept is related to its changing empirical basis, as in articles on heat and thermodynamics, light, and tides.

The 131 entries for philosophy of science include materialism and metaphor in science, realism and reification. In the 43 entries related to historiography of science the externalist view of scientific development is well represented, including discussions of the Hessen, Morton and Needham theses.

On the whole a professional level of historical treatment has been maintained by the three editors (each active in the history of medicine), assisted by eight subject editors and 86 more contributors (mainly British scholars). The philosophical discussions tend to reflect the "realist" orientation; the Marxist outlook appears explicitly (and sketchily) only in a few articles, sometimes marred by distortion. Under historical materialism, for example, undue emphasis is given to the relativist argument by Alfred Sohn-Rethel that science itself "should be seen as a transient social form." Among articles that could have been improved by reference to the pages of *Science and Nature* are those on causality, dialectic, philosophy of mathematics. and sociology of knowledge.

Overall, this new reference volume fills a general need though its $40 price tag means that most users will have to go to a library to use it. Too bad, because it makes for good browsing as well as for exploratory reference on a specific subject. [It is also highly regrettable that most of our readers have no easy access to that excellent but long out-of-print reference work, *A Dictionary of Philosophy* (Moscow, Progress 1967), a volume which provides clear Marxist statements on many of the same subjects.]

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## Continuity and Discontinuity in Evolution

Donald R. Griffin, *The Question of Animal Awareness: Evolutionary Continuity of Mental Experience*. Rockefeller University Press, 2nd ed. 1981, $13.95.

Charles J. Lumsden and Edward O. Wilson, *Genes, Mind and Culture: The Coevolutionary Process*. Harvard University Press 1981, $20.00.

Is there any process more fascinating than the constant interplay between the social and biological which we experience in our everyday lives? And how vast are the controversies that rage over how much of animal nature operates within our own human nature! A particular question concerns how much of an evolutionary gap has been created by the emergence of the human mind and culture. The books reviewed here approach that question from different directions. Donald Griffin shares with us his upward struggle out of the bogs of behaviorism, while Edward O. Wilson and his young physicist collaborator Charles J. Lumsden want to drag us down into the lower depths of sociobiological reductionism.

Griffin, seeking to open the eyes of fellow ethologists to the theoretical possibility that animals have significant "mental experiences", states his position on evolutionary continuity of consciousness with laudable humility:

> Most people not indoctrinated in the behaviorist tradition take it for granted that animals do have sensations, feelings, and intentions. This intuitive impression is based on our experience with patterns of animal behavior that appear sufficiently analogous to some of our own behavior to permit us to emphathize. The dilemma of contemporary behavioral scientists results from our indoctrination that *as scientists* we must put such notions behind us as childish sentimentality unworthy of a rigorous investigator. Yet the behavioristic and reductionistic parsimony typified by [J.B.] Watson and [Jacques] Loeb may have led us down a sort of blind alley . . . [p. 116, emphasis in original here and throughout.]

Griffin then proceeds to make a good case for evolutionary continuity of such consciousness, discussing the comparative evidence for social communication, animal semantics, and elementary forms of cognition where animal behavior "suggests awareness, conscious intention, or simple forms of knowledge and belief" [p. 171]. Griffin will no doubt be surprised to learn that what he has accomplished with so much effort was anticipated by Frederick Engels a century ago. In a brief but theoretically comprehensive note, using Hegel's term *understanding* in the same sense as Griffin's *awareness*, Engels wrote:

> All activity of the understanding we have in common with animals: *induction, deduction,* and hence also *abstraction* ([my dog] Dido's generic concepts: quadrupeds and bipeds), *analysis* of unknown objects (even the crack of a nut is a beginning of analysis), *synthesis* (in animal tricks), and, as the union of both, *experiment* (in the case of new obstacles and unfamiliar situations). In their nature all these modes of procedure — hence all means of scientific investigation that ordinary logic recognises — are absolutely the same in men and the higher animals. They differ only in degree (of development of the method in each case). The basic features of the method are the same and lead to the same results in man and animals, so long as both operate or make shift merely with these elementary methods. [*Dialectics of Nature*. N.Y. 1940, p. 203.]

But this is as far as the agreement between Griffin and Engels extends. Faced with the question of evolutionary *discontinuity* marked by the emergence of human language, Griffin remains a prisoner of his empiricist background. Though he devotes an entire chapter to the question, "Is Man Language?", and refers throughout the book to authors who assert the unique character of human language, Griffin fails to deal in any significant way with the function of language in abstract thought as the basis for emergent human culture. By ignoring the social ability of humans to carry out vast economic and political projects, Griffin can simply dismiss the wide range of philosophical and psychological thought on the qualitative differences between the cognitive potentials of animals and humans, maintaining that it "is indefensibly circular to argue that language is unique to man and, therefore, no matter how complex animal communication turns out to be, it cannot possibly be comparable to human language" [p. 112]. Let us see how Engels treated this question. In the passage quoted above, concerned with the Hegelian distinction between *understanding* and *reason* (according to which only dialectical thought is reasonable), Engels continues:

On the other hand, dialectical thought — precisely because it presupposes investigation of the nature of concepts — is only possible for man, and for him only at a comparatively high stage of development (Buddhists and Greeks), and it attains its full development much later still through modern philosophy. [*Ibid.*]

Thus, while Engels and Griffin would tend to agree on the ability of a dog to "think" in terms of simple concepts (bipeds versus quadrupeds) and even adapt such concepts to changing experience, there are severe limitations on the complexity of the concepts that a dog can master. (In this sense, the symbolic dance language of bees should be considered as relatively much more limited in conceptual content.) In Engels' view, we would not expect the dog to be able to consider the origins or dialectical development of a concept (its historical conditioning). On the other hand, for Engels, as for Griffin, there should be no occasion for great surprise or consternation in the discovery that chimpanzees can communicate with one another in an abstract sign language created for them by Big Brother *Homo*. The ability of apes to use symbolic language, at about the level of a six-year old human, should only serve to emphasize the gap of discontinuity between animal *awareness* (whether in ape or human) and the developed human faculty for sustained and purposeful social action based on abstract reason.

Unable to understand the qualitative discontinuity introduced by human language, Griffin also fails to appreciate fully the component of evolutionary discontinuity represented by the development of human biological equipment for language processing—the anatomical specialization of brain, larynx and tongue which accompanied the transition from ape to man. Engels has pointed out the crucial role of cooperative labor, based on manual dexterity, in effecting the transition to biped existence with a new level of social consciousness that does not appear in Griffin's discussion:

our simian ancestors were gregarious; it is obviously impossible to seek the derivation of man, the most social of all animals, from non-gregarious immediate ancestors. The mastery over nature, which begins with the development of the hand, with labour, widened man's horizon at every new advance . . .

First comes labour, after it, and then side by side with it, articulate speech — these were the most essential stimuli under the influence of which the brain of the ape gradually changed into that of man . . .

The reaction on labour and speech of the development of the brain and its attendant senses, of the increasing clarity of consciousness, power of abstraction and of judgement, gave an ever-renewed impulse to the further development of both labour and speech . . . strongly urged forward, on the one hand, and . . . guided along more definite directions on the other hand, owing to a new element which came into play with the appearance of fully-fledged man, viz. *society* . . .

By the co-operation of hands, organs of speech, and brain, not only in each individual, but also in society, human beings became capable of executing more and more complicated operations, and of setting themselves, and achieving, higher and higher aims. With each generation, labour itself became different, more perfect, more diversified. Agriculture was added to hunting and cattle-breeding, then spinning, weaving, metal-working, pottery, and navigation. Along with trade and industry, there appeared finally art and science. [*Ibid.*, pp. 282-289.]



Summing up, we can say that Griffin is right concerning the high level of mental activity in animals. Thus, human consciousness has its biological preconditions and, with respect to simple cognition, there is no unbridgeable gap between man and his animal ancestors. This is the point on continuity of awareness that Griffin makes effectively, if rather stiffly. But beyond Griffin's view lies a gulf of evolutionary discontinuity. On the other shore is found highly-developed human language, human intellect, and human culture — all social phenomena with their own laws of development (something else missing in Griffin's account), but all nevertheless dependent on that good old human *gray matter* and human biological equipment for communication by language.

It is relevant here to review some of Griffin's reviewers. Sir Peter Medawar (Nobelist in Medicine, 1960) found Griffin's arguments for evolutionary continuity to be "quite sound" but did not seem to know where to go from there, so ended with a lecture on being kind to experimental animals. [*The Sciences*, Dec. 1981, p. 25] Gerald Zuriff, psychologist, criticized both Griffin and Medawar for their failure to heed the "cogent" argument of John B. Watson, founder of behaviorism, that "the concept of consciousness is neither useful nor scientific" [*The Sciences,* Dec. 1982, pp. 10-11]. Jack P. Hailman, reviewing the 1976 edition, praised Griffin for getting across the message to ethologists and psychologists: "Stop studying only those things easily measured and devote more effort to difficult and important problems of animal awareness" [*The Auk* 95: 615f; 1978].

Let us turn now to the Lumsden-Wilson volume. From its title, the unsuspecting reader might hope for some recognition of evolutionary discontinuity, especially since the authors define the term *coevolution* to include the reciprocal effects of genetic and cultural evolution [p. 367]. But their treatment, unfortunately, bears no resemblance to the reciprocal process described by Engels above. Instead, they have simply carried the mechanistic absurdities of Wilson's *Sociobiology* treatise to their "logical" conclusion. They begin by rejecting the sensible prevailing concept that genetic coevolution provides a basis for culture "only in the sense of creating the capacity to evolve by culture" [p. 1]. Proceeding to deny that culture represents a higher level of organization of human existence with laws of its own, they assert that culture is merely "the product of vast numbers of choices by individual members of society," a strategem that permits them to model culture in terms of "epigenetic rules at the level of one person" which can be expressed "through the procedures of statistical mechanics" [pp. 176-177]. Surely this is the ultimate in the bourgeois ideology of individualism! And the whole book is nothing but an elaborate effort to justify such a one-sided, simplistic and mechanistic approach to the human mind and culture.

The end result is short on empirical data, long on meaningless mathematical equations, and heavy with misrepresentations achieved often enough by exploiting the weaknesses of other mechanistic materialist scholars. For example, Noam Chomsky is cited frequently. Brazen misinterpretations are common. Even Marx supposedly supports the Lumsden-Wilson sociobiological thesis ("Marx went so far as to speak of history itself as part of natural history, and the inevitability of a union between the natural sciences and the science of man." [p. 356]). But, in their not so humble opinion, "the key error of Marxism as a scientific theory of history is its tendency to conceive of human nature as relatively unstructured and largely or wholly the product of external socio-economic forces" [p. 355] and they are willing to help us Marxists "shift emphasis of social theory so far from the isolated themes of class struggle and economic determinism as to bring Marxian scholarship close to the mainstream of Western social science and blend the two together . . . in a biologically fundamental picture of human nature" [p. 356]. Predictably, there is no place for class interest in their view of culture. In fact, the only vague allusions to class in the whole volume seem to be a mention of "ruling families" that practiced brother-sister incest in ancient societies (Egypt, Incas, etc.), and a mention of "socioeconomic structure" in relation to societies subject to extreme environmental fluctuations (as in the Kalahari Desert) [pp. 85, 208].

The reception of this book in the scientific community has been markedly derisory. A news story by Roger Lewin [*Science* 212: 908-910; 1981] noted that it "is certain to attract a good deal of attention, because Harvard University Press is promoting it unusually vigorously for what essentially is a research monograph, and because Edward Wilson's name is attached to it." Lewin then asks "how sound a contribution" the book makes, and gets some highly critical answers from scholars. The formal review in *Science* [213: 749-751; 1981] by Cloninger and Yokoyama concludes that the theory "is open only to weak tests at best" and that, "like the psychodynamic theories of Freud and his faithful disciples, will enjoy wide retrospective explanatory power but can make only limited testable predictions." The review in *Nature* [291: 267-268; 1981] by Edmund Leach (Fellow of King's College, Cambridge University) was less polite: "This book comes so close to being a parody of the genre to which it belongs that I have had difficulty in believing that it is not intended as an academic hoax." Richard C. Lewontin [*The Sciences*, July 1981, pp 23-26] gave a penetrating Marxist analysis of the book, emphasizing the ahistorical approach to culture. A rebuttal by the authors [*ibid.* Nov 81] was no more convincing or informative than the book itself. But letters printed in *The Sciences* [Nov 81 and Mar 82] show not only the sociobiology community up in arms to defend Wilson but also a general lack of understanding on the relation of human biology to human culture which only Marxism seems able to clarify.

Neither of these books gives a satisfactory account of emergent human consciousness in terms of the evolutionary gap or discontinuity compared to other animals. In some static sense, the two approaches may even be the same in the end. For example, Griffin finds the assumptions of sociobiology to be "plausible" [p. 146], while Wilson writes a blurb of praise to promote Griffin's book. Nevertheless, there seems to be a substantial difference in their direction of motion. Where the Lumsden-Wilson sociobiological approach would reduce humankind to the level of other animals, Griffin's tends to raise animals to the level of *Homo sapiens*. Is it just sentiment on my part to find Griffin's motivation the healthier?

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## Recombinant DNA in Social Context

Sheldon Krimsky, *Genetic Alchemy, The Social History of the Recombinant DNA Controversy*. MIT Press 1982. 445 pages $24.95

Krimsky introduces this case study with the claim that scientific decisions cannot be understood adequately without taking account of factors external to scientific inquiry, i.e., the social context and social relations of science. The subject, recombinant DNA (hereafter rDNA) research, is identified as a crisis of risk with attendant debate much like the debate among scientists over the development of the hydrogen bomb. (A particular risk many feared was escape from a laboratory of a bacterium into which a tumor virus had been implanted, a bacterium that could easily become resident in the human intestinal tract.) Krimsky wants to know about changes that take place in science and in the positions held by scientists during such crisis periods. The rDNA controversy offers a superb opportunity to study such changes and their causes. One method Krimsky uses is to trace these changes chronologically with, here and there, an acute and illuminating analysis of the arguments advanced by various participants in the debates. Here the author, trained in both physics and philosophy, is at his best. What emerges from this study is not only alchemistic, as the title suggests, but a veritable litany of scientific fallacies which, dramatically exposed, serve as hinges holding together a first rate historical narrative.

For the most part, the scientists who took the lead in calling attention to the potential risks involved in these new gene splicing techniques were those who had in the late 1960's been engaged in discussions and organizations concerned with the social responsibilities of science and scientists.

In general, these critics were more inclined to a collective and social treatment of the problems even though individual scientists frequently differed in their opinions on precisely where the limits of responsibility lay. But none of the critics of the research were invited to participate in the first major, international conference on the risks of rDNA research at Asilomar Conference Center; in fact, they were not even invited to attend as non-participants. The Asilomar Conference organizers also rejected the recommendations of the Genetic Engineering Group of *Science for the People* which, in a solicited letter, made the following five observations:

1. The combination of rDNA techniques, cell fusion, and in vitro fertilization are converging toward human genetic engineering.
2. The public should be informed about rDNA research; decisions about who benefits and who bears the risks should not be left in the hands of scientists.
3. Science is a value-laden activity.
4. Genetic engineering does not arise out of general social needs; scientific interests are not always synonomous with social interests, e.g., research into the cure of diseases continues to take precedent over research into the prevention of diseases.
5. Broad public participation in the decision making process at the Asilomar Conference is desirable.

Not only was that broad public participation at Asilomar ruled out, but even scientists who advocated it were excluded.

Though no consensus concerning risks resulted from the Asilomar Conference, its leaders and organizers gave due heed to a remark of David Baltimore: "If scientists cannot reach consensus, the issue will be taken out of their hands". This warning became the basis and stimulus for a contrived political (not scientific) consensus among rDNA research scientists. As Krimsky points out:

> To secure the goal of disciplinary autonomy, the organizers of Asilomar had accomplished two objectives: (1) they defined the issues in such a way that the expertise remained the monopoly of those who gain the most from the technique, and (2) they chose to place authority for regulating the use of technique in the agency that is the major supporter of biomedical research in the United States. As the controversy developed, these objectives came under attack from persons both inside and outside the scientific community. [p. 153]

The vested scientific interests stressed that the concern over risks should be based on current knowledge, not on current ignorance. If there was no "evidence" of risk, which there could not be in principle because of the very nature of the research, these interests argued, scientists bear no responsibility for proving that what they are doing is safe. This was (and is) the position of most scientists engaged in rDNA research; in effect, they refused to confront the problems discussed by Krimsky:

> For a research program in its early stages, it is highly likely that unexpected results will appear. Science, after all, does not advance through a continuous path of predictable outcomes. But how does the unexpected in science bear on the problem of risk assessment . . . [In most cases of rDNA research,] since the recombinant microorganisms had never been created . . . there was no empirical evidence from which to proceed. This is where the term 'prediction' took on political overtones. Those who wanted to see the research stopped or substantially slowed down emphasized the primitive stage of biological prediction when new biotypes were being considered. According to this view, there were too many variables and too many exceptions to make *a priori* judgments.
>
> A different position on the efficacy of prediction in biology came from those scientists who were concerned about restraints on free scientific inquiry. Their arguments drew heavily on analogies between natural recombinations and what was being planned in the laboratories of molecular biologists. They approached the problem of risk assessment as reductionists. Beginning with a concrete scenario for a hazardous event, they estimated the probabilities of each sub-event in the causal chain. Those critical of the reductionist hypothesis argued that catastrophes do not conform to a linear process. Furthermore, the reductionist hypothesis treats biology like a mechanistic system and takes no account of emergent events." [pp. 89-90]



The conversion process by which the vast majority of scientists came to support unfettered rDNA research involved factors beyond scientific rationality and argument. It was, in fact, a tense ideological struggle. Krimsky reports testimony before the Senate Subcommittee on Health chaired by Edward Kennedy in which Willard Gaylin, a physician-bioethicist connected with the Hastings Center, stated that

> . . . the conventional wisdom would approve regulating scientific activities if they impose a threat to society . . . The knowledge engendered by science is a social product because of its historical roots, its public resources, and "because it has become an indispensable part of our common culture." We have a right to control science not because of its failure, but because of its success. This is a fundamental departure from the laissez faire conception of scientific pursuit underpinning the rationale of progress and the liberal view of intellectual freedom. Gaylin's position has been associated with radical groups like Science for the People, but is rarely heard in the liberal circles of the Hastings Center. His testimony before the Health Subcommittee upset some members and associates of the Hastings Center. Maxine Singer wrote a five-page, single-spaced critique of Gaylin's remarks point by point. Daniel Singer was concerned that Gaylin's testimony would give Hastings an antiscience reputation. [p. 168]

As the debate proceeded, two antagonistic philosophical perspectives surfaced. These were the *reductionist* and *organismic* theories of molecular biology. The reductionists argued that the properties of the whole can be completely explained in terms of the properties of component parts. The organismic position was that the additivity principle did not apply to organisms, that the parts of an organism are mutually determining and interdependent giving rise to the possibility of unpredictable emergent properties. If the phenomenon of emergent properties was plausible, " . . . then the factors that determine expression of DNA in an organism are transferable between divergent species. If there are any natural barriers to expression of eukaryotic genes in prokaryotes that cannot be overcome in this manner, then most of the hazards of such gene transplants are zero. But it is precisely the expression that scientists were anticipating and that allows the technology to revolutionize the field of molecular biology." [p. 175]

The reductionist thesis proclaimed that "from nonpathogens, pathogenesis will not emerge." This reductionist doctrine reflects what Marcuse called the one-dimensionality of bourgeois reason which makes it incapable of grasping elements in their interrelatedness. One wonders how scientists who take such a position account for their own existence—as a product of evolution (emergence) from hydrogen, carbon, nitrogen and oxygen. It is significant in this regard that the Marxist approach to science which is emergentist, i.e., dialectical, has brought the central contributions to theories of the origin of life in this century (the work of A.I. Oparin, J.D. Bernal and J.B.S. Haldane). Indeed, it is *precisely* at the biomolecular level that one might expect to encounter emergent phenomena in relatively frequent occurence!

From the onset of the controversy, it was generally agreed that a "safe" variant of the bacterium E. Coli was the best vehicle for rDNA implanting. This organism, in the wild state, is an endemic pathogen, the cause of infection and death in many developing countries. It was chosen on the basis of its relative impotence in the United States where hygienic control of water supplies prevails to minimize the chances of widespread infection. But in some countries the escape of an altered and more pathogenic E. Coli organism could be catastrophic (bacteria do not recognize national boundaries). The decision on the part of the scientific establishment to "clear" E. Coli K12 for genetic manipulation was the height of scientific and cultural imperialism. *O sweet commerce and the freedom of enterprise!*

Still, for some years modest guidelines for experimentation had been imposed by National Institutes of Health. Then, in September 1981, the Recombinant DNA Advisory Committee of N.I.H. did away with its guidelines. This was accompanied by threat, intimidation, and character assassination directed against the critics of the research. It originated in and was executed by the biological establishment which sought to speed up research with an eye toward huge personal financial gains. Scientists began to form private corporations that would enable them to market their products with maximum profit. In this drive for *free market* science, the arguments and evidence against restrictions on research and against social control were contrived, fabricated, phoney:

> The stakes in the rDNA controversy were very high: the control of science and the control of an immensely powerful and potentially profitable technology. [Human insulin produced by this technique was cleared for sale as this review is written.] Scientists wanted to keep that control to themselves, and commercial interests were satisfied to give it to them. It was a tradition with which both were comfortable. But others believed that this kind of technology was too powerful, both for its positive and negative potentials, to leave to scientists. It is no wonder that the actual nature of the evidence should be secondary since control, not "safety", was to a large extent the main issue. "Safety" was only the strategic hilltop whose possession would help win the war. [p. 243]

In 1977 a citizens' committee in Cambridge, Massachusetts, the Cambridge Experimental Review Board, of which the author was a member, issued the following declaration:

> While we should not fear the increase of our knowledge of the world, to learn more of the miracle of life, we citizens must insist that in the pursuit of knowledge appropriate safeguards be observed by institutions undertaking this research. Knowledge, whether for its own sake or for its potential benefits to humankind, cannot serve as a justification for introducing risks to the public unless an informed citizenry is willing to accept those risks. Decisions regarding the appropriate course between the risks and benefits of a potentially dangerous scientific inquiry must not be adjudicated within the inner circles of the scientific establishment." [p. 307]

The withdrawal of the research guidelines by N.I.H. in 1981



marked the end of the grim story. A most undesirable outcome of the conflict was the increasing tendency toward secrecy and competition—the antithesis of free science—as research converged on industrial applications and profit. A significant sector of academic biology was being integrated into the system of capitalist commodity production. Many, perhaps most, of the scientists involved, because of their bourgeois background and their individualistic ideology, subscribed to a narrow, crude and one-sided conception of what it means for science to be free, i.e., unregulated, citing the Lysenko episode indignantly. They were quite unable or unwilling to understand that a free science has no secrets or patents, is open to public and collegial scrutiny and criticism, is a social and historical creation of the collective labor of many contributors, and is inherently democratic. Free science as an instrument created by society is subordinate to the needs of society—it should not be used as a tool for amassing profits. The narrow subjectivist ideology of bourgeois science also undermines science itself; a science mystified by concealment behind the pseudo-ethic of the free market invites popular attack, especially so when it fails to meet social needs while serving the interests of a small minority. Biology courses in creationism are one result.

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**Books Received**

Lorin Anderson, *Charles Bonnet and the Order of the Unknown*. Studies in the History of Science, v. 11, Reidel 1982. $37.00.

Mario Bunge, *Scientific Materialism*. Reidel 1981.

Joan Lisa Bromberg, *Fusion: Science, Politics and the Invention of a New Energy Source*. MIT Press 1982. $30.00.

Dmitry P. Gorsky, *Definition*. Progress, Moscow 1981. $7.20.*

Martin Harwit, *Cosmic Discovery: The Search, Scope and Heritage of Astronomy*. Basic Books 1981. $25.00.

John Losee, *A Historical Introduction to the Philosophy of Science*. Oxford Univ. Press. 2nd edition. 1980. $6.95 paper, $14.95 cloth.

Ilya Novik, *Society and Nature*. Progress, Moscow 1981. $6.00.*

V.V. Nalimov, *Faces of Science* (ed. by Robert G. Colodny). ISI Press, 1981. $22.50.

Bertell Ollman and Edward Vernoff, editors. *The Left Academy: Marxist Scholarship on American Campuses*. McGraw-Hill 1982. Paper, $8.95.

*The Philosophical Forum*, "Sociobiology: The Debate Evolves" (special double issue). Vol. 13, Nos. 2-3, 1981-82. $7.50.

Herbert A. Simon, *Models of Discovery*. Reidel 1977. $19.95 paper, $39.50 cloth.

Marx W. Wartofsky, *Models: Representation and the Scientific Understanding*. Reidel 1979. $14.95 paper, $36.00 cloth.

**Sean Sayers, "Contradiction and Dialectic in the Development of Science."** ***Science and Society*** **45 (4): 409-436; 1981.**

This is an effective explanation of dialectical contradiction for answering the typical questions raised by scientists. Simple examples and straightforward arguments make clear the difference between formal and dialectical logic. Discussions of the ideas of Popper, Lakatos and Kuhn are also used to clarify this difference. Sayers' paper is recommended to those who have been confused by the Mussachia article which appeared previously in same journal (41: 257-280; 1977 and 42: 185-198; 1978). See also criticism of Mussachia in *Science and Nature* (No. 4: 1-2, 77-78) and exchange of letters in this issue.

Sayers' paper may nevertheless have its own contradictions. A careful re-reading, occasioned by questions from Erwin Marquit, reveals what may be interpreted as an inconsistency. Sayers' main theme concerns the concrete nature of dialectical contradictions. "Formal logic," he says, "just because it excludes all considerations of content, is indifferent to truth . . . the minute the *content* of what is being said is taken into account the situation changes. Now the contradiction becomes concrete . . . [and] in concrete circumstances one may well have good reasons for asserting both sides of a contradiction" [p.425]. Elsewhere, however, in discussing Lakatos' approach to the logic of science, Sayers refers to an unresolved conflict between theory and experiment as "a contradiction in the full logical sense" [p.420]. To this observer it seems that, while such a conflict can be represented in terms of a formal contradiction (excluded middle), the concrete and practical nature of the problem makes it inherently a dialectical contradiction. Perhaps Sayers has given us here an excellent if unwitting example of dialectics in scientific thought—the case where a contradiction is both formal and dialectical at one and the same time!

Saul Birnbaum adds that there are some very good things to be found in *Dialectical Contradictions: Contemporary Marxist Discussions*, ed. by Erwin Marquit, Philip Moran and Willis H. Truitt (Marxist Education Press, Minneapolis 1982).

**Marx, Engels, Lenin,** ***On Dialectical Materialism.*** **Progress, Moscow, 1977. 422 pages. $3.95***

Provides lengthy passages from works of the masters, dealing with many aspects of Marxist philosophy. It would be more useful as a reference work if it included a subject index and if it included more from Lenin's *Philosophical Notebooks*. Recomended as a supplement to *Reader in Marxist Philosophy*, edited by Selsam and Martel (International, New York 1963).

**Ronald L. Numbers, "Creationism in 20th-Century America".** ***Science*** **218: 538-544; 1982.**

Traces the development of the creationist pretensions as science—from William Jennings Bryan up to the present "claim to scientific respectability" with new philosopher heroes Karl Popper, who contended that evolution theory could not be falsified, and Thomas Kuhn, who "described scientific progress in terms of competing models or paradigms rather than the accumulation of objective knowledge." Provides useful historical background for ideological engagement with the enemy.



**Nathan Rosen, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?". In** ***Albert Einstein: His Influence on Physics, Philosophy and Politics*****, ed. by Peter C. Aichelburg and Roman U. Sexl (Friedr. Vieweg & Sohn, Braunschweig 1979), pp. 57-67.**

This is the Rosen of Einstein-Podolsky-Rosen fame, re-examining the controversy with Niels Bohr over the 1935 paper by the trio (with the above title). The recent experimental investigation of Bell's inequalities has convinced him that a complete description of microworld reality cannot be obtained by some simple modification of quantum mechanics such as hidden variables. Rather than accept Bohr's narrow description of reality (and consequent idealist interpretation), Rosen concludes that the question of completeness no longer has physical meaning unless it can be re-interpreted to call for a complete new theory, which "is likely to involve revolutionary changes in concepts and principles—perhaps even changes in our concepts of space and time." Not too sanguine on the prospect for this, he ends by understating: "The consequences of a revolution in physics are hard to foresee."

**Yu. A. Kharin,** ***Fundamentals of Dialectics.*** **Moscow, Progress, 1981, 256 pages. $6.40.***

Saul Birnbaum says this book is slanted towards India and Buddhist readers, answering a trend to mysticism in physics (*The Tao of Physics, The Dancing Wu Li Masters*, etc.) which gives it added value. This brief, popularized exposition of dialectical materialsm is also noteworthy for its attempts at diagrammatic representation of subjects such as the structure of Marxist-Leninist philosophy, the forms of motion of matter, and individual consciousness. May give you ideas for developing your own blackboard representations. But it does not deal so directly with the real philosophical problems of science as in A.P. Sheptulin, *Marxist-Leninist Philosophy* or in the much more comprehensive though no longer available *Fundamentals of Marxist-Leninist Philosophy*.

**Jorge Ruda,** ***Estudios de Psicologia Dialectica.*** **EDUCA, Centro-america, 1980.**

Based on courses given at University of Costa Rica, this text deals with the origins and development of Soviet psychology, including a detailed survey of contemporary Soviet psychology. The author is professor of psychology, University of Ottawa. The publisher EDUCA represents national universities of six Central American countries.

**Paul Forman "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists to a Hostile Intellectual Environment."** ***Hist. Stud. in Phys. Sciences*** **3: 1-115; 1971.**

*Corrigendum.* The volume and year were given incorrectly in *S&N* No. 2, p. 65, where it was noted that this scholarly paper, providing material for a Brechtian drama, is required reading for those interested in the philosophical problems posed by quantum mechanics.

*Lester Talkington*

## *Science under Hitler: A Bibliography*

**(Compiled by Bob Broedel, PO Box 20049, Tallahassee, Fla. 32304)**


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# On Causality in Quantum Mechanics: A Mathematician's View

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COMMENTS by Erwin Marquit,
Lester Talkington, David Schwartzman

The debate between Marquit, who favors the viewpoint of Hörz *et al.* [1980a], and Talkington [1980], who favors the views of Svechnikov [1971], has forced me to go back to some basic texts of quantum mechanics while reading and re-reading their papers. Thinking about what I have read has led to some conclusions that I now submit as my contribution to the discussion. My conclusions about the debate are these: Marquit and Hörz *et al.* are right that causality in quantum mechanics must differ from causality in classical mechanics. They are wrong, however, in that they look for quantum-mechanical causality in the wrong place and ascribe to it the wrong form. Talkington is right in saying that Hörz *et al.* ignore the dynamic underpinnings of the Schroedinger equation. He is wrong, however, in supporting the view of Svechnikov that causality cannot be expressed in a law governing changes of state but must deal with forces and an action or interaction of bodies.

To support my conclusions I shall refer to some crucial facts about quantum mechanics (Appendix A), about the role of real and complex numbers in describing reality (Appendix B), and about relations of dependence between physical quantities (Appendix C). Since these facts have been overlooked in some of the philosophical discussions, I urge that the appendices be read before sections I and II.

## I. The Basic Postulate of Materialism

Materialist philosophy is not tied to any fixed conception of the nature of matter. Scientific conceptions of the nature of matter change with each new discovery, especially as science explores deeper and deeper levels of the reality underlying phenomena that are directly observable by our instruments and senses. The only unchanged postulate about matter of both science and dialectical materialism is that it exists and develops independent of the perceiving mind. This was already understood by Lenin [1927] in his discussion of the twentieth-century revolution in physics:

> Only one thing is, from Engels' viewpoint, immutable—the reflection by the human mind (when the human mind exists) of a world *existing and developing independently of the mind.* No other 'immutability,' no other 'essence' or 'absolute substance,' in the sense in which the idle official philosophy portrayed these, existed for Marx and Engels. The 'essence' of things or their 'substance' is also relative, it expresses only the degree of man's power penetrating into and knowing objects; and even if yesterday this penetration did not go any further that the atom, and today, no further than the electron and ether, then dialectical materialism insists on the temporary, relative, approximate character of all these milestones on the road of knowledge of nature, through the progressive science of man. The electron is as inexhaustible as the atom, nature is infinite, but it *exists* infinitely: and only this categorical, unconditional recognition of its existence beyond the consciousness and sensation of man, distinguishes dialectical materialism from relativist agnosticism and idealism.

In classical mechanics, matter is conceived as particles with independently assignable position and momentum. In electromagnetism, matter is pictured via the vectors **E** and **H** assigned to each point in space at each instant of time by a solution of the Maxwell equations. In quantum mechanics, matter is represented as having properties defined by the wave function, which assigns a complex number to each point in space at each instant of time.

## II. A Critique of Both Sets of Views

I stated above my conclusions about the positions taken by Marquit and Talkington in their debate about the nature of causality in quantum mechanics. On the basis of the facts outlined in the appendices I can now develop the arguments that support these conclusions.

Marquit and Hörz *et al.* say that causality in quantum mechanics must differ from causality in classical mechanics. This is correct because the way the state of a system of particles is specified in quantum mechanics is different from the way the state is specified in classical mechanics. In classical mechanics, a state is specified by giving the values of the position and momentum of each particle. In quantum mechanics, a state is specified by giving the value of the wave function for each particle. In both systems the principle of causality is expressed in the statement that the initial state, combined with the differential equations that describe how the state changes, determine the state at any later time. In classical mechanics, since a state is specified by the position and momentum of the particles, these are the quantities that figure in the principle of causality. In quantum mechanics, since a state is specified by the value of the wave function, it is this value that must figure in the principle of causality. When Marquit and Hörz *et al.* look for the principle of causality in probabilistic statements about position and momentum, they are looking for it in the wrong place for these reasons: a) The position and momentum of a particle do not define its state. The wave function does. b) The measures $\psi^2$ and $\phi^2$ which predict the probability density of the particle at a given position and momentum respectively contain less information than the wave functions $\psi$ or $\phi$. Only the latter have the most complete information about the behavior of the particle.

When Marquit and Hörz *et al.* reject the wave function $\psi$ as the determinant of causality in favor of $|\psi^2|$ they are insisting that causality must be expressed in terms of position and momentum, as it is in classical mechanics, as if the particle character of matter were somehow more real than its wave character. To this extent they are still prisoners of the concepts of classical mechanics, and their viewpoint is not free of the influence of mechanical materialism. By choosing the real number $|\psi^2|$ rather than the complex number $\psi$ for expressing the principle of causality they are victims of an outmoded prejudice that only real numbers can express real properties of the material world. But complex numbers are equally capable of expressing real properties of the material world, and in some situations, such as this, are the most appropriate numbers for that purpose.

When Talkington stresses the dynamical underpinning of the Schroedinger equation he is on firm ground because the potential term in the equation does take into account the forces acting on the particle. Moreover, the equation does deal with the state of a quantum-mechanical system, and does describe how the state changes with position and time. The combination of initial value of the wave function and the Schroedinger equation is fully deterministic and therefore embodies the principle of causality. However, Svechnikov, whose ideas are supported by Talkington, is on less firm ground when he makes a distinction between causality and the relation of states, and insists that "The cause is of a dynamical (force) character and is expressed in an action or an interaction of bodies." This viewpoint is defective on several grounds. a) The distinction between causality and a relation of states is a distinction without a difference. The principle of causality is expressed precisely in equations that link an initial state to future states. b) The insistence that causality must be expressed in terms of "forces" overlooks the fact that "The forces are merely shorthand expressions for the complex interactions between various wave-particle systems, which in modern physics are usually referred to as *fields*." [Merzbacher 1970, p. 31] Statements about forces can always be replaced by equivalent statements about potentials or acceleration. c) The insistence that causality must be expressed in terms of "an action or an interaction of bodies," like the Hörz *et al.* insistence that it must be expressed in terms of the position and momentum of particles, reveals a predisposition to think that only "bodies" are real, and, to that extent, represents a failure to break with mechanical materialism.

## Appendix A. Characteristics of Quantum Mechanics

1. Matter on the atomic and nuclear level is characterized by a wave/particle duality: An electron, for example, is detected as a particle with a definite charge, mass, and spin. On the other hand, interference phenomena in the diffraction of electrons show that the electron also has a wave character. Quantum-mechanical theory must take this wave/particle duality into account.



2. A single wave whose amplitude is assigned to infinitely many points in space does not assign a particle to any particular position in space. Localization of a particle can be produced by wave interference in two ways: a) superposition of many waves with different wave numbers to form a wave packet; b) by means of suitable boundary conditions, superposition of a wave on itself to produce a standing wave. In both cases, the particle is located in a finite region with a certain probability.

3. For a system that is in a bound state (spatially confined), measurable properties can take on only certain discrete values. For unbounded systems, possible values of measurable properties are discrete where the particles are standing waves, and are continuous where the particles are wave packets. The theory must account for the occurrence of discrete values and/or continuous values in the corresponding circumstances.

4. In the theory, the state of a wave/particle is defined by a wave function $\psi(r,t)$ which assigns a value of the function to each point in space for each instant of time. The function $\psi$ is a solution of the Schroedinger wave equation, a partial differential equation which specifies how the value of the wave function changes with time and with distance, and relates the corresponding rates of change to the potential energy of the particle derived from forces that are present. The Schroedinger equation has many possible solutions. Boundary conditions, which specify what the values of $\psi$ should be on some initial surface at some initial time $t_0$, select from among these many possible solutions the one for which $\psi(r,t)$ has the specified values on the boundary at the time $t_0$. The initial $\psi(r,t_0)$ at time $t_0$ then determines the state $\psi(r,t)$ at any other time $t$.

5. The statement that the initial state $\psi(r, t_0)$ at time $t_0$ determines the state $(r, t)$ at time $t$ "is the quantum-mechanical form of the principle of causality." (Merzbacher, p. 334)

6. If the causality principle is combined with the assumption that $\psi$ is the product of a time-dependent factor and a position-dependent factor, then it follows that the values of $\psi$ cannot be restricted to real numbers. Complex numbers must be used. There is an even deeper reason why complex numbers must be used, showing that they are essential for describing the properties of elementary particles: The operation known as *charge conjugation* which relates a particle to its anti-particle makes use of the complex conjugation operation which replaces the complex $a+bi$ by the complex number with reversed imaginary part, $a-bi$.

7. A complex number is a two-dimensional vector which may be pictured as an arrow in a plane. The wave function assigns an arrow (in a plane) to each point in space at each instant in time. This assignment is roughly analogous to the solution of the Maxwell equations in electromagnetic theory, which assigns an electrostatic vector **E** and a magnetic vector **H** to each point in space at each instant of time.

8. In physical optics, the intensity of interference fringes is given by $\mathbf{E}^2$ and $\mathbf{H}^2$. Analogously, in quantum mechanics, the intensity of what may be called *electronness* at a point is given by $|\psi|^2$, the square of the absolute value of $\psi$. When applied to a single particle, this measure of intensity can only mean the probability density of finding the particle at that point.

9. Observables (measurable physical properties) are represented by linear operators operating on the wave function. Because of the properties of partial differential equations, the equation for the state when an observable denoted by an operator $A$ is measured has only certain solutions called eigenfunctions corresponding to certain discrete values of $A$ called eigenvalues. The eigenvalues are all real numbers.

10. The behavior of a material particle is described completely by its wave function $\psi$.

11. Two observables can be sharply defined independently of each other if and only if their operators commute. The operators for position and momentum *do not commute*. Hence position and momentum cannot be sharply defined simultaneously. The sharpness of definition is governed by the Heisenberg uncertainty relation, $(\Delta x) \cdot (\Delta p) = \frac{1}{2} h/(2\pi)$. This relation, which can be derived rigorously, asserts that the more precisely we know the position of a particle, the less precisely we know its momentum, and vice versa.

12. The wave function $\psi\ (r, t)$ is defined in coordinate space. The behavior of a particle can also be described by means of another wave function $\phi\ (p, t)$ defined in momentum space. The two functions are related to each other reciprocally by Fourier integrals. Thus, on the level of phenomena described by quantum mechanics, the position of a particle and its momentum are not independent measures. *They are related to each other*.

13. The probability density that the momentum has the value p is given by $|\phi|^2$. Note the analogy to the probability density at a point in coordinate space. In both cases, a positive real number is obtained by squaring the absolute value of a complex number. This in effect throws away some of the information given by the complex number, since the complex number, as a vector, has *direction* as well as magnitude. Thus, measures of position and momentum contain less information than the wave function. That the information that is missing is essential information can be seen from the fact that addition of positive real numbers alone cannot account for interference phenomena, while addition of vectors can.

14. Two elementary particles of the same kind are indistinguishable. Interchanging them does not produce another state but the same state. In this respect the states described by a wave function that characterizes the simultaneous occurrence of two or more particles of the same kind are analogous to flag signals formed by arranging colored flags in a line. For example, a signal formed by two red flags and three black flags arranged in a line, with no other distinguishing mark on each flag except its color, remains the same if the two red flags are interchanged. Because of this property of being indistinguishable, particles of the same kind do not obey Boltzman statistics (the statistics used in the molecular theory of gases). Instead they obey Bose-Einstein statistics if they have integral spin, or Fermi statistics if they have half-integral spin. Hence the names *bosons* and *fermions*. The latter obey Pauli's exclusion principle. The indistinguishability of elementary particles of the same kind, and Pauli's exclusion principle, mean that they cannot be thought of as being independent of each other: the properties of one particle



**(e.g., momentum and angular momentum) cannot be arbitrarily assigned without regard to the properties of the other. The particles are local manifestations of a global phenomenon. In the case of electrons, they are like local bumps in the distribution of the quality we have called "electronness."**

15. If two protons in the singlet state are allowed to separate, and the same component of spin is subsequently measured on both particles, they will always be found to be in opposite directions, which may be designated as + and −. If this fact is combined with the assumption that each particle has a definite spin-component parallel or antiparallel to each of three orthogonal axes A, B, and C before it is measured, and that the negative correlation between the components of protons separated from the singlet state is maintained no matter how far apart they are, then a certain inequality known as the Bell inequality can be derived which would connect the numbers of protons from among many singlet pairs that would have given spin components in any two out of the three orthogonal directions with the numbers for the other possible choices of two out of three components. For example, $n(A^+B^+) \leq n(A^+C^+) + n(B^+C^+)$. However, the rules of quantum mechanics, constituting a different set of assumptions about elementary particles, predict that for some choices of the orthogonal axes $A$, $B$ and $C$ the Bell inequality would be violated. Most of the experiments performed with the spin of protons or with the analogous property of polarization of photons support the prediction of quantum mechanics. The results of these experiments can be understood if a pair of protons moving apart from the singlet state are regarded not as independent entities but "as the elements of a single physical system that . . . becomes progressively more extended in space," and more generally, "in some sense all these objects [particles or aggregates of particles] constitute an indivisible whole" [d'Espagnat]. The individual particles are best understood as local manifestations of a global phenomenon.

## Appendix B. Numbers, Reality and Intuition

From ancient times the concept of "number" has been embedded in humankind's perception of reality. But with evolving technology and economic life, and accompanying increased experience and deeper penetration into the nature of matter, three things have also evolved in tandem: a) the concept of "number" itself; b) our ability to grasp intuitively number relations originally formulated as abstractions; c) our understanding of the kinds of "number" structures that are appropriate for describing real properties of matter.

In a food-gathering economy, the concept of number was restricted at first to counting numbers. The development of agriculture and subsequent urbanization made it necessary for people to measure distances, lengths of cloth, volumes of stored seed, etc. Experience with measurement made it necessary to expand the concept of number to include fractions. The development of geometry led to the discovery that fractions do not suffice to give the length of every line segment: There is no fraction, for example, that can represent the length of the diagonal of a square whose side has unit length. This discovery gave birth to the concept of "incommensurable quantities" out of which emerged the concept of irrational number. Later experience with quantities that have both magnitude and direction (displacements, forces, temperature, etc.) made it necessary to expand the concept of number again by including negative numbers. The system of counting numbers, extended by appending zero and negative whole numbers, became the system of *integers*. The system of fractions, extended by appending zero and negative fractions, became the system of *rational numbers*, which includes the system of integers as a substructure. The system of possible lengths, rational and irrational, extended by appending zero and negative lengths (lengths measured in the opposite direction) became the system of *real numbers*, which includes the system of rational numbers as a substructure. The real number system can be pictured as the ordered set of points on a line extending indefinitely in both directions and with no gaps.

Meanwhile the development of algebra forced a new expansion of the concept of number. In order to be able to solve so simple an equation as $x^2 + 1 = 0$ it was necessary to invent the number i which is a square root of $-1$. Since no "real number" represented by a point on a line can be the square root of $-1$, this number, and the complex numbers $a + bi$ formed with its help were called "imaginary." Imaginary numbers were thought of at first as convenient tools having no physical counterpart. But it wasn't long before it was discovered that imaginary numbers do have a physical counterpart. A complex number can be pictured either as a point in a plane or as an arrow in a plane. Complex numbers are *two-dimensional vectors*. They also have the additional property that they can be multiplied and divided. This makes the complex number system a *field*. The development of the technology and theory of electricity and magnetism established a new connection between complex numbers and physical reality. The rotation of an armature in a dynamo can be represented by the rotation of a unit arrow, $e^{i\theta} = \cos\theta + i \sin\theta$, and the real and imaginary components of this arrow are needed in the description of wave motion. Thus, imaginary numbers are not "real" only in the technical sense of not being part of the "real number system." They are very real in the sense of having physical counterparts, so that they do correspond to real physical relations of certain aspects of matter.

During the nineteenth century the concept of number was expanded again when Hamilton discovered the *quaternions* $n = r + ai + bj + ck$. Quaternions, originally conceived as an abstract structure, are now known to have a multitude of physical applications. The best known of these are the use of the elements of the form $ai + bj + ck$ as three-dimensional vectors, and the use of the full four-term quaternions to represent the most general rigid motions in space.

There is an underlying reason why the real numbers, the complex numbers, and the quaternions play a special role in descriptions of physical reality. It has been proved that if a mathematical structure is a field (its elements can be added, subtracted, multiplied and divided), is a topological space (a continuum in which a concept of "neighborhood" is defined), is connected (all in one piece), and is locally compact (each point has a neighborhood such that every cover of it contains a finite subcover), then that structure is essentially either the real number, complex number, or quaternion system.



The evolution of our concept of number to more and more general forms, and the discovery of physical counterparts of these forms has been accompanied by an evolution of our ability to think intuitively about these newer forms. Our number intuitions are derived from two sources, our hereditary brain structure developed by evolution, and our cumulative experience, passed on from generation to generation through both informal and formal modes of instruction, and added to by each generation. Whereas in earlier times people could grasp intuitively number relations involving only counting numbers or only real numbers, those experienced with complex numbers have no difficulty working with them intuitively. There is no mystery involved in what is meant by multiplying by a complex number once it is understood that it is equivalent to a rotation of a vector and a stretch or contraction of the vector (De Moivre's Theorem). The fact that the intuitive use of complex numbers is a recent acquisition of the human mind should not stand in the way of their use in the construction of theoretical models of physical reality. The principal criterion that is relevant in choosing the numbers and number relations to be used in a model is their appropriateness for representing the physical properties being modeled.

## Appendix C. Relations of Dependence in Physics

One of the characteristics of the growth of the science of physics has been the discovery that some properties of matter formerly thought to be independent are actually dependent on each other. Another characteristic has been an increasing awareness of the fact that every observation is a relationship between the observer and the observed and that therefore there is a dependence of the observation on the frame of reference of the observer and the activity of the observer. In many cases these two characteristics are linked to each other.

Before the development of Newtonian mechanics it was thought that the weight of a body was an invariant property of the body independent of its position. Classical mechanics revealed that the weight of a body and its position are linked, because the weight depends on the distance of the body from the center of the earth. The center of the earth enters into the concept of weight because we, the observers, live on the earth. If we were living on Mars, the distance from the center of Mars would be the relevant parameter of position. In Newtonian mechanics, the concept of invariant weight was replaced by the concept of invariant mass, and, in a closed system, it was assumed that the total mass and total energy of the system were independent constants, representing separarate laws of conservation: conservation of mass, and conservation of energy. But with the insights derived from relativity theory, so dramatically confirmed by the atomic bomb, we now know that the mass and the energy are not independent quantities, but are, in fact, convertible into each other, so that the two separate laws of conservation have to be replaced by one law: conservation of mass/energy.

Before the development of relativity theory, it was assumed that if any event $E_1$ occurs at position $(x_1, y_1, z_1)$ and time $t_1$, and an event $E_2$ occurs at position $(x_2, y_2, z_2)$ at time $t_2$, that the distance between the events, $[(x_2 - x_1)^2 + (y_2 - y_1)^2 - (z_2 - z_1)^2]^{1/2}$, and the time interval between the events, $t_2$-$t_1$, are invariant properties that are independent of each other. Now we know from relativity theory that these two properties are actually related to each other and are related to the frame of reference of the observer. What is assumed to be invariant in relativity theory is the interval between the events, $[c^2(t_2 - t_1)^2 - (x_2 - x_1)^2 - (y_2 - y_1)^2 - (z_2 - z_1)^2]^{1/2}$. The distance between the events and the time interval between the events are the space and time components respectively of one quantity, and they vary with the speed of the observer.

Quantum mechanics has also uncovered relations of dependence between properties formerly thought to be independent of each other. The position and momentum of an elementary particle, as pointed out in Appendix A, 11 of this paper, are related by the Heisenberg uncertainty relation, and, as pointed out in Appendix A, 12, are reciprocally related by Fourier integrals. Energy and time are also related by a Heisenberg uncertainty relation.

## ERWIN MARQUIT COMMENTS:

Adler focuses on a number of very important points. For many readers, his discussion should help demystify the role of imaginary numbers, and consequently of complex variables, in the description of the physical world. His discussion of this should not be misread as stating that complex variables are themselves a form of matter. It is important to note that he gave the example of the application of the techniques of complex algebra for the representation of alternating currents. He points out further that complex numbers are two-dimensional vectors. In fact, we can generalize this to state that every representation of the physical world which is based on the use of complex variables can be replaced by a representation based on two-component real variables, though not always without mathematical inconvenience. And therefore Adler's statement that complex numbers *must* be used in quantum mechanics is not strictly true, though there is no question that they are a convenient mathematical device for the representation of processes in quantum physics. It can also be pointed out that the conceptual difficulty is not with the use of imaginary numbers for the representation of physical variables, but with the need to represent physical variables in the microworld in two-component form, where the two components are not of a geometrical character.

In classical physics we do have three-component fields, each component of which can vary in space and time (for example, as functions of x, y, x, and t). Such fields are vector fields, each component having the spatial directions of the three coordinate axes. In quantum mechanics, the two components can also be expressed as functions of space and time, but the components are not vector directions in geometrical space.

The reason for dwelling on this point is to stress the fact, as Adler does, that the functional description we use in quantum mechanics is not directly observable phenomenologically, but not simply because



imaginary numbers are involved. We can make a sort of analogy with Marx's analysis of exchange value, a value that does not manifest itself phenomenologically—it is price that is observed in the market place. Adler seems to give the wave function the role of *essence*, just as Marx gives an essential nature to exchange value; then, as implied in Adler's arguments, the square of the wave function interpreted as a probability density would be the phenomenal component, just as price would be the phenomenal expression of exchange value. The analogy, it is true, was not made by Adler explicitly; it will turn out that this approach has some shortcomings.

Adler's tendency to embrace causality one-sidedly leads to oversimplification of the views of Hörz and co-authors as well as of my own views. For example, Adler writes that "Marquit and Hörz *et al.* look for the principle of causality in probabilistic statements about position and momentum."

Let us put the comments of Hörz and co-authors [1980b,p.85] in their more complete context:

> Concepts such as law, causality, and structure embrace certain specific forms of interaction. Engels points out in a number of places that the principle of causality can only be understood as a general interaction.

Further:

> The category causality contains the direct influence of one phenomenon of the objective world on another phenomenon, the conditioning of one phenomenon (effect) on another (cause) and its unity. [p.103]

For the domain of quantum physics, they write:

> The form of causality characteristic for the microworld can be defined as follows: Cause, as the real phenomenon which appears with the probability $p_a$, gives rise to and conditions another real phenomenon, effect, with the probability $p_b$. [p.112]

The whole stress of Hörz *et al.* here is that causality is associated with interaction and the connection among phenomena that exists before, and occur as a result of, the interaction. Adler appears to confuse causality with law. He identifies the solution of the Schroedinger equation with causality and does not see the Schroedinger equation as a law of quantum mechanics in its distinctness from a causal principle. According to Hörz *et al.*,

> While causality can be understood only as a moment (essential aspect) of interaction, and in this sense represents the simplest form of connection, the concept of law represents complex and complicated forms of connection, which, in turn, presupposes the causality principle. [p.104]

Nor is it true that the viewpoint of Hörz and co-authors and myself ignore the dynamic underpinnings of the Schroedinger equation. In a paper entitled "Statistical Processes and Causality," I wrote:

> The fact is that the Schroedinger equation is just as mechanistic as Newton's equation. Both describe changes as due to external actions. The cause-effect bond, however, is unique in one case and statistical in the other. . . It is the inner nature of matter that gives rise to the difference, not the external forces. [Marquit 1977.]

Adler's error is to set the wave function obtained from the solution of the Schroedinger equation against the probabilistic meaning given to the square of its amplitude. In this way Adler introduces an artificial separation between essence and phenomena. When we solve the Schroedinger equation for the case of a particle in specific physical situations, for example, in a given potential field, we have to impose what are called boundary conditions to determine constants resulting from the general solution of a differential equation. These boundary conditions are known phenomenologically or are assumed theoretically on the basis of knowledge of the concrete physical situation. Adler explains that the square of the absolute value of the wave function "can only mean the probability density of finding the particle at that point." We ask, however, what does "finding" mean? It cannot mean anything else than the probability density for an interaction, as a phenomenon, occurring at that point. From that phenomenon, actual or potential, we fix the wave function. It can even be stated that the set of all probability densities associated with a given physical system, if known as phenomena, give us more information than the solution of the Schroedinger equation, since from the former the wave function can be more completely reconstructed, while the converse is not true if we keep in mind the need to fix the constants through the boundary conditions.

On the level of quantum mechanics, the wave function does give an adequate representation of the physical reality, but it cannot be obtained without coupling the solution of the Schroedinger equation to the concrete physical process it describes. This coupling cannot be effected without the inclusion of the probabilistic character of the square of the wave function amplitude. The range of energy levels for the hydrogen atom, for example, is obtainable from the solution of the Schroedinger equation, but the process of transitions from one level to another, which is described by means of the wave function, requires the probabilistic aspect to come to the fore.

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**DISCUSSION**. I disagree with Marquit's formulation that "every representation of the physical world which is based on the use of complex variables can be replaced by a representation based on two-component real variables." Complex numbers and functions are not only a mathematical convenience but plain necessity. The most explicit formulation of a quantum mechanical system is by representation of states by vectors in a complex infinite-dimensional Hilbert space. Restriction to real numbers would eliminate real eigenfunctions of Hermitian operators; hence, a precise value that can be observed would be eliminated (rather than an expected value).

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While it is true, as Marquit says, that complex numbers can be constructed as ordered pairs of real numbers, the complex numbers nevertheless constitute a higher-level number system with structure and properties qualitatively different from those of the real number system. To visualize the difference



between the two systems, think of real numbers as points along a line, complex numbers as points lying on a plane. The calculations of quantum mechanics require the use of complex numbers, however they are constructed. The real number system alone is not sufficient for this purpose since it constitutes only a subset of the complex number system.

*Irving Adler*

## LESTER TALKINGTON COMMENTS:

Irving Adler's contribution to our discussion is valuable because it reaffirms the dynamic role of causality within the very formalism of quantum mechanics itself, showing that the Schroedinger equation and the wave function of a particle are formulated in terms of classical dynamics (with the potential treated as an alternative expression of dynamic force). Adler's arguments are especially welcome because of their *physical* character and their sound basis in philosophical materialism.

There is a question, however, whether Adler's position will be fully persuasive to those who have been influenced by the concept of "statistical causality" which is at issue here. This concept is congruent with the Copenhagen interpretation which, over the past half century, has been shown to be logically (and ideologically) a complete and closed system, quite impervious to any formal criticism on its own terms. It seems that Adler's position would be stronger if he had also addressed the larger question of whether microphysics is subject to further dialectical development through the inner contradictions of quantum mechanics, a process in which the present formalism may well be transformed drastically or even eliminated.

If Adler had taken this further step, examining microphysics from outside the formalism, he might have reached quite a different conclusion concerning the Svechnikov proposal for a research program which treats an experimental apparatus (such as the twin slits of the usual "interference" demonstration) as an assembly of microparticles interacting with the experimental microparticles. The empirical justification for such an investigation can be found in the many experiments demonstrating that, for a given macroscopic configuration, the statistical distribution of microparticles remains unchanged if the rate of flow of these particles is reduced to the point where physical interaction ("interference") between such experimental particles is no longer possible. One obvious interpretation of such experimental results is that the statistical distribution of microparticles depends primarily on their interaction with the macroscopic apparatus, and does not arise from some innate stochastic motion of individual particles as postulated by many physicists.

Such an investigative approach is, of course, not conceivable within the formalism of quantum mechanics nor can it be justified in any way on the basis of the "statistical causality" concept. There is, however, ample philosophical justification for undertaking such an investigation if one sees quantum mechanics as a temporary stage of a microphysics which is *mutable* in the Leninist historical sense, i.e., subject to further change and development. The *mutability* of all scientific knowledge was discussed by Lenin in the passage quoted by Adler (above) in relation to the materialist basis for preserving dynamic causality in microphysics. Thus, the passage provides an excellent illustration of how the dialectics of change and development go hand in hand with scientific materialism in the Marxist approach to philosophical problems.

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## DAVID SCHWARTZMANN COMMENTS:

I found Adler's paper provocative and clarifying on a number of important issues. However, where it mentions Bell's Theorem and quotes Bernard d'Espagnat (Appendix A, point 15), the paper glosses over a crucial question: whether the microworld, the object of quantum mechanics, exists with objective properties, independent of human consciousness. The orthodox view, following Bohr, emphasizes the necessary unity of the observer and the observed, with the experimental arrangement determining the elements of reality of a given system. Some go even further. Among these is d'Espagnat [1979]. The subtitle of his paper, not quoted by Adler, says: "The doctrine that the world is made up of objects whose existence is independent of human consciousness turns out to be in conflict with quantum mechanics and with facts established by experiment."

According to d'Espagnat, if the apparent experimental violation of Bell's Theorem holds up [cf. Robinson 1982] and if quantum mechanics is an adequate and complete theory of the microworld, then one of two assumptions must be discarded: either give up realism (materialism), or else give up the assumption of superluminal transfer of information. (The current debate on Bell's Theorem and its implications is, in some essential aspects, a replay of the classic debate between Eintein and Bohr over the "EPR paradox", so-called from the 1935 paper by Einstein, Podolsky and Rosen—though there is an important difference in that Bell's Theorem permits actual experimental tests, including the possibility of superluminal transfer of information, while the Bohr-Einstein controversy was fought over thought experiments.)

Adler's paper, otherwise fine, fails to bring out the profoundly idealist position taken by d'Espagnat or the implications of the current debate over Bell's Theorem. This is a significant omission because several popular books continue the same line of idealist argument. For example, Paul Davies, a distinguished theoretical physicist, wrote that experimental testing of Bell's Theorem "deals a decisive blow to theories based on the concept of an independent reality." And Heinz Pagels [1982], who is currently president of the



New York Academy of the Sciences, goes almost as far: "The world just isn't 'there' independent of our observing it; what is 'there' depends in part on what we choose to see—reality is partially created by the observer." Pagels, providing a very clear description of the experimental test, refers to the "quantum weirdness" in the "non-objectivity" of the quantum properties of individual photons—taking the position that objectivity and locality are complementary concepts, in the sense that the assumption of a definite state for photons implies their nonlocality while the converse assumption of local causality implies giving up the objectivity of individual photons (even if a pair of correlated photons are light years apart!).

Another suggestion, perhaps not quite as unpalatable, was made by Pitowsky [1982]: probability theory must be revised for the microworld, analogous to Einstein's use of Riemann's geometry. This sounds similar to the proposals for a quantum logic, specially designed for the microworld; Bunge [1967] has pointed out that, as a theory of the microworld, such a quantum logic is full of inconsistencies because it must use ordinary logic as well.

d'Espagnat's idealism was the target of a critique by Victor Weisskopf [1980] who pointed out the obvious: "Quantum mechanics deals with the . . . processes in the interior of distant starts or with the nature of rocks that existed before humankind evolved". Since radioactive decay is clearly a process of the microworld and so are nuclear reactions within stars, how could those damned electrons ignore the fact that no human consciousness was around while they were doing their thing? Unfortunately, this argument still leaves an opening for objective idealists of the Hegelian type: Consciousness, infinite in time and space, pervades the Universe acting as the Eternal observer. (This, however, is not a scientific argument!)

Still another possibility, of course, is that quantum mechanics is not a complete theory but a limiting case of a larger theory (*deja vu*?). To recognize the objectivity of human consciousness, its historical development and its nonexistence for almost all of the last 15 billion years, is basic to scientific knowledge and materialist philosophy. It demands that any scientific theory of the world should be observer-free in the sense of the objective existence of the object/process outside of our consciousness. Bunge [1967] has attempted to develop such a version of quantum mechanics. If the experimental test of Bell's Theorem is really in conflict with a materialist version of quantum mechanics such as Bunge's, then one must conclude as a materialist that quantum mechanics has reached its limits and must be superseded. From the conclusions of Rosen [1979, see Bibliographic Briefs, this issue], it appears that hidden variable theories are also ruled out, and any new theory will involve revolutionary concepts of space and time. This subject deserves much closer attention from dialectical materialists. The experimental test of Bell's Theorem may prove as crucial to physics as did the Michelson-Morley test, but the results need careful scientific and philosophical scrutiny. In any event, to use Mark Twain's euphemism, the purported experimental refutation of materialism is premature.

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**DISCUSSION.** I am glad that Schwartzman has commented on the subtitle of d'Espagnat [1979] which says in effect that realism (materialism) must be rejected. This statement is nowhere supported in the body of d'Espagnat's article. On the contrary, within the text he says: "When all the consequences of abandoning realism are considered, however, it is too great a renunciation to have much appeal . . . If this refusal to seek underlying causes of observed regularities is applied consistently, it trivializes the entire scientific enterprise . . . Given the extreme consequences of abolishing realism, one is inclined to cling to this first premise" [p.177]. He then concludes that the experimental violation of the Bell inequality implies a violation of Einstein separability, and adds: "The violation of separability seems to imply that in some sense all these objects constitute an indivisible whole" [p.181]. This might make it seem that d'Espagnat was not responsible for the subtitle and does not agree with its anti-realist position. However, in his reply to the comments by Weisskopf [1980], d'Espagnat makes it clear that he does agree with it. Evidently, consistency is not one of his strong points!

*Irving Adler*

Wonder what the editors of *Scientific American* have to say about such an inconsistency between subtitle and concluding paragraphs of an article!

*Editor, Science & Nature*

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***A Byte of Logic***

**Edward O. Wilson, dean of sociobiology, says that the loss of genetic and species diversity by destruction of natural habitats would be a much worse catastrophe than limited nuclear war. [*Sci. Amer.* July 1982, p. 121.]**





# THE NATURE OF HUMAN KNOWLEDGE*

## In Marxist Theory, Knowledge Reflects Experience

What is knowledge anyway? What does it mean to humankind? Are there laws governing the development of knowledge? Is objective truth attainable, and, if so, how? By what process? What are the criteria for true knowledge? Philosophical questions such as these are dealt with in the theory of knowledge, or *epistemology* (from the Greek words *episteme* for knowledge and *logos* for theory or doctrine).

Such questions arose with the very beginning of philosophy In Greek philosophy, analysis of the nature of knowledge began with Democritus, Plato, Aristotle, the skeptics and the stoics. Contributions to the theory have been made by Bacon, Descartes, Locke, Spinoza and many others in the modern era.

The problem of knowledge occupies a central place in Marxist-Leninist philosophy. Th.e basic assumptions of the dialetical-materialist theory of knowledge were formulated by Lenin in his *Materialism and Empirio-Criticism*:

> (1) Things exist independently of our consciousness, independently of our sensations, outside of us. . . .
> (2) There is definitely no difference in principle between the phenomenon and the thing-in-itself, and there cannot be any such difference. The only difference is between what is known and what is not yet known. . . .
> (3) In the theory of knowledge as in every other sphere of science, we must think dialetically, that it, we must not regard our knowledge as ready-made and unalterable, but must determine how *knowledge* emerges from *ignorance,* how incomplete, inexact knowledge becomes more complete and more exact.[5]

The theory of knowledge owes to Marxism two things that have changed it fundamentally: (1) the extension of materialist dialectics to the sphere of knowledge; (2) introduction into the theory of knowledge of practice as the basis and criterion of true knowledge. Materialist dialectics has put an end to the isolation and separation of the laws of the objective world from the laws of thought, because it is the science of the most general laws of motion both of the external world and of human thought. There are, as Engels writes, ". . . two sets of laws which are identical in substance, but differ in their expression in so far as the human mind can apply them consciously, while in nature and also up to now for the most part in

* Adapted from *Fundamentals of Marxist-Leninist Philosophy* [Moscow 1974]. An appendix herewith, discussing agnosticism and subjective versus objective dialectics, previously preceded this essay.

human history, these laws assert themselves unconsciously. . . .

Knowledge and the laws of its motion (subjective dialectics) are thus the reflection in the thinking brain of the laws and properties of objective reality itself. The content of our knowledge is brought into harmony with the objects and processes existing outside it in the process of cognition, which is predicated on man's material practical activity, his practical efforts to master the phenomena and processes of nature.

### Subject and object

Knowledge does not exist in a person's brain from the outset, it is acquired in the course of his life, as a result of his practical activity. *The process of acquiring new knowledge is called cognition.*

In order to understand the essence, the laws of cognition one must decide who is its subject, that is, who is the knower of objective reality. This would seem to be no great problem; naturally the subject of cognition is man. But, in the first place, the history of philosophy tells us that there have been thinkers who believe that it is fundamentally impossible for man to know the essence of things. *Secondly, there is today a widespread opinion that cognition, and such a form of it as theoretical thinking, can be done not only by people but by the machines they build, such as computers.* And finally, it is not enough merely to assert that man is the subject of cognition; one must find out what makes him the subject, and for this purpose one must know his essence.

Ludwig Feuerbach criticised the idealist concept according to which the subject of cognition is consciousness, correctly noting that consciousness itself, being a property of man, is predicative. Man for Feuerbach was a physical, corporeal being, living in space and time and possessing by virtue of his material nature the ability to know reality. It would seem that Feuerbach in his concept of cognition had in mind a concrete person possessing natural essence, but, as Marx observed, Feuerbach "never arrives at the really existing active men, but stops at the abstraction 'man' and gets no further than recognising 'the true individual corporeal man' emotionally. . . ."[7]

How does man acquire his concrete, real essence? Man possesses the inherent properties of an immediate natural being including sensory perception, but he creates his second, social nature—culture, civilisation. By means of labour he creates himself, not simply assimilating the objects of nature, but changing them in accordance with his needs. Man can do this only because he is a social being, in definite relations with his own kind. " . . . Man," wrote Marx, "is not an abstract creature inhabiting some extra-mundane sphere. Man is the world of man, the state, society."[8]

Outside society there is no man, and consequently, no subject of cognition either. But the reader is quite entitled to ask, surely it is not all mankind, society as a whole, that gets to know things, but separate individuals. Of course, society cannot exist without individuals, who think, produce, possess their own features and abilities But these individuals can be the subjects of cognition only thanks to the fact that enter into certain social relations with one another and acquire the instruments and means of production accessible to them at a given level of social organisation.

The level of knowledge is not determined solely by people's natural and individual features; the main factor is social conditions and possibilities. No matter how splendid a genius Newton was, he could not have created the theory of relativity. Objective idealism, having shown that consciousness in society does not depend on any one concrete individual, made a mystery of this fact by taking the total result of people's activity as registered in the forms of consciousness and presenting it in the form of an independent essence moving according to its own logic. Thought was thus divorced not only from its specific vehicle—the subject—but also from the object—the things and phenomena outside the subject.

The process of cognition, however, needs not only a subject, but also an object with which the subject (man) can interact. The phenomena and processes of objective reality exist independently of consciousness. Man himself, the subject of consciousness, can be judged by what becomes the object of his cognition and practice. For example, in the time of Democritus and Aristotle, and even in the time of Galileo and Newton, the electron, although it existed in reality, did not come within the range of human knowledge. Man was not capable of discovering it and making it the object of his thoughts and actions. Only by knowing the level of development of social practice can we infer what object of nature will become an object of human cognition, that is, become an element of social life. For example, social practice is now at such a level that the exploration of the space surrounding our planet, and exploration of other planets of the solar system, is gradually entering the sphere of human activity. Man lives in a more or less humanised natural environment. He is forever bringing new phenomena of nature into the orbit of his being, turning them into the objects of his activity. In this way the human world is made wider and deeper. Criticising Feuerbach's concept of reality, Marx writes: "He does not see how the sensuous world around him is not a thing given direct from all eternity, remaining ever the same, but the product of industry and of the state of society. . . . The cherry-tree, like almost all fruit-trees, was, as is well known, only a few centuries ago transplanted by *commerce* into our zone, and therefore only *by* this action of a definite society in a definite age it has become 'sensuous certainty' for Feuerbach."[9]

All this goes to show that both the subject and the object upon which the subject acts acquire a social character and depend on human practical activity. This activity creates culture, an element of which is knowledge.

## Practice. The social and historical nature of knowledge

The indispensable condition on which knowledge depends is the influence that the objects of nature and social processes exert upon man, but man can only develop his knowledge by acting, intervening in objective phenomena and transforming them while experiencing their influence. We can understand the essence of human cognition only by deducing it from the peculiarities of this practical interaction of subject and object.

Mankind and nature are two systems of different quality, but they are both material. Man is a social and objective being and acts in an objective way. His possession of consciousness and will exerts a substantial influence on his interaction with nature, but this interaction does not thereby lose its material essence. Man acts with all the means at his disposal, natural and artificial, on the phenomena and things of nature, transforming them and at the same time transforming himself. *This objective material activity of man is known as practice.*

The concept of practice is fundamental not only to the theory of knowledge of Marxism-Leninism, but also to Marxist-Leninist philosophy as a whole. Practice cannot be confined entirely to the sphere of production. If it is, man becomes merely an economic being, satisfying by means of labour his needs for food, clothing, habitation and so on, and his consciousness becomes purely technical in character. Economic production has its place in practice, as its basis, but human practical activity cannot be reduced merely to the practice of production. Practice, in the broadest sense, includes the totality of objective forms of man's activity; it embraces all aspects of his social being, in the process of which his material and spiritual culture is created, including such social phenomena as the class struggle, and the development of art and science.

In his production labour activity man treats nature not as an animal does, obtaining only what it and its offspring immediately require; man is a universal being, he creates things that do not exist in nature, he creates on his own scale and by his own yardstick according to constantly emerging and developing aims. Such activity is impossible without consciousness.

All forms of man's objective activity are built on the foundation of labour and production, and it is these forms that engender such a phenomenon as knowledge of things, processes, and the laws of objective reality. Initially, knowledge was not separated from material production: the one was part of the other. As civilisation developed, however, the production of ideas broke away from the production of things, and the process of cognition became an independent, theoretical activity of man with its own subject-matter and specific features. This subsequently gave rise to the opposition between theory and practice, which is in fact only relative in character.



If we set out to explain the interrelation between theoretical activity and practice, we shall see the dependence of theory on practice and at the same time its relative independence. For the theory of knowledge both are important. *The dependence of knowledge on practice explains to us the social and historical nature of knowledge.* All aspects of cognition are connected and determined by society. The subject of cognition is man in his social essence, the object is a natural object or social phenomenon selected by man and included in the sphere of his activity; practice is the objective, material activity in which man, far from losing his essence, acquires it, thus creating himself and his history.

From nature man has inherited certain biological factors on which the functioning of consciousness depends; these are the brain and a fairly well developed nervous system. But man's natural organs have changed their purposes and function in the process of social development. "Thus the hand," wrote Engels, "is not only the organ of labour, *is is also the product of labour*,"[10] It is thanks to social activity that the sensory organs, the brain and hands, have acquired the ability to create such marvels as the pictures and statues of the great artists, the compositions of brilliant musicians, the masterpieces of literature, science and philosophy.

It follows from the social nature of knowledge that the development of knowledge is caused by the changes in man's objective activity, in his social needs, which determine the aim of knowledge, its target, and stimulate people to strive for an ever deeper theoretical mastery of knowledge.

The relative independence of knowledge allows it to go a little further than the immediate demands of practice, to anticipate practice, to foresee new phenomena and actively influence production and other spheres of human life. For example, the theory of the complex structure of the atom arose before society had consciously set itself the goal of making practical use of atomic energy.

Knowledge can move ahead of practice because it has its own laws that differ from the laws of development of production. The connection between knowledge and the practical tasks that the individual and mankind as a whole set themselves is often of a complex and indirect nature. For example, the results of contemporary mathematical research are mainly applied in other branches of science, such as physics and chemistry, and only afterwards in engineering and the technology of production.

Of course, there is always the possibility of theoretical activity becoming divorced from practice. In the field of cognition this may lead to its becoming a closed-circuit system without any outlet in human practice. Knowledge may then lose its connection with its target and thus be deprived of its main function, that of enriching people with new knowledge, helping them to master objective processes and place them at the service of man. The systematic application of knowledge to practice is, therefore, the guarantee of its objectivity, of its deeper penetration into the essence of the things and processes of objective reality.

**The concept of reflection in Marxist theory**

The result of the process of cognition is knowledge. The concept of knowledge is extremely complex and full of implications. Many epistemologists have concentrated on one or another aspect of knowledge and presented this aspect as expressing the whole nature of knowledge. This one-sidedness has led to exclusion of major factors comprising the very essence of knowledge, with the result that some concepts of knowledge are incomplete and even misleading.

The first definition of knowledge establishes its place in the process of social life. In knowledge man masters an object theoretically, transforms it to the plane of the ideal. Knowledge is ideal in relation to the object outside it. It is not the thing that is known, the phenomenon or property that is cognised, but a form of assimilation of reality, man's ability to reproduce things and processes in his thoughts, aims and desires, to operate with their image and concepts.

This means that knowledge, since it is ideal, exists not in the form of sensuously material things or their material copies, but as something opposite to the material, as a feature or aspect of the objective interaction of subject and object, as a form of man's activity. As something ideal, knowledge is nevertheless interwoven with the material in the motion of the nervous system, in the signs created by man (words, mathematical and other symbols, etc.).

This is what gives rise to the ideas that express man's spiritual mastery of objects, the images, the yardsticks that he evolves for things and processes which exist or may exist.

If we say that the specific nature of knowledge lies in the grouping of ideas, we must also pose the question of their content, their relationship to objective reality. The dialectical-materialist solution to this problem was formulated by Marx in the following general terms: "... The ideal is nothing else than the material world reflected by the human mind, and translated into forms of thought."[11]

*The relationship between knowledge and objective reality is expressed in the concept of reflection.* This concept was proposed by philosophy in ancient times. The modern materialists made wide use of it, and in a number of cases gave the process of reflection a mechanistic colouring; reflection was regarded as the influence of objects on man, whose sense organs registered their imprint, their form, like wax.

Although reflection is not a concept peculiar to the Marxist-Leninist theory of knowledge alone, it has gained its place there, been rethought and acquired new content. Why is such a concept needed? When discussing the content and source of knowledge, the form in which it is connected with objective reality, we cannot uphold the



positions of materialism without understanding knowledge as the reflection of the things, properties and laws of objective reality.

Materialism in the theory of knowledge proceeds from recognition of the existence of an objective reality independent of man's consciousness, and of the knowability of that reality. Recognition of objective reality, which forms part of the content of knowledge, is directly connected with the concept of reflection. Knowledge reflects the object; this means that the subject creates forms of thought that are ultimately determined by the nature, properties and laws of the given object, that is to say, the content of knowledge is objective.

The idealist theory of knowledge avoids the concept of reflection and attempts to substitute for it such terms as "correspondence", presenting knowledge not as the image of objective reality but as a sign or symbol replacing it. Lenin firmly protested against this because "signs or symbols may quite possibly indicate imaginary objects, and everybody is familiar with instances of *such* signs or symbols".[12] The idealists themselves, such as Ernst Cassirer, the neo-Kantian, make no secret of the reasons for their dislike of the concept of reflection. Defending the concept of knowledge as a symbol in relation to the object, he wrote: "Our sensations and ideas are symbols, and not *reflections* of objects. From an image we demand a certain *likeness* to the reflected object, but we can never be sure here of this likeness".[13]

The idea of knowledge as reflection is today opposed by the philosophers of various schools, and also by philosophising revisionists. The latter reject reflection as allegedly a concept of metaphysical materialism incompatible with Marxist philosophy, which proceeds from recognition of the activeness of the subject in the process of the practical and theoretical mastering of the object. The theory of reflection is thus presented by these philosophers as the basis of dogmatism.

Of course, reflection, seen as the lifeless copying of existing things and processes and considered apart from the subjective, actively creative influence of man, cannot serve as a characteristic of knowledge. The very meaning of human life lies in free creative activity, in the practical transformation of the world, and knowledge serves the aims and tasks of this activity. But knowledge can be the instrument of transformation of the world only when it is the objective and active, practically oriented reflection of reality. Knowledge is the mastering of objectively existing reality, it has reality as its content, that is, it reflects the properties and laws of phenomena and processes existing outside it.

Thus the dialectical-materialist theory of knowledge reveals the nature of knowledge, substantiating it with the principle of reflection; it endows the concept of reflection with new content, including in it the sensuously practical, creative activity of man. Knowledge is the coincident reflection of reality, tested by social practice.

**Language as the form of existence of knowledge.**

Knowledge is ideal but in order to exist in reality it must have a sensuous, material form. Man as an objective being acts only objectively, and his knowledge also exists in objective form. One may operate with knowledge only in so far as it takes the form of *language*, a system of sensorily perceptible objects-signs.

This organic link between knowledge and its existence in the form of language was noted by Marx: "From the start the 'spirit' is afflicted with the curse of being 'burdened' with matter, which here makes its appearance in the form of agitated layers of air, sounds, in short, of language. Language is as old as consciousness, language *is* practical consciousness that exists also for other men, and for that reason alone it really exists for me personally as well. . . ."[14]

On the surface, knowledge takes the form of a system of signs denoting an object, event, action, etc. That which the sign denotes is its meaning. Sign and meaning are indivisible; there can be no sign without meaning and vice versa.

A distinction must be made between linguistic and non-linguistic signs, the latter including signals, markings, and so on. Knowledge exists in linguistic signs, whose meaning is contained in cognitive images of the various phenomena and processes of objective reality. (Modern formal logic makes a distinction between "meaning in extension" and "meaning in intension". The former is the class of objects denoted by a certain word, the latter its logical connotation. For example, the "meaning in extension" of the word "whale" is all the whales that ever were, are or will be; its "meaning in intension" is a mammal inhabiting the ocean, etc. Here the term "meaning" is used in the broad sense, both extensionally and intensionally.)

There is no intrinsically necessary, organic link between the sensorily perceived object acting as a sign and its meaning. The same meaning may be attached to different objects performing the function of a sign. Moreover, artificial formations created for a special purpose—symbols—may also act as signs.

The development of knowledge has brought into being a highly ramified system of artificial, symbolic languages (for example, the symbol language of mathematics, chemistry, and so on). These languages are closely connected with the natural languages, but are relatively independent systems of signs. Science more and more often resorts to the use of of symbols as a means of expressing the results of cognition. The words of the natural language are not always suitable for expressing scientific concepts because they have their own specific sensuous meaning, connected with everyday usage. Symbols are used to provide the close definition that is essential to strict and unambiguous thinking.

Knowledge as a linguistic system forms a world of its own, with its own specific structure in which separate elements are connected together according to certain rules. This system has its laws of structure and functioning, is constantly being enriched with new elements, changing its structure, and so on. Moreover, the laws of the functioning of the system are relatively independent and not directly connected with the things and processes of objective reality and their reflections in the mind of man.



**Symbol and meaning**

Symbolism is widely used by certain philosophical schools to defend idealistic notions. Indeed, if knowledge exists in the form of systems of signs, and these signs are more and more often replaced in modern science by symbols, does this not confirm the idea that knowledge is a symbol and not the reflection of reality?

Contemporary positivists constantly stress the idea that the adoption of artificial language by science has entailed a loss of objectivity in knowledge. "The new physics," writes Philipp Frank, "does not teach us anything about 'matter' and 'spirit', but much about semantics. We learn that the language by which the 'man in the street' describes his daily experience is not fit to formulate the general laws of physics."[15] Yes, of course, physics has its own language which is unlike any natural national language, but it creates such a language not in order to move away from the processes it studies, but to investigate them more deeply and thoroughly.

Knowledge is becoming increasingly symbolical in its form of expression, and scientific theory often appears in the form of a system of symbols, but the importance of these symbols and equations is that they give a more accurate and profound reflection of objective reality. It is not the symbols themselves that are the result of knowledge, but their ideal meaning whose content is the things, processes, properties and laws studied by the given science. The symbols in Einstein's formula $E = mc^2$ are not knowledge; real knowledge is the meanings of the symbols that comprise this formula and the relationship between them (expressing one of the laws of physics—the connection between energy and mass).

Admittedly, it is not always easy to decide the meaning, that is, the class of objects, to which certain symbols and theories as a whole refer. The time has passed when all knowledge was, in effect, self-evident and a definite sensuous image or object could be perceived in every concept. It is no accident therefore that we are now urgently confronted with the problem of interpretation, the elucidation of the theories expressed by a more or less formalised symbolic language.

The very term "interpretation" has acquired a non-traditional meaning. It now signifies not only scientific explanation, implying a search for the laws and causes of phenomena (science has never relinquished that task and it is still the most important element of scientific research), but also the logical operation of defining the cognitive significance of abstract, symbolic systems and the content both of the individual terms (symbols) and statements (expressions), and of theory a a whole.

The logical thinking of the 20th century has been much concerned with questions involving the interpretation of abstract theoretical systems. At first glance this would not seem to be an intricate task. We have a certain scientific theory with its own specific language; in order to understand the theory we must reduce its language to another language, a more universal and formalised one, for example, the kind of language provided by modern formal logic. In general such a comparison of two languages is extremely fruitful because it allows us to test a scientific theory by rigorous linguistic criteria, to establish its non-contradictoriness, the accuracy of the term used, and so on. But this method cannot be used to elucidate the objective sphere of theory, i.e., its cognitive significance and objective content.

There is another means of interpreting scientific theory: this is to compare its language with the language of observation, of experiment, to seek not only the abstract objects behind the terms and expressions of theory, but also the empirical sensuous objects that can actually be observed. This operation, known as empirical interpretation, allows us to relate an abstract theoretical system to the phenomena of objective reality; but even empirical interpretation does not solve the crucial problem, the elucidation of the whole cognitive significance of the theoretical system.

One and the same theory may be interpreted through different experiments which, even taken together, cannot replace the knowledge it contains of the laws of phenomena.

Some schools of contemporary philosophy, particularly logical positivism, assume that knowledge is built up of two elements—the rules of operating with linguistic signs and sense perception. Therefore, say the positivists, scientific theory can also be interpreted only by the linguistic means of formal logic or by reduction to the language of observation, of experiment, which is nearer to the natural language and consequently to our sensory images. The weakness of these positivist concepts lies in the fact that, in analysing the language of science, they do not actually deal with knowledge itself in its specific form, because since the days of Kant it has been accepted in philosophy that knowledge must include a content that goes beyond the use of symbols and beyond empirical observation (things outside us). This implies that to understand theory and grasp its cognitive significance, to understand the knowledge it contains, we must not confine ourselves to its interpretation by means of the language of formal logic and empirical observation, but include it in the general process of development of knowledge and of human civilisation in general.

By this means we can understand the part played by theory in intellectual development, in the intellectual mastery of the phenomena and processes of objective reality, and where it is leading human



thought and activity. In this revealing of the cognitive significance of theory a tremendous part is played by the categories of philosophy.

From the above the conclusion may be drawn that *knowledge is the spiritual assimilation of reality essential to practical activity. Theories and concepts are created in the process of this assimilation, which has creative aims, actively reflects the phenomena, properties and laws of the objective world and has its real existence in the form of a linguistic system.*

**Objective truth**

*Knowledge is a result of human activity.* In acquiring knowledge man is active, he is guided by his own aims and makes special instruments, tools and other devices that help him to know reality. Man's intervention in the processes that he studies is constantly increasing. For practical activity we need knowledge that reflects with the greatest degree of fullness and accuracy the objective world as it exists in itself, independently of man's consciousness and activity. Here we are confronted with the question of the truth of knowledge. What is truth? How is it possible? Where are the criteria by which we can separate true knowledge from the untrue, the false?

Long-standing tradition that goes back to the philosophy of ancient times tells us that the truth is what corresponds to reality. But this definition is so broad and ambiguous that it has been virtually accepted by nearly all philosophical schools, both materialist and idealist. Even the agnostics (see Appendix) agree with it, while putting their own interpretation on the terms "correspondence" and "reality". The agnostics say they are not against knowledge in general, but against knowledge as the reflection of things and processes as they exist in themselves. So the general conclusion is that all philosophers have believed the attainment of truth to be the aim of knowledge and have recognised its existence.

For these reasons the Marxist-Leninist theory of knowledge could not rest content with such an abstract definition of truth; it had to go further. Marxism-Leninism has developed the more concrete concept of *objective truth*, which means knowledge whose content does not depend on the subject, does not depend either on the individual or on mankind as a whole.[16]

As we have noted, there can be no knowledge, and consequently no truth, independent of man's practical activity. This is where the objective idealists are wrong in their conception of taking truth beyond the sphere of man and mankind into some transcendental world.

But on the other hand there can be no truth outside its objectivity; if objective reality does not form the content of knowledge, knowledge loses its basic quality, that of reflecting the object as it exists in itself. Thus, such statements as "the electron forms part of the structure of the atom of any element", or "any capitalist society is based on the exploitation of man by man", are objective truths because their content is taken from objective reality, from the state of things that exists independently of the consciousness of the people who seek to know it.

Objective truth expresses the dialectics of subject and object. On the one hand, the truth is subjective because it is a form of human activity; on the other, it is objective because its content does not depend either on the individual or on mankind as a whole.

For the materialist "... the recognition of objective truth is essential", while for the agnostic, the subjective idealist, "there can be no objective truth",[17] because he rules out the possibility of phenomena and processes being reflected in thought as they exist independently of the consciousness of the thinker.

Denial of objective truth takes various forms. Kant and his followers believed the attribute of true knowledge to be necessity and universality, whose source lay not in the objective world but in the nature of sensuality and intellect; consequently, in their view, there was in fact no objective knowledge. Machism regarded true knowledge as that in which the most economical and simple connection of sensation was achieved. Marxism does not deny the importance of the desire for economy and simplicity but, as Lenin wrote, thought "...is 'economical' when it *correctly* reflects objective truth...."[18] Pragmatism deduces truth from practice, which is understood as subjective activity designed to achieve utility. It is, of course, an objective fact that true knowledge is necessary and useful to society, including separate individuals, but utility and practical considerations are not the source of truth; rather knowledge can only be useful, can only become the instrument for transforming things, when it is objectively true. Through practice knowledge comes into contact with objective reality, and draws its content from the latter.

Bertrand Russell, a prominent figure in British neo-positivism, believed truth to be a form of faith. "... It is in fact primarily beliefs that are true or false; sentences only become so through the fact that they can express beliefs."[19] Russell sees truth as a belief to which a certain fact corresponds; the false is also a belief, but one that is not confirmed by fact. The question of what constitutes a fact that confirms belief is left open; it may be some external association, and so on. In other words, the objectivity of the content of knowledge as the decisive moment of truth does not figure in this theory.

Popular among positivists is the concept that reduces the content of knowledge to the means of testing and proving it. In this case the objective significance or content of knowledge is confused with the means of its proof or testing. But proof is not the content of knowledge; it is the process of establishing its objective truth. The same statement with one and the same objective content may be proved by completely different means; in doing so we change not the content of the statement that has to be proved but only our attitude to it. Therefore, without detracting from the significance of proof and verification in the process of cognition (we must not only obtain objectively true knowledge, but also be subjectively confident that it is such), we must draw a strict distinction between truth and the means of proving it; the one may not be taken for the other. Truth is knowledge the content of which is determined by the object, its properties and laws.



## What Is Absolute Truth?

Objective truth is not something static. It is a process that includes various qualitative states. There is an accepted distinction between *absolute* and *relative truth.*

The term "absolute truth" is used in philosophical literature in various senses. It often implies the notion of complete and ultimate knowledge of the world as a whole. This is truth in the last instance, the ultimate realisation of the strivings and potential of human reason. But is such knowledge attainable? In principle man is capable of knowing everything in the world, but in reality this ability is realised in the process of the practically infinite historical development of society. "... The sovereignty of thought," writes Engels, "is realised in a series of extremely unsovereignly-thinking human beings...."[20] Each result of human knowledge is sovereign (unconditionally true), inasmuch as it is a moment in the process of cognition of objective reality, and unsovereign as a separate act, inasmuch as it has its limits which are determined by the level of development of human civilisation. Therefore the desire to achieve truth in the last instance at all costs is like going on a wild goose chase.

Sometimes the term "truth in the last instance" is used to describe factual knowledge of individual phenomena and processes the authenticity of which has been proved by science. Such truths are also sometimes called eternal: "Leo Tolstoy was born in 1828", "birds have beaks", "chemical elements have atomic weight".

Do such truths exist? Of course, they do. But anyone who would limit cognition to the achievement of such knowledge would, as Engels remarks, not get very far. "If mankind," he writes, "ever reached the stage at which it should work only with eternal truths, with results of thought which possess sovereign validity and an unconditional claim to truth, it would then have reached the point where the infinity of the intellectual world both in its actuality and in its potentiality had been exhausted, and thus the famous miracle of the counted uncountable would have been performed."[21]

Science has developed through overthrowing various assertions that claimed to be absolute but turned out to be true only for their time (for example, "the atom is indivisible", "all swans are white", and so on). Actual scientific theory quite often contains an element of the untrue, the illusory, which is revealed by the subsequent course of cognition and the development of practice.

But do we not then set foot on the perilous path of denying objective truth? If in the process of cognition a moment of illusion is discovered in what was thought to be true, if the opposition between the true and the false is relative, then perhaps there is no general difference between them? This, in fact, is the argument of the relativists, who absolutise the relativity of knowledge. If truth is relative, it may be considered from their standpoint that science moves from one truth to another, or, which is the same thing, from one error to another.

Relativism is correct in one respect—its recognition of the fluidity, the mobility of all that exists including knowledge, but it metaphysically divorces the development of knowledge from objective reality. "The materialist dialectics of Marx and Engels certainly does contain relativism, but is not reducible to relativism, that is, it recognises the relativity of all our knowledge, not in the sense of denying objective truth, but in the sense that the limits of approximation of our knowledge to this truth are historically conditional."[22]

The Marxist theory of knowledge, while opposing both dogmatism and relativism, acknowledges the existence of both absolute and relative truths, but in doing so it establishes their interconnection in the process of achieving objective truth. "To be a materialist," Lenin writes, "is to acknowledge objective truth, which is revealed to us by our sense-organs. To acknowledge objective truth, i.e., truth not dependent upon man and mankind, is, in one way or another, to recognise absolute truth."[23]

*Absolute truth* exists because in our objectively true knowledge there is something that is not overthrown by the subsequent course of science, but is only enriched with new objective content. At the same time at any given moment our knowledge is *relative*; it reflects reality truly in the main, but not completely, and only within certain limits, and with the further movement of knowledge it becomes more accurate and more profound.

Objective truth is the process of movement of knowledge from one stage to another, as a result of which knowledge is filled out with content taken from objective reality. It is always a unity of the absolute and the relative. "Each step in the development of science," Lenin writes, "adds new grains to the sum of absolute truth, but the limits of the truth of each scientific proposition are relative, now expanding, now shrinking with the growth of knowledge."[24]

In ancient Greece a geometry was invented that is known in science as Euclidean geometry. Is it true or not? Of course, it is an objective, absolute-relative truth, because its content is taken from the spatial relationships existing in objective reality. But it is true only up to a certain point, that is, while it remains abstracted from the curvature of space (regarded in Euclidean geometry as zero). As soon as space is considered with a positive or negative curvature, scientists have recourse to non-Euclidean geometries (Lobachevsky's or Riemann's), which have extended the limits of our knowledge and contributed to the development of geometrical knowledge—along the path that leads us ever deeper into objective truth.



## Criteria of true knowledge

In seeking objective truth, man experiences a need for criteria to help him distinguish it from error.

This would seem to be quite simple. Science yields objective truth and man has worked out many ways of proving and testing it. But this is not the whole story. Proof in the strict sense of the term is the deduction of one knowledge from another, when one knowledge must necessarily follow from another—thesis from arguments. Thus in the process of proof knowledge does not go beyond its own sphere, but remains, as it were, confined within itself. This is what has given rise to the idea of the existence of *formal* criteria of truth, when truth is established by collating one set of knowledge with another.

The so-called theory of *coherence*, which has been much publicised in the 20th century by the neo-positivists proceeds in general from the proposition that no other criterion exists, and that truth itself is the agreement of one set of knowledge with another set of knowledge established on the basis of the formal logical law of inadmissibility of contradiction. But formal logic can guarantee us the truth of a deduced statement only if the premises from which it follows are true; *A* follows from *B, B* follows from *C,* and so on *ad infinitum*.

But from where, we may ask, do we obtain the general principles, the axioms and even the rules of logical deduction that form the basis of any proof? This question was asked by Aristotle. If we follow the theory of coherence, we can only accept them as conventional agreements (conventions) and thus write off all attempts to establish the objective truth of knowledge, thereby submitting to subjectivism and agnosticism in the theory of knowledge.

The history of philosophy records various approaches to the problem of the criteria of true knowledge. Some philosophers saw the solution in empirical observation, in the sensations and perceptions of the individual. Of course, empirical observation is one of the means of testing knowledge. But in the first place, not all theoretical concepts may be tested by direct observation. Secondly, as Engels wrote, "the empiricism of observation alone can never adequately prove necessity. . . . This is so very correct that it does not follow from the continual rising of the sun in the morning that it will rise again tomorrow. . . ."[25] But knowledge that lays down laws must contain in itself both necessity and universality.

Of course, scientific practice does sometimes test statements and theories by sensory experience. But this cannot serve as the ultimate criterion of truth, because from one and the same theory there may follow quite different consequences that can be tested experimentally.

The fact that one such consequence, or several of them taken together, corresponds to experience still does not guarantee the objective truth of the whole theory. Besides, not all propositions of science can be tested by direct recourse to sensory experience. This is why even the neo-positivists, who champion the prinicple of *verification* (testing of knowledge by comparing it with the data of experience, obseration and experiment), have felt its unreliability as a general criterion of the truth of knowledge, particularly when dealing with scientific theories that possess a large degree of universality. To rescue the principle of verification, they go on inventing ever wider interpretations of the concept of "experimental verifiability", on the one hand, while limiting the sphere of its application (not all true ideas can be tested experimentally, etc.), on the other. Some of them, the British philosopher Karl Popper, for example, have proposed that verifiability should be replaced by falsifiability, that is, the attempt to find experimental data that refute rather than confirm the theory.

Disqualifying facts are, of course, essential to science, particularly as a means of establishing the limits of applicability of a given theoretical system. But this method cannot be used to prove its objective truth.

Marxism has solved the problem of the criterion of truth by showing that it lies ultimately in the activity which is the basis of knowledge, that is, *in social historical practice*. "The question whether objective [*gegenständliche*] truth can be attributed to human thinking is not the question of theory but is a *practical* question. In practice man must prove the truth, that is, the reality and the power, the this-sidedness [*Diesseitigkeit*] of his thinking."[26]

What gives practice its strength as a criterion of truth? The criterion of true knowledge must possess two qualities. First, it must undoubtedly be sensuous and material in character, it must take man out of the field of knowledge into the objective world, because it is the objectivity of knowledge that must be established. Second, knowledge, particularly the laws of science, has a universal character, and the universal and infinite cannot be proved by one individual fact or even by any number of them taken together. Man's practical activity, the nature of which is intrinsically universal, possesses this special feature.

As Lenin said, a person "finally" grasps objective truth, "... only when the notion becomes 'being-for-itself' in the sense of practice".[27] Moreover, in practice the universal acquires the sensuously concrete form of a thing, a process, and so it has in itself "not only the dignity of the universal but also of the *simply actual*".[28] In other words, in practice the objectivity of knowledge which is universal in character acquires the form of sensuous authenticity. And there is no need to depart into the bad infinity of enumerating examples and facts. The steam-engine which man built on the basis of knowledge proves



the proposition of physics concerning the conversion of thermal energy into mechanical energy and, as Engels observed, "100,000 steam-engines did not prove this more than *one* ..."[29] By learning how to use atomic energy in industry, agriculture and medicine man proves the objective truth of the physical concepts of the structure of the atom.

This does not mean, of course, that from the standpoint of Marxist-Leninist epistemology every concept, every act of knowledge must be directly tested in practice, in production or in some other form of material human activity. In reality the process of proof takes the form of deducing one set of knowledge from another, that is, the form of a logical chain of reasoning, some of whose links are tested by application in practice. But does not this suggest the idea that besides practice there exist criteria based on the logical apparatus of thought, on the collation of one set of knowledge with another? Of course, the forms and laws of logical deduction do not depend on separate acts of practical activity, but this does not mean that they are in general unconnected with practice and not engendered by it. As Lenin wrote, "... the practical activity of man had to lead his consciousness to the repetition of the various logical figures thousands of millions of time *in order that* these figures *could* obtain the significance of axioms".[30]

Practice is not a fixed state, but a process formed of individual elements, stages and links. Knowledge may overtake the practice of one or another historical period. There may not be enough available practice to establish the truth of the theories that are advanced by science. All this indicates the relativity of the criterion of practice. But this criterion is simultaneously absolute because only on the basis of the practice of today or tomorrow can objective truth be established. " ... The criterion of practice can never, in the nature of things, either confirm or refute any human idea *completely*. This criterion too is sufficiently 'indefinite' not to allow human knowledge to become 'absolute', but at the same time it is sufficiently definite to wage a ruthless fight on all varieties of idealism and agnosticism."[31] As it develops practice overcomes its limitations as a criterion of knowledge. Developing practice cleanses knowledge of all that is false and urges it on to the new results that we need.

# APPENDIX

## Materialist Dialectics Versus Agnosticism

Dialectical materialism reveals the bankruptcy of the school of thought that denies man's ability to obtain objective knowledge of reality (known as *agnosticism*). The ideas of agnosticism are to be found in a mild form in the scepticism of ancient Greece (Pyrrho, Carneades and Aenesidemus): separation of knowledge from external reality, doubts as to the reality of the external world growing into denial of the existence of things themselves, and so on

The theoretical foundations of agnosticism were worked out by the English 18th-century philosopher David Hume, who maintained that all knowledge was, in essence, nonknowledge. "The most perfect philosophy of the natural kind only staves off our ignorance a little longer: as perhaps the most perfect philosophy of the moral or metaphysical kind serves only to discover larger portions of it. Thus the observation of human blindness and weakness is the result of all philosophy. . . . "[1] Hume recommended faith and force of habit rather than knowledge as the basis for practical action.

Kantianism is the next variety of agnosticism. Kant produced a detailed analysis of the cognitive process, its separate elements: the senses, intellect, reason. This was an important contribution to the theory of knowledge. But the direction and general conclusion of all his theoretical reasoning are incorrect. Kant revealed the complex and contradictory world of knowledge, but he divorced it from the things of the real world. ". . . Of what they [the things—*Ed.*] are in themselves," he wrote, "we know nothing, we know only their appearances, that is, the notions they evoke in us, acting on our senses."[2]

Kant is right in saying that knowledge begins with experience, with sensation. But experience, as he understands it, instead of bringing man into contact with the world of things in themselves, separates him from it because Kant presumes the existence in the consciousness of *a priori* knowledge, i.e., forms of sensation and intellect that exist prior to and independently of experience. According to Kant, knowledge is built up out of that which is given by experience and out of these *a priori* forms. Apriorism brings him to an inescapable agnosticism.

Agnosticism does not disappear when we come to the philosophy of the 19th and 20th centuries. It was accepted by various schools of bourgeois philosophy, particularly the positivists and such varieties of positivism as Machism and the related philosophy of pragmatism. Recent bourgeois philosophy has contributed nothing "original" to the premises of agnosticism; it merely reproduces the ideas of either Kant or Hume, and more often than not presents a mixture of the two as the latest thing in philosophy.

How does agnosticism treat the basic trends in philosophy—materialism and idealism? It would be an oversimplification to assume that all idealist philosophers are agnostics. Descartes, Leibnitz, Hegel and others were not. Hegel, as Engels observes, overthrew agnosticism " . . . in so far as this was possible from an idealist standpoint".[3] But the idealist criticises agnosticism inconsistently, makes concessions to it and himself gravitates towards agnosticism in dealing with a number of fundamental questions of the theory of knowledge. On the other hand not every agnostic is a determined, consistent advocate of idealism. Often he tries to occupy an ambivalent, compromise position in the struggle between materialism and idealism.



> For the materialist, the 'factually given' is the outer world, the image of which is our sensations. For the idealist the 'factually given' is sensation, and the outer world is declared to be a 'complex of sensations'. For the agnostic the 'immediately given' is also sensation, but the agnostic *does not go on* either to the materialist recognition of the reality of the outer world, or to the idealist recognition of the world as our sensation. [Lenin[4].]

Agnosticism, as a theoretical conception of knowledge which divorces the content of our sensations, perceptions and concepts from objective reality, is idealism when it comes to solving the second aspect of the basic question of philosophy. Admittedly, not everyone who calls himself an agnostic actually is one. Some naturalists, such as the Englishman Thomas Huxley, who in the 19th century introduced the term "agnosticism" declared themselves agnostics without actually adopting agnostic philosophical positions. In the conditions of bourgeois society the term "agnosticism" was a convenient disguise for their scientific materialism, and they simply declared everything that went beyond the bounds of the scientific discovery of their day to be unknowable. Their views were primarily opposed to religious faith in the existence of God.

The attitude of agnosticism to dialectics and metaphysics is complex. Agnosticism speculated on the dialetical nature of human knowledge. It is true that a certain degree of scepticism and doubt is essential to propel knowledge forward, to overcome dogmatism. Since the days of the Greeks scepticism has contained a certain dialetical element. The sceptics often perceived the richness, complexity and contradictoriness of the progress of knowledge towards truth. But agnosticism absolutises the mobility and relativity of knowledge and its scèpticism acquires a negative bias. The agnostics reassure themselves by asserting the relativity of knowledge, its contradictoriness, and refuse to proceed any further towards the laws of the objective world. The separation of subjective dialectics (motion of knowledge) from objective dialectics (motion of matter) is the basic epistemological source of agnosticism.

Agnosticism was rightly criticised as soon as it appeared. Its opponents were quick to point out the contradictory nature of its statements and the absudity of its ultimate conclusions. But in this criticism there was often more wit than solid argument. The agnostic concept of knowledge arises as a reflection of the complex character, the contradictory nature of the process of acquiring knowledge, the difficulties involved in defining the criteria of true knowledge. But agnosticism also reflects the position of certain classes of society, their world view. The overcoming of agnosticism therefore presupposes both the solution of complex theoretical problems and overcoming (exposing, eradicating) its social roots. Neither the old contemplative materialism nor idealist dielectics can cope with this problem. It can be solved only on the basis of materialist dialectics, which is also the theory of knowledge of Marxism-Leninism.

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[25] F. Engels, *Dialectics of Nature*, p. 229.
[26] K. Marx and F. Engels, *Selected Works*, Vol. 1, p. 13.
[27] V. I. Lenin, *Collected Works*, Vol. 38, p. 211.
[28] Ibid., p. 213.
[29] F. Engels, *Dialectics of Nature*, p. 229.
[30] V. I. Lenin, *Collected Works*, Vol. 38, p. 190.
[31] Ibid., Vol. 14, pp. 142-43.


***Change the world to fit human nature***

**If man draws all his knowledge, sensation, etc., from the world of the senses and the experience gained in it, then what has to be done is to arrange the empirical world in such a way that man experiences and becomes accustomed to what is truly human in it and that he becomes aware of himself as man. If correctly understood interest is the principle of all morality, man's private interest must be made to coincide with the interest of humanity. If man is unfree in the materialistic sense, i.e., is free not through the negative power to avoid this or that, but through the positive power to assert his true individuality, crime must not be punished in the individual, but the anti-social sources of crime must be destroyed, and each man must be given social scope for the vital manifestation of his being. If man is shaped by environment, his environment must be made human. If man is social by nature, he will develop his true nature only in society, and the power of his nature must be measured not by the power of the separate individual but by the power of society. — The young Marx and Engels, 1844, "The Holy Family." *Coll. Works* iv, 130.)**

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