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SCIENCE AND NATURE

The journal of Marxist philosophy for natural scientists

IN THIS ISSUE

SCIENCE AND NATURE

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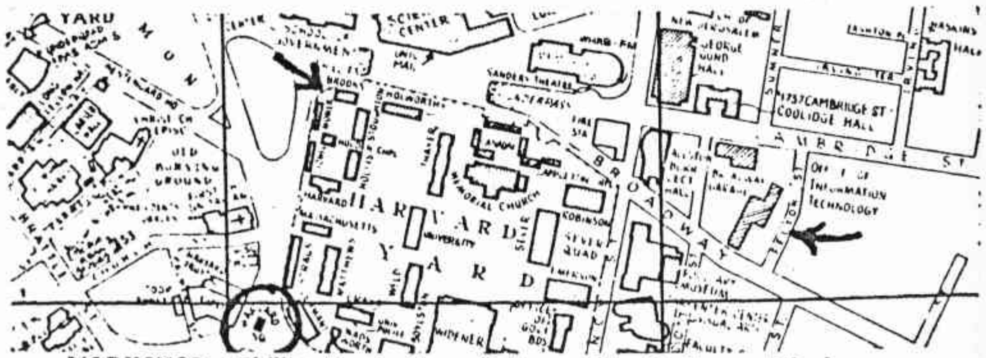

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1978, Number 1

SCIENCE AND NATURE

SCIENCE NEEDS A USEFUL PHILOSOPHY

An Editorial Statement on Our Reason for Being

PURPOSE. This journal is dedicated to helping the investigator in developing new knowledge and the teacher in transmitting this knowledge to a new generation. The functions of the journal will be:

1) To show how the Marxist theory of knowledge illuminates basic philosophical questions concerning the scientific process and thus helps to raise the level of consciousness of those participating in the process.

2) To demonstrate that the principles of dialectical materialism provide a fruitful conceptual orientation for research processes such as model building, hypothesis generation, and interpretation of experimental results.

3) To report on the ways in which historical materialism provides useful understanding of external social forces that influence the way scientists think as well as the economic basis of their activity.

By such methods we hope to make scientists into better philosophers and philosophers into better scientists, bringing the two communities into a closer, more effective working alliance for the advancement of science. We believe that this is also the best way to refute the false charges that Marxism is only dogma.

Along the way we hope to explore vital questions such as the relationship of thought processes to other processes of nature. And we will be delighted if, through such exploration, our journal contributes to the further development of Marxist philosophy of science.

Thanks to the examples provided by Marx, Engels and Lenin, the role of Marxist philosophy is already well established in the social sciences. *Science and Nature* will concentrate on achieving the same status for the natural sciences.

POLICY. This journal will be a medium of communication on the philosophical problems of the physical, biological and formal (mathematical) disciplines. The main effort will be to publish material that is both interesting and relevant to practitioners of these sciences.

Relevance will be judged primarily by the degree to which a contribution brings out the relationship of philosophical principles to scientific practice. For this purpose, authors should make an effort to discuss their scientific problems in a manner that can be grasped by those outside of the particular discipline involved.

Contributions of all types are welcome—brief communications or in-depth articles, bibliographic notes or more lengthy reviews The size and frequency of journal issues will depend on the quality of editorial material available as well as the funds on hand. (Our current goal is two issues per year.)

PROPOSAL. We solemnly promise that any investment you make to help get this new journal off the ground will bring no monetary reward whatsoever. But you may expect lasting satisfaction from helping us to take advantage of the currently favorable climate for bringing to the academic community a broader and deeper understanding of Marxism.

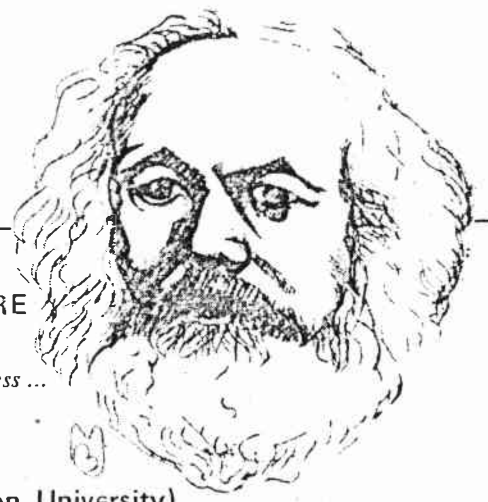
As a minimum investment, we urge that you not only subscribe for yourself but also bring *Science and Nature* to the attention of friends, colleagues and students. Any investment in the form of an added contribution will, of course, be especially helpful in this first year of our philosophical enterprise. The time you invest in giving us helpful suggestions and critical comments will also be greatly appreciated. ☐

Philosophy Against Nature

Naively, Galileo believes that simple truth will conquer, and the old order will collapse under its pure light. He does not at first recognize the depth of the problem he is confronting. Patiently Galileo explains to the court and its philosophers how burdensome the old theory has become: "As your highness no doubt knows, we astronomers have for some time been encountering great difficulties in our calculations. We are using a very old system which seems to be in agreement with philosophy but unfortunately not with facts." All that is necessary is that one look through the telescope and one will see with his own eyes that Aristotle and two thousand years of tradition are wrong. The court philosopher disputes at length with Galileo on the nature of truth ... no one looks through the telescope.

– *James Lawler,* "Bertolt Brecht on Revolution in Science and Society," *Political Affairs, July 1978.*

A Workshop Discussion
Washington DC
12 February 1978



KARL MARX ON SCIENCE AND NATURE
(EXCERPTS)

Science as Social Process ...

INVITED SPEAKER
Robert S. Cohen (Boston University)

The aims of an organization of this sort [Dialectics Workshop] are primarily to bring out the relevance of Marx to the present time, to the science of today. I am talking here not on my view of what Marx means for today but on what I think he meant for his time. The relevance of this for science and philosophy today is for you to develop ...

Marx and Science

The principal contribution of Marx was to emphasize the social character of science. Although he acknowledged the cognitive successes of the sciences, he nevertheless comprehended them as social phenomena, part of the general social and economic processes of their times (that would be his hypothesis): and if at times they were isolated from social forces, then they were to be understood as a product of social conflicts and pressures which allowed for such isolation. Even the pure scientists, responding solely to inner motivations (perhaps curiosity) would be in a social situation which produced that kind of curiosity, that kind of isolation.

To be social meant, further, to respond to socially produced motivations and purposes, and to do so with socially stimulated modes of inquiry and of explanation, and with criteria of success or failure which themselves changed from century to century ... The sciences were not in any full sense promoted by pleasurable motivations for, in the development of the sciences, Marx saw a central, perhaps the *essential*, contribution to the grim and practical task of mastering nature. * * *

On the deliberately conscious linkup of science with industry, and the social implication. Marx says (*Capital*, v. 1): "The implements of labour, in the form of machinery, necessitate the substitution of natural forces for human force, and the conscious application of science, instead of rule of thumb" (p 382) ... and "Intelligence in production expands in one direction because it vanishes in many others. What is lost by the detail labourers, is concentrated in the capital that employs them ... modern industry makes science a productive force and presses it into the service of capital" (p 355). * * *

You may remember this passage from *The Communist Manifesto:* "The bourgeoisie has stripped of its halo every occupation hitherto honored and looked up to with reverent awe. It has converted the physician, the lawyer, the priest, the poet, and the scientist into wage labourers."

On the distinction between science and cooperative labour, there is an interesting passage from vol. 3 of *Capital:* "Universal labour is scientific labour, such as discoveries and inventions. This labour is conditioned on the cooperation of living fellow beings and on the labours of those who have gone before. Co-operative labour; on the other hand, is a direct co-operation of living individuals." * * *

Marx early recognized that, like prejudices and religious beliefs, so also ideas have social function and determinants, not least of them scientific ideas, even those of the most confirmed and objectively established sort. He was an admirer of Charles Darwin whose work he saw as a penetrating insight and próof indeed of the historical character of biological nature. But he also noted with amusement that Darwin's hypothesis saw nature through a social image. He wrote to LaSalle, at a time when he was still friendly with LaSalle, "Darwin's book is very important and serves me as a natural-scientific basis for the class struggle in history. One has to put up with the crude English method of development, of course. Despite all deficiencies, not only is the death blow dealt here for the first time to 'teleology' in the natural sciences but its rational meaning is empirically explained" (*Selected Correspondence*, p. 151). Note that you don't throw out teleology, you *explain* it.

Then, in a letter to Engels, he says: "It is remarkable how Darwin recognizes, among the beasts and the plants, his English society with its division of labour, competition, opening up of new markets, 'inventions,' and the Malthusian 'struggle for existence.' It is Hobbes' *war of all against all*, and one is reminded of Hegel's *Phenomenology* where civil society is described as a 'spiritual animal kingdom,' while in Darwin the animal kingdom figures as civil society" (Ibid. pp 156-7).

Marx also saw Darwin's work as suggestive for human history, and for the instrumental role of the human body and of technology and science. Here is another of those simply marvelous footnotes in *Capital:* "Darwin has interested us in the history of Nature's Technology, i. e., in the formation of the organs of plants and animals, which organs serve as instruments of production for sustaining life in the plants and animals. Does not the history of the productive organs of man, of organs that are the material basis of a social organization, deserve equal attention? And would not such a history be easier to compile since, as Vico says, human history differs from natural history in this, that we have made the former but not the latter."

Marx consistently treated science under the general heading of labor and he understood scientific conceptions to be joined with the material basis of human existence, with practical life, and with social relations among men and women. The previous footnote continues: "Technology discloses man's mode of dealing with Nature, the process of production by which he sustains his life, and thereby also lays bare the mode of formation of his social relations and of the mental conceptions that flow from them." An extraordinary claim! If one really understood the history of technology and the science that goes with it, that would be the basis for understanding the mode of formation of social relations and the mental conceptions, the ideologies of the times! That's in *Capital*, so it's the fully mature Marx. "The weak points," he goes on, "in the abstract materialism of natural science, a materialism that excludes history and its process, these weak points are at once evident from the abstract and ideological conceptions of its spokesmen, whenever they venture beyond the bounds of their own specialty." I think that's no longer true. Today the liberal as well as the radical spokesmen go well beyond ... but this was something quite appropriate for his time.

Marx and Nature

He saw very well, as I'm sure you know, that capital "first creates bourgeois society and [with it] the universal appropriation of nature", the key point of bourgeois society, universal in the sense defined before, that nature takes an instrumental role in human history, nature becomes an instrument for humanity. That's from the *Grundrisse*, the huge preparatory notes for *Capital*. "For the first time," he continues, "nature becomes purely an object for humankind, purely a matter of utility, ceases to be recognized for itself. The theoretical discovery of any autonomous law of nature appears merely as a ruse, a trick, so as to subjugate nature to human needs, whether as an object of consumption or as a means of production."

Now such an attitude towards technology and science, I think, obviously leads to what Marx considers to be the correct notion of freedom, the now familiar theme of Marx, in which we reverse the domination of human beings, either by the "blind" forces of nature or by the blind forces of society, and turn them upside down.

It is technology that is fundamental because technology is the mediation between man and nature. He says (*Capital*, vol. 3): "the realization of freedom consists in socialized man, the associated producers, rationally regulating their material interchange with nature and bringing it [nature] under their common control instead of allowing it to rule them as a blind force." Now that must lead beyond craft technology to science with the impressive modification of human life which is made possible by the cognitive achievements of science when and if, in Marxist terms, nature is *appropriated*. Marx says (in *The Grundrisse*), "it is neither the direct human labour he himself performs nor the time which he works, but rather the appropriation of his own [scientific] productive power, his understanding of nature and his mastery over it by virtue of his presence as a social body, it is, in a word, the development of the social individual [as scientist] which appears as the great foundationstone of production and of wealth."

Now to understand what he meant by the phrase "the social individual" is to understand what he meant by the whole of the Marxist theory of society and I can't pursue that here ...

Marx and Method

Unfortunately even today 100 years later, research into what Marx meant by the scientific method, the methods of thought and investigation, as shown in his works and as expounded by him, has brought no general agreement anywhere in the world. There are, however, several explicit texts by Marx on the scientific method [see Bibliographic Notes, this issue] ... and then there are other things to be understood, if you're clever enough, from everything else that Marx wrote.

Engels often praised Marx's method, even above Marx's achievements. In a letter of 1895 to Sombart, a soon-to-be reactionary German economist, Engels wrote: "Marx's whole manner of conceiving things is not a doctrine, but a method. It offers no dogmas, but rather points of reference for further research, and the method of that research ..." * * *

Discussing the dialectical method in *Capital* (Vol. 1 Afterword) Marx said: "Of course, the method of presentation must differ in form from that of inquiry. The latter has to appropriate the material [the empirical data] in detail, to analyze its different forms of development, to trace out their inner connection. 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." ... So Marx distinguishes the method of inquiry from the method of exposition.

By the way, exposition is not just the method of teaching or pedagogy, it is the actual second stage of scientific work. Inquiry is factually realistic, beginning with initially uninterpreted data that are subjected to analysis in stages of complexity which demand insightful abstraction, simplification and, of course, some subtlety. The factual data are the concrete entities or the wholes, not the individual partial things, the whole factual data. And the results of analysis are abstract principles analyzed into theoretically formulated "parts", i. e., selected out, hypothetically guided by theories that have been based upon and more or less tested in previous investigations. So inquiry is a complicated stage of empirical research, and of inductive as well as hypothetical analysis.

Then comes *presentation* which gives the results their necessary development – or some notion of development anyway -- which aims to be a conceptual return from abstraction back to the concrete reality, and brings the component parts or qualities of any subject matter together in what we would call their organic relatedness and interrelatedness and into their evolutionary or historical movements. This return will be mediated by expository as well as theoretical demands so as to clarify the separate qualities and the various relations among them, including their relations with their environment.

So, for Marx, the truth will be the whole in its changes. And these changes in turn will be related by historical processes. This is the Marxian dialectic of contending and negating "forces" within history ... negative meaning changing, denying or destroying what was ...

DISCUSSANT:

Irving Adler (North Bennington, Vermont)

Fifty years ago many people rejected as absurd the idea that Marx and Engels had created a science of society. They argued that there couldn't be any such thing as a science of society or a science of history because (a) controlled experiments are not possible and (b) the student of society is part of the phenomenon he is studying: hence, objectivity in the study of society was presumed to be impossible. This argument was the basis of a supposed absolute distinction between the study of nature, which could be scientific, and the study of social institutions, which could not.

This distinction has been eroded by what is happening in the natural sciences themselves. First, all the sciences are now historical sciences. In the nineteenth century, geology and biology had already become historical sciences, studying processes and evolution. Since then, physics applied to astronomy, has become an historical science, studying the origins and evolution of stars and galaxies and the forms of matter they contain. Chemistry, too, has become an historical science, studying prebiotic chemical evolution and the origin of life. Secondly, it isn't only in the social sciences that the spectator is part of the act; the spectator is now part of the spectacle in physics as well. Relativity theory teaches us that measures of length and duration depend on the speed of the observer, and quantum theory tells us that the more accurately we determine the position of an electron, the less accurately we can determine its momentum, and vice versa, because every act of observation is an act of interference with the thing observed.

The erosion of this presumed distinction between the study of nature and the study of society has two important implications. The first is that the scientific study of society is no less possible than the scientific study of nature. The second is that dialectical materialism, developed initially in the social sciences for the study of complex dynamical processes undergoing evolution, is also the appropriate method for the study of processes and evolution in the natural sciences. * * *

In what way is dialectical materialism of use to the scientist? To answer this question, it is important to note first the characterization of dialectical materialism by Engels that has been called to our attention by Professor Cohen. Engels said in 1895 in a letter to Sombart, "Marx's whole manner of conceiving things is not a doctrine, but a method. It offers no finished dogmas, but rather points of reference for further research and the method of that research." ... Awareness of the principles of dialectical materialism is a kind of insurance against dogmatism. Each of the principles is a generalization from past experience in the sciences. But it never takes the form of telling us what we *must* find in a particular area of study. It tells us only what we *may* find. Then, this reminder of what is possible, becomes the point of reference, as Engels says, for some research. ...

Each of the principles of dialectical materialism is a danger signal, a red flag, if you will, warning of the existence of some common booby traps for unwary investigators. Among them are: 1) the metaphysical fallacy, trying to characterize an object by some abstract, absolute essence rather than by studying its internal and external relationships, its history and its dynamics; 2) the supernatural or external cause fallacy, failure to recognize that some motion can be understood as self motion resulting from internal conflict; 3) the mechanistic fallacy, failure to recognize that qualitative differences do exist; 4) the reductionist fallacy, ignoring the fact that in a complex structure or process

the whole is greater than the sum of its parts; 5) the eclectic fallacy, failing to recognize that qualitatively distinct factors may be related; 6) the extrapolation fallacy, extrapolating a law beyond the time and context for which it has been established, thus unjustifiably converting what is particular and temporary into what is allegedly general and eternal.

Recent experience in biology provides a good example of the usefulness of dialectical materialism as insurance against dogmatism. After Pasteur's experiments settled a 200-year dispute about spontaneous generation, his principle that life comes only from life and Virchow's principle that cells come only from cells were elevated into a dogma assumed to be valid for all times and circumstances.

Adherence to this dogma was an obstacle that stood in the way of initiating any meaningful study of the origin of life. The scientific study of the origin of life began when Oparin, Bernal and Haldane, consciously using one of the principles of dialectical materialism, challenged this dogma, pointing out that a law that applies for the period after the origin of life need not be valid for the period before the origin of life.

Now another dogma has arisen, to take the place of this old one. This is the dogma asserted as late as 1971 by Jacques Monod that the transfer of information between nucleic acids and protein can only go in one direction, from DNA to RNA to protein. The dogma has already been shaken by the demonstration that in some cases information goes from RNA to DNA but the faith of the dogmatists has not been shaken. Adherents of dialectical materialism would keep an open mind on this question. The fact that the transfer of information goes only in the direction from nucleic acid to protein in living cells does not preclude the possibility that it may have gone the other way at some stage in prebiotic chemical evolution.

If the principal function of dialectical materialism is to serve as an antidote for dogmatism, any formulation of its principles as a set of rules that nature *must* obey is a misrepresentation of these principles. In this category is the assertion on purely philosophical grounds that "space and time are boundless and infinite" made in the book "The Fundamentals of Marxist-Leninist Philosophy," published in the U.S.S.R. in 1974. In relativistic cosmology, the answer to the question "Is space finite or infinite?" will be obtained only by measuring the relevant parameters, and not by making some arbitrary presupposition.

I would add only one more point at this time. Dialectical materialism, like any other theoretical construct, cannot be assumed to be perfect for all time. It should be reexamined constantly, to be corrected and refined in the light of experience. It is now over 100 years since its principles were first formulated in the work of Marx and Engels. More precise formulations of its principles that are based on the great advances of the sciences in the last 100 years should now be possible.

DISCUSSANT
Garland E. Allen (Biology Dept., Washington University)

[Dialectical materialism] is, of course, more than a method, but as a method it has an enormous amount of applicability in everything we do and I would like to urge that when we think about learning this method, we think about its application, too – what it can be used for – that we not think about it only in terms of its application to the natural sciences although that may be the main concern we here more or less directly share. I think it is obvious that a bench natural scientist who is asking questions and trying to come to answers can profit from using the method we call dialectical materialism. In my own work in the history of science, that method has raised questions of a different sort than I would otherwise have raised ...

I think the only way to develop the method is to apply it and see its inner connection in a number of different realms. That means extra-academically as well as academically. I think it can help us, for example, understand and criticize phenomena such as the rise of sociobiology theory ... in fact, if one applies the dialectical method to the content of that so-called theory, one sees that there are many questions it has left unanswered and many methods which it has not even looked at...

I'm not sure that I agree with Bob Cohen that Marx's statement is applicable more to his day than ours–namely, the statement that the idealistic nature of much of science is evident when scientists step out of their own specialty and begin to make comments on other things. I certainly see an enormous amount of that today among my colleagues. I see it in sociobiology where the basic method is to *invent* genetic components which then are elaborated into enormous explanations of why they might exist. As Marx and Engels both pointed out, science deals not with what might be conceived to exist but with trying to understand what in fact does exist.

I think also that developing our dialectical method involves putting these things into practice, both in criticizing things like sociobiological theory and in terms of our own activism in contemporary social fields. By that I mean that we learn more about the method, to apply even in our intellectual endeavor, by being on the streets, by means of protest, by standing up ... talking about sociobiology ... doing what we have to do in our contemporary lives. That's another field of the interaction between theory and practice in dialectical materialism which I think is absolutely essential ...

I don't mean to sound mechanical about it but I think there's no such thing as theoretical Marxism... I tell "theoretical Marxists" [sociologists] that I think it's a contradiction in terms. Perhaps the analogy with theoretical physics works on people's minds but I don't think there is such a thing as theoretical physics really. Ultimately, things must be tested in some way. The division of labor between those who theorize and those who test is a mistake in any realm of human activity, especially in dialectical materialism. It is a method that has to be practiced in every realm because it is basically one which sees interconnections, whether in day-to-day political experience, classroom teaching, lab bench science, or the study of the history of nature.

I want to close with this notion: the theory of dialectical materialism is powerful and rich because it is first and foremost a method which grows, is not static or unchanging, but a method which teaches certain kinds of questions to ask and certain ways of trying to answer them.

AUDIENCE INTERACTION

David Schwartzman (Howard University). Some writers have maintained there is a contradiction between Engels' formulation of dialectical materialism and Marx's thinking on the subject. Dr. Cohen, what are your views on this?

Cohen. I think there are differences but not contradictions and not conflicts... Engels treated questions that Marx did not treat and which Marx would not have treated. Engels was more concerned, on behalf of both of them, with his studies of contemporary natural science.

It's most unfortunate that Engels' notes [in *Dialectics of Nature*] were taken as though they were his considered results. He was writing about historical processes in nature. It's not at all clear that specifically dialectical relationships were found by him in nature. Historical transitions were found but, if one keeps to the word dialectic as referring more or less to self-changing, inner-generated transitions, one can't find *that* in nature. If you define dialectic in the form that it takes place in human history, you don't find it...

Engels raises this question a bit in *Anti-Duhring*, then time and again in *Dialectics of Nature*. He gives examples, pedagogical or provocative but, at any rate, unanalyzed examples... and says that this is a future research problem... These are not considered or fully worked out things.

I think Lenin picked it up very well in that very brief, also incomplete, business where he says that what we need is a bunch of scientists who understand Hegel and will still be scientists. Then maybe someday we'll have a material equation of dialectic...

Lenin wrote that without knowing of Engels' work [on the dialectics of nature] but it fits very well. I think it's a very incomplete, open research problem as to what would be an adequate meaning for an autonomous nature having a dialectic.

I think that, for Marx himself, it was of no interest. He really thought of nature in its relation with mankind and, since nature is transformed by mankind, there was quite obviously to him a dialectic of *that* nature. Whether there was a dialectic in nature itself, independently, he didn't get to that. I think he would have if he hadn't died so early, because he shared with his friend Engels this very Eddingtonian type of interest in how nature gives rise to its own contradictions.

In that sense, one can say with some neoMarxists that, on one side Engels is *more* scientific because he talks more about autonomous nature, and Marx is more man-centered in dealing with nature. You can push this contrast but I don't believe in it myself. I think Engels was more Marxist than that...

Stephen Jay Gould (Harvard University). I've always read *Dialectics of Nature* as just a series of notes, undigested examples. I've never understood whether he just put them down as reminders to work out later... The problem of quantitiy into quality is where Marxist theory intersects my own work on the nature of rapid change in geology ... if someone found my notes for [an unpublished] article, it would be a silly series of lists.

Cohen. Besides, there were no xeroxes then, and people had to write down all kinds of quotations.

Gould. On the other hand, if you look at some of the more finished products, such as *The Part Played by Labour in the Transition from Ape to Man,* there Engels is willing to make particular historical deductions from Marxist theory. There's one thing that's not appreciated about that essay: Engels didn't just make up right from theory the notion that the hand represents labor and labor is the ground for society, therefore the hand must have evolved before the brain. He writes as though he had deduced it from that principle but, in fact, he cribbed it from Haeckel who had been arguing that upright posture came first. Nonetheless, in that essay Engels was willing to make a historical hypothesis and justify it on the basis of philosophy.

Lester Talkington (Science and Nature). It seems to me that as soon as historical processes enter the picture, dialectics come in too, whether in nature independent of man or in purely social processes. Probably the biggest source of difficulty arises from the lack of clarity on this in *Dialectics of Nature.*

Cohen. I think there's a lack of clarity in understanding Marx on this. If he were Hegelian, it would appear that the word dialectical would mean something with the word history. If it means the same thing as the word historical, we'd just have a synonym. But if it means something additional to historical... In Hegelian usage, at least, it would seem to be talking about internally generated changes, changes which come from internal forces and internal relations. Internal to what? Well, whatever the entity is that you're looking at, society or whatever.

And there could be dispute over whether dialectical changes, dialectically formulated history of change in the stars and galaxies, are of that sort. Or would all the forces of change be external to the entities involved? Well, that's an astronomical or cosmological problem, not one that Marx or Engels was competent to deal with or were interested in enough to deal with it. Engels speculates that there could be some such thing but he didn't pursue it. He was already in the last years of his life and editing Marx's manuscripts instead. He had consciously made that choice.

Hegel had done very little on this... His only serious work [in this area], on chemistry, was published only two years ago. So the Hegelian notion of natural processes is an open research problem for which no doubt Marx and Lenin and others would have been glad to give research grants. They didn't have a finshed doctrine at all.

Talkington. For example, take plate tectonics and the motions generated within the earth that give rise to change, to historical processes, having nothing to do with man or man's understanding. Would you consider those possibly dialectical processes of nature?

Cohen. I don't know what the word dialectical would add to just calling them processes. Perhaps it depends on how it is formulated.

Talkington. Wouldn't it possibly give a scientist some understanding to know dialectics in investigating such a phenomenon?

Cohen. I agree entirely with Gar Allen on the helpfulness [of dialectical materialism] in pinning down the questions to raise. In that sense, yes.

Ullica Segerstrale (Harvard University). Does the word dialectic have any meaning aside from the question of how a human being looks at something?

Garland Allen. Is it meaningful to ask whether there is a really objective dialectical process independent of people's interpretation of it? There's almost no way to answer that question.

Cohen. But there's a very strong hint in Marx and in Hegel on this: that there's an objective dialectic, not a subjective epistemological way of dealing with it but an objective dialectic in human history. The *invisible hand* is just a useful way of referring to the fact that objective processes exist independently...

There's an objective dialectic, yes.

Schwartzman. I think there has been work since Engels on the question of dialectics in nature... But it is, indeed, a project which is not developed so much as, say, historical materialism. ... Marx anticipated a lot of systems ideas. Some of the meta-theories in systems theory, aspects of cybernetics and so forth could really be the raw materials for a materialistic dialectics, for developing dialectical materialism as the most generalized theory of science—not simply being reduced to metaphors and to the old three laws, which really need a lot of work. It's no longer adequate to use these laws and say that this contributes to a methodological approach, warning us about certain things. We need to develop it as a formal science... I don't think it's correct to reduce dialectics simply to something evolving through internal processes. We've come beyond that in terms of looking at things as systems and their environments. But certainly a star is an entity in itself and undergoes certain internal processes. So does any other object that natural scientists study.

Cohen. Well, the entire part-whole process can be looked at through the use of systems theory... In the general methodology of science in the last 25 years, certainly in the Soviet Union and among Marxists elsewhere, it's exactly as you're saying. □

On the Alienation of Science ——————————————

In sixth-century Ionia man faced Nature in the confident hope that by his unaided powers he would be able to wrest from her her secrets; and in his bold enterprise he came to feel himself engaged upon an ethical as well as a scientific task. Conscience acquired a new scope as man realized that his progress in knowledge meant submitting his mind to the acceptance of external fact, of external law; and that the understanding of this law gave him power to help or harm his fellows. *Philanthropia*, love of his fellows, became his inspiration as much as *theoria*, disinterested curiosity.

But the obstacles to the growth of this new knowledge and to the exercise of this new power proved greater, and other, than had been anticipated. Not only did Nature prove more complex than man had supposed, but political obstacles also intervened. If democracy dimly and fitfully perceived that its fate was linked with science, oligarchy had no manner of doubt that ignorance was its shield...

Aristotle saved his scientific soul by a breach with Platonism where Platonism had lost touch with Nature, but he retained from Platonism the view that truth is the preserve of the élite, and that social order must be based on acknowledged superstition

— *Benjamin Farrington,* Science and Politics in the Ancient World.
Barnes & Noble Inc. (a Division of Harper & Row), New York 1968, pp. 229-30.

A Workshop Discussion
Columbia University
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BARRY COMMONER AND THE SECOND LAW OF THERMODYNAMICS

Technology fetishism?

CONTRIBUTED PAPER
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Is the direction of scientific research governed by philosophical assumptions? Scientists sometimes deny that philosophy influences their work, maintaining instead that they are guided strictly objectively by their empirical, experimental approach. Frederick Engels maintained otherwise:

> Natural scientists believe that they free themselves from philosophy by ignoring or abusing it. They cannot, however, make any headway without thought, and for thought they need thought determinations. But they take these categories unreflectingly from the common consciousness of so-called educated persons, which is dominated by the relics of long obsolete philosophies, or from the little bit of philosophy compulsorily listened to at the university (which is not only fragmentary, but also a medley of views of people belonging to the most varied and usually the worst schools), or from uncritical and unsystematic reading of philosophical writings of all kinds. Hence they are no less in bondage, but unfortunately, in most cases to the worst philosophy, and those who abuse philosophy most are slaves to precisely the worst philosophers. *(Dialectics of Nature,* International, 1940, p. 184.)

The misplaced influence of certain philosophical conceptions is often found in science and philosophy of science. In their time Marx and Engels noted how the influence of Newtonian conceptions of motion served to justify a view of the universe as a closed system devoid of any real history and how this view began to give way under the impact of the work of LaPlace, Herschel, and Darwin.[1] Today the generally held conceptions of matter and energy recognize to a degree the richness and diversity of motion and change. However, the failure of many investigators to acquaint themselves with dialectical materialism and its conception of nature may well account for the growth of views inconsistent with this recognition. As an example, I offer Barry Commoner who, in his published book, *The Poverty of Power* (Knopf, 1976), gives what purports to be a scientific analysis of energy resources together with certain social and political conclusions. I focus on this particular work because the reputation of its author as a socially concerned scientist signifies that it is likely to have a great impact on many thoughtful individuals concerned with environmental deterioration and the waste of resources. Marxists and other progressive persons will find Commoner's book particularly interesting because, unlike most other well-known environmentalists, he lays the blame for the problems he discusses squarely at the doorstep of monopoly capitalism. He gives an eloquent and often moving description of capitalist plunder including the imperialistic rape of the third world. In the end he calls for socialism as the only way to an ecologically sound environment. However, as we will try to show, the content of his "socialism" and the scientific analysis upon which it is based is open to some serious questions. Although Commoner makes use of Marx in part of his analysis, it is evident that he is either unfamiliar with or does not choose to make use of the Marxist conception of nature.

Commoner develops his view from the standpoint of the Second Law of Thermodynamics which notes the tendency of energy systems to move towards a state of maximum *unavailable energy* or *entropy*. Applying this principle to the consumption of energy resources, Commoner maintains that such resources are being "irretrievably lost" (p. 26). He goes on to show how the production of most synthetic materials, including petrochemicals, uses up large amounts of energy which cannot be retrieved except at the price of using even greater amounts to retrieve them (pps. 186-94). Because the mass production of such synthetics as plastics is highly profitable, plastics have gradually been substituted for other more "energy-efficient" materials. According to Commoner, the use of synthetics and other modern "energy-intensive" production techniques wastes both capital and labor as well as energy. Capital invested in such systems does not regenerate jobs to the degree possible in less mechanized environments. Although modern production brings higher productivity rates, it accomplishes such while requiring less "participation" of labor in the production process (pps. 226-27).

Commoner advocates a return to more organic production techniques. He states that the economic development of underdeveloped countries lies in their being able to develop their potential for producing natural materials such as cotton and rubber. Growth in this area would be possible if the developed world turned away from its dependence on synthetics (p. 236).

Commoner, despite his claim of disagreement with those who prescribe austerity in the name of ecological limits to growth, nevertheless associates himself with the growing number of bourgeois writers who condemn excessive technology and who hold that the future of the human race depends on convincing masses of people to do with fewer modern conveniences (pps. 232-33).[2] There are obviously many profound political and social implications of this view, yet what is most striking is its seemingly scientific basis from which much of its credibility stems. Rather than discuss the political and social aspects here, let us look at its scientific and technological side.[3]

It should be noted that nowhere in his discussion of the Second Law of Thermodynamics does Commoner indicate any awareness of possible limits in the operation of that law. Although humans can create what Commoner calls "local islands of order", in his view, the law mandates a process of increasing entropy and "disorder" as the "fate of the universe" (pps. 18-28). Other scientists, however, do not quite share Commoner's view. Jacob Bronowski, for example, notes that while a decrease in available energy (an increase in entropy) may be the most "probable" occurrence, the universe is sufficiently vast to accommodate a substantial number of less probable entropy-decreasing activities, such as the development of living things (*The Ascent of Man*, Little Brown, 1973, pps. 347-48). J.D. Bernal points out that entropy can generally be said to increase only in what are called "closed systems" and that the Second Law cannot apply to such "open systems" as living things (*Science in History*, Vol. 3, 1971, pps. 903-4).

It is interesting to note that one of the principal themes developed by Engels in his *Dialectics of Nature* is that the infinite diversity of matter and motion suggested by the progress of science mitigates against the existence of such singular terminal processes as implied by the interpretation of the Second Law adopted by Commoner and others. For Engels, "All motion consists in the interplay of attraction and repulsion (p. 38). Such opposites can never cancel each other out. To suggest that attraction gain the upper hand over repulsion, which is what the total disappearance of available energy would mean, is as inconceivable to Engels as having a magnet with a north pole and no south pole (p. 39). Commenting on the view of Rudolf Clausius, the physicist who is usually credited with formulating the Second Law, Engels suggested that his work had, in view of the principle of conservation of energy, posed the *problem* rather than the solution, namely how does energy in the form of uniformly distributed heat (entropy) become "*utilizable* again" (pps. 157, 201-02, 216). Engels criticizes the conception that *force* is to be treated as an independent phenomenon separate from matter. Rather energy and matter are to be seen as inseparable from each other (p. 168). Engels notes how the "thermal death" interpretation of the Second Law with its implied reliance on the need for an "*impulse from outside*" or an act of creation to start the "world clock" going leads one to a supernatural rather than a scientific account of nature (p. 126).

Engels is concerned throughout his discussion in *Dialectics of Nature* to point out the variety of forms matter and motion take. It has, however, been a common mistake of idealist philosophy of science to stress the quantitative side of physical phenomena to the exclusion of their qualitative aspects. Thus, for example, physical motion has sometimes been reduced to nothing more than the value of variables in an equation. A major portion of Engels' attempt to illustrate the qualitative differentiation of motion is devoted to a discussion of the measure of motion, that is, to *work*. *Work* has both a qualitative as well as quantitative side and is defined by Engels as a "change of form of motion regarded in its quantitative aspect" (p. 72). Yet, in one of his unelaborated notes, Engels points out the tendency to import "the thermodynamical category of work" (that is, a purely quantitative conception of work as simply the expenditure of energy) into physiology and even economics without due attention to the special qualitative features of these areas (pps. 211-12).

Human labor involves, naturally, the expenditure of energy and, in the traditional language of thermodynamics, the *dis-organization* of the environment, but unlike the general situation with sub-human forms of energy consumption, conscious human labor has the potential to extract information from the environment. Barry Commoner, however, falls into the common position of equating information with lack of entropy. Although any *organized* physical system may have the potential of *informing* when appropriate conscious efforts are directed towards it, the system in question is not to be equated with the elements of consciousness which constitute the *information* concerning it. And, although information stored in the brain is indeed a form of highly organized matter which consumes energy in the process of organizing itself, its degree of organization is not to be equated with the degree of organization of whatever external order it may reflect. A materialist perspective requires that we distinguish thought from the external objects it reflects. In this connection the following observation by the Soviet biologist, M. M. Kamshilov, concerning the tendency to confuse the informational and energetic aspects of entropy is instructive:

> Extraction of energy and substance inevitably exhausts the source of substance and of energy. Extraction of information, on the contrary, does not impoverish its source. The reading of a book (extracting information from it) does not turn it into an empty album. Since in life's evolution the decisive role belongs to accumulation of information rather than to the energy aspect or the material aspect, life, accordingly, must not be a disorganizing factor (*Evolution of the Biosphere*, Mir, Moscow, 1976, p. 222).

Just as a fully materialist perspective regarding physical nature requires us not to impose in advance a restricted conception of matter, so this same standpoint requires us to recognize the richness of living organisms and human beings. The potential import of the anti-entropic activities of conscious human beings is given by the fact that humankind is now unlocking the enormous energy potential of the atom.[3] Moreover, new discoveries showing what seems to be a limitless diversity of atomic structure raises serious questions regarding the scope of the Second Law of Thermodynamics. The discovery of atomic power and atomic structures is the product of labor considered in *both* its qualitative and quantitative dimensions.

Since dialectical materialism views nature as an infinitely rich totality of interconnected entities and processes, it holds that any given stage of knowledge is a partial reflection of the material world. Such partial reflections can be continuously refined as active investigation ensues. To the extent that actual external reality is reflected our ideas are objectively true, but to the extent such reflection is partial our ideas are incomplete. Even error must be seen as having an objective relation to reality, as being a distorted and incomplete reflection. Science is therefore to be viewed as equally an *activity* as well as a body of accepted facts. However, just as capitalist productive relations obscure the social nature of production, that is, the fact that the source of wealth lies not in capital but in the collective labor of individuals, so too is the social nature of science and technology "fetishized."[4] Humankind's creations, technological and otherwise, stand "estranged" from their creators and even set in opposition to them. Relations involving both individuals and things are misperceived as being solely relations among things. Marx commented on this problem in the following passage:

> In our days everything seems pregnant with its contrary. Machinery, gifted with the wonderful power of shortening and fructifying human labor, we behold starving and overworking it. ...Even the pure life of science seems unable to shine but on the dark background of ignorance. All our invention and progress seem to result in endowing material forces with intellectual life and in stultifying human life into a material force. ("A Speech in London," in Selsam, Goldway, and Martel, eds., *Dynamics of Social Change*, International, 1970, p. 335.)

The present-day tendency to perceive the effects of a runaway social system, capitalism, as runaway technology instead, is an updated form of the *technology fetishism* of which Marx speaks.

Those like Commoner who write of energy sources and their waste, fail to view human labor itself as the special resource that it is. The refinement of technology includes the refinement of labor, yet as Marx and Engels both show, the development of capitalism and the growth of machinery and technology generate a surplus labor force for which capitalism has no use.[5] Yet, it is not the growth of machinery and technology *per se* that causes such a surplus labor force to be formed, but rather the need of capitalism to exploit labor power in order to extract profits under conditions where an increasing portion of capital is invested in machinery and other means of production and thereby does not generate any additional *surplus value*. As Engels observes, every grown person is capable of producing more than he or she consumes else humankind could not multiply ("Outlines of a Critique of Political Economy," in Marx-Engels, *Collected Works*, Vol. 3, New York, 1975, p. 438.) Moreover, the growth of scientific and technical knowledge increases the efficiency of the laborer. Yet Commoner sees modern productive methods as antagonistic to full employment, holding instead that even a socialist economy ought to rely on primitive labor-intensive production techniques. But how does Commoner basically view labor? His examples suggest that he views it primarily as sheer physical force, just the sort of simplification about which Engels warned.

It is not that Commoner and others sharing his approach should abandon their criticisms of the use and waste of energy under capitalism. Rather, it is necessary to recognize that it is capitalism's need to exploit labor power, not its need to waste energy, which is the driving force. Exploiting labor power under advanced productive conditions negates the development of the full potential of labor. It is against this full potential that the use of energy sources needs to be weighed. Reducing labor to a simple unidirectional expenditure of energy and reducing physical processes to a similar linearity are the fruits of undialectical thinking and technology fetishism. A given conception or model of any natural process may well yield important theoretical and practical results. What dialectical materialism teaches us is not to commit the idealist error of substituting the model for that which it reflects. Much of the "bad" philosophizing concerning science of which Engels spoke consists in making this mistake.

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NOTES

1. For the Marxian conception of science and technology, see Engels *Dialectics of Nature* (esp. Ch. 1) and *Anti-Duhring* (esp. Chs. 5-8), Marx *Capital* (Vol. 1, Ch. 15).

2. For the view that ecology dictates "limits to growth" and general austerity see esp. Donella H. Meadows, et. al., *The Limits to Growth*, Universe Books, 1972, 1974; Paul Ehrlich and Anne Ehrlich, *Population, Resources, Environment: Issues in Human Ecology*, Freeman, 1970, 1972: Robert L. Heilbroner, *An Inquiry into the Human Prospect*, Norton, 1974, 1975. Commoner's claim that he is not advocating a view based on ecological limits is not terribly convincing as his reliance on labor-intensive technology would seem to preclude the use of much modern labor-saving technology. It is on this technology that the modern standard of living and its attendent growth rests.

3. From the Marxist perspective, the view that austerity is a consequence mandated by scientific necessity diverts attention from the role of the capitalist drive for profits as the real source of the pressure for austerity. To his credit Commoner does suggest that it is indeed this drive for profit which is the cause of this pressure and of the waste of resources. However, his recommendation of an absolute cessation of the production of synthetics hardly accords with the general socialist aim of all round improvement of the standard of living and a lightening of the burden of labor. Also, to recommend to under-developed countries that their only path to a higher standard of living lies not through industrialization and the employment of modern technology but rather through the cultivation of labor-intensive industries such as the production of natural fibers would seem to echo the time-worn pronouncements of the colonialist.

4. For Marxist discussions of the social aspect of science, see Engels and Marx, as noted above, also the works of J. D. Bernal: *The Social Function of Science* 1967, and *Science in History* 1971 (both MIT Press); *Science and Industry in the Nineteenth Century* 1970 (Indiana University Press).

5. On the central Marxian notions of surplus-value, labor power, constant and variable capital, see Marx, *Capital* (Vol. 1, Ch. 8) and his pamphlets *Wage, Labour, and Capital* 1953, *Value, Price, and Profit* 1935; also, Engels "Outlines of a Critique of Political Economy," in Marx-Engels *Collected Works* (Vol. 3, 1975, pp. 418-43)—All International, New York.

AUDIENCE INTERACTION

David Schwartzman (Howard University. I have a problem with the often-stated idea that living things violate the Second Law. This debate has been going on for some time and I maintain that you have to consider the organism and its environment as a system. In that system, the entropy will increase.

Lloyd Motz (Columbia University). You have to be certain what kind of system you're talking about. The Second Law applies essentially to an isolated system. In an isolated system entropy at best can remain constant. If there are any irreversible processes, it must increase. Since no living organism is an isolated system, we cannot speak of the entropy of the living organism by itself. We have to consider the entropy of the organism in its environment.

Certainly the total entropy would increase but there's no reason why the entropy of the individual organism cannot decrease at the expense of increasing entropy in the environment.

Morgan. On this matter of a system, one of the problems is not taking into consideration the operation of the infinities involved here—not only infinity in what we might call the horizontal sense of the universal law but also infinity in what I would call the vertical sense of descending to smaller and smaller particles of matter, and so forth. Whereas the law may operate and not be violated in that sense, drawing certain conclusions from the law seems to me a different thing. To postulate the so-called thermal death, to postulate that there is some sort of immediate threat from a process that we can infer from the Second Law seems to me to overlook the infinity of matter. It's the sort of thing that results, I think, when people try to impose a very simple conception of matter.

Motz. The only thing that's important is whether the entropy can still increase. The point is that entropy is increasing. There's nothing wrong with that, as long as you're certain about it. You can never speak about how big the entropy is. That's not the important thing, but whether it can become bigger. And it can always become bigger.

Morgan. Engels, writing before all the work was done, seems to imply that while the entropy is increasing, entropy in various spots must necessarily decrease, that there is a corollary to the Second Law that, as repulsion is overcome by attraction in certain parts of the world, that attraction is overcome by repulsion elsewhere. There are only a few places where he specifically talks about the Second Law, but my impression is that the overall thrust of the work is aimed against just this kind of singular reading of the Second Law.

Tony Dominski (New Haven, Conn.). I work as an ecologist. Commoner gives the best critique in print of energy policy under capitalism. So he's useful with my students. His position vis a vis labor and labor intensive, of course, is crazy. Where this comes out very practically right now is in the case of nuclear energy versus solar energy. Commoner and labor leaders have been arguing that to solarize the economy would put a lot of workers to work and that when the panels were up they would reduce the utility costs for consumers, which is something that we're for. While thinking of solar energy and nuclear energy, and balancing the total capital costs and labor costs and health costs, Commoner comes out for solar power. Now, politically, we have to get public ownership of utilities and then develop an energy policy. But, in the meantime, what position should a progressive scientist take on nuclear vis a vis solar and jobs? Your critique of Com-

moner's work is very timely.

Morgan. We could get off into a whole debate about some of these other energy sources. I'm not sure I agree with you or Commoner on solar energy as a panacea or about nuclear power per se. But I do think that we have to be aware in this debate of a kind of anti-technology that has crept in and influences even progressive people. In other words, if one likes what Commoner says about waste under capitalism, should one therefore accept *scientifically* what Commoner says about nuclear power or solar energy or petrochemicals? Perhaps even under socialism? It bespeaks an anti-Soviet notion of socialism. But that's another big area...

Benjamin DeLeon (Springfield, N. J.). Dialectical materialism, from your viewpoint, seems to be opposed to entropy-death of the universe. Is dialectical materialism then against the big bang theory of creation?

Morgan. The ultimate first big bang or just any big bang?

DeLeon. *The* big bang!

Morgan. There was a comment made before about how we cannot in advance rule out any discovery. Dialectical materialism tells us not to impose any preconception on things, so that if someone were to discover evidence suggesting something like that, I don't think it would refute dialectical materialism. Engels seemed to feel that such things as singular ends and singular beginnings were not really warranted because of the observable evidence for constant opposition within things. So I would be against the idea, but I don't think we really know.

Danny Goldstick (University of Toronto). I think we're in danger of "staying in round rooms so we can't be cornered" and watering down our dialectical materialism just to make sure that whatever happens won't refute it. If something is borne out by empirical evidence—the way constant opposition within things is—we needn't be afraid of asserting it definitely just because it *could* be empirically refuted. On the basis of the evidence to date, dialectical materialism opposes any metaphysical absolutizing of natural phenomena—whether that of the "big bang" or the "universal heat death" or *any* process thought of as going on through all eternity or tending to an absolute end of everything. It seems to me you *can* put your finger on what is undialectical in Commoner and others without having to worry about the fact that your own statements themselves are naturally going to be fallible. On the other hand, let me counter what I have just said—dialectically but not logically (I mean, this won't be logically inconsistent with it)—by saying also that the cosmological sciences which draw out zany consequences from putting a bunch of laws together and absolutizing them for the sake of argument are not at all useless merely because we ought to take with more than a grain of salt the utterly absolutistic consequences so derived. You can learn where you have a problem in your theories by seeing what you get when you put them all together. In fact, the history of science often shows progress to have occurred in the past when people, in a zany enough way, took certain theoretical conceptions seriously and pushed through the formal-logical derivation of their consequences to the very end. What results from such an absolutization can in fact be quite useful scientifically. The only thing, however, is not to altogether *believe* it really.

John J. Dropkin (Brooklyn Polytechnic). I think we have to make a distinction between the consequence *we* draw of the laws of nature and the fact that nature is subject to law. There is no such thing as heat death because other factors come in—but this does not say that there is no such thing as the Second Law of Thermodynamics, out of which heat death has been predicted. Though it is a basic and fundamental law, it may turn out false because we cannot say that we have absolutely exhausted every phase of the determination of natural law. However, the Second Law is one of the real fundamental laws which should not be thrown away just because heat death is thrown away.

Saul Birnbaum (Bronx Community College). I don't think we're throwing it away. The question of entropy and negentropy is dealt with exhaustively by Kuznetsov in the new book, *Philosophy of Optimism.* Another point: in the reference to dialectical materialism viewing nature as an infinitely rich totality, I would add the adjective "ever-changing". Not just infinitely rich but an infinitely rich and ever-changing totality of interconnections.

See Books Rcvd. p. 62.

Schwartzman. In his previous book, *The Closing Circle,* Commoner raised some important ideas that are now being taken very seriously in socialist countries, in particular the need for closed-cycle technology and understanding the impact of advanced technology on the environment. I would go along with you on the question of labor-intensive versus capital-intensive, particularly because, while he does mention in *Poverty of Power* the need to cut the military budget, it's not emphasized. It's quite clear that, if the budget is reordered, we will have all the jobs we can handle in this economy. I think that's symptomatic of the avoidance by the ecological movement of dealing with the big question.

Morgan. Like the anti-nuclear movement. Regardless of what you think of nuclear power, the movement does not place sufficient emphasis on nuclear bombs.

Joel Swartz (University of Illinois School of Public Health). A quick comment on the Second Law. I agree that biological systems don't violate the Second Law because entropy is created in the whole of the environment. Entropy decreases in the organism but there is a *net* increase in entropy. On the other hand, there is something qualitatively different in that you are forming areas where *organization* is increasing. This is not in contradiction with the Second Law. The Second Law doesn't tell you anything about it. Nor do any laws of physics. Presumably, there is some law—of evolution or something else—that is operating. So you get a phenomenon which doesn't violate the Second Law because it's totally different from the Second Law.

Morgan. There are interpretations of the Second Law which possibly would say that ultimately living systems will disappear or at least that they will not come to predominate. But I think that the law is sufficiently vague to accommodate the possibility that there will be no end-point, that living systems will become more and more a major factor. This is not to say that it *will* happen but that it's *not* to be precluded from happening.

Swartz. I notice that a lot of the ecology people, some of whom are progressive, keep dwelling on the Second Law. There was an exchange a year ago in *Scientific World* with J. B. Neilands who kept talking about the Second Law. People from the socialist countries answered by saying this was reductionist... I don't understand why the Second Law is important to the study of ecology or why it's raised to that point.

Dominski. The most useful point that Commoner raised about the Second Law is what engineers say: that you don't use a high quality source of energy like electricity to heat a home where you could use a heat pump that puts more Btu's into your house. He's saying that to design an energy-efficient economy you use energy at the appropriate temperature and that, in a nutshell, is why ecology is concerned.

Morgan. I can certainly agree with Commoner on using the Second Law to make energy choices in a context of limited options, though its use in an "extrapolated" sense is incorrect.

Editor's query: What are the implications for Marxist categories in the work of Nobel laureate Ilya Prigogine on thermodynamics? See "Time, Structure and Fluctuations," *Science* 201:777, 1978, and "Thermodynamics of Evolution," *Physics Today,* Nov. and Dec., 1972. ☐

Unleashing the Intellect

The uncertainty principle is obtained as a practical limitation on the possible precision of measurements. This limitation is not, however, inherent in the conceptual structure of our interpretation.
— David Bohm, Phys Rev 85: 180, 1952.

A study in the dialectics of creativity

ON INTUITION VERSUS DIALECTICAL LOGIC*

Nikolai N. Semyenov
Member of Presidium USSR Academy of Sciences

A physical chemist tells of first-hand experience with the process of scientific discovery

TASS FROM SOVFOTO

Shared 1956 Nobel with Hinshelwood of Oxford

A Philosophical Preface

The birth of dialectical logic is connected with the names of Kant and Hegel. Kant had already demonstrated that the appearance of a contradiction within a scientific concept was not the result of some regrettable error of reasoning, logical carelessness or imprecision, but a very natural state of the human mind at which the mind arrives because it has observed most painstakingly all the postulates and demands of strict formal logic, or definiteness of concepts. Developing this point of view, Hegel began to examine the logical contradiction as an internal motive force of development, as the "motor" of man's cultural development, in the spiritual and theoretical sphere above all.

Marx purged Hegel's dialectics of its idealistic bias and gave it a materialistic interpretation, thereby laying the foundation of materialist dialectics.

*Adapted from an article in *The Scientific and Technological Revolution: Social Effects and Prospects.* Progress, Moscow 1972.

We find that, on the whole, Marx's theoretical thinking ran on the same lines that we observe in the development of natural science, with the one difference that Marx reasoned quite consciously, whereas in natural science the dialectical movement of thought is still mainly spontaneous. Hence the fact that natural scientists very often have an inadequate conception of the true logic of their own reasoning. Not having mastered the system of concepts of dialectical logic, they consider their own actions in terms of inadequate concepts, and this hampers, at the critical points in the development of natural science, their quest for a way out of the blind-alley of contradictions.

Engels was instrumental in developing and applying the Marxist dialectical method to the problems of natural science.

Lenin's behest that we should establish and consolidate an alliance between philosophy and natural science, an alliance equally necessary for both sciences, calls for a clear conception of how they can and must enrich each other.

Further reflection upon this question inevitably suggests the conclusion that the Marxist dialectical method of reasoning is philosophy's most valuable achievement, an achievement it can and must share with natural science. It is from this standpoint that philosophy appears above all as Logic with a capital L, as a theory of knowledge that corresponds with the present level of development of the 20th century natural and socio-historical sciences and their current needs.

Lenin regarded this as the main principle of dialectical materialism. Agreeing with Engels, he stated this concept most emphatically in the following words: "Dialectical materialism 'does not need any philosophy standing above the other sciences'. From previous philosophy there remains 'the science of thought and its laws—formal logic and dialectics'. Dialectics, as understood by Marx, and also in conformity with Hegel, includes what is now called the theory of knowledge, or epistemology."[1]

The categories of materialist dialectics are meaningful; they reflect the objective world with all its contradictions and interrelations. They are not stagnant concepts, but continue to develop and acquire greater meaning. That is why the application of the system of dialectical categories, dialectics as a method of cognition, to various spheres of science stimulates and develops thinking in these sciences and thus leads to the practical transformation of objective reality.

[1] V. I. Lenin, *Collected Works*, Vol. 21, p. 54.

Logic with a capital L (which is, as I have said, Lenin's definition of logic, dialectics and the theory of knowledge as a unity) takes account of the legitimate rights of formal logic. But dialectics, like logic and the theory of knowledge, brings out the true role of formal logic in the development of scientific cognition. The role of formal logic in the advance of cognition is most clearly revealed in mathematics, especially when applied to the processing of data supplied by the other sciences. That is why the relation between logic and mathematics has attracted the attention of both mathematicians and philosophers specialising in logic.

It is rather widely accepted that mathematics is in general identical with formal logic: both mathematics and logic are regarded as a purely formal apparatus of reasoning, as a "language of science" (its "vocabulary" and "syntax"). This view was given its most consistent expression in Bertrand Russell's dictum that logic is the youth of mathematics, and mathematics the maturity of logic. This would be an incorrect view of mathematics, taken as a whole, as a distinct science in its development.

However, in the application of the available mathematical apparatus to the processing of data supplied by other sciences mathematics is indeed a formal-logical apparatus. The apparatus of mathematical logic—precisely because of its purely formal character—has served as the theoretical basis for the creation of modern computing techniques. In principle, all the automated and strictly formalised operations of the human brain without exception can be transferred to a machine, thereby relieving man of a mass of work which requires time rather than intelligence and creative ability. Thus, despite (and, to some extent, thanks to) the circumscribed character of formal logic, its application has had tremendous consequences, which are already engulfing the social sphere as well.

But it is already safe to say that machines will prove powerless in every case involving contradictions which cannot be resolved by purely formal means.

For all the importance of formal logic, it is, however, by no means the main part of Logic with a capital L. Here is the opinion of a group of French mathematicians: "The mode of reasoning which consists in building a chain of syllogisms, is only a transforming mechanism which can be applied regardless of premises.... In other words, it is only the external form ... a language, you might say, which is proper to mathematics and no more. To regulate the vocabulary of this language, to make its syntax more precise, is to do a very useful thing.... But—and we insist—*this is only one side* and the least interesting one at that."[1]

The limited role of formal logic in the development of the sciences springs from its "indifference" both to initial premises and to the composition of the "concepts" subjected to demonstrative exposition (that is, to the "content" side of the matter in general, to the "extralinguistic factors"), and this allows it to be used for the most diverse purposes, some unscientific and essentially retrograde. Let us recall, for instance, that the scholastic interpretation of Aristotle's logic has served theologists as a formal apparatus of reasoning and has been used by them for their unscientific purposes (especially in the Middle Ages). One need give no other examples than the scholastics' struggle against the concepts of Giordano Bruno and Galileo.

These premises, and the concepts of the natural and social sciences which reflect them, arise through the interpretation of experiments, the experience of real human activity, and the practice of transforming nature.

The Nature of Natural Science

The natural sciences study the properties of matter and set themselves the immediate task of helping man to understand the material world. In the past, this entailed improving the active, purposeful, and clearly reproducible contact between man's thinking and the objects of the surrounding world. Only such contact could lead to formation of the basic postulates and concepts in theoretical mechanics, physics and chemistry, and determine the advance of natural science as a whole. By the close of the Renaissance conscious, purposeful contact between thinking and the surrounding world, expressed in the shape of experiment, had developed into the definitive instrument of scientific cognition.

Experiment differs essentially from the contemplation and observation of nature to which the thinkers of ancient Greece mostly confined themselves. Experiment must be purposeful, if it is to wrest from nature the answer to a question formulated according to strict theoretical principles. (The result, admittedly, is sometimes quite unexpected, and instead of an answer, nature sets the scientist another problem.) This means that experiment can play a revolutionary role only where it is closely linked with the development of theoretical thinking. It was this close contact

[1] Nicolas Bourbaki, "L'Architecture des mathematiques. Les grands courants de la Pensée mathematique", *Cahiers du Sud*, 1948, pp. 35-47.

between the development of theoretical thinking and the development of scientific experiment that marked the birth of the natural sciences in the modern meaning of the word. Experimentally verified premises were systematically put into the basis of scientific knowledge. And the subsequent development of science assumed the form of a dramatic dialogue between the existing system of concepts and the data yielded by new experiments.

Theory usually develops in such a manner that a new experiment (or more precisely, the old one, now interpreted) causes, or, rather, brings out and reveals the contradictory situation inherent in the existing system of concepts. This necessitates creative thinking of a kind that formal logic no longer provides, namely, dialectical thinking.

Experiments are usually staged to clarify some particular aspects of theory within the framework of the existing concepts. Such inquiries are very useful for the verification and expansion of theory, and for establishing the conditions for its application in practice. However, they do not go beyond the framework of existing concepts; nor do they lead to revolutionary advances in science. Substantial advances in science follow discoveries that come into conflict with existing systems of concepts. The resolution of such contradictions leads to the emergence of new scientific concepts, which are sometimes epoch-making and revolutionise science as a whole. But much more often experiments are of limited significance, ensuring a substantial advance only with reference to some particular scientific question. Nevertheless, taken together, all these discoveries, major or minor, do in the main determine the revolutionary advance of scientific knowledge.

A Case History in First Person

The Marxist-Leninist theory of knowledge objectively reflects the process of creative scientific endeavour even when that endeavour is spontaneous. When this theory is applied consciously by the scientist, natural science tends to develop at a more rapid rate. This relationship between the scientist's work and the theory of knowledge is much more clearly expressed in the brilliant, though rare works of epoch-making significance.

At the moment, however, I should like to go into this matter with reference to some of the very common "minor" discoveries, confining myself, of course, to those that entail the emergence of a new, albeit specific concept. I intend to cite a concrete example from the history of those "minor" discoveries, and to follow closely the train of thought that

leads from the contradiction brought out in the experiment to the emergence of a new concept. As a rule, scientists, in their treatises, never deal with this preliminary process of reasoning. Purely because of this (and not because I attach any special importance to the experiment), I have chosen as an example one of our early studies, namely, the discovery, between 1925 and 1928, of what is known as limiting phenomena in chemical kinetics and the establishment of the concept of branched chain reactions. This discovery was made by myself and my closest pupils, then very young (among them are the present Academicians Y. Khariton and V. Kondratyev and Corresponding Members of the USSR Academy of Sciences A. Kovalsky and A. Shalnikov).

We were to study the phenomenon of chemiluminescence that occurs during the oxidation of phosphorus vapour by oxygen. To detect optimal light emission, the experiments were carried out under low oxygen pressures. Quite unexpectedly we discovered that with the reduction of the initial pressure of the gas mixture down to a certain pressure P_1 (we called this pressure the lower limit), the mixture completely failed to react and, therefore, to emit any light. In that state it could be kept for days without any signs of a reaction. When the pressure was slightly above this limit, the reaction was very rapid, with intensive emission of light. The rapid reaction at pressure above the limit came to a complete halt as soon as the pressure of the reactive mixture fell to a certain residual level, slightly below the lower limit.

We observed the same phenomena with various other mixtures of oxygen and various combustible gases. The value of the lower limit proved to be dependent on a number of other parameters besides pressure, such as temperature, the radius of the vessel, dilution of the combustible mixture with the inert gas argon, and the number of active admixtures slowing down the reaction. Each of these parameters, when varied smoothly under constant pressure and constant other parameters, has its own limiting value, separating the region of the very rapid reaction from that of chemical inertness.

A phenomenon outwardly similar to our discovery was already known. This is the spontaneous explosion of combustible gases when the temperature rises above a certain critical point. We found it necessary to make a special study of this phenomenon. It turned out that the mixture reacted mildly at a slow, but entirely measurable speed, which increased with a rise in temperature. When the temperature reached a certain critical value an explosion followed. It was found that at a constant temperature there is a critical pressure and even a critical vessel size. In other words, everything looked very much like the discovery I have described.

Our group then began to study the causes of this phenomenon and so arrived at the theory of thermal explosion. It showed that this type of explosion had nothing in common with the limiting phenomena we had observed in the phosphorus oxidation type of reaction. An attempt to dismiss our phenomenon as a thermal avalanche failed (although the thermal explosion theory paved the way for the general theory of combustion and explosion).

Consequently, in our work on phosphorus oxidation we had discovered some absolutely new and unusual phenomena in chemical kinetics which could be called "all-or-nothing" phenomena, with a marked boundary between them. These phenomena were in basic contradiction to all the fundamental propositions of chemical kinetics of the time, which held above all that the rates of all chemical reactions varied smoothly with temperature and pressure, in accordance with certain universal regularities.

Our first report, published in early 1926, was sharply criticised by the eminent German Professor Bodenstein, then the doyen of chemical kinetics. He wrote that our results were impossible theoretically and contained gross methodological errors experimentally.

We had to go back to our experiment and eliminate all the methodological errors pointed out by Bodenstein. In 1927, we published another and longer article which confirmed and enlarged on the 1925 experiments. Bodenstein thereupon withdrew all his objections, first in a private letter, and then in a public statement. The new facts could be considered well established. The contradiction between them and the existing concepts in chemical kinetics stood out with ample clarity.

Since we had no idea how to resolve this contradiction, we turned to experiment once again to establish with the utmost precision the empirical regularities of limiting phenomena, mathematically expressed. We discovered that all these regularities fitted into the rather simple formula $\Phi\delta = 1$, where δ is the value characteristic of each type of reaction, and Φ is any given, fairly simple combination of the parameters mentioned above (pressure, temperature, vessel radius, etc.). At first, this provided no explanation of the phenomenon in question.

In our case, the molecules of oxygen and phosphorus below the given pressure limit were inert in respect of each other. This could naturally be attributed to the high energy of activation and the low temperature of the experiment. But this implied that such a reaction ought not to occur even above the limit. Consequently, the rapid reaction above the limit, which we had actually observed, had an entirely different mechanism. At this point, we recalled Bodenstein's remarkable discovery, made between 1913 and 1916 in his study of the photochemical reaction producing HCl from the gaseous H_2 and Cl_2. He had demonstrated that for each quantum of light absorbed by a Cl_2 molecule there was produced up to one million HCl molecules (known as the quantum yield), instead of a pair of molecules, as Einstein's formula had suggested and as had frequently been confirmed in experiments with other photochemical reactions. Bodenstein had called this remarkable phenomenon a "*chain* reaction". After three years of search and failure, Nernst and Bodenstein produced a correct reaction mechanism, which was a brilliant description of all the experimentally discovered kinetic regularities. It introduced into chemical kinetics for the first time the concept of particles with a high reactivity—free atoms and radicals—which are produced when one of the bonds of a molecule is ruptured.

Mechanism of $H_2 + Cl_2$ Reaction

Cl_2 + quantum of light $\longrightarrow Cl + Cl$ } generation of chain

$Cl + H_2 \longrightarrow HCl + H$
$H + Cl_2 \longrightarrow HCl + Cl$ } chain propagation
$Cl + H_2 \longrightarrow HCl + H$

etc.

$Cl + Cl \longrightarrow Cl_2$ } chain termination
Cl + active admixture

$$H_2 + Cl_2 = 2\,HCl$$

Fig. 1

From the kinetic regularities developed by Bodenstein, it is easy to determine the length of the chain ν, i.e., the value proportional to the rate of the reaction, which is made up of the number of elementary reactions in the hain from its generation to its termination. Both experimentally and theoretically, this value varies smoothly with the variations of all the parameters, and that is why it was of no direct assistance to us in explaining our limiting phenomena, which were characterised by a sharp difference in reaction rates.

Nevertheless, we were haunted by the idea or rather, a vague feeling, that the phosphorus oxidation reaction was somehow connected with the concept of Bodenstein's chain reaction.

In our experiments, we were amazed by the fact that the limiting pressure depended on such parameters as the vessel diameter or the pressure of the inert gas admixture, which, seemingly, should have nothing to do with the elementary steps of reactions. According to our experiments, the limiting pressure P_1 was inversely proportional to the square of the vessel diameter. Here one could make the purely mental experiment of enlarging the vessel ad infinitum. In that case the limiting pressure would tend towards zero. In other words, the limiting pressure would disappear, and the rapid reaction would occur under negligible pressures. This meant that the development of the reaction was hindered by the walls of the vessel. It then occurred to us that the vessel diameter might exert a similar effect on Bodenstein's chain reaction as well. If Bodenstein's chains could be terminated as a result of the capture of atoms and radicals by active admixtures, there was the probability—greater or lesser—that they would be captured by the vessel's walls, through chemosorption. This type of termination of Bodenstein's chains should naturally appear under reduced pressures, when termination in the gas phase was less intensive. This called for an experiment to study the dependence of the rate of the photochemical reaction of hydrogen and chlorine on vessel diameter and pressure. In 1928, these experiments fully bore out our hypothesis. The termination of chains on walls later turned out to be common for all chain reactions.

Towards the end of 1927 we adopted our hypothesis as a basis, without waiting for the results of these experiments. Under this assumption it was not hard to find a mathematical expression for Bodenstein's length of chain. It was at this point that we unexpectedly realised that the combination of parameters Φ in our empirical expression for limiting phenomena was identical with the expression ν, provided the chains were terminated on the vessel walls. A connection between our discovery and Bodenstein's chain reactions became increasingly probable. Our empirical equation $\Phi\delta^2 = 1$ could thus be rewritten as $\nu\delta = 1$. This, as I have said, did not, of course, directly lead us to the solution of the main question. For the length of chain in the Bodenstein-type reaction varied quite smoothly with the diameter, whereas we had a critical diameter d_1 (P_1 remaining constant) below which the reaction did not occur at all, while developing very rapidly above it.

Psychologically, the benefits, however, were very great. The contradiction had become even more precise and acute. If earlier we had had to discover the reason why the reaction could reveal limiting phenomena, we were now faced with the question: why was it possible for a chain reaction, capable of termination at the vessel wall, to show limiting phenomena? The sum of our reasoning and experimenting suggested a one-way path at the end of which, and nowhere else, lay the answer.

The Myth About Intuition

At this point, we had a flash of inspiration, seemingly intuitive, though in the light of what had gone before we cannot call it a revelation, for it had been prepared by everything I have described above. When a scientist writes about his discovery he is usually hesitant about revealing the personal aspects of the quest which led to the emergence of a new basic concept. He usually begins with that concept. Hence the myth about intuition, in which he himself may later come to believe.

The really important thing in epistemology, however, is the description of the scientist's preparatory mental work, for that is based on a study of the whole history of thought, beginning with the ancient Greeks. But the Greeks were much less inhibited about describing their process of reasoning than our modern scientists are. Perhaps we should change this; at least, I shall now try to do so. What interests me particularly is the meaning of the vague concept of intuition in the light of dialectics.

I wish I could recall what I was thinking just before that flash of inspiration. I may have been thinking that the properties of the free atoms and radicals in Bodenstein's chains were analogous to the actions of bacteria, which, so to speak, swallow up the original molecules, turning them into the products of the reaction. Suddenly it occurred to me that bacteria were able not only to eat but also to multiply. Just a minute, I said to myself. What if the free atoms and radicals were also capable of multiplying? There it was: there was the answer!

This culminating point set me arguing with myself. Why, I asked myself, should they be capable of multiplying at all? That would call for the appearance of more than one radical in the given elementary act of development of Bodenstein's chain. There should be at least one more or, rather, two more, because in the final count the whole thing comes to dissociation of a molecule into two free radicals. But dissociation requires sufficient energy. Where does that come from? Well, coincident with the elementary reaction there could be a release of a large amount of energy which some time later, a very short time later, it is true, is diffused into heat. But before that happens it could be used, like a quantum of light, to dissociate a molecule of the initial substance, thereby causing a branching of the chain. But how, precisely? That, I decided, could wait. I was sure that the answer to the contradiction lay in the possibility of the chain's branching.

I do not recall exactly how it was; it may have actually been by analogy with bacteria. In Newton's case we are told it was a falling apple. In other cases it was something else. That is not so important. If a gun is loaded and you play about with it long enough, something will cause it to go off. What mattered was the long train of thought that came before, which clearly brought out and sharpened the contradictions, and not what actually triggered that flash of inspiration.

Now that we had our answer, the task was to formulate our hypothesis properly. Let us assume, then, that each link of Bodenstein's chain may produce with probability δ a branching, giving a secondary Bodenstein chain. In that case, over the whole length of the Bodenstein chain, consisting of ν links, there will appear $\nu\delta$ new chains. This will apply not only to the primary but also to the secondary chains generated in the branching. The expression $\nu\delta = 1$ which determines the limit, means that each Bodenstein chain with a length ν, when terminated, produces an average of one branching which starts a secondary chain, etc. Every termination of the chain is compensat :d by one branching, making the chain as a whole infinite, so to speak.

Let us assume that we inject into each cubic centimetre of gas one primary free radical to start such an infinite chain. Taking τ to designate the time of the radical's entry into each elementary reaction, we find that we have $\frac{1}{\tau}$ reactions a second. In t seconds we shall have $X = \frac{t}{\tau}$ = molecules of the initial substances reacting. Owing to the great reactivity of atoms and radicals, τ is usually small.

Let us take, for example, $\tau = 10^{-3}$ sec. Assuming further that the pressure $P_1 = 1/100$ atm., i.e., $3 \cdot 10^{17}$ initial molecules in a cubic centimetre, let us calculate the time it will take to bring the reaction up to 30 per cent of conversion: $X_{30\%} = 10^{17} = 10^3 t$; hence, $t_{30\%} = 10^{14}$ sec $\approx$ 3 million years (see Fig. 2).

Let us now assume the initial gas pressure to be above the limiting pressure. In that case, more than one branching will arise on each section of Bodenstein's chain. More chains will originate than are destroyed. As a result, one primary free radical admitted into the gas will produce a chain

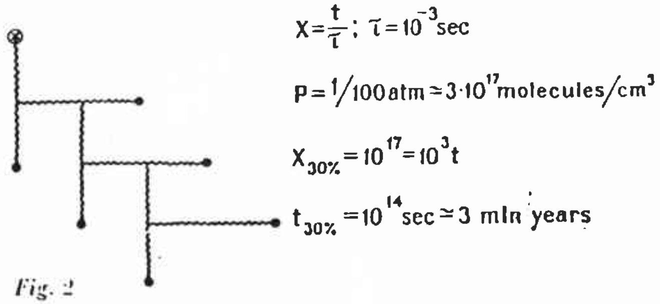

Fig. 2

avalanche, in accordance with the $Ae^{\varphi t}$ law, where the reproduction coefficient φ is proportional to the difference $\delta\nu - 1$ and inversely proportional to the length of Bodenstein's chain ν and time τ.

Even with a minor change in the initial pressure of the mixture, say, by 1 per cent, above the limit and respectively with $\nu\delta - 1 = 0.01$, the avalanche will develop so rapidly, that 30 per cent of the substance will react in roughly 4 minutes (see Fig. 3).

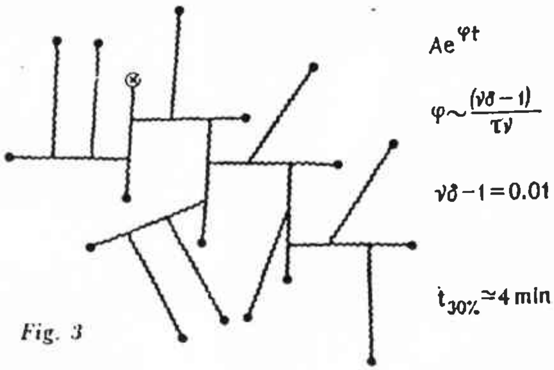

Fig. 3

Naturally enough, below the limit, when $\nu\delta < 1$, and the number of terminations is in excess of the number of branchings, the admission of one radical cannot result in a reaction at all and the incipient chain will quickly be extinguished.

However, in most cases, the reaction above the limit proceeds even faster. After all, the majority of rapid branching chain reactions proceed quite differently from the long Bodenstein chains with rare branchings. The branchings take place virtually on every link of the chain; the chains turn out to be almost continuously branched (see Fig. 4).

In fact, the concept of Bodenstein's chain disappears altogether. The amazing thing is that this concept, which was

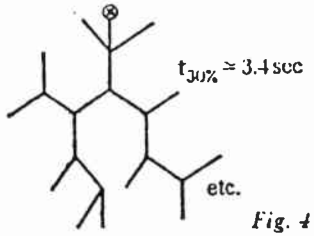

Fig. 4

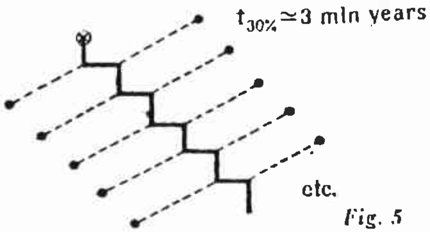

Fig. 5

such a great help in solving the contradiction we had discovered, turned out in the end to be irrelevant.

In the continuously branching chain reaction the propagation of the chain is automatically connected with its branching. Each free atom or radical is capable either of disappearing (termination of chain) or propagating the chain, as it enters the reaction. This being so, it is simple to find the conditions for the limits and the rate of the chain avalanche development, both above and below the limit, where pressure varies by ± 1 per cent.

We find that here again there arises at the limit something like a single infinite chain (see Fig. 5), so that the time $t_{30\%}$ continues to be 3 million years. The reaction above the limit will develop in accordance with the avalanche law $e^{\varphi t}$, but here φ will be higher than for chains with rare branchings.

If in this case we increase the initial pressure by 1 per cent above the limit P_1, the admission of one primary free atom or radical into one cubic centimetre of gas will produce a reaction which takes not 4 minutes, as we have seen above, but roughly 3 seconds, instead of millions of years at the limit.

We have examined an ideal case, where the spontaneous origination of free atoms in a reactive gas does not take place at all, or takes place very rarely, and where the reaction is started by admitting at least one free radical from outside.

If η_0 such primary free radicals appear in each cubic centimetre in a second, it is easy to produce an expression for the rate of the branched chain reaction, and the quantity of molecules X which have reacted in time t, as has been done above. The results are shown in the following table.

	$\eta_0 = 1$ free radical/sec.cm^3	$\eta_0 = 100$ free radical/sec.cm^3
$P = P_1$	$t_{30\%} \simeq 1$ year	$t_{30\%} \simeq 2$ weeks
$P = 1.01\,P_1$	$t_{30\%} \simeq 4$ sec	$t_{30\%} \simeq 3$ sec
$P = 0.99\,P_1$	$t_{30\%} \simeq 30{,}000$ years	$t_{30\%} \simeq 300$ years

Consequently, our hypothesis furnishes a good explanation why some chemical reactions are based on the "all-or-nothing" principle.

It would be wrong to assume that once the hypothesis was clearly formulated our work was over. On the contrary, that was when it really began. And it is still going on, in a measure. Step by step the theory has been given increasing precision and clarity, becoming a quantitative theory that has predictive power.

So it was that we succeeded in establishing the occurrence of two types of avalanche-like chemical processes which, in certain conditions, lead to explosion: the thermal explosion, which arises as a result of the build-up of the thermal avalanche, which I have dealt with here in brief, and the chain explosion, which results from the avalanche-like reproduction of active chemical particles and free atoms and radicals, whose concentration in the development of the chain avalanche attains, theoretically and experimentally, tremendous values comparable with the concentrations of primary molecules. It later turned out that only these two types of explosion occur in nature. Even in atomic physics, blasts can be only of the chain type (atomic explosion) or of the thermal type (thermonuclear explosion).

I should like to add another remark in connection with my analysis of this discovery. Experiments have shown that the general regularities governing branched chain reactions, notably the "all-or-nothing" limit itself, and the development of chains in time depend very little on the actual mechanism of these reactions. The important fact is that they branch out and that chains terminate. Quantitatively, these regularities also depend on certain constants which can be determined, to their first approximation, from experiments according to the optimal values.

This applies not only to chemical branched chain reactions but also to physical reactions, which include nuclear fission chain reactions and also, in essence, the reactions of multiplication of light quanta in lasers and masers. In the former, the active particles of the chain are the excited compound-nuclei, which arise in the capture of neutrons by the atomic nuclei of active substances, and the neutrons emitted, numbering three for every act of fission of the compound-nucleus. The termination of the chain takes place through the emergence of neutrons beyond the limits of the active body (similar to the termination of the chain on the wall) or the capture of neutrons by certain admixtures. In density, dimensions, admixtures of active substances, dilution with U-238, etc., limiting phenomena are identical with those which are observed in chemistry. The formation of vast quantities of neutrons in the course of the reaction, the exponential growth of the rate of reaction in time during an atomic blast are similar to the corresponding phenomena in chemical branched chain reactions. The "all-or-nothing" principle is here manifested in an especially clear-cut form, and in essence provides the sole basis on which one can build atomic bombs and piles without fear of explosion, and set off an explosion through the insignificant alteration of one of the parameters. The formal relations in our insignificant-scale chemical phenomena remain true even for these powerful reactions. From the analysis of this discovery it can be seen that it is not so much the confirmation of existing concepts that is of especial importance to scientific cognition as the emergence of concepts contradicting the former. These contradictions are the main stimulus in the development of science. It is a gift of fortune for a scientist to come up against a contradiction, major or minor, and he should seize upon it. Yet, it is so easy to overlook it, to brush it off, especially when a deadline looms for the publication of an article or the presentation of a thesis.

Discussion in General

Let us now pass from "minor" to "major" contradictions, which reveal much more strikingly the principal logical components of scientific discoveries at the time of decisive scientific revolutions. When it comes to changes in the basic scientific concepts supporting the whole edifice of a theoretical system, not only the concepts themselves change but also the logic of thinking itself, the very understanding of what is "logical" and what is "illogical". Here is what Max Born says about it: "The situation here (in quantum mechanics—*N.S.*) is so confused that the only option is this: either to rest content with the feeble adaptation of concepts to the system of formulas ... or to modify the rules of thinking, of logic itself."[1]

At such moments, the theoretical physicist begins to work as a pure logician, as a transformer of Logic. He has to work in the sphere of such contradictory concepts as continuity and discontinuity, interconnection and establishment, time and space, probability and necessity; for the specific purposes of natural science he must modify, develop and review the primary logical categories. This is a subtle business and

[1] Max Born, "Bemerkungen zur statistischen Deutung der Quantenmechanik", *Werner Heisenberg und die Physik unserer Zeit*, Braunschweig, 1961, S. 106.

in the absence of sufficient erudition and philosophical training it is very easy to repeat former mistakes. Engels used to say that materialism changes its form with every great new discovery in science. This is where a developed and properly mastered logic of historico-philosophical thinking ceases to be luxury, a mere pleasant supplement to a training in natural science, and becomes a matter of primary and most acute necessity. One such revolutionary logician was Einstein when he worked out his theory of relativity (revising the concept of time as a logical concept). Another leading revolutionary in logic was Niels Bohr, who, to all intents and purposes, was the founder of the modern quantum theory. His quantum theory of the atom emerged as a result of a bold resolution of the contradiction between Rutherford's planetary model of the atom, introduced virtually straight from the experiment, and classical electrodynamics.

Bohr's principle of complementarity went even further in revolutionising the logic of physical cognition, for it tacitly introduced into the very structure of physical theory the idea of contradiction; at the same time, Bohr's conception of the fundamental epistemological importance of the "instrument-object" relationship to some extent corresponded to Marx's conception of the active epistemological role of the instrument in the cognition of things.

The dualism of wave and corpuscular concepts, discovered by Planck and Einstein in respect of light, was taken by de Broglie to be a *universal* contradiction of microobjects and applied to a description of the motion of electrons (which was shortly confirmed experimentally). De Broglie's conception was one of the sources that gave rise to quantum mechanics.

Our last example is Dirac's anticipation of the positron. Attempts to unite quantum mechanics and Einstein's relativistic mechanics had run into the difficulty of having to recognise the existence (because there were expressions containing a square root) of particles carrying a plus and a minus sign, i.e., positive and negative energy. But particles with negative energy seemed to be an absurdity, pure nonsense. It was, therefore, necessary to invent a principle which would rule out their existence in nature and which at the same time would admit the possibility of their existence. The contradiction was formulated with the utmost logical incision. Using Pauli's exclusion principle, Dirac introduced his "holes in a vacuum" concept (a vacuum filled with a vast number of what were virtually electrons in a state of negative energy). This somewhat obscure concept, literally invented on the basis of a most strictly formulated antinomy, was then concentrated into the rational concept of a fully material particle, "the electron with a positive charge", i.e., the positron. But the initial vague and even logically contradictory concept was in fact the nutrient medium, so to speak, which produced not only the concept of the positron but modern relativistic quantum mechanics as a whole in its new and even more striking but, I regret to say, not so stringently formulated antinomies.

The example of Dirac's discovery is a very characteristic one and provides a summing up, as it were, of the creative process of theoretical thinking. From this example, or, rather, this food for thought, one can obtain the clearest possible picture of some of the primary concepts of dialectical logic.

As for the significance of these concepts, let us see whether it is possible to define the actual process of scientific endeavour (say, in the aspects dealt with above) as a process subject to the laws of Logic with a capital L. Every scientist knows that theoretical work is anything but a smooth movement forward and only forward. It may appear to be so from afar, just as the Earth, for example, appears to be an ideal geometric sphere when viewed from outer space, but certainly not to a mountaineer climbing Mount Everest.

The harder a scientist tries to recall "how it all actually happened", the stronger is his impression that it is in general quite impossible to discover any "rational" principle or logic in the development of scientific knowledge and that this development is governed by nothing more than the whims of unrestrained will with its "mad notions". Thus Louis de Broglie writes in his *Paths of Science*: "Human science, essentially rational in its principles and in its methods, cannot achieve its most remarkable conquests except by executing sudden and perilous leaps of the mind, involving the play of faculties called imagination, intuition and perception, released from the hard constraints of rigorous reasoning. Let us perhaps say that the scientist carries out the rational analysis and goes over link by link of the chain of his deductions; he is bound by this chain up to a point where he suddenly escapes from it, and the liberty of his imagination, once recaptured, enables him to look out onto new horizons."[1]

This, one might say, makes it clear that formal mathematical logic, while being an effective and invaluable instrument for the solution of tasks of a definite type, proves to be powerless when it comes to explaining the actual process of scientific work leading to the production of new concepts.

[1] Louis de Broglie, *Sur les sentiers de la science*, Paris, 1960, p. 354.

If we assume that scientific thinking is "logical" and "rational" only insofar as it proceeds in strict accord with the axioms, postulates and theorems of formal mathematical logic, the scientific thinking that actually takes place inevitably seems to be irrational, so that science itself appears to be a madhouse where only superficial order is maintained by the logician-attendants but by no means by the inmates, whose sole aim is to disrupt it.

If this were so, the whole theory of scientific knowledge would prove to be a purely outward and absolutely inexplicable fusion of two different and irreconcilable sciences—formal mathematical logic and the purely psychological description of intuition.

Dialectics of the Hypothesis

It would appear that there ought to be trends in science that would provide an exact and specific description of certain universal laws governing the process of scientific reasoning. From the viewpoint of dialectics it is clear that these so-called "mad notions" are essential, logical processes of reasoning.

In fact, whenever the result of a new experiment (or a more thorough analysis of a previous experiment) leads to a basic contradiction within the system of existing concepts, that very contradiction constitutes a determination of the conditions engendering a *hypothesis*; that is to say, the contradiction prescribes a vector of reasoning in the formation of the *hypothesis*.

In short, in any given case we may expect to find the following "mechanism". At first the contradiction within the old theory appears to be rather generalised and vague. Clearly, the new experimental fact, if and insofar as it is understood, contradicts the old theory and the old concepts in general; but it is far from clear where the apex of the contradiction is located and *exactly what* old key principle has to be modified. Gradually, as a result of further experiments and of the refinement of the old concepts themselves (no such refinement had been needed before), the contradiction is sharpened and narrowed down until it becomes apparent *exactly which* old concept must be modified first. The contradiction acquires the acuteness of an antinomy formulated with the utmost stringency. But this is also a formulation, although only implied, of a negative definition, as it were, of the new concept. It then remains to understand the *positive* content of the new concept, to define it not only as a clearly formulated question, but also as an answer, as a new concept. The new concept is usually a qualitative and basic reformulation of the old initial concept, though it is simultaneously the embryonic form of a new theoretical system. Such is the origin of a hypothesis.

At this point the following new logical cycle begins. Parallel with the verification and confirmation of the hypothesis in the course of countless experimental variations and mathematical concretisation (let us assume that our hypothesis is correct) there follows an examination and enrichment, as it were, of the initial concept alone, and its articulation into a series of interrelated, more specific auxiliary concepts, with the result that the hypothesis develops into a detailed, experimentally verified theory.

From the concrete material I have introduced into this article it will be seen that this logical picture is a legitimate idealisation of the specific process of creative reasoning.

The initial contradiction which destroys the old theory is thereby resolved ("removed") within the new, more profound and specific theoretical understanding which includes the old theory as a particular limiting case. Intuition, thus understood, emerges as nothing else but the form in which a perfectly rational process of reasoning takes effect. The contradiction, therefore, destroys not the theory in general, but only the old, limited theory, or, to be more precise, the illusion that the old theory was a final, complete and concrete ("absolutely true") reflection of objective reality. The contradiction brings out the nodal points within the system of the old theory, in which its *growth points* are concentrated and in which its ability to "grow through contradiction" becomes apparent. Moreover, it is the strictest and most formally perfect movement of thought that arrives at those growth points in which the basic (dialectical) contradiction begins to show, a contradiction which confronts intuition with the task of constructing a hypothesis, that is, reaching a point beyond which any purely formal movement becomes impossible.

We have tried, by analysing the process of scientific discovery, to show how dialectical logic works and how it helps the scientist understand and refine the actual process of creative scientific thinking.

* * *

Marxist in his physics and history of science as well as in politics

J. D. BERNAL (1901-1971)

(QUOTED WITHOUT COMMENT)

The Insufficiency of Physical Theory

[A]t the moment [cosmological] observations, experiments, and theories are in such a state of flux that all that seems established is that the universe has a history. In tracing it out as much is likely to be learned about the nature of matter and radiation as about the distant heavens. Indeed, the new discoveries, especially those of the fundamental particles and their transformation, have put a very considerable strain on existing physical theories, especially those of the laws of interaction of elementary particles and of the constitution of the nuclei. Such theories as exist – and it must be admitted that for many phenomena there are no theories – are built on *ad hoc* analogies with the quantum theory applied to the much stronger forces and smaller distances in nuclear physics. Involving 'cloudy crystal ball' models, 'magic numbers', and 'strangeness' quantum numbers, they even have a somewhat magical – cabalistic – flavour.

It may well be, however, that a far more radical revision of the relativity and quantum theories needs to be made, not by tinkering with the present theories while accepting the assumptions that underlie them, but rather by making a fundamental attack on their logical and philosophical bases. It was in this way that the older theories were overthrown, first by the accumulation of material experimental evidence which they could not explain, and secondly by questioning the bases of the arguments which had led to the classical theory. Any new theory must of course account for all or most of the existing facts, but it will be accepted only if besides explaining them it serves to link together more successfully even wider fields of experience.

We are just entering a new phase of criticism 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. Though the new theories are various they have common aims. One is to generalize a field theory that will unite the hitherto disparate relativity and quantum theories. Another is to remove the basic indeterminacy of the new quantum theory of 1925 especially associated with Bohr and Heisenberg. The victory will go to whoever can explain satisfactorily the new and fuller range of physical phenomena, the intranuclear forces, and the behaviour of the range of ephemeral and protean particles. It is too early to say what will ultimately emerge, but it is bound to be very different from the accepted orthodoxy of the last forty years.

— *Science in History*, Cambridge, Mass. 1971, Volume 3, pp. 772-73.

Detente in Psychobiology at U.S.-Soviet Meeting on Irvine Campus

His name loomed largest

"Science is supposed to be free of ideology," writes Constance Holden in *Science* 12 May 1978, "but a subject with such far-reaching implications as the study of brain and behavior is hard to keep totally clear of political perspectives." She quotes one participant at a recent Soviet-American conference on experimental psychology as saying: "Understanding the neural and behavioral sciences touches on our philosophies of life and science in general." Noting a strong deterministic and environmentalist cast in the Soviet approach, reflecting a political philosophy that human beings are shaped by society, Holden concludes that "this does not mean the Russians are not doing good science," though Americans tend to find an "imbalance in their interests."

The name looming largest at the meeting (at University of California at Irvine) turned out to be that of Ivan Petrovich Pavlov (1849-1936): American participants were surprised to learn how much the Russian physiologist and Nobel laureate still dominates Soviet psychology, said the report.

The standard-bearer for Pavlov was E. A. Asratyan, observed Holden. He is the 74-year-old Armenian-born director of the Institute of Higher Nervous Activity and Neurophysiology, who escaped the Turkish massacre of 1915 by disguising himself as a girl and fled to Russia where he was educated and spent 10 years in Pavlov's laboratory. His role at the meeting was characterized by Holden as follows:

> A staunch Communist of enormous energy and cordiality, he lost no opportunity to propangadize his fellow conferees. Whenever he managed to score a point for the Soviet Union he cried "Dialectic!" and laughed hugely. Similarly, he would jump up after a paper had been presented and declaim (to the amusement of the audience), "Thank you for showing Pavlov right."

A new generation of Post-Pavlovian thought has come to the fore, Michael Patterson of the University of Ohio told Holden. The difference is that a Pavlovian sees conditioned reflexes as the basis for all behavior while the followers of P. K. Anokhin, who died a few years ago, see reflexes as one level of organization in a hierarchy of "functional systems." They view every response in the brain as resulting from an interaction between selected aspects of the environment and an internal representation of a "goal." (Title of the conference, "Neurophysiological Mechanisms of Goal-Directed Behavior and Learning," reflected this Soviet slant.)

The Soviets regard their approach to brain functions as more "holistic" than that of the Americans, says Holden:

> They [the Soviet conferees] talk much of systems and little of synapses... They put great emphasis on the interaction between organism and environment in the formation of these systems. They have a fondness for grand theories into which to fit their findings. Americans, on the other hand, are more molecularly oriented, more empirical, and wary of global hypotheses...
>
> E. Roy John, one of America's few global hypothesizers, stirred a great deal of interest among the Russians. He says the activities of single neurons have no meaning except as they contribute to larger neuronal "ensembles," which in learning become parts of larger "representational systems," ... John said he was "delighted at the Soviet response to my work" and had not been aware until now that he and the Russians were doing work that confirmed each other's.

During the eight days of intensive intellectual and personal contact, the initial skepticism of some American participants is reported to have vanished. For example, John Lacey of Fels Research Institute commented icily on arrival: "They haven't got a thing to teach us so far as I'm concerned. They're not doing a thing we're not doing and they're doing some things far worse." At the end, though his basic assessment had not changed, his attitude had been transformed by finding the quality of most papers "astoundingly good." Moreover, Lacey said: "I've changed my negative view of meetings – that all they do is take research money," and now he is all in favor of more detente-type activities.

Richard Thompson, chairman of Irvine's psychobiology department, was reportedly ecstatic over the outcome of the conference he hosted and looking into possibilities of exchanging scientists between the Irvine and Soviet laboratories. □

Before Sociobiology

The fascist philosophers ... recognise at bottom no categories other than those of biology. National imperialist ambition is to be founded on theories of racial superiority ... purely biological. The totalitarian principle depends on the analogy between society and a metazoan organism or between society and a hive of colonial hymenopters.

– Joseph Needham, in Foreword of Biology and Marxism *by Marcel Prenant, New York 1938, p. ix.*

THE CUSP CATASTROPHE AND DIALECTICS*

Martin Zwick

A mathematical interpretation of the classical principles of dialectics can be found in the newly developed Catastrophe Theory of René Thom (1975) and E.C. Zeeman (1976, 1977). Thom's theory describes seven ways ("elementary catastrophes") in which continuously-varying causes can produce discontinuous effects. This note considers only the "cusp" catastrophe, in which one effect ("behavior") variable depends upon two causal ("control") parameters.

The cusp can be illustrated with the model shown in Figure 1a. Two rubber bands are attached to the periphery of a disk which rotates about its center. The other end of one rubber band is fixed while the second has a free end (the "control point"). The disk rotation angle (x) is the behavior variable, and the control parameters, known as "conflicting factors," are the coordinates (a, b) of the control point. If this point is moved across the cusp-shaped "bifurcation set" from point 1 to point 7, thereby decreasing the parameter a, and increasing b, the disk angle will change gradually until point 6, where it will undergo a discontinuous rotational jump ("catastrophe").

Figure 1b explains what is happening. The system is assumed always to be at equilibrium in a local minimum (initially, α) of some energy function, E. As the control point moves, the function changes. At point 2, i. e., upon entry into the bifurcation set, an inflection point appears, which, at 3, deepens into a second minimum, β. This minimum corresponds to a different equilibrium value for the angular variable x. At point 4, the depth of the two minima of E are equal, but at 5, the second is deeper and thus preferred. But if the control point moves gradually, the system does not switch to the new state until its own local minimum disappears (6). This is called "delay." If one reverses the direction of motion, the disk remains in minimum β until point 2, where it undergoes a discontinuous flip to α. Thus, on either side of the bifurcation set (1 or 7) there is one minimum. Inside the bifurcation set, the system is "bimodal," i.e., it can occupy *either* minimum, depending upon the previous motion of the control point.

* The present note, a substantially abridged version of (Zwick, 1978), is intended only as a brief introductory sketch. The reader should be cautioned that a quite lively controversy now exists over the validity of many applications of catastrophe theory.

Figure 1c represents another perspective. As the control point moves across the bifurcation set, a "behavior point" moves on a folded "behavior surface" directly above it. If the control point passes both boundaries of the bifurcation set, as shown, discontinuous jumps of the behavior point will occur.

An example of the cusp is the phenomenon of phase transitions (Poston & Stewart, 1978), where pressure and temperature are the conflicting factors, density is the behavioral variable, and the two modes are the gas and liquid phases. (In this phenomenon, the delay does not usually extend to the far boundary of the bifurcation set.)

Quantity and Quality. The relationship of the cusp catastrophe to dialectics should be fairly transparent. The first dialectical law, "the transformation of quantity into quality," is close to, though not synonymous with, the transformation of continuous causes into discontinuous effects.

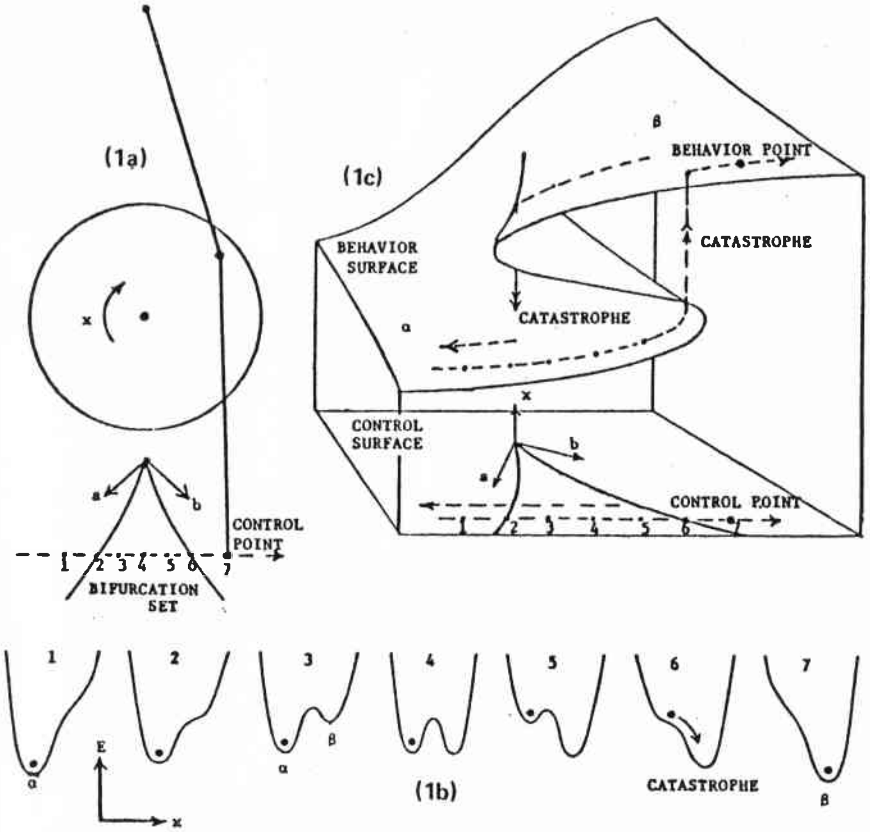

Figure 1. A model illustrating the cusp catastrophe (Zeeman 1976).

The behavioral modes of the cusp are usually qualitatively different, as in the phase transition example. Indeed, this example is given by Hegel, and cited by Engels, as an archetypal illustration of dialectics. Within the dialectical framework, change of a quantitative nature, involving "mere" increase or decrease, which does not alter the basic character of the system cannot go on indefinitely, but at a certain point (Hegel's "nodal line"), always leads to a qualitative transformation (or "leap"). Water, when heated, does not go on getting hotter and hotter indefinitely, but at a certain critical temperature, undergoes a qualitative change from liquid to gas. The dialectical exposition is essentially identical to the catastrophe-theoretic conception. Hegel's "nodal line" is the bifurcation set, and his "leap" is the catastrophe.

Interpenetration of Opposites. The interpenetration – or unity and struggle – of opposites appears in the cusp in several ways: (1) In the bimodality of the behavior suface, and in the overlapping of its two behavioral modes. (2) In the conflicting control factors. If one factor is exclusively present or dominant it will lead the system into one of the alternate modes. However, the regions of dominance overlap, and if both factors are approximately in balance, we have a struggle of opposites. (3) Inside the bifurcation set, the potential function has two minima, which arise from the interpenetration of the domains of influence of the conflicting factors. One minimum corresponds to the actual state of the system, the other to a potential alternative state. Dialectical "development" occurs via the changing relative strength of the two minima, which is the internal content of the transformation of quantity into quality.

Negation of Negation. The third dialectical law can be given two different interpretations: (1) By concatenating two or more cusps, one can represent the on-goingness of the process beyond the first transformation. (2) Within a single cusp, we have the following sequence: Initially, there is a single energy minimum, and corresponding behavioral mode, and the uncontested dominance of one of the conflicting factors. Upon entry into the bifurcation set, this condition is negated, and replaced with contradiction (bimodality) and strife. Finally, the struggle of opposites culminates in a qualitative jump: unimodality is restored, but in a new state.

Conclusion. The cusp catastrophe can be used to model the three dialectical laws. Without doubt, some conceptions of dialectics require the more complex catastrophe known as the "butterfly," which has three equilibrium minima, and allows the emergence of an independent qualitative synthesis (not merely the victory of one of the opposites). The analysis may be expanded in many directions. The bewildering array of examples which have been cited as instances of dialectical laws, and the many alternative theoretical interpretations of dialectics, could be examined and possibly clarified in terms of Thom's theory.

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E.C. Zeeman, *Catastrophe Theory*, Addison-Wesley, Reading, MA, 1977.

M. Zwick, "Dialectics and Catastrophe," presented at the Fourth International Congress of Cybernetics and Systems, Amsterdam, 1978.

Editor's note: The familiar physical example of qualitative transformation – water to steam or ice, and vice versa – was originated by Hegel (see Engels, *Dialectics of Nature*, New York 1940, p. 30). Some phase transitions of this type are modeled to good accuracy by the Van der Waals equation, represented geometrically by the folded *behavior* surface of figure (1c). Perhaps graphic representation of quantitative data for other types of qualitative leaps would be a useful means of exploring dialectics in nature. (The controversy over catastrophe theory, mentioned in author's footnote, may be sampled in *Science News* 111: 218, 1977, *Science* 196: 287, 1977, *Nature* 269:759, 1977; 270: 381, 1977; 271:401 & 486, 1978.) □

Ideology in Mathematics

Euclid's famous ELEMENTS, which laid the foundations of geometry, rest on premises (axioms or postulates) which are clearly non-formal. Euclid's axioms are based on reality, that is to say, on the practice of surveying, architecture, road-building, shipbuilding, military science, and other similar branches of material culture in antiquity. In other cases it is not so easy to trace the roots of theoretical premises and concepts in mathematics, and it is therefore no accident that the neo-positivists appeal to modern mathematics in their effort to prove that our knowledge has in general nothing in common with objective reality and is purely a mental contrivance. It would be highly important for Marxist philosophers, working in close contact with mathematicians to elaborate this important epistemological and ideological problem.

– *Nikolai N. Semyenov, p. 34, in* The Scientific and Technological Revolution, *Progress, Moscow, 1972.*

A book review by IRVING ADLER

STEPHEN JAY GOULD
Ever Since Darwin: Reflections in Natural History
W. W. Norton & Co., New York 1977

This book is a product of much learning, very good thinking, broad culture, sparkling wit, and fine writing. The value of dialectical materialist insight is apparent as Professor Gould explores the history of evolution, some of its puzzling problems, and the nature of scientific thought. The 33 chapters are packed with information on biological evolution, geological history, human social development, and their interconnections. Along the way, it is studded with apt quotations from Shakespeare, Goethe, Milton, Dickens, Dryden, Pope, Freud, Hardy, W.S. Gilbert, Simone de Beauvoir, Aldoux Huxley, Bruno Bettelheim and John Ciardi.

The theory of evolution is the study of the emergence of new and distinct species in the self-development of the system of living things interacting with their environment. Dealing as it does with a process of development in which each separate part (whether it be organ or organism or species) is enmeshed in complex relationships, both internal and external, it can be studied successfully only by a mind that habitually uses dialectical modes of thought. This is demonstrated implicitly in Professor Gould's masterly discussion of some of the fascinating puzzles of evolutionary theory, such as: What accounts for the Cambrian explosion (the sudden appearance 600 million years ago of a great diversity of biological forms after 2.5 billion years of only bacteria and blue-green algae)? What explains the rapid extinction of half the families of marine organisms 225 million years ago (during the Permian period) and the loss of one quarter of them 70 million years ago (during the Cretaceous period)?

The use of the ecological "cropping principle" in the discussion of the Cambrian explosion shows that predator-prey relationships affect other species besides the predator and prey. The explanation of the Permian extinction with the aid of the modern geological theory of plate tectonics shows the inseparability of biological development and geological change. The complex interplay of sexual reproduction, nutrition and predator-prey relationships are discussed in terms of the 12-foot antlers of the Irish Elk, parthogentically-produced gall midges that eat their mother, cicadas emerging from the ground for sexual reproduction every 17 years, and certain bamboo plants flowering simultaneously every 120 years.

In some chapters, Professor Gould finds it necessary to refute common misconceptions about the nature of scientific

thought. He makes a valuable contribution by formulating some explicit statements about what science is and how it is developed: "Creative thought in science is... not a mechanical collection of facts and induction of theories, but a complex process involving intuition, bias and insight from other fields. Science, at its best, interposes human judgment and ingenuity upon all its proceedings." (p. 125). "Science is no inexorable march to truth mediated by the collection of objective information and the destruction of ancient superstition. Scientists, as ordinary human beings, unconsciously reflect in their theories the social and political constraints of their times. As privileged members of society, more often than not they end up defending existing social arrangements as biologically foreordained." (p. 15). "The success of a scientific hypothesis often involves an element of surprise. Solutions often arise from a subtle reformulation of the question, not from the diligent collection of new information in an old framework" (p. 12).

In a particularly valuable section, Professor Gould discusses some old and new theories of biological determinism: theories of the alleged inferiority of the darker races of mankind; biological theories of criminality; and the theory that war is a product of man's biologically determined aggressive tendencies. In short, swift strokes Professor Gould skillfully refutes these theories and exposes their roots in class bias and imperialist apologetics.

The discussion in both the first and last sections of the book revolves around the central question of the relationship between biological evolution and human development. Professor Gould argues correctly against the twin fallacies that a) because man is unique, he is not part of the animal world, or b) because man is part of the animal world, he is not unique, and is only an animal. His discussion of these fallacies would have been strengthened had he explored more fully the significance of Benjamin Franklin's great insight, foreshadowed in the myth of Prometheus as told by Plato in his *Protagoras*, and further developed by Marx and Engels, that man is a tool-making animal.

Developing more fully a statement by Marx (*Capital* vol. 1, New York, International, p. 372, footnote 3, lines 5-9), Anton Pannecoek in his pamphlet *Marxism and Darwinism* pointed out that tools are detachable, replaceable, modifiable inanimate extensions of man's bodily organs, and they are designed and used through patterns of learned social behavior. Darwin demonstrated that natural selection leads to the modification of a species' means of interacting with its environment, viz., its organs and inherited patterns of behavior. When this principle is applied to man, it becomes transformed into a new principle: the modification of man's inanimate extensions of his organs and of his learned patterns of behavior. This evolution for man is principally technological progress and the accompanying evolution of social institutions. Historical Materialism, Marx's theory of social evolution, is thus seen as the result of the application to man's specific nature of the general principle of Darwinism. Man's unique form of evolution, social evolution, is different from biological evolution, but is at the same time the result of the operation of the same general principle that underlies the biological evolution of all other organisms. In the short space of a review, this point cannot be developed in the detail that it requires and deserves. This reviewer hopes that his brief reference to it may inspire 1) a reprinting of Pannekoek's valuable but long unavailable pemphlet, and 2) a discussion of its principle idea by so eminently qualified a scientist as Stephen Jay Gould.

Ever Since Darwin is meant for the general reader, its essays having appeared previously in the monthly *Natural History*, published by the American Museum of Natural History. The book is also excellent for supplementary reading at high school or college level.

North Bennington, Vermont

Hierarchy in Science and Nature

The reductionist hypothesis may still be a topic for controversy among philosophers, but among the great majority of active scientists I think it is accepted without question. The workings of our minds and bodies, and of all the animate or inanimate matter of which we have any detailed knowledge, are assumed to be controlled by the same set of fundamental laws, which except under certain extreme conditions we feel we know pretty well. It seems inevitable to go on uncritically to [a seemingly] obvious corollary of reductionism: that if everything obeys the same fundamental laws, then the only scientists who are studying anything really fundamental are those who are working on those laws. In practice, that amounts to some astrophysicists, some elementary particle physicists, some logicians and other mathematicians, and few others ...

The main fallacy in this kind of thinking is that the reductionist hypothesis does not by any means imply a "constructionist" one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less to those of society. The constructionist hypothesis breaks down when confronted with the twin difficulties of scale and complexity. The behavior of large and complex aggregates of elementary particles, it turns out, is not to be understood in terms of a simple extrapolation of the properties of a few particles. Instead, at each level of complexity entirely new properties appear, and the understanding of the new behaviors requires research which I think is as fundamental in its nature as any other.

—P.W. Anderson, "More is Different: Broken Symmetry and the Hierarchical Structure of Science." Science 177: 393, 1972.

Poor Symmetry, Violated Again!

OR, MARXISM AGAINST THE MAGNETIC MONOPOLE

Today the dictionary says:

> **magnetic monopole** Hypothetical magnetic particles (analogous to the electrical particles, the electron and proton) with a magnetic charge of either north or south. They have been postulated on conservation and symmetry principles... Neither quantum theory nor classical electrodynamic theory bars the existence of the magnetic monopole. Maxwell's equation [sic!] would prove completely symmetrical if such particles did exist... Despite all [the] reasons for their existence, and intensive searches for them, so far no individual monopoles have been detected. [Gray and Isaacs, *A New Dictionary of Physics*, Longman, London 1975.]

A century ago, Frederick Engels said:

> Dialectics has proved from the results of our experience of nature so far that all polar opposites in general are determined by the mutual action of the two opposite poles on one another, that the separation and opposition of these poles exists only within their unity and inter-connection, and, conversely, that their inter-connection exists only in their separation and their unity only in their opposition. This once established, there can be no question of a final cancelling out of repulsion and attraction, or of a final partition between the one form of motion in one half of matter and the other form in the other half, consequently there can be no question of mutual penetration or of absolute separation of the two poles. It would be equivalent to demanding in the first case that the north and south poles of a magnet should mutually cancel themselves out or, in the second case, that dividing a magnet in the middle between the two poles should produce on one side a north half without a south pole, and on the other side a south half without a north pole. Although, however, the impermissibility of such assumptions follows at once from the dialectical nature of polar opposites, nevertheless, thanks to the prevailing metaphysical mode of thought of natural scientists, the second assumption at least plays a certain part in physical theory. This will be dealt with in its place... Two poles whose activities did not completely compensate each other would indeed not be poles, and also have so far not been discovered in nature. [*Dialectics of Nature*, International, New York 1940, pp. 38-39, 44.]

Was Engels right in this dialectical proof? Would completion of his study have made a contribution to physics? Do recent headlines (e. g., *Science News* 109: 122, 1976 and *Physics Today* p 15, April 1976) about fruitless new searches for monopoles reflect needless waste of research effort and money? □

BIBLIOGRAPHIC NOTES FOR NATURAL SCIENTISTS

GENERAL REFERENCES

Annotation is by the editor except as noted.

*Asterisk designates an item from a socialist country available through Imported Publications Inc., 320 West Ohio Street, Chicago Ill. 60610.

SERIALS AVAILABLE ON SUBSCRIPTION

Science and Society. A quarterly, $8.00 per year ($9 outside USA).

Room 4331 John Jay College, CUNY, 445 W 59th St, New York N Y 10019.

A significant forum for debate between dialectical materialists and new leftists. The occasional articles dealing with philosophy in the natural sciences may be on either side of the fence.

Social Sciences. A quarterly published by USSR Academy of Sciences, Moscow. $7.50 per year*.

Despite the name, essays on philosophy of the natural sciences appear regularly in this scholarly journal that covers a wide spectrum of disciplines.

SOME BOOKS CURRENTLY IN PRINT

Bernal, J.D. 1972 *Science in History.* 4 volumes, paper, MIT Press, Cambridge Mass. $15.00

A historical materialist treatment of science as it developed through the ages in changing social contexts, with the late great physicist saying his thing on the dialectics of the scientific process.

——— 1967 *The Origin of Life.* Cloth, World, Cleveland $12.50.

Surveys the experimantal results and theoretical problems up to 1967. Includes the revolutionary papers of Oparin (1924) and Haldane (1929) that opened up this exciting field. – *Irving Adler.* [Also see remarks by Adler in the Workshop Discussion on Marx, this issue. *Editor*].

Blauberg, I.V., et al. 1977 *Systems Theory: Philosophical and Methodological Problems.* Cloth. Moscow $4.10*.

An excellent book that gives due attention to the contributions of Bertalanffy and discusses the paradoxes of systems thinking in dialectical terms. – *Saul Birnbaum.*

Bohm, David 1957 *Causality and Chance in Modern Physics.* Paper, Harper Torchbooks, New York.

A profound Marxist critique of prevailing mystical interpretations of quantum mechanics, with a rewarding discussion of the dialectics of qualitative change in physical processes. A proposed explanation of quantum phenomena, however, demonstrates anew that perceptive philosophical analysis is no substitute for the *physical* insight still sought in this pivotal problem of modern physics.

Cornforth, Maurice 1972 *Dialectical Materialism.* 3 volumes in paper titled *The Theory of Knowledge, Materialism and the Dialectical Method, Historical Materialism.* International, New York $5.00.

A good general introduction to the concepts of Marxist philosophy that deals some with problems of natural science.

Engels, Frederick 1878 *Anti-Duhring (Herr Eugen Duhring's Revolution in Science).* International, New York 1966; cloth $6.95, paper $2.85. Moscow, cloth $2.50*.

The polemic that became the first more or less connected exposition of Marxist materialist dialectics. Emphasizes interaction of science and philosophy.

——— 1873-86 *Dialectics of Nature.* International, New York, 1940 cloth $7.50, paper $2.85.

The brilliant first effort at systematic discussion of this subject, with editor J.B.S. Haldane pointing out some errors in the posthumous working notes.

Fedoseyev, P.H., et al. 1977 *Philosophy in the USSR: Problems of Dialectical Materialism.* Cloth, Moscow $5.25*.

The problems are non-trivial, emphasizing unresolved questions of Soviet Marxists. The ten sophisticated essays are each relevant to scientific workers.

Gould, Stephen Jay 1977 *Ever Since Darwin.* Cloth, Norton, New York $9.95.

See review by Irving Adler in this issue.

Hodes, Robert 1952 *Aims and Methods of Scientific Research.* Occasional Paper No. 9, AIMS, 20 E 30th St, New York NY 10016, $1.00.

A brief essay by an outstanding neurophysiologist discussing matters such as the dialectical unity of clinical observation and experimental intervention. With memoirs of the author by Dirk J. Struik, Theodor Rosebury, Irving H. Wagman and Jane Hodes.

Hoffman, John 1975 *Marxism and the Theory of Praxis: A Critique of Some New Versions of Old Fallacies.* Paper, International New York $3.25.

A timely and successful polemic against "new" leftists who retreat to the "young Marx" minus Marxism and so forth. Quite useful in today's philosophical environment.

Kahane, Ernest 1961 *The Thought of Claude Bernard.* (Tr. with an introduction by Harry Chovnick MD and Paule M Prebus.) Occasional Paper No. 3 (1966), AIMS 20 E 30th St New York NY 10016, $1.50).

Fascinating insights on the anti-positivist and unconsciously dialectical views of a great 19th century physiologist. Points up the loss to science from his ignorance of Marxist philosophy.

Konstantinov, F.V. et al. 1974 *The Fundamentals of Marxist-Leninist Philosophy.* Cloth, Moscow $4.50*.

Deals with the basic propositions and problems of dialectical materialism, emphasizing interaction of philosophy with scientific process. A comprehensive textbook and good reference source.

From Lenin's *Philosophical Notebooks*, Moscow, 1961, pp. 196-7.

The *totality of all* sides of the phenomenon, of reality and their (reciprocal) *relations*—that is what truth is composed of. The relations (= transitions = contradictions) of notions = the main content of logic, by which these concepts (and their relations, transitions, contradictions) are shown as reflections of the objective world. The dialectics of *things* produces the dialectics of *ideas*, and not vice versa.	Hegel brilliantly *divined* the dialectics of things (phenomena, the world, *nature*) in the dialectics of concepts #
# This aphorism should be expressed more popularly, *without* the word dialectics: approximately as follows: In the alternation, reciprocal dependence of *all* notions, in the *identity of their opposites*, in the *transitions* of one notion into another, in the eternal change, movement of notions, Hegel brilliantly *divined* PRECISELY THIS RELATION OF THINGS, OF NATURE.	indeed divined, not more

Lenin, Vladimir Ilyich 1909 *Materialism and Empirio-Criticism.* International, New York 1970; cloth $7.50, paper $2.95. Moscow cloth $2.25*.

Lenin took off the year 1908, even neglecting the newspaper, to study physics in order to combat Machian idealism in Bolshevik ranks. He produced a model of materialist polemics that served its purpose.

——— 1895-1916 *Philosophical Notebooks.* Volume 30 of *Collected Works.* Cloth, Moscow (1972) $2.25*.

Traces the development of Lenin as dialectician, especially his study of Hegel in 1915-1916 while writing *Imperialism* and other history-making works. Useful insights are to be found in his notes and commentaries.

Marx, Karl 1887 *Capital.* Volume One. Paper 1967 International New York $4.25.

Demonstrates the tremendous methodological power of materialist dialectics used consciously in the process of scientific discovery rather than unconsciously and inconsistently. Peppered with references to natural science.

Polikarov, A. 1973 *Science and Philosophy.* Cloth, Sofia $4.75*.

Brief essays discuss the heuristic approach to problem solving and raise provocative questions concerning the relation of philosophy to some current problems of physics and cosmology.

Selsam, Howard and Harry Martel, eds. *Reader in Marxist Philosophy.* International, New York cloth $7.50, paper $3.50.

Marvelous sampling of cogent excerpts from Marx, Engels and Lenin on materialism versus idealism, the dialectical method, theory of knowledge and scientific method, etc.

ROBERT S. COHEN

MARX ON HIS SCIENTIFIC METHOD

The following references seem to be the only explicit statements by Karl Marx on his own methodology.

Marx, Karl 1844 *The Holy Family:* Section 2.

Marx was still fairly young.

——— 1857 *The Grundrisse:* Section 3 of General Introduction.

A fairly long passage on the method of political economy.

——— 1858 *A Contribution to the Critique of Political Economy:* The Preface.

Partly duplicates the statement in *The Grundrisse.*

——— 1873 *Capital,* Volume 1, Second edition: The Afterword or preface.

Devoted in part to methodology.

——— 1879-80 *Werke,* Bd 19: Notes on Adolph Wagner

This unpublished critique of an obscurantist textbook on political economy includes what is probably the last writing by Marx on method.

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MARX'S MATHEMATICAL MANUSCRIPTS

At the burial of Karl Marx, 17 March 1883, Friedrich Engels noted that Marx had worked in many fields and "in each, even in that of mathematics, he made independent discoveries." Engels later intended to publish some of Marx's mathematical writings, but was unable to do so, and the first publication came only in 1933, fifty years after Marx's death. Marx had abandoned an early attempt to apply mathematics to his economic theory, but in the last decade of his life he became interested in the foundation of differential calculus, which, in the books at his disposal was inadequately explained. In an attempt to arrive at a more satisfactory explanation, Marx applied his dialectical method and arrived at results that justified Engel's graveside remarks.

Marx was not in the mainstream of mathematics and was unaware of contemporary work in the foundations, including the work of Cauchy earlier in the century, so that he had no influence on that development. But his concept of the differential as an operational symbol anticipated some twentieth century developments and he independently arrived at the interpretation of the differential as principal linear part of an increment, an interpretation that is standard in modern textbooks. Further, his use of dialectics in explaining the derivative is a model use of the principle of the negation of the negation.

The following bibliography is intended as an introduction to this area of Marx's thought. It is incomplete; I have included only items with which I am familiar and which refer directly to Marx's writings. Thus, related material, such as the excellent article by Chandler Davis, "Materialist Mathematics" (in *For Dirk Struik* edited by R. S. Cohen, J. J. Stachel, and M. W. Wartofsky, D. Reidel, Boston, 1974), have not been included. In transliterating Russian names and titles the system of the journal *Historia Mathematica* has been used.

THE MANUSCRIPTS THEMSELVES

Marx, Karl 1933 "Matematicheskie rukopisi," *Pod znamenem marksizma,* Nr. 1, 15-73.

Incomplete Russian translation of two articles on the derivative and the differential, plus historical comments and other fragments. This material was newly translated for inclusion in [Marx 1968].

——— 1968 *Matematicheskie rukopisi,* edited by S. A. Yanovskaya, Moscow: Izd. "Nauka".

Contains most of the some 1000 pages of manuscripts in the Marx-Lenin Institute. Part 1 contains all the original mathematical writings of Marx, the most important being the articles on the derivative and the differential and the essay on the history of the differential calculus. His commentary on the "mystical" period of Newton and Leibniz has often been quoted (for example, by A. P. Yushkevich in *Istoriya matematiki s drevneishikh vremen do nachala XIX stoletiya,* Vol. 3, Moscow 1972, p. 256.) Part 2 consists of Marx's commentary on the work of other mathematicians. All this is given in the original languages (mostly German) with Russian translation on facing pages. There is an excellent preface by S. A. Yanovskaya, extensive commentary and description of writings not included, and copious notes—all in Russian. This volume is the basic source for the study of Marx's mathematical writings.

——— 1972 "Sul concetto di funzione derivata," *Critica Marxista—Quaderni,* n. 6, 278-286

Italian translation by Lucio Lombardo Radice of Marx's article on the derivative.

——— 1974 *Mathematische Manuskripte,* edited by Wolfgang Endemann, Kronberg Taunus (Federal Republic of Germany): Scriptor Verlag.

A mulitated photocopy of the German half of Part 1 of [Marx 1968].

——— 1975 *Manoscritti Matematici*, translated and edited by Francesco Matarrese and Augusto Ponzio, Bari: Dedalo Libri.

Italian translation of Part 1 of [Marx 1968]. There are separate introductions by the editors, the one by Ponzio being worthwhile.

WORKS ABOUT THE MANUSCRIPTS

Glivenko, V.I. 1934 "Ponyatie diferentsiala u Marksa i Adamara," *Pod znamenem marksizma,* Nr. 5, 79-85.

Points out that Marx anticipated Jacques Hadamard in stressing the role of the differential as operational symbol.

Gokieli, I.P. 1947 *Matematicheskie rukopisi Karla Marksa i voprosy obosnovaniya matematiki,* Tbilisi [Tiflis]: Izd. AN Gruzinskoi SSR.

An excellent early study of Marx's mathematical manuscripts, but overestimates the influence that the publication of all of Marx's mathematical writings would have on Marxist mathematicians.

Kennedy, Hubert 1976 Review of [Marx 1974] and [Marx 1975], *Historia Mathematica* 3, 490-494.

——— 1977 "Karl Marx and the foundations of differential calculus," *Historia Mathematica* 4, 303-318 (correction in *Historia Mathematica* 5, 92).

Traces the history of the writing and publication of Marx's mathematical manuscripts and analyses Marx's explanation of differentation as an example of the negation of the negation and of the differential as an operational symbol. Similar to [Struik 1948], but takes into account the material of [Marx 1968].

Kolman, E. 1932 "Eine neue Grundlegung der Differentialrechnung durch Karl Marx," *Verhandlungen des Internationalen Mathematiker-Kongresses, Zurich 1932, II. Band, Sektions-Vorträge,* 349-351.

A brief description of Marx's mathematical manuscripts, including some quotations. (Kolman announced the existence of the manuscripts at the International Congress of the History of Science and Technology, London, 1931.)

——— 1933 "Eine neue Grundlegung der Differentialrechnung durch Karl Marx," *Archeion. Archivio di storia della scienza* 15, 379-384.

Essentially the same as [Kolman 1932].

——— 1968 "K. Marks i matematika (O 'Matematicheskikh rukopisyakh' K. Marksa)," *Voprosy istorii estestvoznaniya i tekhniki* 25, 101-112.

Discusses the motivation Marx found in political economy and philosophy for his study of mathematics, and shows how he overcame the difficulties of the "mystical" period in the development of differential calculus. Kolman concludes: "For historians of mathematics and for philosophers concerned with the philosophical problems of mathematics, the statements of Marx – not in the form of quotations, each letter of which one follows, ostensibly considering it inviolable, but in the form of a matchless example of a concrete creative application of dialectical thinking – will serve as a guide."

Katolin, Lev 1973 *"My byli togda derzkimi parnyami ... ",* Moscow: Izd. "Znanie".

This is a popular, but thorough treatment of Marx's enthusiasm for mathematics, in particular for differential calculus, and of the fate of his manuscripts, of their deciphering, and of their study and preparation for publication by Soviet scholars. There is no bibliography. A second edition is in preparation. ("Lev Katolin" is a team pseudonym of Karl Levimin and Anatolii Melamed.)

Lombardo Radice, Lucio 1972 "Dai 'manoscritti matematici' di K. Marx," *Critica Marxista–Quaderni*, n. 6, 273-277.
Introduction to [Marx 1972], stressing Marx's explanation of the derivative as a negation of the negation.

Rybnikov, K.A. 1954 "Matematicheskie rukopisi Marksa," *Bolshaya Sovetskaya Entsiklopediya*, 2nd ed., vol. 26, 496-498.
Excellent brief article on Marx's mathematical manuscripts by a man who wrote his doctoral dissertation on Marx's mathematics. (This article was omitted in the 3rd edition, and hence will not appear in the English translation of the *Great Soviet Encyclopedia* now in progress.)

——— 1955 "Matematicheskie rukopisi Marksa," *Uspekhi Matematicheskikh Nauk* 10(1), 197-199.
Report of a conference held at the Moscow Mathematical Society, 20 May 1954. Notes that Marx's concept of the differential as an operational symbol was later extended by Maurice Fréchet to functional analysis.

Struik, Dirk. J. 1948 "Marx and Mathematics," *Science and Society* 12, 181-196.
The first report in English on Marx's mathematics. Struik was acquainted only with the material of [Marx 1933], but did have access to the original German. A noted Marxist scholar, mathematician, and historian of science, he brought his considerable talents to the task of producing a lively introduction to this aspect of Marx's thought.

——— 1975 Review of [Marx 1968], *Archives internationales d'Histoire des sciences* 25, 343-345.

Yanovskaya, S.A. 1933 "O matematicheskikh rukopisiyakh K. Marksa," *Pod znamenem marksizma*, Nr. 1, 74-115.
A commentary on [Marx 1933], superceded by her preface to [Marx 1968].

——— 1969 "Karl Marx' 'Mathematische Manuskripte'," *Sowjetwissenschaft. Gesellschaftswissenschaftliche Beiträge* (1), 20-35.
German translation of the preface of [Marx 1968].

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BOOKS RECEIVED

Kuznetsov, B.G. 1977 *Philosophy of Optimism.* Cloth. Moscow $5.30*.

Fedosoyev, P.N. et al. *Lenin and Modern Natural Science.* Cloth. Moscow $5.75*. □

Hypotheses Fingo ————————————————————

I think it is worth while to demonstrate the kind of speculations into which Marxism leads the scientist. For an acceptance of this philosophy inevitably induces novel types of action and thought.

– *J.B.S. Haldane,* The Marxist Philosophy and the Sciences, *London 1938, p, 8.*

ON DIALECTICS IN NATURE AND SCIENCE

Did you take a second look at our front cover or speculate on the meaning behind its design? The emblem was conceived to represent the *dialectical interpenetration* of science and nature, hinting at the manifold interconnections between the ideal form of scientific knowledge and the material nature it reflects. If you never thought about this before, consider, for example, how scientific instruments and experimental procedures are integral aspects of the knowledge gained through them, yet they also are integral components of the material nature reflected in that knowledge.

To me the most fundamental aspect of this interconnection was clearly stated by Frederick Engels: "the dialectical laws are really laws of development of nature, and therefore are valid also for theoretical natural science" (*Dialectics of Nature,* New York 1940, p 27). He showed that the laws of dialectics operate in the physical processes of nature as well as in the mental processes of scientists and the social processes of which science is part.

Of course, not everyone agrees with Engels on this. Robert S. Cohen, for example, expressed sharp disagreement in our workshop discussion on Marx (this issue), saying that Engels dealt only with historical processes or transitions while "self-changing" dialectical processes are something different and *not* found in nature. He poses the essential question thus:

> In Hegelian usage, at least, [the term dialectic] would seem to be talking about internally-generated changes, changes which come from internal forces and internal relations. Internal to what? Well, whatever the entity is that you're looking at , society or whatever.

Bob Cohen has thus presented us with a well-defined and challenging position that invites debate. May our readers strive for equal clarity in stating their own positions on the matter. I will open the debate with two questions concerning natural entities within which dialectical processes might occur: 1) What external forces bring abour the internal polarization in the mitotic process of a dividing cell? 2) What external force produces the emission during radioactive "decay" of a natural atom, say, of radium?

David Bohm, from a searching study of the entity problem in his *Causality and Chance in Modern Physics,* concludes that

> ... every entity, however fundmental it may seem, is dependent for its existence on the maintenance of appropriate conditions in its infinite background and substructure [which], however, must themselves evidently be affected by their mutual interconnections with the entities under consideration. Indeed, this interconnection can,

under appropriate conditions, grow so strong that it brings about qualitative changes in the modes of being of every kind of entity known thus far. This type of interconnection we shall denote by the name of *reciprocal relationship*, to distinguish it from mere interaction (p. 144).

Bohm thus sees dialectical change within an entity as dependent both on internal relations of the substructure and external relations with the background. In effect, he sees all qualitative change resulting from contradictions within a system at some level of organization on some time scale. His discussion of the conditions for qualitative change will be rewarding to anyone concerned with dialectics in nature.

Bob Cohen's position raised a question in my mind that Bohm has helped to clarify. I now ask whether Marxists ever see dialectical processes occurring entirely independent of that "infinite background" external to the entity involved? Whether in nature, in thought, or in society, are not all inner processes of dialectical change somewhat dependent on external influences? In the concrete circumstances under which a new entity emerges from the old, are there not external as well as internal connections involved? For example, could the self-motion of mitosis take place without a proper physiological environment? And why should it be surprising to find the half-life rate of inner-generated radioactive beta emissions influenced by external conditions?*

Recalling that Bob Cohen conceded, in the workshop, that there could be a dispute over whether the history of stars and galaxies represented *dialectical* transformations, I respond with the following serendipitous quotation:

In the constellation Orion is a fuzzy-looking star. A telescope reveals that it is a cloud of cosmic gas and dust, and that within it stars are forming, living and dying. Some stars quietly fade away, but others explode with the light of a billion suns–becoming pulsars or possibly mysterious black holes in space. Such colossal explosions enrich the surrounding gas with elements made in the nuclear fires of those first stars–and from this gas form new stars and planets and possibly life. *(Calendar, May 1978,* American Museum-Hayden Planetarium.)

The above passage reads to me like the spontaneous dialectics of an able astronomer describing the objective dialectics of a negation of a negation occurring in a process of nature.

Hank

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* J.L. Anderson and G.W. Spangler, *Jour Phys Chem* 76:3603, 1972 and 77:3114, 1973 [Thanks to Donna Duncan and Raymond Peat for calling my attention to this report of anomolous results].

WHERE WE COME FROM

The philosophical concepts of dialectical materialism have their origins in science. These origins go back at least as far as the early Greek philosopher-scientists such as Democritus. In the nineteenth century, these concepts were shaped by Marx and Engels under the direct influence of classical science which flowered with the industrial revolution. The further development of these concepts proceeds today under the revolutionary impact of modern science as well as the social upheavals of our era. Clearly, the natural sciences have influenced the development of dialectical materialism much more than vice versa. *Dialectics Workshop* was conceived as a means of bringing the interaction of Marxist philosophy and natural science into a better balance...

There are many objective reasons why the time is now ripe for dialectical materialism to assume its proper role of guidance in the process of scientific research. First is the noteworthy fact that positivism has lost its vaunted position of respectability. Despite all the claims made for it, positivism has proved incapable of explaining how new knowledge emerges. The bloom is also gone from its reductionist linguistic version that seeks to convert philosophy into an analysis of the language of science. A second favorable circumstance today is the widespread interest of the academic community in things Marxist, an interest which has mainly political origins (especially the Vietnam experience).

But enthusiasm for Marxism has not yet penetrated deeply into the departments of biology, physics and mathematics. Why is this? Why is Marxism so much more widely incorporated into the social sciences than into the natural? Asking the question helps to find part of the answer. Neither Marx nor Engels nor Lenin was a practitioner of a natural science. They were superb practitioners of the social sciences–including even military science. Each also studied the natural sciences, and each came up with some brilliant insights on relationships between natural science and philosophy, but none ever worked in a laboratory or prepared an experimental report of a new discovery. They did not leave us with practical examples for the useful application of dialectical materialism in the natural sciences.

What we need then is to provide working examples that will inspire and point the way for a new generation of Marxist practitioners of natural science. This is a primary goal of *Dialectics Workshop.*

–– From remarks by the editor at Dialectics Workshop Conference 12 Feb. 1978.

Addendum: For a natural scientist the usual path to the Marxist mode of thought is through practical experience in political activity. Indeed, since metaphysical thinking is so deeply ingrained in scientific training, the further transition of the scientist to becoming a Marxist in her/his professional work may be very slow and even not get very far. Hence the need for *Dialectics Workshop* to concentrate on developing the application of Marxist philosophy to this particular area. What's more, our effort may help some scientists traverse the path in the opposite direction–since Marxist insight into professional problems can lead to new understanding of political realities. ☐